A PHILOSOPHICAL APPROACH TO THE FOUNDATIONS OF HUMAN GEOGRAPHY

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Thesis submitted for the degree of Doctor of Philosophy

at The University of Adelaide

June 1984

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SUMMARY

This dissertation takes as its primary objective the task of establishing theoretical grounds on which human geography might securely base itself as an integrated discipline. It approaches the state of contemporary fragmentation of the discipline, and its separation from its traditional roots, through an historical perspective on twentieth-century human geography. Three periods of significance are identified: traditional, positivist, and post-positivist, the latter including behavioural, humanist, and prescriptive schools. Central to these changing trends, it is concluded, is the idea of science and the idea of human functioning. The aspirations associated with both these ideas--to frame the discipline as a science, and to base it on a comprehensive view of human functioning--are held to be valid; and a programme of conceptual analysis directed at establishing the conceptual foundations of the discipline in the integration of these two notions, and in the integration of contemporary and traditional human geography, is proposed.

The analysis first examines the nature of scientific inquiry. It begins by noting that human geographers have not dealt adequately with the idea of science, even though it has been an assumed notion at the heart of their theory and practice. A systematic approach to the idea of a science is, it is concluded, prerequisite to the conceptual stability of the discipline. The hypothetico-deductive model of scientific inquiry, associated in particular with Popper and Hempel, is therefore outlined. The powerful challenge to it launched by Kuhn, in which notions of logic in science are asserted to be

subsidiary to the context of community in which science is practiced, is examined. Responses to that challenge by Popper and Feyerabend have not, it is concluded, satisfactorily met its essential critique. The idea of a scientific research programme, on the other hand, as developed by Lakatos, does seem to account for the historical processes of science identified by Kuhn, within the traditional structure of logic. It is therefore provisionally adopted as the most coherent and best supported model of science for the purposes of this inquiry.

Since, however, human geography deals with human phenomena, the question of the applicability of this model to the social sciences inevitably arises. Again it is noted that the conceptual matters associated with the idea of a social science have not been canvassed either widely, or in depth, by human geographers: a systematic approach is thus held to be essential. The positivistic approach to social science, predicated on the principle of a unity of method between the physical and social sciences, is outlined. Critiques of this position are then examined. The most influential of these is that developed by Winch within the context of a philosophical analysis of the intelligibility of social phenomena. Positivistic responses to this critique are found to be unconvincing. An associated view, in the context of historical studies, is that of Collingwood: here, however, the response of theorists such as Carr is more effective. It is concluded that epistemological and methodological difficulties in the Winch-Collingwood position are substantial; and that Carr's response raises the possibility of a genuine unity of method based on the Lakatosian model.

In applying this model to human geography, the idea of scientific theory is examined, and found to rest on the notion of primitive concepts. An

analysis of methodological holism and methodological individualism suggests a reciprocal relationship between the geographic collectivity and the geographic individual. The approach to human geography through the individual is, however, asserted on logical grounds to be the more fundamental, and is therefore adopted.

Having established, in a provisional way, a legitimate model of science and social science, the discipline of human geography itself is approached. An integrated model of the geographic individual, which attempts to preserve the holistic structure of individual functioning is proposed. Its major elements reflect a hierarchy of functioning, from overt behaviour and decision-making, to cognition and cognitive structures, to identity and consciousness. Each level of functioning is found to be associated with particular schools of, or approaches to, the discipline. The treatment of these different levels within the discipline is outlined with reference to the leading figures and main works of the schools.

In assessing this model in terms of the criteria for scientific theory already established, significant areas of conceptual weakness are identified. Interestingly, however, from the perspective of each of the different criteria it is one level of the model, that of identity and consciousness, which emerges as most in need of development. It is concluded, therefore, that although the conceptual difficulties at this level are substantial, a resolution of them would do much to establish the coherence and power of the integrated model.

The ability of the proposed integrated model to account for some central insights of traditional human geography is then explored. Two historically significant conceptual dichotomies--between the relational and holistic views, and between determinism and possibilism--are discussed, and both are found to

be susceptible of accomodation. The Vidalian tradition is examined in some detail, and its fundamental principles located. The ongoing analysis of the relationship between man and environment within the traditional theme of human ecology is discussed. In summary, two themes are identified as characteristic of traditional views: the physical and biological foundation of the man-environment interaction; and the idea of geographic holism. Taking the reductive approach to these insights at the collective level to be legitimate, the first is found to constitute a genuine modification of the integrated model of the geographic individual. The modified model is then capable, it is proposed, following a cybernetic mechanism, of generating a reasonable account of geographic holism. Again, however, weaknesses of the model at the level of identity and consciousness emerge, and are seen to require significant development.

Approaches to the idea of consciousness in cognate disciplines are then outlined, with a view to strengthening the proposed integrated model. On the one hand, evolutionary biology seems to concur with the location of consciousness at the basis of human functioning, and emphasizes its self-reflexive nature. On the other, substantive contemporary psychological research seems to have been directed towards the establishment of a coherent account of human consciousness: it is found to be still largely exploratory, but to hold promise of advance. It is therefore concluded that the strengthening of the proposed model in the areas identified as central may reasonably be predicted.

In conclusion, while recognizing the incomplete nature of the proposed model, an attempt is made to sketch the form of the discipline of human geography to which it might give rise. The continuation and enhancement of

existing schools within an integrated theoretical framework, and the integration of traditional and contemporary approaches, is foreshadowed. The structuring of the discipline along scientific lines is seen potentially to confer major benefits, including cumulative development of theory; cooperative exchange between schools; greater relevance for the gathering of empirical fact; and a more powerful thrust of theoretical development. The content of the theory of the discipline is seen to assert the priority of holism in the account of the geographical functioning of individual and of group; and thus to enhance the comprehensiveness and applicability of the intervention strategies developed on it. The possibility of greater, and more valuable, communication with cognate disciplines is raised, and the participation of human geography in the cross-disciplinary task of elucidating the science of human life is urged. In conclusion, the first stages of a scientific research programme developed from this theoretical position are tentatively outlined, and prospects for the future conduct of the discipline explored.

DECLARATION

Except where otherwise acknowledged in the text, this thesis represents the original research of the author.

Geoffrey Alexander Wells

ACKNOWLEDGEMENTS

The author wishes to record his indebtedness to the following people and institutions who helped him in various ways. The dissertation was supervised by Professor Fay Gale, Department of Geography, The University of Adelaide, and by Professor Murray McCaskill, Department of Geography, Flinders University of South Australia. The author is deeply and principally grateful to them for their advice, critical insight, and support. Fellow post-graduate students and faculty of the Department of Geography, The University of Adelaide, provided the stimulus for many ideas worked out in this thesis in a number of illuminating discussions and seminars. Maharishi International University, Fairfield, Iowa, kindly provided both time and facilities to bring the writing and production of the thesis to a conclusion. Finally, the author's wife and children supported him through an unusually long period of gestation and bore with the inevitable vagaries of that process. To all of the above, grateful thanks are due.

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CHAPTER I

INTRODUCTION

1.1 Approaching human geography

There is a natural tendency, for one engaged in academic work, sooner or later to stand back from immediate intellectual concerns and attempt to view the discipline as a whole. Not everyone feels the need for such a perspective, or actually makes the attempt, but it is a sufficiently common practice to be regarded as reasonable and natural. Its motivating concerns are various. One may want to gain an understanding of the kind of an enterprise one is engaged in, to identify the general characteristics that lie within the particulars of everyday intellectual activity, to describe the enterprise in more abstract terms that can be compared with similar descriptions of other disciplines. One may want to survey the area of its domain, to locate core and periphery, and the varieties of intellectual landscape that lie within its boundaries. One may want to investigate traffic across its borders, its links with other disciplines, the intellectual milieu within which it is located. Or one may want some or all of these perspectives together. Whatever the reason, the intention is the same: to locate the smaller enterprise within the larger; and that larger, perhaps, within some still greater whole. Research activity within localized domains may be valid and useful in its own terms; but it seems

to be essential to the health and vigour of any intellectual endeavour that it proceed in the light of the objectives and characteristic methods of the discipline as a whole.

A comprehensive overview of this kind requires both cross-sectional and longitudinal perspectives. Partly this is a practical consideration: it makes sense to avoid duplicating the work of others, whether it is contemporary with, or antecedent to, one's own. But more important is the development of a sense of intellectual identity, of the cooperative and cumulative nature of the enterprise, the shared angle of vision and commitment one has in common with one's fellow workers, both past and present. It is this confidence, born of a secure intellectual identity, that allows energy and attention to be directed outwards, towards real world problems, rather than continually inwards, with an eye always to areas of possible weakness in the foundations on which one builds.

It is notoriously difficult, however, to view a contemporary enterprise, in which one is oneself engaged, dispassionately. To begin with, it is not always easy to disentangle one's own beliefs, feelings and predispositions from the observations made and conclusions derived. Even more problematic, however, is the attempt to develop a considered, historically valid assessment of events and trends that are close to one in time. Whatever the degree of one's historical intuition and experience, it is extremely difficult to make reliable judgements about the historical durability of contemporary changes and developments in a discipline. It is just not possible to know to what degree they will, in hindsight, be considered fads or fashions, rather than genuine changes of direction or advances in the discipline. For that kind of judgement is needed simply a lapse of time, a settling out, the emergence of enduring

patterns. The irony is that always the present is the time to which such judgements would be most useful; but there is no help for it, and we are left to do what we can with such facts and such judgement as we have at our disposal.

These are considerations which inevitably present themselves to the human geographer in search of an holistic perspective on his discipline. Human geography is peculiarly vulnerable to these difficulties. The last two decades have seen a degree of intellectual turmoil in the discipline that is unprecedented in the course of its long and honourable history. It is difficult to avoid the observation that an advanced degree of fragmentation characterizes contemporary human geography. Some have seen nothing new in this: James (1972) notes that diversity has been characteristic of geography--in ancient Greece, for instance, or in the thought of Humboldt and Ritter--and has grown consistently over the last century. He goes on to remark:

> This state of affairs has bothered some scholars and from time to time there have been efforts to provide narrow definitions of the field that exclude considerable numbers of active workers, past and present, from membership in the field. These efforts have not been successful. (James 1972, p.530)

Some geographers, notably Sauer (1956a), have, indeed, required such diversity as necessary to the vigour and identity of the discipline. In the same spirit, Berry (1978b, p.19) remarks upon

> the mosaic quality of modern geography which results from the many points of origin in the field;

an image which follows Mikesell's (1969) characterization of the discipline as a "mosaic within a mosaic."

It may be that these assessments of the current trends will in time be seen to be historically valid. In the present intellectual climate, however, they are not descriptions of the discipline that naturally present themselves. A more accurate account, we may feel, is given by Ley (1981, p.209), who refers rather to a

> . . . condensation of intellectual history, which in other disciplines was spread over a period of two or three times as long, (that) has created a turbulence which, while exciting, can also be confusing, as the intellectual half-life of not only theories but also whole paradigms shrinks rapidly.

Compounding this fragmentation of the discipline into rapidly changing and often widely separated intellectual frameworks is the degree of emotional attachment with which proponents of the different schools have defended their own positions and attacked others: intensity of debate, high feeling, refusal to compromise, the assertion of mutual exclusivity, and the pursuit of dichotomy have become, if not characteristic of the discipline, at least not unknown to it. A standard specimen of such critical exchange is the debate--unworthy, one may feel, of two such eminent geographers--between Harvey (1974b) and Berry (1974).

In such a turbulent intellectual environment, it is not easy to gain a balanced perspective of the contemporary state of the discipline as a whole. Yet it is clear that the task is an urgent one. Most modern societies have come to accept the view that knowledge for its own sake is not an acceptable justification for the continuance in the universities and colleges of a contemporary discipline. Increasingly, the demand has been for relevance, and for commitment, to the solution of real world problems. For all its intellectual diversity and searching, there is a growing feeling that human geographers have not been able satisfactorily to meet this demand. In parts of

the academic world, contraction and retrenchment of the discipline have accelerated. The contemporary geographer may well seek to know why.

The difficulties of coming to terms with the human geography of the present are compounded by the marked contrast it exhibits to the human geography of the past. Certainly debates between different intellectual positions have been a part, and often a fruitful part, of the discipline in the twentieth century (as they have been of every discipline worthy of the title). The extended debate, of over three decades, between determinist and possibilist perspectives is an obvious example, though perhaps less productive than it might have been. But here the debates occurred within a shared framework of concern, which centred on the relationship between human groups and their environments. The analysis was jointly conducted at the collective level of resolution, and sought macroscopic, holistic understanding. About this fundamental framework there was no disagreement: it defined the domain of the discipline, and lay outside of the reach of criticism. In modern times, on the contrary, it is just these kinds of assumptions that have been questioned; and the resulting turbulence and mutual unintelligibility has been, ~~inconsequence~~ in consequence, altogether of a more significant order. Philosophical underpinnings, long considered to be a priori givens on which the discipline might uninterruptedly proceed, have been exposed and subjected to scrutiny; and in the light of that analysis, none of the fundamental parameters of the discipline--its domain, its concerns, its goals, or its methods--have escaped reconsideration.

An example which makes graphic the contrast between current and traditional human geography is what have come to be known as environmental concerns. On the one side is the conference held at Princeton in 1955, entitled "Man's Role in Changing the Face of the Earth" (Thomas 1956). In

retrospect it seems that this was a meeting of international significance, bringing together scholars of considerable eminence--some of whom, such as Sauer, could trace their intellectual histories back to the roots of twentieth-century human geography--and perhaps marking the beginning of international recognition of the global nature of the environment, its significance for the human species, and the urgency of halting, even of reversing, its deterioration. Looking to the underpinning of this intellectual monument, one finds a stable, widely accepted view of geography, whose theory was well articulated, and apparently deeply rooted in the reality of the natural and human world of its time. It has all the marks of a discipline in full flower, secure in its identity, confident of the essential validity of its theory, and certain of its capacity to make major contributions to the solution of critical global problems.

On the other side is the inability of the discipline to respond to the challenge of the early 1970s, when academic and community identification of environmental concerns, of an ecological crisis, seemed most to demand it. To those outside the discipline, it must have seemed almost incomprehensible that the one discipline which for decades had taken as its focus the relationship of human societies and their environments should find itself bankrupt of insight at just that time when, because of the receptivity of the academic and general community, it might have had most to contribute. Instead, the biological sciences took up the challenge, and new departments of environmental studies were created to fill the gap; and geographers, fully absorbed in the attempt to resolve the growing internal contradictions of their discipline, effectively abandoned the field. On environmental matters, with the exceptions of a few brave forays, such as Haggett's (1972), geographers, powerless in the disarray of their discipline, remained silent.

It is tempting to try to evaluate the vagaries of twentieth-century human geography in terms of Kuhnian paradigms, traditions of theory and research engendered by particular examples of practice in the discipline held to be most successful and most valuable (Kuhn 1970a; and see extended discussion in section 2.3 below). Some geographers have explicitly attempted such a commentary. From one perspective, for instance, the history of twentieth-century human geography is able to be subsumed under the operation of three dominant paradigms: environmental determinism, diffusion, and spatial interdependence (Berry 1978a,b). Other authors, however, identify different paradigms (Johnston 1979; Mikesell 1978). The fact seems to be that, valuable as Kuhn's thesis is to the understanding of science, it cannot be applied to other fields of human endeavour without considerable risk: outside the domain of science, for which the theory is specifically developed, it is difficult to identify the operation of its key concepts. The ideas of normal science, of paradigms, of anomalies, of crisis and resolution become open to interpretation, and hence give rise to a very different account of the history of the discipline. In human geography, the idea of an exemplar has value, as witness the enduring influence of the regional monographs of the French school; but the systematic application of Kuhn's theory does not seem to be possible. Kuhn himself expresses wariness of such applications, popular as they have become, while accepting the general usefulness of the idea of periodization in reviewing the history of ideas (Kuhn 1970a, p.208).

However, the historiographic method espoused by Kuhn, the attempt to locate an underlying pattern in changing intellectual modes, is of clear value, and appropriate to the appreciation of contemporary human geography. Here the appeal is not so much to the details of the flow of ideas as they arise in the

normal course of the development of the discipline. Rather the concern is to locate the conceptual dynamic of which the more superficial fluctuations are an expression, to deal more generally, and therefore more abstractly, with the history of ideas in the discipline. Such a methodology would seem to be particularly appropriate to human geography, since, as has been observed, it is specifically at these fundamental conceptual levels that the significant changes in the discipline have occurred. We may feel encouraged in the view that, if there is an ordered perspective of the discipline to be gained, this kind of approach to the history of its ideas may materially assist in achieving it. To this task, therefore, we now turn.

1.2 Human geography in the twentieth century: an overview

1.2.1 Traditional human geography, 1900-1956

It is a fundamental principle of historical analysis that different commentators will select different facts as significant, and will place different interpretations upon them, according to their personal predilections and locations in particular societies and historical periods. There can be, therefore, no one correct view of human geography in the twentieth century. The past changes shape according to the perspective of the present. Certainly assessments of the history of the discipline in the first half of the twentieth century were quite different in 1950 than they are in 1980: the intervening upheavals now throw altogether different characteristics, such as stability and continuity, into sharp relief.

From our perspective, given our stated intention to seek an understanding of contemporary human geography, three divisions of the twentieth-century history of the discipline into periods suggest themselves. The first embraces what may be termed traditional human geography, beginning with the onset of the century itself, and ending perhaps with the 1956 conference on "Man's Role in Changing the Face of the Earth," as representing an apogee, a culmination of more than fifty years of almost continuous development, which soon after declined and faded from view in the face of the new positivist challenge. This date is not to be seen as a distinct boundary, however: intimations of things to come were in evidence well before it (Schaefer 1953), and its influence continued--indeed, still continues--to be felt after it. But it does seem that at about this time the traditional view ceased to be the dominant mode of thinking in the discipline, and hence it is not an unreasonable date to fix upon.

In the light of the experience of the last two decades, the stability, continuity, and certainty displayed by the discipline in this period is remarkable. From its beginnings to its end, in all four of its principal national foci--the U.S.A., Great Britain, France, and Germany--one can trace long and stable traditions of human geographic thought and practice which exhibit not only continuous and cumulative development within each tradition, but a remarkable degree of comparability, stage for stage, between them. In the nineteenth century roots established by Humboldt (1849), Ritter (1852-59; 1862) and von Richtofen (1883) were codified and elaborated by Ratzel (1896; 1912), and gave rise to a strong tradition, of which Hettner (1905; 1927) and Schlüter (1920), in the first subsequent generation, and Troll (1949) and Bobek (1949; 1962) in later generations, are representative. In France, the work of Vidal de la Blache (1911; 1913; 1926; 1928) is the undisputed origin of the national school, and gave rise to an immensely rich and coherent body of geographic theory and practice which for some decades remained the standard by which all work in the discipline was judged. Among its best-known members are Bruhnes (1920) and Febvre (1925), in its early phases, and Demangeon (1905; 1927; 1940; 1962) and Sorre (1922; 1961; 1962a,b) in the next, and more recently George (1951; 1956; 1966; 1976). In Britain a uniquely national school did not so obviously develop, though much fine work was produced: Mackinder (1902; 1931) and Herbertson (1905; 1915; 1963; 1965) were leading figures of the early days, and were followed by such thinkers as Fleure (1919; 1927-36; 1952) and Roxby (1926), and then by the widely influential Stamp (1950) and Wooldridge (1956). In the U.S.A., Marsh's (1863) contribution went largely unnoticed until much later, and the national school unquestionably began with the formidable Davis (1903; 1906; 1915; 1924; 1932; 1954) and Semple

(1897; 1901; 1903; 1911; 1931), although the approaches of the two to the discipline were very different. Their views were elaborated by such figures as Barrows (1923; 1962), and, in his own unique manner, by Huntington (1907; 1915; 1920; 1924; 1945). The fullest development of a truly human geography, however, awaited the work of Bowman (1916; 1924; 1931; 1934; 1938); and, above all, the towering figure of Sauer (1925; 1932; 1941a,b; 1944; 1952; 1956a,b; 1958; 1962a,b; 1974), whose length and productivity of career, originality of thought and research, literacy and accessibility of written works, and influence on a whole generation of human geographers (Wagner 1960, 1962, 1972; Mikesell 1969, 1978a,b; Ackerman 1963; Zelinsky 1973, 1975; and so on) arguably ranks him at the head of twentieth-century human geographers. One would want also to mention Whittlesey (1925; 1927; 1929; 1936; 1954) as an original and influential theoretician, and James (1929; 1942; 1949; 1952; 1967; 1972). From Australia, although he lived for many years in the U.S.A., there is Taylor (1941; 1951).

The theoretical positions adopted and developed by many of these well-known geographers is examined in some detail in section 5.2 of this dissertation, and will not be rehearsed here. What is important to note is the essential and enduring agreement among these national schools about the fundamental notions of human geography. This uniformity of opinion is all the more remarkable in that it is maintained during a period of history when great and rapidly accelerating changes of thought at all levels--in the sciences, the social sciences, the arts and the humanities--were typical rather than exceptional of academic disciplines. A number of central elements of this common ground may be identified. The first is an undeviating and universal acceptance that the business of the human geographer is to explicate the

relationship between man and the environment.* Traditionally, this is stated in a variety of ways: "the mutual relations between man and his natural environment" (Barrows 1923); "human societies in relation to the earth" (Bowman 1934); "the earth as the world of man" (Hartshorne 1959); "the man-environment system" (Ackerman 1965); and so on (James 1972, pp.508-509); but the intent is the same, and varies only a little in the more than half a century of its elaboration.

Associated with this theme is the notion of area and association, the chorological tradition best expounded by Hettner, and identified by Hartshorne as the characteristic mode of geographic thought. It finds its most impressive form in the notion of the region, first as a natural grouping of phenomena on the earth's surface, as elaborated, for instance, by Herbertson, Roxby, and Stamp in the U.K., by Barrows, Platt, and Bowman in the U.S.A., and above all by Vidal and his followers in France; and later, in the work, for instance, of James and Whittlesey, as an intellectual concept, a tool for approaching spatial patterns on the surface of the earth from particular perspectives,

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* It is necessary to state clearly that the term 'man' is employed here and throughout this dissertation in its historical sense, as a generic characterization of the human species, an interpreted meaning which is in accord with its etymological roots (Skeat 1978). Rather than seek circumlocutions that are neutral with respect to gender, it was felt to be necessary to retain the phrase in the interests of historical veracity. No pejorative connotation is therefore intended by its use.

independently of any inherently "natural" organization (James 1972, pp.460-471). In the earlier of these two approaches to the region, the idea of geographic holism is a widely shared principle of geographic theory, a view which finds an echo, albeit muted, in the systems approaches that flourished for a time in the late 1960s and the early 1970s (Stoddart 1967; Haggett 1972).

Common to all these themes was an understood conception of the nature of geographic methodology, which emphasized the value of field work, of intimate and sustained contact with the actual milieu of investigation (James and Sauer 1915; Platt 1935, 1959; West 1979), combined with the gathering, analysis, and presentation of basic statistical matter about the region in question (Wittlesey 1925, 1927; Wright 1936, 1937). The integration of field observations--physical, biogrographical, and human--and statistical matter, together with historical research, where appropriate, into a coherent form that, through the judicious blending of verbal, numerical, and visual description, strove to recreate the holism of the region, required the precision of the scientist and insight of the artist. Where successfully accomplished, as in the French regional monographs, it is often held to represent the height of traditional geographic achievement. Perhaps because of this achievement, physical and human geography, although increasingly recognized as separate disciplines in their own right, remained firmly integrated at the regional level, until the onset of the notion of the conceptual region towards the end of the period. Certainly it can be said that human geographers for some decades maintained a view of man as inseparable from his environment, even if the contrary is not true to the same degree of physical geography.

It is all too easy, from the vantage point of the 1980s, with the experience of over two decades of conceptual and methodological debate in the discipline, to depreciate the achievement of the traditional geography of this period. There is much of a fundamental nature to criticize; and those criticisms in fact were made, and ~~lead~~ led to the positivist revolution of the 1960s. Thus, Gould, as an unrepentant representative of the positivist movement, decries what he sees as

> . . . bumbling amateurism and antiquarianism that had spent nearly half a century of opportunity in the university piling up a tipheap of unstructured factual accounts. (Gould 1979, p.140)

In recent years, however, there has increasingly grown an awareness of the real and lasting achievement of this period; of the deep insight, and the high degree of analytic and synthetic skills displayed by these geographers. The possibility of incorporating both insights and skills into a reconstructed contemporary human geography has been raised (Hart 1982), particularly within the humanist school (Buttimer 1971, 1978). It is a project with which, as will become clear, the author is in sympathy, and to which the intent is to make here a contribution.

1.2.2 The positivist revolution, 1957-1968

It seems legitimate to use the term "revolution" to describe the change that occurred in the discipline in the late 1950s and early 1960s, not only because there are precedents for it (Davies 1972), but because it came after such a long period of stability and continuity in the foundations of the discipline. Again, the dates defining the period are not absolute. As has been described, the first blows in the positivist campaign were struck well

before 1957, but it was at about this time that the traditional perspectives lost their unchallenged pre-eminence. Similarly, although the positivist movement is often allocated squarely to the 1960s, it is clear that articulated attempts to soften its strictures and move it in more humanly oriented directions, had emerged before the end of the decade (Cox and Golledge 1969), although the influence of positivism has continued powerful in the discipline, and remains so today.

Depsite its revolutionary posture, the positivist movement can trace its roots back to earlier geographers of the traditional period, Christaller, von Thünen, and Jefferson, in particular, and even to Schlüter, Hettner, and Gradmann (Müller-Wille 1978), who began to develop spatial models in the context of urban and central-place studies. A more immediate predecessor was Hägerstrand (1952; 1953-67), whose studies on innovation diffusion had demonstrated the effectiveness of modelling and of statistical techniques in dealing with human spatial phenomena. The effective beginnings of the positivist movement, however, can unambiguously be located in Schaefer (1953), who, in accordance with the logical positivist approach to the physical sciences developed in Vienna during the 1920s, advocated the search for general laws capable of generating a nomological structure of explanation by logical inference (Gregory 1978, pp.32-34; and see section 2.2 below). Although at the time of its publication, Schaefer's article did not produce further published work, later systematic attempts to structure the discipline along positivist lines appealed to it as seminal to the newly emerging tradition. An extended journal article by Garrison (1959-1960) preceded the first substantial textbooks, those of Bunge (1962) and soon after of Haggett (1965). Of their significance, both intellectually and emotionally, to the fledgling positivists, it is sufficient to quote Gould (1979, pp.140-141):

> With the exception of one or two works of scholarship in historical geography, it was practically impossible to find a book in the field that one could put in the hands of a scholar in another discipline without feeling ashamed. Only when Bill Bunge's doctoral dissertation and Peter Haggett's synthesis were published was there something that one could offer without apologies.

Other leading figures were Berry (1964, 1967), Marble (Berry and Marble 1968), Morrill (1970), and Gould (1969; Abler and Gould (1971). Most importantly, for the purposes of this dissertation, was the conceptual underpinning of the positivist movement provided by Harvey (1969a): this attempt to demonstrate the validity and universal applicability of positivism as a foundation for human geography, to ally the discipline with the great traditions of the physical and biological sciences, was an enterprise of the greatest significance in legitimizing the revolution that had by then occurred. So powerful was this combined thrust of theory, technique and methodology that the discipline adopted it quickly and pervasively. An entire generation of human geographers was educated according to its principles.

It has often been remarked that the central core of the positivist revolution was methodological. James (1972, pp. 485-495) points to advances in observational techniques made possible by technological innovations: for example, air photography, radar and infrared imagery, and, more recently, the widespread availability of satellite data and imagery. More significant, however, and more characteristic, was the change in methods of analysis developed in part to gain full advantage of the rapidly increasing bank of data. With its basis in the econometric methods developed by von Neumann and Morgenstern (1944) and others in the 1930s, on the formal foundation of probability theory, it seems to offer a rich and flexible mathematical language to the new generation of human geographers bent on creating their discipline as

a science (King 1969). In human geography, it resulted in the emergence to dominance of minor themes of the discipline that, though little known, had been worked upon for some decades. Such notions as the gravity model, central-place theory, diffusion, and regional science (James 1972, pp. 516-524), long familiar to the geographer but isolated from each other, were brought together and subsumed under a new and powerful theoretical framework: locational analysis (Haggett 1965; Berry 1978a, p. vii). Gould (1969, p.2) identifies, as central to its methodology, developments in area sampling and the gathering of data, and thoroughgoing quantitative analysis, with its elaborate technical apparatus of multivariate analysis, inferential and descriptive statistics, spatial modelling, and mathematical methods. Chisholm (1975, ch. 2) is in agreement with this assessment, as is Ambrose (1969, ch. 1). As a geographic methodology, it stands in marked contrast to the wide range of methods characteristic of traditional geography in the first half of the century in which, as has been noted, empirical investigation was understood to include personal experience in the field, intuition and insight, and generalized description, as well as the gathering of statistical data and the use of map comparisons to establish spatial correlations.

Important as these methodological developments were, however, of greater significance is said to be the change in the conceptual structure: the adoption of logical positivism, and its search for universal laws from which, under the appropriate logical inference, prediction of empirical fact could be generated (Harvey 1969a; James 1972. pp. 471-477; Gregory 1978, pp. 32-34). Szymankski and Agnew (1981, ch. 2), on the other hand, have convincingly demonstrated that although the trappings of positivism were there, the central element--criticism, or scepticism--was not. As is examined in detail below

(section 2.2) the hypothetico-deductive model developed pre-eminently by Popper (1959) takes as its primary assumption the idea that scientific work is essentially critical; indeed, that to the degree it is critical work, it is scientific. Szymanski and Agnew argue that, in the headlong pursuit for order which possessed the positivist movement, the critical thrust that ought to have characterized it became blunted. They identify eight "indicators" of the dogmatic pursuit of such order: the acceptance of nomothetic, rather than idiographic, explanation; the use of positivist language, such as "theory," "hypothesis" and "model"; the decline in the use and acceptance of the regional concept; the effort to link geographic theory to General Systems Theory; the attempt to employ case-studies as confirmations of theory; the adoption of an instrumentalist position, as against realist; the attempt to test theories and hypotheses; and the display of accumulated knowledge, in textbooks, and in the formation of systematics. They argue that most of these positivist aspirations were not fulfilled in practice. "Theories," "models" and "hypotheses" did not meet the criteria established by the natural sciences. Systems theory proved of limited applicability only. Confirmation was widely regarded as adequate on the basis of one or two favourable case studies. Instrumentalism fostered theory which, despite its alleged commitment to the tribunal of fact, lacked interpretation in the real world. Reports of testing suppressed counter-instances, and selectively employed data to support theory. In addition, all the pressures that participation in a scientific community are likely to produce--as analyzed in the context of the physical sciences by Kuhn (1970a, see section 2.3 below)--which threaten to distort every aspect of a scientific enterprise, were experienced by positivist human geographers; and more often than not, in the view of Szymanski and Agnew, acceded to.

For the particular purposes of this dissertation, it is also worth noting the constriction of domain that accompanied the change in methodology. It has been the usual practice of positivist geographers to deny such a constriction: Gould (1969, p.2), for instance, refers to "new ways of tackling old problems"; and Ambrose (1969, p. 1) affirms that

> So far as the subject matter of geographical enquiry is concerned little has changed, although changes of emphasis have taken place . . .

Yet it is certainly arguable that the drive to quantify the subject matter of the discipline resulted in its spatial aspects being thrown into relief, simply because these were the aspects that lent themselves to quantification; and that attempts to build logical foundations for the discipline as a spatial science, as in Bunge (1962), were ex post facto justifications, and not the axiomatic development they too often claimed to be. Certainly the quantitative revolution will continue to be valued for the techniques and tools which it has provided, and which may be essential to the balanced progress of the discipline. It should be noted, moreover, that in many, even most, areas of contemporary human geographic teaching, spatial analysis and quantitative methods still remain the backbone of both theory and practice (Haring and Lounsbury 1983; Cliff and Ord 1981; Wrigley 1979). But the substantial reduction in the applicability of human geographic theory to the realities of human life seems to be incontrovertible, and is of central importance to the understanding of the reactions to undiluted positivism which soon began to emerge.

1.2.3 Reactions to positivism, 1969-1984

Again, as we have noted, the date 1969 is not a fixed boundary, but marks the emergence of the first codified responses to the positivist movement in the discipline. Thus Cox and Golledge's (1969) seminal survey of behavioural approaches appeared at this time; and it is of interest to note that Gould's (1969) summary of the methodological developments that had occurred during the positivist era included a category of behavioural geography, defined at that time largely in terms of spatial behaviour and environmental perception. In retrospect it is clear that the reactions to positivism had deep roots in the discipline, particularly in the cultural and historical traditions, that reached back to its very beginnings. In many ways, therefore the reactions to positivism that have emerged in the last fourteen or fifteen years can be seen as the reassertion of traditional priorities and insights in a modern form. That is not to say that new directions, arising from and expressing the contemporary human condition, have not been developed; they have, and aggressively so. But the continuity of perspective, if not of the actual content of theory, is inescapable to the historically-minded commentator, and is integral to an understanding of this contemporary period. Perception studies, too, such as those of Saarinen (1966), were, as Gould noted, authentic and immediate predecessors of it, as was the hazard research associated particularly with White (1964).

The behavioural school. The fundamental ideas of the behavioural school are analyzed in some detail under sections 4.2.1 and 4.2.2 below, and will therefore not be pre-empted here. They are well summarized by Johnston (1979, pp. 113-125). The notion of rational decision-making, developed initially in

the context of economics by Simon (1957), and based on the principle of optimization, was fundamental to positivist locational analysis: the spatial patterns analyzed were held to be the aggregate outcome of such rational decisions. Wolpert (1964, 1970, 1972) and others modified this principle, primarily in the context of migration studies, to allow for sub-optimal or satisficing behaviour, which did not require the assumptions of perfect information flow nor that of fully rational decision-making processes. The emphasis was on adaptation to a personal and social environment of varying degrees of certainty and desirability. Underpinning the behavioural approach to spatial behaviour were attempts to identify the cognitive structures of which such behaviour was thought to be an expression: thus Golledge (1969, 1979; Golledge and Amedeo 1968; Golledge and Rushton 1976; Golledge and Zammaras 1973; Golledge, Brown and Williamson 1972; Golledge, Rivizzigno and Spector 1976), for instance, and Cox (1972; Cox and Golledge 1969, 1981) have been in the foretront of the attempt to integrate the insight of contemporary cognitive psychology and those of the spatial science developed by positivist human geographers. Hägerstrand's (1952, 1953, 1967) seminal work on time-space geography has also been taken up by the behavioural school (Pred 1967, 1969; Parkes and Thrift 1980). Methodologically the approach of positivist geography remained intact: the attempt was still to employ statistical methods and stochastic models in systematically formulating and testing hypotheses about the nature of human behaviour and the spatial patterns to which it gives rise.

It would seem, therefore, that the behavioural school could equally as well have been placed within the fold of the positivist movement as outside it. Certainly it has been widely identified with positivistic spatial science and therefore seen as subject to the same critique. Gregory (1978, pp. 38-41), for

example, points to the almost complete dependence of positivist theory in human geography on theory developed in other disciplines; to the assumption of methodological universality that positivist theory had perforce to adopt; to the instrumentalist philosophy fundamental to it; and to the restrictiveness of its view of spatial processes, and therefore of its potential application. All these criticisms can be applied to behavioural geography, which borrowed more or less uncritically from cognitive psychology for its theory, made the same assumptions of universality, took the same instrumentalist position, and adopted a superficial, and therefore largely inapplicable, view of the nature of human functioning. Yet it has been linked here with other less ambiguous reactions to the positivist movement because it recognized the fundamental difficulties of human geographic theory which dealt exclusively in aggregates of ideal decisions and behaviours, and the necessity to build the foundations of the discipline in a more comprehensive and realistic account of the human individual. In attempting to build those foundations within the positivist framework, it satisfied neither the positivist nor the anti-positivist; but it represents the beginning of a direction in the discipline which has strengthened and become more uncompromising in the last decade.

The humanist critique of the behavioural school is simply that, although in the right direction, it did not go far enough in rejecting positivist modes of thinking and developing a theory of human functioning more expressive of reality. Thus Ley (1981, pp.211-218) castigates its links with psychological behaviourism; its adherence to 'naturalism' (unity of method in investigating natural and human phenomena); its "shrunken and essentially passive view of man" (Ley 1981, p.213), in which human consciousness is seen as the product of external factors, or as essentially irrelevant to a scientific enterprise; and

the inevitably subjective nature of its selection of data, and their interpretations. These theoretical inadequacies are seen to lead to an inability to develop applications that work in the real world, just because they do not have an effective grounding in the reality of human life. This critique, in turn, has given rise to prescriptions for valid human geographic theory which may now be considered to form a fully-fledged reaction against the positivist movement, and to constitute a school of human geographic thought in its own right.

The humanist school. Again, leading ideas of this school are detailed in sections 4.2.2 and 4.2.3 below, and will not be examined here: our concern is rather to gain a broad view of this development in the discipline. As has been noted, some contemporary humanist geographers have located the roots of their approach to human geography in the traditional school (Buttimer 1971; 1978). Certainly it seems clear that there exists an almost unbroken continuity between historical geographers such as Darby (1936; 1941a,b; 1951; 1952; 1956); cultural geographers of the Berkeley school, such as Wagner (1960; 1972; Wagner and Mikesell 1962), Glacken (1967), Sopher (1978; 1979), and Meinig (1979b); and the earlier work of Sauer (previously cited) and Wright (1966c). An early outlier of the modern school--bearing much the same relationship to it as Scheafer (1953) did to the positivist movement--is Lowenthal's (1961) article, which laid out a surprisingly comprehensive theoretical and methodological framework for the understanding of geographic experience; and his subsequent work, initially with Prince (1964; 1965), and later in areas that he made characteristically his own (Lowenthal 1967; 1968; 1975; 1976; 1979; 1982; Lowenthal and Bowden 1979) has set, in erudition and scholarship, a standard for the field. However, his was very much a lone enterprise for over a decade;

and the effective beginning of the modern humanist school seems to lie with Buttimer's (1974) impassioned appeal for the restoration of the idea of human values to the discipline. Significantly, in an appendix to this monograph, are outlined approaches to phenomenology and existentialism, closely allied philosophical positions which have been elaborated as potential foundations for the discipline by Relph (1970; 1976a,b; 1981a,b), Entrikin (1976; 1980), Samuels (1978; 1979; 1981), as well as by Buttimer (1976a,b; 1978; 1979; Buttimer and Seamon 1980) herself, and by others (Walmsley 1974).

Another seminal figure was Tuan (1974; 1975; 1976a,b; 1978; 1979a,b,c), whose elegant excursions into literature and myth opened many of the now standard directions for the field. Sack (1972; 1980; 1983) was early in recognizing the importance of the subjective dimension of space, as were Mercer and Powell (1972) in identifying the potential of phenomenology for the emerging humanist perspective. Olsson (1974; 1978; 1981) is a late convert, interesting given his orthodox intellectual background (Olsson 1969), and it is easy to be unkind about his somewhat undergraduate rediscovery of Joyce; but he is at least attempting to deal with problems of language and meaning that human geography has in common with all the humanities, and which have no simple answers. More recently, the humanist school has acquired an eloquent and scholarly commentator in Ley (1974; 1978a,b; 1979; 1980; 1981; Duncan and Ley 1982), whose incisive critiques, on a variety of issues, have done much to identify areas of strength and weakness in the theoretical foundations of the school. Guelke (1971; 1974; 1981; 1982a,b), too, as discussed in section 3.1.1 below, has recently intensified a long advocacy of idealism, in the Collingwood mould, as a foundation for pursuing historical studies in geography.

This eclectic perspective on human geography has certain central themes that are commonly accepted as fundamental by its proponents. Ley and Samuels (1978, pp.10-14) identify in particular two such groups of themes, the first ~~centring~~ centreing on epistemology and the second on methodology. The epistemological position of the humanist school, to the extent that a common stance is taken by its proponents, emphasizes the active, rather than the passive, view of the acquisition of human knowledge. The primary organizing reality is subjective, not objective. The world does not have a separate, independently verifiable existence, since it is always perceived through human eyes:

> No object is free of a subject; whether in thought or in action each phenomenon is part of a field of human concern. (Ley and Samuels 1978b, p.11)

Attention is therefore directed to the nature of subjective knowing, to the structure of values, attitudes, and beliefs that organize the nature of the cognition of the external world, and to the ground of identity and consciousness in which that structure is embedded. Moreover, the humanist analysis attempts to retain the holism of human experience, which is seen to result from the interaction of man and the environment:

> . . . the synthesis is not functional but dialectical, not abstract but contextual. . . . As man and environment engage each other dialectically, there is no room in a humanistic perspective for a passive concept of man dutifully acquiescing to an overbearing environment. But neither is man fully free, for he inherits given structural conditions and, indeed, may be unaware of the full extent of his bondage. (Ley and Samuels 1978b, p.12)

Both of these epistemological principles are elaborated below, the first in chapter IV, the second in chapter V.

A second shared position, deriving from this epistemological stance, relates to methodological matters. Here emphasis is placed on understanding,

rather than on explanation. There is an explicit rejection of the unity of methods appropriate for the investigation of natural and human phenomena, and an adoption of a wide range of methodological techniques, including archival research; the interpretation of literature (Tuan 1976a; Pocock 1981a); landscape appreciation; the analysis of visual artefacts, such as architecture; and, most importantly of all, participant observation in the tradition of Weberian verstehen. Empirical data are used where appropriate, but subsumed under these subjective approaches, as supports, rather than leading edges of insight. In this the traditional influence of the Vidalian school is clear. The philosophical controversies associated with these methodological matters are examined in detail in chapter III.

It should be noted, too, that, although it is not yet strongly developed in the school, there are directions for practical intervention inherent in this analysis. A different conception of human functioning in the environment necessarily implies differences in the nature of its application. Thus the ramifications can be, for instance, economic (Duncan 1978), political (Western 1978) or social (Ley 1974; Lemon 1978): because of the fundamental nature of its theoretical revision, indeed, all fields traditionally regarded as part of the geographer's domain can be reconstructed both for theory and for application. It is this consideration which supports the standing of the humanist approach as a school in its own right. Its critique is sufficiently powerful to challenge traditional, positivist, and behavioural theory in every domain they have adopted, and it has the potential, not yet fully realized, to derive practical implications on every substantive human geographic issue.

Prescriptive human geography. Finally, the emergence of prescriptive approaches to human geography during the 1970s and 1980s should be noted

(Stoddart 1981). It has been observed earlier that a dominant theme of the contemporary perspective on academic disciplines has been the demand for demonstrable relevance. Human geographers have responded to this demand in two ways. The first is the development of a Marxist school ~~centreing~~ centering on the journal Antipode, under its mentor Peet (1974; 1977; 1979), and with such luminaries as Hurst (1973) and Slater (1977a,b). Among its most impassioned advocates are former leaders of the positivist movement, who have accepted the Marxist rather than the humanist critique of positivism, and have attemped to align human geography with the radicalized critiques of the political and social sciences of the late 1960s. Harvey (1972; 1973; 1974; 1979; 1982) is the most eminent representative of this trend: his standing within the positivist movement, together with the comprehensiveness and clarity of his major Marxist works (Harvey 1972; 1982) has played no small part in legitimizing this approach with the mainstream of contemporary human geography. Bunge (1973a,b), too, has lent the weight of his authority to the radical critique, though in a more practical and less doctrinal manner. Recently the structural Marxism associated with Althusser and the French school has been incorporated into the Marxist critique, though not, it is felt, with any conspicuous success (Duncan and Ley 1982).

The Marxist critique stands apart from the behavioural and humanistic responses to positivism in human geography in that it is derived entirely from theory outside the discipline, rather than developed in response to demands within it. Moreover, it is by nature not so much a working hypothesis, an angle of perspective from which is to be developed successively more elaborated theory and practice, as a comprehensive account of human existence, both individual and social, that seeks documentation, rather than analysis and

criticism. It is therefore difficult to treat the emergence of this school historically: the conceptual continuity which might link it to the other developments in the discipline is not easy to locate. It can be broadly associated with the rejection of positivism and the demand for relevance that have been identified as characteristic of the period: beyond that, with its highly articulated theory, immense body of associated literature, and uncompromising stance on the possibility of modifying and developing its positions, it stands, one may feel, apart from the main thrust of the contemporary critique of positivism. It represents, in the context of twentieth-century human geography, a graft, rather than a natural outgrowth, which may or may not take: at this stage of its development it is difficult to say which is the more likely.

A second approach to prescriptive human geography is the welfare geography exemplified by Smith (1975; 1977) and Hall (1981). Here the demand for relevance in the discipline is not allied to any particular ideological framework: the tradition it draws on is a liberal one, which simply asserts the necessity of directing the discipline to the solution of the urgent real world problems, such as inequitable resource distribution, poverty, hunger, epidemic, and so on. It stems from what is seen as the persistent reluctance of human geographers to deal with issues such as these rather than pursue the internal development of their own theory:

> . . . great potential . . . exists for basic descriptive research on geographical variations in the quality of human existence. To "quantify quality" seems an appropriate challenge to place before the geographer with skills in numerical methods. It is a necessary preliminary to improving the quality of life, and to reducing the spatial inequalities which are increasingly becoming matters of social concern. (Smith 1975, p.351)

The emphasis, in other words, is on a redirection of the methods developed in the positivist period to matters of general human concern, particularly those that bear on the survival of people. The Marxist would want to see these concerns in the wider context of ideology and society. The humanist would regard the central concept of "human well-being" as theoretically primitive, and in need of substantial redefinition and expansion. Neither would take issue, however, with the intent of Smith and others to use geographic insights to ameliorate the human condition: the debate is how best to go to the roots of that condition, and effect fundamental and enduring changes in it.

We may note finally two unrelated trends that are of significance to an overview of the contemporary state of the discipline. The first is the present marked degree of severance of physical from human geography. There had been, during the traditional period, a slow trend in this direction, particularly as physical geographers felt the need to investigate their domain more on its own terms, rather than as a backdrop for human activity. The 1956 conference "Man's Role in Changing the Face of the Earth," however, saw the two spheres still effectively integrated. It was the positivist movement, with its attention to spatial patterns, and its location of the roots of those patterns in human behaviour, rather than in the physical environment, which effected the separation. Human geography became, in a single stroke, a social science, and began to pursue theoretical and methodological directions characteristic of the social sciences and the humanities. Perhaps, from this perspective, separation laid the ground for the emergence of a Marxist school, which could only be seen as relevant once the discipline had eschewed its physical tradition.

Secondly, it is worth remarking upon the rise in self-reflexive analysis of the discipline. It is a tradition which has been a particular characteristic of human geography in the twentieth century--indeed, throughout

its history--but in recent times the proportion of the discipline's output devoted to it has risen sharply. It has included historical assessments (James 1972; Berry 1978a; Johnston 1979); collections of newly emerging work and assessments of its implications (Ley and Samuels 1978a; Mandal and Sinha 1980; Brown 1980; Harvey and Holly 1981); strongly felt individual statements (Zelinsky 1975; George 1976; Gould 1979); and reassessments (Cox and Golledge 1981). The idea of a 'philosophy' of geography has been raised to the forefront of attention (Gale and Olsson 1979). The Annals of the Association of American Geographers, for example, a central and traditional forum for the discipline, has recently seen a steady stream of such articles, amongst them Hart (1982), Duncan and Ley (1982), Gale and Golledge (1982), van der Laan and Piersman (1982), Knight (1982), Sack (1983), and Couclesis and Golledge (1983). It is difficult to avoid the conclusion that the fundamental conceptual debates that have characterized the discipline over the last decade are as fervent and unresolved as ever. As Ley (1981, p.210) remarks:

> In true North American fashion, obsolescence is setting in more and more speedily; North American geography, too, has its annual ritual of spring cleaning and its Easter parade of new models each April, when last year's motifs are cleared from the intellectual wardrobe.

One concludes that the resolution of this conceptual confusion is a central, perhaps the central, task facing the discipline today. Diversity of theory and practice in an academic discipline can be a sign of intellectual health and vigour; but there comes a point beyond which, if fundamental conceptual disagreements cannot be resolved, fragmentation and eventual disintegration becomes inevitable. It may be part of the natural order of things that such a process should continue and reach its predictable end; but human geographers can be forgiven for hoping not. Certainly there is little sign of acquiescing

in such a decline; but, equally certainly, no resolution of the fundamental order required seems to be in sight.

1.2.4 Summary

It will be valuable now to attempt a summary of the historical roots of the contemporary state of the discipline as revealed by this brief overview. It is not possible to provide a definitive analysis of these changes, which have developed over eight decades in which the entire structure of the global society and its mores have radically changed. A comprehensive explanation would seek to embed these changes in the wider dynamics of social and cultural change; a task which awaits some future historian with the advantage of the perspective of time.

We may, however, begin by noting that if there is a conceptual continuity to be identified in twentieth-century human geography it seems to lie in an enduring theme: the relationship between man and environment. Certainly Hartshorne (1939; 1960) has argued for the centrality of the chorological, rather than the relational theme; but it seems, nevertheless, that the notion of areal association can be readily subsumed under those of man and environment. Viewed from the side of man, rather than from some framework external to both categories, the notion of 'environed man' has continued to underlie much of the research even of the last two decades, through variation in the definition of 'environment.' In spatial analysis, environment became, firstly, space; and later, behavioural space. In the humanist perspective, it became environment as subjectively experienced and lived. Even Gould (1963, p.290), writing in the full flower of the quantitative revolution, could assert:

> Without cataloguing the many and various definitions of human geography by professional geographers over the last few decades, it is safe to say that most have included the words Man and Environment. Traditionally, geographers have had a deep intellectual curiosity and concern for the face of the earth and the way it provides, in a larger sense, a home for mankind.

We may, therefore, provisionally accept that the idea of man and environment delimits in a general way the domain of twentieth-century human geography, and direct the historical analysis to the way in which this theme has been treated.

The broad historical overview sketched here has attempted not to present a comprehensive review of twentieth-century human geography, but to identify pivotal points in the progression of its ideas on which the discipline as a whole has turned to new directions. Two such historical fulcra, identified by the conceptual and methodological discontinuities that frame them, have been specified. The first is the move of the discipline to structure itself as a science, in the pattern of the physical sciences. This move is based on a deliberate adoption of logical positivism for its philosophical underpinnings, and characterized by the embracing of a conceptual apparatus centred on nomological explanation and on mathematical and statistical techniques of analysis. It clearly separates the traditional human geography of the first half of the century from that of the last three decades, since, even though it has been challenged, it remains the dominant source of theory formation, conceptual analysis, research methodology and technique in the discipline. It is still the pervasive foundation of geographical education at secondary and tertiary level: every year the steady stream of textbooks elaborating its central principles strengthens. It has been subject to substantial modification and development since its first statements in Bunge and Haggett, particularly in the area of its behavioural underpinnings; but its pre-eminence is, one may feel, an established fact.

The second fulcrum, subsidiary to the first, is the reaction against positivism: the attempt, in the behavioural school, to bring the abstract, aggregated theory of spatial science into closer accord with human realities; and the outright rejection, in the humanist school, of positivistic assumptions and priorities as altogether incapable of comprehending the essential nature of human functioning and the richness of human existence. Associated with this reaction is the demand for relevance, for the drawing out from theory of prescriptive implications, for commitment to ameliorate the human condition, as represented in the liberal school of welfare geography and in the radical Marxist school. Of these responses to positivism, the humanist school seems to represent the most comprehensive critique, even though eclectic and loosely formalized, within the domain identified as characteristic of human geography. Its central commitment to the humanizing of geographic theory and understanding has proved to be enduring, and has given rise to progressively more incisive critiques, and more codified theory; though its interpretations and implications for human geographic practice are still obscure.

We note, in conclusion, that both of these conceptual foci turn on the idea of science. It is in the name of science that the positivist movement challenges the long-standing traditional perspective; and it is against precisely that idea of science that the reactions to positivism are directed. If, therefore, we accept the view that these two historical discontinuities are fundamental to the contemporary state of the discipline, it becomes quite central to come to an understanding of science, in the original context of the physical sciences, and as it has been employed in the study of social phenomena. Our overview seems directly to imply that both to the understanding of the historical roots of the present state of fragmentation of the

discipline, and to the resolution of the internal debates which fuel that disintegration, the notion of science is indispensable. It is therefore to this idea, in particular, that chapters II and III are principally addressed.

1.3 The conceptual foundations of human geography: a proposed programme of inquiry

The overview that has been gained from this brief historical analysis seems to suggest that two concepts, each associated with a significant historical discontinuity in the discipline, are central to the understanding of the state of contemporary human geography. The first, and most important, is the idea of science and its derivative, the idea of social science. The second is the idea of human functioning, both individual and collective, in the environment. These, it is proposed, will be the two poles of our inquiry. Bearing on them, not as separate themes, but in the sense of bringing into relief aspects implicit to both, is the idea of relevance, of the demand for the applicability of theory in the discipline to the substantive problems of the human global society. It is hardly necessary to assert that this is not the only way to come to an understanding of the contemporary state of the discipline: obviously there are a number of ways one could proceed. But it does seem to be an approach that is rooted in the historical development of the discipline, and is not only, in this sense, valid, but is addressed to the central areas of contemporary debate, and is of at least potential value in resolving that debate. We may therefore proceed to the framing of a programme of inquiry, based on these concepts, in the confidence that such an inquiry has its roots in both the past and present of the discipline.

A principal assertion of our inquiry will be that the positivist aspiration to place human geography on a scientific foundation, to frame it after the model of the physical sciences, was not misconceived. Looking at the four centuries or so of scientific endeavour, it is difficult to avoid the conclusion that scientific inquiry has been, in its own terms, a resounding

success. It seems to offer real and considerable advantages to the framing of a discipline. The public nature of its enterprise gives not only the possibility of cooperation within a particular generation of scientists, but of cumulative growth from generation to generation. These attributes are the result of a systematic and precise approach to the building and testing of theory, to which the relationship between theory and empirical fact, and between theory and theory--embodied in the ideal of a deductive system--is fundamental. It further offers the possibility of developing systematic links with other bodies of theory, and even with other disciplines. Most importantly, the demand that theory meet the facts, together with the deductive development of theory--its vertical rather than its horizontal thrust--provides for the growth of the power of theory, the ability to organize and utilize increasingly expanded areas of natural laws and natural phenomena. It seems, at first glance, therefore, that there are good reasons to seek to reconstruct the discipline as a science, and reap thereby both the structural and the practical benefits which it is said to confer. There are, as will be seen, some real difficulties to be resolved before accepting unreservedly this account of science, and even more intractable problems in transferring it to the study of social phenomena (see chapters II and III). At this stage, however, it is simply asserted that, if these problems can be resolved, the aspiration to frame human geography as a science seems to eminently reasonable, and certainly worth pursuing.

We have seen, however, that the humanist critique has been based in large part on a rejection of logical positivism, the philosophical position which underpins the scientific enterprise. Central to that critique is the idea that a positivistic view of human functioning--the human individual stripped to meet

the stringencies of objective investigation-- removes from it most of what is characteristic of human life, which is essentially subjective in nature. Again, the aspiration is difficult to question. It is clear that the depth of understanding developed in the discipline is dependent on the degree to which the reality of human functioning in the environment is encompassed. Moreover, the effectiveness of application and intervention deriving from that understanding is a function of the depth of its insights. If, therefore, the structure and methods of science are to be retained, the primary challenge which faces the discipline is to do so in such a way that the humanist demands are met.

We need not accept uncritically the assertion, made by both positivists and humanists, that their positions are irreconcilable. It seems to have been a characteristic of the discipline, particularly in contemporary times, to conceive of different perspectives solely in terms of dichotomies, which are regarded as opposed in principle, mutually exclusive of each other, so that the only possible outcome of debate is to accept one or the other, or, if necessary, neither. James (1972, p.507), for instance, identifies a number of such dichotomies "harmful to the clarity of geographic thought": idiographic-nomothetic; physical-human; topical-regional; deductive-inductive; science-art. Buttimer (1978, p.73) notes the effect of this kind of intransigence on the structure of the discipline as a whole, as new conceptual and methodological positions have been developed and immovably adopted:

> Ideally, each new offspring should mean an enrichment for the "family" as a whole as unexplored or forgetten elements of household business are brought to light. . . . As each component member seeks its own identity, however, and develops its own special language and social world, communication becomes increasingly difficult and harvest time finds an extended family awkwardly facing (or avoiding) the challenge of synchronizing its collective efforts.

On the contrary, therefore, in directing attention to the apparent opposition of positivism and humanism in the discipline, in this dissertation we take the position that the aspirations of both are legitimate; that an a priori assertion of their mutual exclusiveness ought not to be accepted uncritically; and that they offer the possibility of valuable mutual enrichment. A primary goal of the inquiry will therefore be to seek an accomodation between them. It is accepted to be unlikely that avenues of potential accomodation within the framework of the theories themselves have been left unexplored: from the perspective of each, the other must appear alien. The approach adopted here is therefore dialectical: rather than recapitulate the separate grounds of opposition, the attempt will be to locate a more fundamental theoretical position in which both positivist and humanist perspectives may assume a legitimate place. We do not know whether it can succeed--the history of the discipline is rather against it--but we can at least assert that it seems to be an attempt which, in the interests of the internal coherence and relevance of the discipline, is worth undertaking.

A similar procedure will be adopted in approaching the second discontinuity which has been identified as pivotal to the understanding of the discipline, that which separates traditional from contemporary human geography. That attitude, embedded in the first full flush of the positivist movement, which seeks to deny value to the traditional geography of the first half of the century (Gould 1969; 1979) is not here accepted. The traditional insights of twentieth-century geography, ~~centreing~~ centring on the idea of the man-environment relationship, and on the conviction of holism, are held to be valuable and fertile. The challenge, therefore, is not to abandon either past or present, but to find a deeper theoretical framework in which to accomodate both. This

is a slightly different approach from Buttimer's (1971; 1978) attempt to revive interest in the Vidalian school of the French geographic tradition, but the intent to conserve the best of the past is the same. Continuity of context, which is prerequisite to the successful pursuit of this accomodation, is held to be available in the theme of man and environment, which is identified as central to both traditional and contemporary human geography.

The question which now presents itself is what kind of inquiry to undertake. In seeking a resolution of the fundamental debates that currently fragment the discipline, one conceivable approach is to attempt to treat the different positions exhaustively, in the terms in which they have presented themselves. Such is the array of theories and conceptual contexts, however, facing the aspiring commentator, particularly in the humanist domain, that the satisfactory elucidation of any one of them would in itself require a dissertation. There is reason to believe, moreover, following our overview of the discipline, that such treatments are not likely to engender the kind of accomodation sought here. The historical evidence suggests that the contemporary debates in human geography, and the development of theory in each school have more often than not been conducted within an a priori framework of assumptions, whose terms and propositions have remained largely unexamined and unchallenged. As analysis of the various positions has become more incisive and demanding, it has become increasingly clear that it is not on the expressed level of the debate that the fundamental issues are to be found. Arguments developed on unsound foundations can be valid without being true, as a fundamental principle of logic asserts (Langer 1967, p.188). Faced with differing theoretical positions--of scientific and humanistic, of traditional and contemporary human geography--each apparently valid within its own

framework, and yet uncompromising in its rejection of the other, one would do well, this principle implies, to look to the assumptions that underpin those positions; to their conceptual foundations.

In general terms this procedure may be termed conceptual analysis. It necessarily operates at a more abstract level of the discipline, by virtue of its seeking the common form which underlies superficial diversity; just as formal logic moves towards increasing abstraction of form and structure in its attempt to find a simple, coherent way of dealing with a diversity of objects and relations. It is not necessary, however, to see in this a move away from the everyday matters that are regarded as the proper domain of the geographer. Thus Langer (1967, p.35) remarks:

> There is nothing abstruse, esoteric or "unreal" about abstract thinking. . . . We all deal with pure forms in a practical, intuitive way. . . . Whenever we draw the ground-plan of a house, we not only see the analogy between the plan and the prospective edifice, but we intend to convey the mere form of the house without any indication or thought of the material to be used in building it. . . . It is the form that interests us, not the medium wherein this form is expressed.

It is, indeed, in the interests of resolution of differences that such abstract assumptions are sought. It is potentially common ground on which direct comparisons can be made; comparisons which cannot be made at the more expressed levels of theory. How, for instance, is it possible to think of relating the idea of spatial patterns produced by human societies to the humanist notion of geographicality without recourse to the more general conceptual categories of which they are expressions? Moreover, in dealing at this level, one deals not just with common forms, but with foundations, which effectively encapsulate the entire structures of theory and practice which are at issue. As an approach to reconciliation it is, then, both valid and effective to proceed in what may be

termed a vertical direction, rather than pursue a horizontal strategy on the surface of the theory of the various schools.

Conceptual analysis of this kind is thus pre-eminently a self-reflexive exercise. Buttimer (1978, p.73) remarks on the desirability that "the discipline as a whole . . . reflect on its own development during the twentieth century" in order to rediscover the wisdom of earlier generations; and, as we have seen, there are important conceptual insights to be gained from such an historical perspective. But there is a deeper sense of the idea of reflexivity which relates to the meanings of the terms in which the theories of the different schools are themselves presented, and which leads analysis in the direction of the more abstract foundations of theory. It is not a process that has been widely given systematic treatment, but the following characterization by Wilson (1963, p.14) is, broadly speaking, accurate and appropriate:

> . . . we are asked to take seriously concepts which hitherto we had taken for granted. We are asked, as it were, to become self-conscious about words which hitherto we had used without thinking--not necessarily used wrongly, but used unselfconsciously. . . . when we deal with questions of concept, we are asked to become aware of the significance of our words.

Wilson goes on to employ the useful analogy (particularly to the geographer) of making a map. One may know a tract of country quite well, he remarks, in that we have travelled through it or around it; but

> . . . we have not become objectively conscious of it in the way that one needs to if one is going to make a map of it. . . . Similarly we have all our lives worked with words, used words successfully to communicate with our fellow; but we have not become conscious of the meanings of words. (Wilson 1963, p.15)

This kind of exercise is an essential component of philosophical method. It is no accident that as soon as one begins to deal with general or more

abstract concepts notions of logic, for instance, present themselves: one of the primary motivations of traditional logic has been to systematize and facilitate such analysis in the abstract domains which philosophers have, over the centuries, made peculiarly their own. In a broadly descriptive way, therefore, this dissertation has been characterized, in its title, as a philosophical approach to human geography. The use of this term is not intended to imply the idea of metaphysical analysis, in the sense of untestable speculation about the nature of things. It is rather an approach to conceptual clarity which goes beyond the idea of definition of terms to the conceptual foundations they assume.

Winch (1958) would associate this procedure with the "underlabourer" conception of philosophy, according to which the primary function of philosophy is "the purely negative role of removing impediments to the advance of our understanding. . . . philosophy is concerned with eliminating linguistic confusions" (Winch 1958, pp.4-5). Winch rejects this view as demeaning to philosophy, whose true role, with respect to other disciplines, is to elucidate the epistemological foundations of all disciplines. We take no position of this argument, which is of concern to pure, rather than to applied, philosophy, as a discipline in its own right. We simply take the view that the techniques of conceptual analysis identified here as valuable to the understanding of human geographic theory have been developed best within the context of philosophy; and that the proposed inquiry, to the extent that it employs such techniques, is therefore philosophical in spirit. Thus Ryan (1970) characterizes philosophy as "second-order inquiry" which

> has elaborated a set of distinctions and elucidated various argumentative techniques, together with the technical vocabulary required for such a task. . . . (Ryan 1970, p.9)

In accord with the approach adopted in this dissertation, which is predicated on the need for this kind of analysis in order to resolve the current theoretical disputes in the discipline, and to construct a sound foundation for its future growth, Ryan goes on to note that

> . . . successful theoretical innovation is often less like the first-order task of factual inquiry than it is like the second-order task of revising or reinterpreting our understanding of what is involved in factual inquiry. . . . (Ryan 1970, p.11)

It is precisely this kind of 'second-order' inquiry we have in mind as an indispensable element of the conceptual and theoretical coherence of the contemporary discipline. As has been noted, there has been recently in evidence a growing awareness of the need for philosophical understanding in the discipline; although notions about what might constitute a philosophic approach have varied widely, to say the least (Gale and Olsson 1979). To those who would want to argue that the realms of philosophy are far removed from the concrete realities of life that are the geographer's true concern, one might simply respond that the current disarray of the discipline has forced conceptual and philosophical matters upon our attention; that the discipline is in its present state just because, as Buttimer (1978, p.73) suggests, it has avoided such matters; and that if the fragmentation of the discipline is to be arrested, and a secure foundation for its future development constructed, these are matters which, however onerous the task, the contemporary human geographer must attempt to resolve.

As has been noted, according to the view of the historical development of the discipline outlined above, in which two discontinuities associated with adoption of positivism and reactions to it were identified as fundamental to an understanding of it, the pivotal notion is the idea of science. For a concept

which has been so central to the discipline in the course of the last three decades, and which has fuelled a change in the discipline so profound that it has come to be called a revolution, it has received remarkably little attention from human geographers. It has been, in the main, one of the chief *a priori* assumptions on the basis of which the discipline has proceeded: it has been little examined by geographers, and where examined, little understood. Only in recent times has this begun to be amended, and then only in the work of commentators on the discipline (Gregory 1978; Szymanski and Agnew 1981), rather than as a foundation for substantive human geographic theory.

The first part of this dissertation thus attempts to deal directly, and in some depth, with the idea of a science; and then, because human geography deals with human functioning in the environment, with the idea of a social science. The foregoing analysis suggests that these are indispensible first steps in the attempt to resolve theoretical debates in the discipline, and to secure its conceptual foundations. If one is persuaded of the potential value of framing the discipline after the model of the natural sciences, one needs to know in detail what that model is, and to be convinced of its essential validity. As it happens, these are not simple questions to pursue, and they have engendered a long and complex debate in which the human geographer, if he or she is seriously intent on resolving this central aspect of the foundations of the discipline, must necessarily engage. Even having established so much, it is an essential second step to decide whether or not the model of science concluded to be valid is applicable to human phenomena; and here the debate is, if anything, longer and more complex than that which relates to science itself. Again, however, it is a debate which cannot be avoided if our inquiry is to proceed at all.

Much of the analysis of these matters has been undertaken within the established traditions of the philosophy of science and the philosophy of social science. It would be presumptuous to attempt full treatments of what are essentially two sub-disciplines in their own right in the context of this dissertation. What is attempted, therefore, is a critical analysis of the main positions upon which these traditions have developed, with a view to establishing firstly, a valid idea of science; and secondly, the applicability of that idea to social phenomena, and hence to the discipline of human geography. Such an idea of science, meeting both of these sets of demands, is, it is suggested, achievable. The necessity of dealing with these traditions of philosophy in order to develop a position on these ideas is a second ground for the characterization of this dissertation as a philosophical approach to the discipline.

It should be noted that there is no attempt here to deal with the idea of science as an ideological formation, following structural Marxist analyses, and other Marxist perspectives on ideology and society, as Mannheim (1952; 1955) and Gramsci (1967) have undertaken. It is accepted to be a legitimate inquiry, and necessary to a full treatment of the idea of science, as Gregory (1978, p.169) concludes; but it arises in a quite separate framework of theory, and requires an extended treatment which is not possible here. To a degree, however, its epistemological foundation may be identified with those of Kuhn and Winch, and these are dealt with at some length in chapter III.

The second part of the discipline then deals directly with the theory of human geography, taking as its point of entry and guiding principle the second of the two notions identified in the foregoing discussion as pivotal: the idea of human functioning. A logical analysis, based on key aspects of the

philosophy of social science, suggests that human functioning may best be approached at the level of the human individual, taken generically; and that accounts of the collective level of human phenomena, which is the accepted domain of the geographer, can be systematically developed on this theoretical foundation. It is suggested that all human geographers necessarily make implicit assumptions about the nature of the generic human individual, whether they explicitly recognize such assumptions or not; and that their theoretical positions have been developed on--indeed, have been made at all possible by--these assumptions.

Rather than adopt any one of these models of the geographic individual--which would, it is implied, entail the acceptance of the priority of the particular theoretical structure to which it gives rise--an integrated model of the individual is proposed. This model follows the principle, developed in the foregoing discussion, of seeking the accomodation of different, even of apparently mutually exclusive approaches, at a deeper level of theory. It is proposed that such a model offers the possibility of reintegrating differing accounts of the geographic individual, and therefore differing structures of human geographic theory erected upon them. It is developed by drawing upon the theory of the main schools of contemporary human geography--spatial, behavioural, and humanist--identified in the historical overview above. This model is then assessed as a candidate for a genuinely scientific theory, in the light of the conclusions of the first part; strengths and weaknesses of the theory are, from this perspective, identified, and possible lines of development suggested.

These strategies are pursued in an attempt to establish an underlying unity between contemporary approaches to the discipline. With a similarly

unifying end in mind, an overview of traditional theory of human geography is then attempted. It should be clear by now that there is no intention in such an approach to return the discipline to a former stage of its development. It is simply an expression of the view that many of the richest of geographic insights can be located in these past traditions; that these are just those insights which have been abandoned as not admitting of a truly scientific understanding; and that they constitute a source of indigenous theory and hypothesis which it is neither logical nor productive to ignore. It is therefore held to be important to determine whether or not the proposed integrated model of the geographic individual is able to generate a theoretical structure that may account for at least the most central of these insights, and is, to that extent, able to begin to effect a reintegration of contemporary and traditional human geography.

Finally, prospects for pursuing the development of the theory along the lines suggested by these analyses are explored. Since the inquiry is undertaken at the level of the conceptual foundations of the discipline--and since, moreover, it reveals substantial theoretical gaps in the proposed approach to the reintegration of the discipline--it is not easy to be specific about the kind of human geographic theory that might be developed on this model of the geographic individual. Nevertheless, it is felt to be both valuable and necessary to attempt an outline of the implications of this approach for the discipline as a whole, primarily in order to confirm that the inquiry has not been undertaken at some artificially isolated level of human geographic theory, but that it has relevance to the practical concerns of the discipline, and is of real potential value in achieving the reintegrative goals adopted in the foregoing discussion.

An important matter which arises from this proposed programme of inquiry into human geography is the extent to which it is reasonable to attempt to encompass both contemporary and traditional aspects of the discipline. It is clear that, interpreted in comprehensive terms, the task is hardly achievable, in a dissertation, for one or other of these domains, let alone for both. There is, however, no attempt here to deal with the various approaches in their entirely: the thrust of the inquiry is directed to underlying assumptions about the nature of human functioning, individual and collective, rather than to the complete range of theories developed on these assumptions. It is an exercise in the foundations of human geographic theory, rather than in its content. At that level, it would seem to be less unreasonable to attempt to deal with a number of contemporary and traditional approaches, since they are held to centre on one theoretical concept common to them all: the generic nature of the geographic individual.

Moreover, it implies that these approaches can be satisfactorily approached through central authors or key works, since concepts at that level are likely to be common to most of those working in the area. The selection is therefore representative, rather than comprehensive. In making the selection, however, the historian of ideas, no less than the commentator on theoretical concepts, has no other recourse than to his or her own necessarily incomplete acquaintance with the literature of the discipline; acknowledging all the while that many other selections may be equally representative and equally valid.

There are some obvious areas of human geographic thought which might have been included in the discussion and were not. One is the contemporary school of Marxist geography. In part this has been excluded from consideration on the grounds of its apparent reluctance to deal with the man-environment theme

identified as lying at the heart of geographic concern. This is not to say that Marxist theory cannot with profit be applied to such matters as, for instance, environmental deterioration, or even to the nature of the geographic individual and the geographical experience; but it must be admitted, one feels, that these are concerns which are marginal to the main thrust of Marxist and neo-Marxist theory. Moreover, the Marxist analysis deals not only with the theory of human geography, but with the social function of that theory, which seems to require a complete meta-analysis in its own terms. For these reasons, together with a recognition of the immense amount of literature associated with it, it was felt that it was not possible, in the space available here, to deal at all adequately with the Marxist school; and that it may be better to omit detailed consideration of it altogether.

A second, and, in the context of traditional human geography, more important omission is that of regionalism, which appears in much of the theory and practice of human geographers during the 1940s and early 1950s, as outlined by Dickinson (1976). It would seem, on the face of it, than an analysis of this material is as important to an overview of the man-environment theme as any of the traditions examined in chapter V. Part of the justification for its exclusion again lies in the bulk of material which must be handled in order to do justice to the development of the regionalist school. but there is a more important theoretical justification for the exclusion of this material. That the regional concept is central to the analysis of man-environment relationships is not here in question: in fact, the roots of that concept, in the work of the Vidalian school, are examined at some length. The Vidalian notion of a region, it will be suggested, is based upon two key principles: that of terrestrial unity, and that of holism. The region assumes

significance through its unique identity as a naturally defined area of the earth; and this is the view here taken, in this dissertation, to be the most valuable component of the regional view in human geography, since it derives directly from a consideration of the relationship obtaining in an area between man and environment. In contrast, the development of the concept in the regionalist school saw the emphasis move to matters of definition and delimitation, which through a shift in the scale of resolution, brought into focus individual elements, with the aim of reaggregating the assemblage that was to constitute the region. This, it may be felt, struck at the heart of the holism of natural regions that characterized the Vidalian position, and led to the sterility of the idea of conceptual regions; which in turn saw the entire regional concept largely discredited, and fade into disuse. These trends may, in retrospect, come to be seen as a tragedy for the discipline, depriving it of an insight as profound as it was complex; an insight in which much of the real identity, richness, and vigour that have characterized geography at its best were invested. To trace the lineaments of this decline is a task that awaits some future historian of geographic ideas; but it was not felt to be appropriate to this dissertation.

A related omission, in the area of traditional human geography, is any detailed account of the German school, identified in the foregoing historical overview as an important source of human geographic theory in the twentieth century, and a corresponding dependence on the Anglo-American and French schools. This omission can be justified in part by the judgement that the German school was perhaps less unified and influential than either the American or French school; but since it was probably more so than the British school, this is not a sufficient ground for exclusion. More germane to the decision

was the limited linguistic competence of the author, which extends to French but not to German, and the relatively restricted body of material that has been made available by translation from the German. The survey of traditional works undertaken is based, as far as possible, on the original works, rather than on commentaries; and in the interests of historical veracity, therefore, the decision to omit sources that were inaccessible to direct inspection seemed a reasonable one to make.

In conclusion, it is important to emphasize, if it is not already clear, the extent to which this dissertation is exploratory rather than definitive. It probes an abstract level of the discipline, often significantly removed from the accustomed domain of the geographer. Its immodest aspirations are to begin to establish a unified and secure foundation for the discipline as a whole, through close attention to the idea of science and to the idea of human functioning, in the context of both contemporary and traditional human geography; and on that basis to sketch possible directions for the discipline to take. Furthermore, the approach to human geography is necessarily a subjective one: it seeks representative, rather than comprehensive, insights in the belief that the pivotal concepts determining the selection are sufficiently fundamental for a thoroughgoing analysis of them to be a valuable contribution to the conceptual foundations of the discipline. At this stage, we are not even sure that the proposed programme can succeed: only that it seems, in the light of the historical perspective on the discipline, to be a legitimate and potentially valuable direction to take towards the fulfillment of these aspirations.

In a very real sense, therefore, this dissertation is the record of a research programme, rather than an exposition of a well-defined, conceptually

complete theoretical structure. It is undertaken in the conviction that waste of insight and experience, whether traditional or contemporary, is not a luxury that a modern discipline can afford; that apparently divergent views can find a common ground in more profound, and more powerful theory; and that in the formulation of such theory lies the potential for greatly more effective, more balanced, and more comprehensive interventions in the urgent problems of global society. Its commitment is thus the commitment of any intellectual inquiry worthy of the name: to ameliorate the human condition, to uplift and enrich the quality of human life.

CHAPTER II

THE IDEA OF A SCIENCE

Outline of the Chapter

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- Bunge
- Cole and King
- Harvey
- Smith
- Amedeo and Golledge

2.2 The hypothetico-deductive model of scientific inquiry

- The limits of science
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2.5 Lakatos on scientific research programmes

- Falsificationism
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- Continuity and tenacity
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CHAPTER II

THE IDEA OF A SCIENCE

2.1 Contemporary geographic views of the nature of scientific enquiry

The idea of science is so much a part of our everyday experience, and the term itself so much a part of common usage, that one may be excused for thinking it unnecessary to explicate the notion further. One such contemporary view of science runs as follows. Everyone knows what science is: it is systematic and logical, uses measurement and quantity, and deals exclusively in the facts established by experiment. As a method of understanding the world, it must be a good one, since we see about us, at every turn, evidence of its success: the amelioration of the global conditions of life, we know, is the gift of science in the modern era. Progress, it has been pointed out, is scientific progress: one is commonly defined in terms of the other (Kuhn 1962a, p. 160). Since the evidence of its value is so overwhelming, it hardly seems worthwhile to conduct any serious investigation of its conceptual foundations. The thing to do is simply to understand its methodological principles, and apply them to the study with which one is concerned. That application may raise difficulties of its own, since every discipline differs in the ease of application of scientific method to its phenomena; but these are difficulties that can, and must, be resolved. Indeed, if the study is to be a reputable one, there is little choice in the matter: only in scientific method

do we have "the setting up and observing of decent intellectual standards for rational argument" (Harvey 1969a, p. vii).

This description may be, in some degree, larger than life, but it is only slightly exaggerated. Looking to the last two decades or so of human geography, it can, we may justifiably feel, be applied there with little modification. By and large it is simply a fact that human geographers have adopted what they understood to be scientific ways of thinking and investigating with hardly a passing glance to the nature of scientific enquiry itself. Certainly human geography has not been alone in this among the social sciences; but it has certainlybeen a prominent characteristic of the discipline in recent times. The roots of this attitude are difficult to trace; perhaps it lies in the traditional commitment of geographers to observation, rather than to reflection. Whatever the reason, it is a central contention of this dissertation that the conceptual confusion consequent upon this attitude is fundamental to the precarious position in which the discipline now finds itself. The corollary of this contention is that if the discipline is to flourish, it must locate itself on a valid and fertile conceptual foundation, to which a profound understanding of the nature of scientific enquiry itself, and of its validity and relevance in the context of human geography, is inescapable. Unwelcome as this conclusion may be to professional geographers, intent on gathering data and pursuing investigations in their chosen field of interest, and unwilling to spend time on abstract philosophical matters that lie well outside the ambit of their discipline and their training, it simply seems unarguable that if they are to regard themselves as scientists, and to employ what they understand to be scientific methods, they need to be certain

that the idea of science on which these notions rest is a valid one; otherwise, they place at risk the legitimacy of the whole enterprise.

Such a geographer may, of course, point to the whole community of scientists and protest that few of them find it worthwhile to undertake such enquiries, and yet they produce valuable work. That is an understandable position, though hardly one of much intellectual integrity: the misconceptions of others, even of most others, cannot be held to legitimize our own. and what may be true of practising scientists is undoubtedly not true of those whose business it is to hold the entire scientific enterprise in perspective, and to investigate the nature of its conceptual foundations: there it is the lack of concensus that is particularly striking. Indeed, it is a matter of some irony that the past two decades, during which human geographers have attempted to frame much of their discipline according to their understanding of the model of the natural sciences, has witnessed the most fundamental debates about the nature of science experienced at any time in this history of the philosophy of science. The failure of most geographers to come to terms with this debate, to become conversant with its central issues, and to establish a position from which to develop valid theory, has compounded their unwillingness to analyze systematically the conceptual foundation of their discipline, and has led, we may feel, to the confusion in which the discipline now finds itself.

Some examples will clarify this point. Although one might draw support for this line of argument from almost any of the major works of quantitative human geography, five in particular are now examined. Three are taken from the 1960's, when the scientific spirit in human geography was at its height: these are Bunge (1962), Cole and King (1968) and Harvey (1969a). The other two are taken from the mid-1970's, when the first humanist challenges to the scientific method in human geography had been launched, and were beginning to be reflected

in the greater commitment of the positivist position to social relevance and to behaviourism: these are Smith (1975) and Amedeo and Golledge (1975). Looked at from a different perspective, it can also be said that Bunge, Harvey, and Amedeo and Golledge are more theoretical in their approach; and that Cole and King, and Smith are more oriented to practical application. Together these five works, then, can be taken to represent the thinking of human geographers over the last two decades about the nature of science.

We may begin with a brief look at an early classic of the quantitative school, William Bunge's Theoretical Geography (1962). In retrospect, the crispness and clarity with which Bunge argues his view of science stands up remarkably well. He takes a firm stand on the need for a priori theory before any description can be undertaken:

> There is an infinitude of facts around us and any description of them is highly selective. This selection can be made at random, but geographers are always seeking facts they judge to be significant. Significance can be judged only in relation to some other phenomenon. The establishment of this relationship means that theory has been formulated. The so-called "mere describers" in geography do not go out into the world with empty heads. (Bunge, 1962, p.6)

He therefore goes on to identify the true business of science as theory construction, and follows Schaefer (1953) in rejecting Hartshorne's argument for the uniqueness and unpredictability of human geographic phenomena. It is part of his position, however, that "the plausibility or intuitive reality of a theory is not a valid basis for judging a theory" (Bunge 1962, p. 3); and in this, he is mistaken. Lakatos (1970) has demonstrated convincingly that, on the contrary, it is the plausibility of theory that constitutes the momentum of scientific research programmes, often in the face of observed counter-instances. Of course, Bunge does not address any of Kuhn's objections as they had only just been formulated (Kuhn 1970a); but it may be said that,

within its limits, his view is as successful as any that have subsequently attempted to take a similar position in geography. It is a matter of some irony that both Bunge and Harvey, the most coherent of his successors, have rejected their early forays into science in favour of Marxist orientations.

Cole and King (1968) exemplify well the cheerful naivety with which human geographers of the 1960's embraced what they conceived to be the scientific method. In a work of some 700 pages devoted to an exposition of quantitative techniques in human geography--to, in other words, the technical aspects of placing the discipline on a scientific foundation--three pages are given to a consideration of what "scientific" may mean. Cole and King (1968, p. 18) begin by distinguishing two uses of the term "science," the first referring to "a certain set of phenomena," the second to "a method of study." They discount the limitation of science to natural as opposed to man-made phenomena, and decide to define science in terms of its method. As we shall see, this apparently simple notion glosses over the whole range of debate that has characterized the philosophy of science and the philosophy of social science since 1960: Kuhn's (1970a) whole point is that there is no neutral way to define science outside the historical period in which is operating, and that therefore appeals to the idea of method have no independent meaning; Lakatos (1970) would wish to place the notion of method in both an historical and logical context of the scientific research programme; and even Popper, the philosopher with whom this position has most in common, would seek to replace the notion of method by a far more rigorous criterion. There is no simple answer to the question of what makes a science a science, and Cole and King, in adopting their position, express just that simplistic attitude described at the beginning of this section.

The steps of the method which Cole and King see as distinguishing run along the conventional lines of the a priori model. They do at least recognize that

> It is desirable, if not essential, to have in mind some objective in a given study, rather than to collect material in the hope of finding an objective. (Cole and King 1968, p. 18)

Again this is closest to Popper's view; and again Popper would want to be more rigorous in saying that all data, without exception, are gathered according to some preconceived idea. However, we must credit Cole and King with here avoiding (though not later) the inductivist fallacy that has traditionally been taken to underpin geographic methodology. They go on to note:

> The interpretation of the findings will as far as possible be given in a form, verbal, numerical or in some other way, that is comprehensible, in the accepted language of science, to other workers, at least in the same field. The interpretation and emphasis is, nevertheless, up to a point bound at this stage to the subjective and personal. (Cole and King 1968, p. 19)

It is striking that this point, which is central to the critique that Kuhn develops, and to the debate in which, as we shall see, Popper, Feyerabend, Lakatos and many others have engaged in, and which is still not resolved, should be raised in passing, and then entirely ignored by Cole and King. For they go on to assert that from these findings may come

> some generalization, around which statements may be made about patterns, regularities or tendencies. Enough repetitions of the same patterns or tendencies may lead to the formulation of laws. Prediction with a given degree of confidence, if not with certainty, is then possible. The ability of a discipline to make predictions is in the view of many the principal criterion of success in a scientific sense. (Cole and King 1968, p. 19)

What, then, has become of the "subjective and personal" interpretation previously alluded to? How has it come about that the variability of such interpretations has given rise to "laws" and to "prediction with a degree of

confidence"? Cole and King do not recognize even the questions, much less provide us with answers to them. Moreover, setting aside the critique of Kuhn and the debate which has followed it, and dealing just with the positivistic model within which Cole and King are here operating, the notion expressed by them that laws and explanation are derived through repetition--through, in other words, an inductive process--has long been discredited by Popper's analysis, which demonstrated the illegitimacy of constructing law and theory prior to prediction and testing (see section 2.2). The account of the development of scientific theory given here by Cole and King is just that popular notion of scientific method one might expect from the layman, but which cannot be justified in those who claim to be practitioners of science, and who therefore ought, one assumes, to know something about its conceptual basis.

This account of science concludes with two minor, but nonetheless significant, misconceptions. The first is that

> The appropriateness of the scientific method for a given problem or hypothesis depends on the number of things being considered. Thus for example, whereas it is impossible to make any prediction about the location of capital cities in their territories simply from studying Paris, it is perfectly feasible to make predictions with considerable confidence about the voting or smoking habits of the population of France, even from quite a small sample. (Cole and King 1968, p. 20)

The confusion here is typical of Cole and King's approach to science. The examples given are valid in terms of statistical inference; they are invalid in terms of science. Statistical inference is not science, although it must be admitted that human geographers are inclined to treat it as such. In the positivist context assumed by Cole and King (a context which, we are obliged to continue pointing out, has been strongly criticized), science is best defined by a structure of logical reasoning which deductively links law statements, particular statements concerning initial conditions, and particular statements

concerning predicted outcomes. In these terms, consideration of Paris in relationship to its territory could quite legitimately constitute a scientific test of a theory or law about the location of capital cities in their territories. Cole and King's misconception is clearly a consequence of the uncritical inductivist thinking which has typified geographic views of scientific method.

Finally, they allude to the idea of experimental control, again a typically lay view of the essential characteristics of science:

> Geography is, however, essentially a field science rather than a laboratory one, so that geographical phenomena are not often susceptible to controlled laboratory experiments as are those in some of the physical and biological sciences. As with many situations in the social sciences, geography is usually concerned with many uncontrolled variables. For this reason relationships are often rather weak. (Cole and King 1968, p. 20)

Yet, as is discussed below (section 2.2), experimental control is not a necessary or a sufficient characteristic for a science; on the contrary, many fully-fledged sciences, such as astrophysics and the atmospheric sciences, manage quite well with _in situ_ experimental situations. The logic of science applied equally well to these situations as it does to experimental ones. One is forced to conclude that Cole and King have not in any sense thought deeply or critically about the nature of the scientific method that they wish to employ; that they do not seem to recognize any other approach to science than the bowdlerized positivistic model they employ; that they do not see as profitable or necessary discussion about these notions, taking them as simple matters beyond dispute; and that, in consequence, all the quantitative techniques offered exist in a theoretical vacuum, in which their role, purpose and interpretation are entirely obscure, and therefore, difficult to use with any degree of conceptual clarity.

Cole and King are not, however, untypical of the approach to science in human geography of the first part of the 1960's. That their work seems now to us, in retrospect, so inadequate is due in no small part to Harvey's (1969a) systematic attempt to come to terms with the nature of Explanation in Geography. So influential has this work been as a conceptual underpinning of positivistic human geography that subsequent authors, if they have referred at all to the problems being considered here, have in the main been content to refer the reader to it, endorse its position, and move on to the specifically geographic matters which they regard of real significance. Smith (1975, p. 348) provides a typical example of this approach:

> Working from description towards explanation involves something more than the application of numerical methods of the kind reviewed in this book. It also requires some understanding of the way in which explanations are sought in the social sciences and the routes to understanding which others such as philosophers and logicians have found effective. David Harvey's Explanation in Geography is an obvious place to begin acquiring this kind of knowledge, supplemented by readings on the conduct of enquiry in the social or behavioural sciences at large.

The response of human geographers to Harvey's work has thus been rather to express relief that someone has undertaken these important questions than to subject his position to critical evaluation. This is all the more surprising since Harvey himself admits in the preface to the book, "I wrote this book mainly to educate myself," terms it an "interim report," and confesses that, even after the writing of it, "relative to what I still have to learn I feel more ignorant than ever" (Harvey 1969a, pp. v, viii). Clearly it was not intended to be a definitive statement; yet, almost universally, that is how it has been treated.

When we come to examine closely the first four chapters of the book, in which is stated the view of science on which the rest of the analysis is

predicated, Harvey's position emerges as far from satisfactory. He begins by making a distinction between the philosophy and the methodology of geography: the first is to be concerned with beliefs, values and objectives; the second with the logic of explanation. Harvey holds that philosophy and methodology, thus defined, can be separated:

> It is thus one of the major tasks of methodological analysis to show how and under what circumstances a particular mode of analysis is appropriate, to specify the assumptions necessary for the employment of a particular technique, and to demonstrate the form of analysis which must be followed if the analysis itself is to be rigorous and logically sound. All this can be accomplished independently of philosophy. (Harvey 1969a, p. 8)

Now there are fundamental difficulties in such a position. Harvey himself touches on them in passing:

> There are, it is true, some issues regarding explanation which cannot be resolved independently of philosophical beliefs--the problem of verification and confirmation being a prime example. Nevertheless, we may rightly insist than an explanation should be logically sound, before we even bother to consider its philosophical underpinnings. (Harvey 1969a, p. 6)

Yet Kuhn's (1962) central thesis is just the contrary of this: that it is impossible to separate the "methodology" and "philosophy" of science, and that the logic of explanation must be embedded in what Harvey terms its "philosophical underpinnings." Harvey may dispute this thesis, with which he is apparently familiar (or perhaps, to be more precise, from which he extracts quotations); but if he wishes to do that, it is difficult to see how he can avoid dealing in some depth with the issues raised by Kuhn. Certainly it is not open to him to assert his position as an <u>a priori</u> axiom of self-evident truth. He must, it would seem, confront Kuhn's powerful attempt to demonstrate that the interpretation of terms in scientific explanation cannot be logically disentangled from the historical and subjective context in which it arises, a

notion which presents a far more comprehensive challenge to Harvey's positivist position than is covered by allusion to "verification" and "confirmation."

The conviction that Harvey has read but not understood the content of Kuhn's work, or its significance, is strengthened on considering the development of his position. He explicitly excludes from methodological considerations the idea of science as "an activity, as a process, as an organized attempt at communicable understanding," and goes on to refer the reader for consideration of these matters to Kuhn who, he says, among others, prefers "to approach the problem of explanation from a behavioural rather than a formal standpoint" (Harvey 1969a, p. 9). Again, Kuhn's point is that to consider science as an activity is not an alternative to considering it as a logical product: it is a necessity to such consideration. Moreover, it is quite wrong to characterize Kuhn's approach as "behavioural," and contrast it to a "formal" approach; as Popper (1970, p. 56) makes clear:

> What are his main arguments? They are not psychological or historical--they are logical: Kuhn suggests that the rationality of science presupposes the acceptance of a common framework. He suggests that rationality depends upon something like a common language and a common set of assumptions. He suggests that rational discussion, and rational criticism, is only possible if we have agreed on fundamentals.
>
> This is a widely accepted and indeed a fashionable thesis: the thesis of relativism. And it is a logical thesis.

Harvey is thus content to bypass the fundamental problem of what constitutes the experiential basis of science--a basis which, in Kuhn's view, is also relative to the framework in which it arises--by simply asserting that he intends to ignore the issue and to assert the following definition of it:

> Experience is thus regarded as some set of statements about reality which are commonly regarded or accepted as being "factual" in a sense that remains undefined. (Harvey 1969a, p. 10)

Such a definition raises more questions than it answers; and, since the notion of experience, or observation, is fundamental to the theory and the practice of science, its deliberate vagueness--which is tantamount to an avoidance of the issue--places at risk the entire structure of science which Harvey attempts to develop. His justification for such avoidance points, firstly, to the more important matter for us, as geographers, of

> . . . elucidating substantive geographical problems and not, as are many philosophers of science, with elucidating the form of explanation per se. (Harvey 1969a, p. 10)

One may wonder why, then, write a book on Explanation in Geography; and how "substantive geographic problems" are to be rationally approached if the conceptual grounds, on which explanations about them are to be developed, are obscure; and whether the two extreme positions here given by Harvey do not have a middle ground--the interface between "substantive geographic problems" and the "form of explanation"--which is precisely the central concern, one would think, of the book. Nor is it sufficient to plead the difficulty of treating adequately the notion of experience

> . . . without either writing a lengthy treatise upon the meaning of experience itself or making substantial presuppositions. (Harvey 1969a, p. 10)

Again, these are extreme alternatives, and there is much that lies between them that would allow for a treatment of experience that is something less than a "treatise," and which still presents a reasoned position: if Harvey is serious in stating that this is not possible, then, given the centrality of the idea of experience to the structure of scientific explanation, the book should not have been attempted at all. It is difficult to avoid the conclusion that it is the "difficulty" of the problems that arise in considering experience that are deterring Harvey here, rather than its inappropriateness in the context of the book. This conviction is strengthened by his attempt to appeal to Lowenthal

(1961) to shed some light on the nature of geographical experience: we may agree with Harvey's characterizing of that discussion as profound, while wondering how a work which emphasizes the importance of the subjective nature of experience, and which has become one of the seminal documents of the humanist school of human geography, is seen to lend support to the objective, realist approach Harvey here seems to be developing. It is the same confusion which has been noted in the attempt to separate "philosophy" and "methodology," and in the understanding of Kuhn's argument.

The confusion deepens in the discussion that follows. Harvey begins his brief account of the "philosophical" problems of explanation by defining explanation as

> . . . reducing an unexpected outcome, which is the source of conflict and surprise, to an expected outcome. (Harvey 1969a, p. 11)

He notes, however, that what is "expected" and what "unexpected" is not a wholly objective matter:

> There is . . . some degree of preselection regarding the experiences we react to with surprise and those that we dismiss. . . . we filter out all kinds of experience and examine just a few that for some reason or other we find surprising. In organised disciplines of learning, the preselection is partly done for us. . . . The questions that we ask, therefore, are partly conditioned by our training. The explanations that we seek tend likewise to be conditioned. (Harvey 1969a, pp. 12-13)

Harvey correctly associates this view with Kuhn's analysis. But instead of pursuing its implications, which present fundamental challenges to the hypothetico-deductive model he is espousing, he simply puts it aside, and goes on to consider some different approaches to the orthodox model--deductive, relational, and analogical--and we are hardly surprised to find that, even here, Harvey does not attempt any critical discussion of these approaches, but accepts for reasons not given, all of them "as valid approaches to constructing

an explanation in given circumstances" (Harvey 1969a, p. 15). He then decides to come back to Kuhn's analysis, which he now summarizes in greater detail, mentioning the ideas of a "paradigm," "normal science," and "puzzle-solving," and noting their utility in viewing the history of geography; but again treating Kuhn's analysis as a way of describing scientific behaviour that supplements, and, indeed, strengthens, the "deductive" model he supports. Finally, he deals briefly with experience and language, and notes the relationship between language and paradigm, but fails to pursue the analysis logically to the idea of the incommensurability of paradigms and scientific theories, as Kuhn does so powerfully. In conclusion, Harvey restates his separation of "formal" and "behavioural" approaches, of the

> . . . hard inner core of methodology--the analysis of explanation as a formal procedure--and a rather more general outer zone concerned with philosophy, speculation, perception, images, and the like. . . (Harvey 1969a, p. 23);

and, while admitting the "unsatisfactory" nature of the discussion of the "outer zone," affirms his intention of proceeding to analyze the "hard core of analytic methodology" (Harvey 1969a, p. 23).

There is little point in pursuing examples of this confusion throughout Harvey's book; yet it should be noted that it does continue, and remains unresolved. The discussion of verification and confirmation in the next chapter (Harvey 1969a, pp. 38-41), for instance, recapitulates all the difficulties outlined above. Moreover, it is worth pointing out that Harvey's habit of trivializing the views he attempts to summarize extends even to the deductive model he wishes to employ as the basis of scientific explanation: Popper's notion of falsification, for example, which is developed throughout that philosopher's work (Popper 1957, 1959, 1963a, 1963b, 1970, 1972a, 1972b, 1972c, 1976), is dismissed in a paragraph of eight lines through a quote of

Kuhn's, which points out that the idea of "degree of falsification" is implied by Popper's theory; an idea, which, in fact, Popper employs at length, and which is integral to his position. Harvey, like Kuhn, is here attributing to Popper that naive falsification which was never his position, and ignoring the methodological and sophisticated accounts of falsification which, we may agree with Lakatos (1970), constitute his real contribution.

As a discussion of the meaning of explanation, and of the fundamental issues associated with the idea of science, this simply will not do. It is clear that Harvey's position--which seems to be that the deductive approach to science constitutes its characteristic form, the understanding of which is supplemented by "behavioural" approaches--is maintained by an unsupported separation of logical and contextual notions, and by a superficial reading of Kuhn which comprehensively misrepresents, as we shall see, the logical power of his position: the compromise, which allows him effectively to do both, is achieved by the distinction between "inner" and "outer" zones, a categorization which again is unsupported by substantive analysis, and seems to arise more from its utility in avoiding a consideration of the conflict of views than from any substantial theoretical position. The simple fact is that, as any careful reading of the main protagonists to the contemporary debate in the philosophy of science must reveal, Kuhn's view of science is not compatible with the hypothetico-deductive model Harvey wishes to adopt: indeed, it challenges its very foundations. The protagonists of that debate, and their followers, are in no doubt about this (Lakatos and Musgrave 1970). Harvey's approach is consistently to embrace conflicting views under a vague and superficial umbrella that is based on no theoretical position, undertakes almost no conceptual analysis, and not only seeks no resolution of conflict, but is apparently unaware of the need for such resolution. Yet this is the account of

science which positivist human geographers have, for over a decade, accepted as the best and most systematic account of the methodological basis of their discipline. Culpability for uncritically accepting such a view of science rests as much with them as with Harvey; and, with the foundations of the discipline in such disarray, its present predicament occasions little surprise.

As we have noted, Smith (1975) endorses Harvey's view of science, and bases his discussion of numerical methods on it. He adopts the "standard model" of scientific explanation, as Harvey (1969a, p. 30) describes it; by which is meant the a priori structure that Hempel and Popper insist upon, in contrast to the inductively based a posteriori method (see section 2.2). Yet he begins his account of scientific method in human geography with the following statement:

> Knowledge begins with observation. We perceive the world around us with our eyes and other senses, and we arrange these observations in a certain relationship to each other so that they "make sense." Repeated observation of the same object or event brings familiarity; repeated observation of a particular association of objects or events creates an expectation. Knowledge imposes order or predictability on the world, reducing uncertainty and helping to make the unexpected consistent with experience if not always entirely unexpected. . . . The cardinal rule in scientific enquiry is that observations should be taken in a way which would lead others to obtain the same results. (Smith 1975, p. 16)

This is a very clear statement of the inductive method which Harvey rightly rejects, and which Popper, in particular, has argued against (Popper 1959). It begins with the tabula rasa notion of knowledge, which asserts the primacy of raw observation, or sensory experience; and yet this is an epistemologically naive idea that cannot be supported. The work of Piaget (1970, 1971), in particular, makes it clear that structures of knowledge are prior to experience (see section 4.2.1); and, indeed, which certain aspects of the environment

might be identified as worthy of attention. Yet, for Smith, these are simply two different alternatives, each equally legitimate:

> Geographers . . . may begin with the collection of facts on a concept or aspect of life which arouses curiosity, then seek empirical or observed relationships which might be expressed as laws, and finally attempt to put these together with other laws to form theory. . . . Others begin with some kind of theory, use it to generate hypotheses or possible explanations for geographical matters, and then test these against observations of the real world. . . . This gives the scientist an alternative to the random collection of facts about everything and anything. . . . It is society's need for <u>useful</u> knowledge . . . that generates criteria of relevance for the selection of the research topic. (Smith 1975, pp. 36-37)

Yet these are not equally legitimate alternatives: only the second can be validly argued. The inductive route to scientific explanation, presented here as a practical and theoretically sound alternative, is an illusion: it was neither advocated by its alleged founder, Bacon (Urbach, 1982), nor practised by those who, like Darwin, publically attested to their adherence to its principles (Gruber 1974). In consequence, Smith's approach to the gathering and analysis of geographical data lack credibility on this fundamental point: it proceeds through description, classification and measurement without resolving the ambiguity, and without explicitly recognizing the theory-laden nature of these operations, except where they become critical, as in the concept of quality of life (Smith 1975, pp. 40-42). And yet, in attempting to separate "numerical methods and quantitative geography" from approaches concerned with "developing theory and building models," he states correctly, following Harvey, that

> All quantitative analysis takes place within the context of some kind of theory or model, explicit or otherwise, for this is what sets the measurement process in motion (Smith 1975, pp. 43-44);

a notion which is elaborated in the conclusion to the book (Smith 1975, pp. 348-349):

> Description in human geography perhaps achieves its highest scientific purpose when the discovery of a particular spatial pattern logically verifies some causal process thought to be at work, in a test of an observed pattern against a theoretical expectation.

Yet even here the confusion continues; for if Smith is adhering, as he here seems to be, to the a priori model advocated by Popper and Hempel, verification is a concept which is not acceptable without substantial qualification, since again it is based on that inductive logic which Popper rejects. In summary, it is clear that the conceptual basis employed by Smith fails to distinguish satisfactorily between inductive and deductive modes of reasoning, and therefore fails to provide the consideration of numerical methods in human geography with a secure and valid conceptual foundation. In addition, inasmuch as this confusion arises within the context of an acceptance of Harvey's approach, it inherits the inadequacies summarized in the preceding discussion, and compounds them. We may conclude, therefore, as with Cole and King, that all the techniques offered by Smith are fundamentally flawed by the conceptual confusion surrounding their interpretation in the context of explanation; and that, in consequence, Smith's attempt to elaborate the technical basis of quantitative human geography obscures more than it clarifies.

Amedeo and Golledge (1975) place their account of scientific reasoning in geography firmly on the account of science given by Mario Bunge (1967). We may first note that the title of the book is a misnomer: there is relatively little that deals with scientific "reasoning," though there is much that deals with different aspects of "theory" within the context of a particular view of science. About that view we may go on to note that it defines science as the method of stating and testing hypotheses, which, through increasing confirmation, achieve the status of laws; and then, in conjunction with other laws of similar status, can be used to build theory from which they can be

deduced (Amedeo and Golledge, ch. 1). None of the substantive problems raised to this model by Popper, much less by Kuhn, are alluded to in this simplistic account; indeed, even Harvey (1969a) is mentioned only in three footnotes, in relatively trivial contexts, and in consequence even his confused view of possible alternatives is not represented. We look in vain for the main figures who have contributed to the deepest understanding of the nature of science over the last decades: Popper figures in one footnote; Nagel in half a dozen; Hempel, Feyerabend, Toulmin, Lakatos, and Kuhn not at all.

The resulting discussion of theory construction, of models, of measurement, and of reasoning in geography (almost exclusively human geography) is thus characteristic of that kind of reasoning which the discipline has often regarded, during the last two decades, as "scientific": but it cannot legitimately claim to be so, in terms of the understanding of science outside the discipline, unless the fundamental issues raised by philosophers of science are addressed; or, at the very least, referred to in such a way that the authors' position on these matters is clear, and is taken clear account of in the account of scientific reasoning in geography that follows. There is even less excuse for Amedeo and Golledge than there is for Harvey, since the six years between their publication had done much to clarify the main positions in the debate. It is difficult to understand how a work such as this, which seems to have as a primary aim the refinement and development of the scientific thrust of human geography to meet the growing discontent with it within the discipline (Guelke 1971; Buttimer 1974), could ignore so completely a debate which is fundamental not only to the conduct of science, but to the intellectual mores of modern academic work as a whole. Nevertheless, the deficiency is inescapable, and the work is irrevocably damaged by it.

These examples may suffice to illustrate the point at issue: that while ostensibly adopting scientific modes of explanation and investigation, human geographers have not approached the idea of science itself with any depth or rigour; nor have they attempted to come to terms with the contemporary debate on the nature of science which has raised fundamental questions about the nature of scientific enquiry with which they wish to be identified. The views of the authors discussed above exemplify the main notions about science held by geographers of the quantitative tradition during the last two decades: and we may look to further examples from the late 1960's (Berry and Marble 1968; Morrill 1970), when the spatial processes model was unchallenged; or from the early 1970's, when the behavioural grounds of such processes began to be sought (Abler, Adams, and Gould 1971; Cox 1972; Kariel and Kariel 1972; Lloyd and Dicken 1972); or from recent times, in which statistical, spatial, and behavioural models are still strongly being developed (Wrigley 1979; Parkes and Thrift 1980; Gold 1980; Cliff and Ord 1981). All suggest that the reluctance of human geographers to deal with these fundamental notions concerning science has remained relatively constant over this time; and that, while notable advances in techniques of analysis have been made, the context of explanation and understanding of those techniques remains confused, and the status of human geographic theory is therefore entirely obscure.

It is evident that there is no quick and easy way to come to that understanding of science and scientific method which, it now appears, is necessary to establish human geography, as a science, on a valid and fertile foundation. The danger of attempting a brief overview of the contemporary debate in the philosophy of science is well illustrated in Harvey (1969a), whose account, while admittedly one of the very few undertaken by geographers, is, as we have seen, so superficial and confused as to compound the conceptual

difficulties, rather than to resolve them. We must therefore conclude that only a systematic approach to the various positions of the debate can hope to understand them in any depth, and seek a valid accommodation between them. As geographers, unfamiliar with philosophical modes of thinking and concerns, such a conclusion may be unwelcome; but it cannot be avoided without continuing to place the discipline, and our own work within it, at risk. We now, therefore, embark on such an investigation of science and scientific method, in the hope that through it a secure conceptual foundation for scientific human geography can be established, or at least outlined; and we accept that if the attempt is not successful, it will be necessary to seek those foundations outside the structure of science altogether.

2.2 The hypothetico-deductive model of scientific enquiry

We begin our analysis of science and the scientific method with an account of that model of scientific enquiry which has been, since the publication of Popper's Logik der Forschung in 1935 (Popper 1959), the prevailing orthodoxy; and which, despite its critique by Kuhn, Feyerabend, Lakatos and others, is still the model most appealed to by those who seek to place their work on a scientific foundation. It is based on an assertion of the invalidity of inductive inference, and attempts to construct both the process and the product of science in terms of deductive inference. In some respects, it can be equated with the "standard model" that Harvey (1969a, ch. 4) espouses, although, as we shall see, it has much deeper philosophical roots than the simplistic algorithm described by him. It is a model which, as Lakatos (1970) points out, has been widely misinterpreted, and which Popper developed through a number of phases. In this section we are concerned with his attempt to place scientific enquiry on methodologically sound foundations: the more sophisticated account subsequently developed by him is outlined in section 2.5. The model described here has been elaborated by a great many writers including, in addition to Popper (1959; 1963a; 1972a; 1976), Hempel (1965; 1966), Nagel (1961), Toulmin (1953), and Smart (1968), from whom some illuminating perspectives are drawn. The following brief account of its essential features focuses in the main on its logical structure, and attempts to elucidate it in sufficient detail and with sufficient clarity to make Kuhn's critique and the subsequent debate intelligible. This limited aim, we may feel, is, despite Harvey's pessimism about the practicality of enterprises of this sort (Harvey 1969, p. 10), an achievable one, even within the confines of a dissertation.

2.2.1 The limits of science

The domain of science is examined at some length by Popper (1959, ch. 1 and 2). He seeks a criterion by which empirical sciences may be distinguished from metaphysical systems (including mathematics and logic): the problem of finding this criterion he terms the problem of demarcation. Having rejected the positivist approach, with its dependence on inductive logic, as reducing science to metaphysical speculation, Popper proposes a convention of demarcation:

> . . . I shall require (of a scientific system) that its logical form shall be such that it is singled out, by means of empirical tests, in a negative sense: it must be possible for an empirical scientific system to be refuted by experience. (Popper 1959, p. 41)

Thus it is not the verifiability of a system that matters: Popper never regards a proposition as finally verified and rejects inductive logic out of hand. Deductive logic, empirical testing, and falsifiability are for him the hallmarks of a truly scientific system.

It is clear, therefore, that although the work of the scientist is based on "his desire to gain ever wider knowledge and ever deeper understanding of the world in which he finds himself" (Hempel 1966, p. 2), there is no question of reproducing that world in its entirety. Scientific enquiry seeks out those aspects of the world that are objective (or, in Popper's phrase, "intersubjectively testable" (Popper 1959, p. 44)), whose relationships are describable in terms of deductive logic, and theories which are capable of being refuted. It rests on the assumption that the world is orderly, in the sense that relations between its elements are logical and predictable; and that this orderliness is a primary characteristic of the world. It is thus a grid through which reality can be viewed--a useful grid, without question, as its

striking explanatory successes and expanding applications show; but still only one of an infinite possible number.

2.2.2 Deductive inference

Scientific endeavour is, on this view, reductionist in outlook. It aims to reduce the apparent randomness of the world to some kind of order. That is, it aims to derive a limited number of principles from which a greater number of actual events can be deductively explained or predicted. Deducibility is "the proof of a logical truth (as) consisting in derivation from self-evident beginnings by self-evident steps" (Quine and Ullian 1970, p. 25). Deducibility leads to the systematization of knowledge:

> All knowledge that we possess can be formulated in propositions, and these propositions are composed of terms. In any science, some propositions can be deduced from or proved on the basis of other propositions. . . . Thus one important relationship among the propositions of a science is deducibility. Propositions that embody knowledge about a subject become a science of that subject when they are arranged or ordered by displaying some of them as conclusions deduced from others. (Copi 1965, pp. 170-171)

Not all terms can be defined, and not all propositions can be proved, without resorting to circular reasoning:

> The ideal of science, then . . . is one in which a minimum number of terms suffice for the deduction of all the rest, and a minimum number of terms suffice for the definition of all the others. This ideal of knowledge is described as a <u>deductive</u> system. (Copi 1965, p. 172)

Two procedures for constructing deductive systems are usually identified (Harvey 1969a, ch. 4). The first, which is said to express the view of philosophers of science since Francis Bacon is the <u>a posteriori</u> approach (Harvey 1969a, p. 34). In it, the hierarchical form of idealized deductive

explanation is transformed into a method. Perceptual experiences give rise to unordered facts, which on subjection to processes of definition, classification, and measurement, become ordered facts. Inductive inference turns ordered facts into laws and theories. In this approach, deductive explanation proceeds from inductively derived laws.

As a model of how scientific laws are derived, the a posteriori approach seems to be inadequate. Two points are of significance. The first is that scientists never come free of preconceptions to the data they are to examine. Apart from the innate structuring mechanisms of perception and understanding, attitudes and values come with the observer and cannot be easily laid aside:

> . . . conjecture or hypothesis must come before observation or perception: we have inborn expectations; we have latent inborn knowledge . . . (Popper 1976, p. 52)

The second is that the derivation of a law from ordered facts, whatever they are, is wholly dependent on inductive inference, which is a logically questionable procedure.

The evident fact that scientists come to their data with something already in mind is the basis of the a priori model of scientific enquiry. Harvey characterizes it thus:

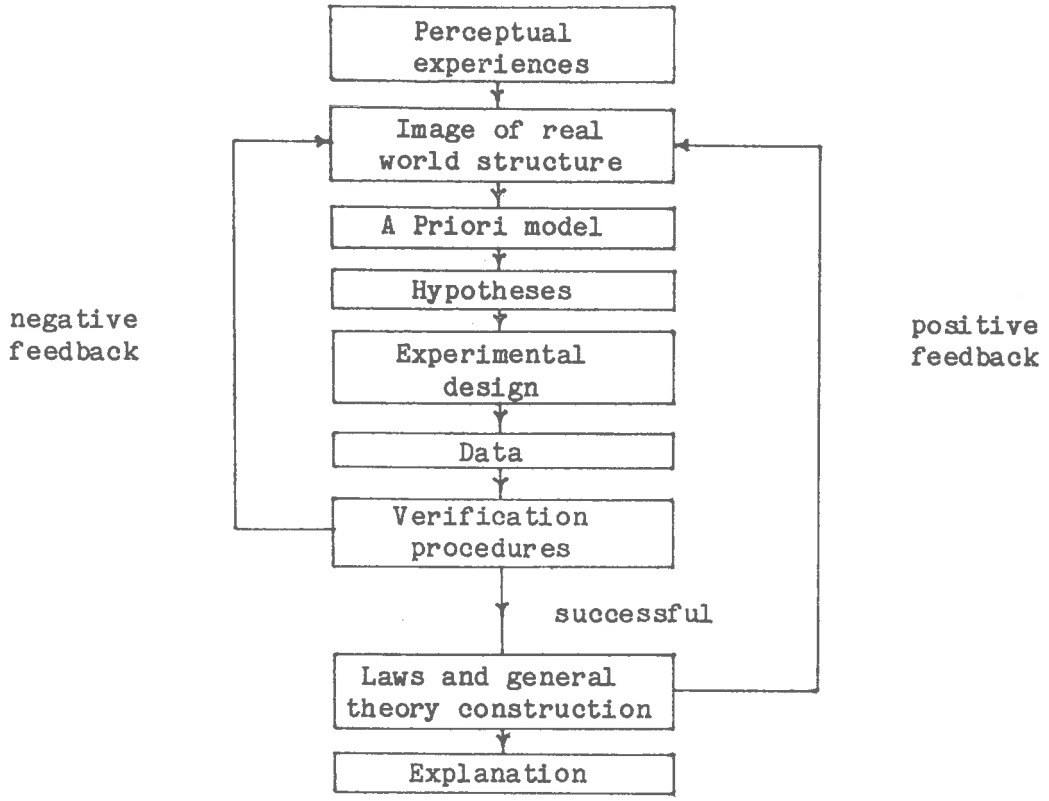

Figure 1. A Priori Scientific Enquiry
(after Harvey 1969a, p. 34)

There are a number of important features of this approach. Firstly, once the a priori model has been constructed, the methodology of investigation proceeds deductively. Thus the derivation of both laws and explanation is deductive.

Secondly, the steps leading to the formation of the model may be accidental, or imaginative, or intuitive, or inductive: they do not affect the logical rigour of the enquiry.

Thirdly, the verification procedures feed back both positively and negatively to that stage which precedes the formation of the model. In the case of negative feedback (refutation of the hypothesis by testing) the only way forward again is to construct a new model: the original is said to have been eliminated.

This last accords well with Popper's views; although in the above diagram he would want to substitute "testing" for "verification," and to deny the possibility of positive feedback. For Popper, the progress of science consists

> not . . . in the accumulation of observations but in the overthrow of less good theories and their replacement by better theories, in particular by theories of greater content. (Popper 1976, p. 79)

Thus once a theory (or model) is falsified, it is gone for good, and must be replaced by a better one. Falsifiability enables the direct comparison of theories:

> . . . the more a theory says, the more it excludes or forbids, and the greater are the opportunities for falsifying it. So a theory with a greater content is one which can be more severely tested. (Popper 1976, p. 79)

Such an approach to the logic of scientific enquiry leads to competition between theories: an almost Darwinian struggle for survival in the face of the most hostile environment that the scientist can provide.

2.2.3 Inductive inference

Inductive inferences are those which lead "from premises about particular cases to a conclusion that has the character of a general law or principle" (Hempel 1966, p. 10). In contrast to deductive inference, in inductive argument the truth of the premises is no guarantee of the truth of the conclusion. However, repetition is to provide the strength that induction lacks on logical grounds. For Popper the problem of induction--that is, whether or not inductive inferences are valid--is simply solved:

> . . . there is no induction, because universal theories are not deducible from singular statements . . . we never argue from facts to theories, unless by way of refutation or falsification. (Popper 1976, p. 86)

It is true that inductive reasoning is not valid: all valid reasoning is deductive. But it may still be true (as opposed to valid), and to dismiss induction out of hand would seem to be to discard a technique of value. Stebbing (1952, p. 166) comments:

> If we were confined to deductive reasoning we should be gravely inconvenienced. . . . Generalization (i.e., going beyond the evidence) is essential to carrying on the affairs of our daily lives; it lies at the very foundation of all the empirical sciences.

The two views are, however, not contradictory; and Popper himself makes this clear:

> . . . there can be no critical phase without a preceding dogmatic phase, a phase in which something--an expectation, a regularity of behaviour--is formed, so that error elimination can begin to work on it. (Popper 1976, p. 51)

In terms of Harvey's schema, induction is the characteristic mode of the first three stages, in which, on the basis of direct experience, an image of the real world develops and is transformed into a testable model. Thereafter the reasoning is deductive. To extend the Darwinian analogy, induction belongs to the creative, diversifying phase of enquiry: deduction to the selective, critical phase. One is essential to the other. There is no contradiction, because they operate independently.

2.2.4 Hypotheses and models

In the conventional view, a hypothesis is a formulation put forward for testing: if it passes the test, and continues to be confirmed in subsequent tests, it achieves the status of a theory, and ultimately, a law. However, for Popper repetition has no logical power, and inductive reasoning no logical

validity. This leads him to reject the logical difference between hypotheses, theories, and laws, since all may be falsified on the next test:

> . . . scientific theories, if they are not falsified, for ever remain hypotheses or conjectures. (Popper 1976, p. 79)

Accepting this point, however, we will still use the term "hypothesis" to denote a generalized statement arrived at inductively, as yet not subjected to empirical test, and adopted "because it would explain, if it were true, some things that [one] already believes" (Quine and Ullian 1970, p. 43).

A hypothesis is framed on the basis of past experience with the intention of using it to predict the future. From a logical point of view, a hypothesis can be any belief, however implausible; but experience has shown that particular attributes seem to be associated with the most successful: conservatism, generality, simplicity, and refutability. The further formalization of the hypothesis will produce a model:

> A model for a theory consists of an alternative interpretation of the same calculus of which the theory itself is an interpretation. (Rudner 1966, p. 24)

The two interpreted theories are thus isomorphic: one is the model of the other. An empirical theory may have either an empirical model, or a mathematical or logical one. The importance of the first lies in the greater familiarity and ease of conception it may have; of the second, in the degree of its deductive elaboration.

A model helps firstly to make precise what may be vague, and thereby to imply some testable situation. Secondly, we can take advantage of a particular calculus or language which has already been explored in some depth, and which therefore provides a flexible framework for the handling of the data.

The process of using a model is as follows:

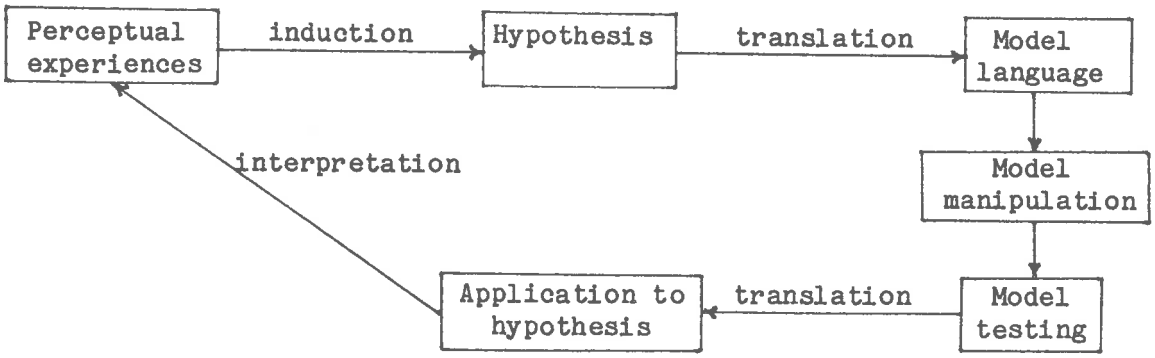

Figure 2. Model Use
(adapted from Harvey 1969a, p. 184)

Clearly the critical phases of this procedure are those of translation. To begin with, there is the problem of identification: to what extent is the model a unique representation of the hypothesis? Ideally, there should be one and only one theoretical explanation of the model. If there are more, the model is overidentified: if there are none, it is underidentified (Harvey 1969a, ch. 10).

Secondly, it is necessary to decide how far the manipulation of the model is legitimate manipulation of the hypothesis. We choose a model largely for its ease of manipulation, but it is essential to demonstrate the isomorphism of model and theory. Moreover, it is not legitimate to take extrinsic information from the domain of the model and translate it into the domain of the theory. In an empirical model this information may relate to the properties of the model substance (such as water): in an analytic model, it may relate to the logical necessity of the statements of the model. Popper, for one, believes that models are no way to full understanding:

> According to this view what matters is the understanding not of pictures but of the logical force of a theory: its explanatory power, its relation to the relevant problems and to other theories. (Popper 1976, p. 93)

2.2.5 Definition

A hypothesis has been articulated, or a model constructed, and now has to be subjected to the demands of the real world. The step which links theory and practice is that of definition. This can be of two types: descriptive (or analytical), by stating the accepted meaning of a term in use; or stipulative, by assigning a special meaning to a new term (Hempel 1966, pp. 85-86).

In practice, definitions do not occur in isolation, but are coordinated in deductive systems. Clearly it is not possible to define every term in the system without resorting to circularity. In all deductive systems, therefore, some of its terms are stated, but not defined: they are the primitive terms, and all other terms are defined on them. Where possible, primitives are selected on the basis of experimentally testable reference.

This point is of particular importance in the construction of a scientific hypothesis (or theory). Obviously definition of terms, if carefully devised, will play a key role in the formulation of a theory; but it is in no way sufficient to give the terms empirical content, and hence make them applicable to the real world:

> For that purpose we need statements that specify the meanings of theoretical terms by means of expressions that are already understood and can be used without reference to the theory. (Hempel 1966, p. 88)

Such a statement is an interpretative sentence.

The procedure of definition is necessary to any further step because it singles out more precisely the relevant portion of the world. The set of individuals, objects, or events with which a scientific enquiry is concerned is the domain of that enquiry. We then proceed to gather the facts about the elements of that domain.

It is important to note that the nature of this gathering varies according to which model of scientific enquiry, a posteriori or a priori, is accepted. In the former, facts are assembled without reference to any predetermined hypothesis or conjecture. In practice this is very difficult, if not impossible; if literally carried out, it would lead to a never-ending mass of information without direction and without organization:

> . . . the maxim that data should be gathered without guidance by antecedent hypotheses about the connections among the facts under study is self-defeating and it is certainly not followed in scientific enquiry. On the contrary, tentative hypotheses are needed to give direction to a scientific investigation. Such hypotheses determine, among other things, what data should be collected at a given point in a scientific investigation. (Hempel 1966, p. 13)

2.2.6 Testing, confirmation, and corroboration

A theory or hypothesis or model may be constructed according to any of the above avenues; but the final demand made of it will always be whether or not it fits the facts. The procedure by which this demand is made is testing. What is normally tested are the implications of a hypothesis for a particular situation; or more formally: "If conditions of kind C are realized, then an event of kind E will occur" (Hempel 1966, p. 19). Commonly we look for crucial tests, which can distinguish between rival hypotheses. This schemata is normally associated with the idea of experimentation. But it is important to note that its logic holds just as well for nonexperimental testing, where we simply wait for the conditions C to occur in situ, and then check to see whether the predicted outcome E does in fact occur.

Furthermore, the notion of experimental control can be misleading. It is said to mean that only one of the qualities dealt with by the hypothesis is

varied at a time, while all other qualities are kept constant. As Hempel (1966, p. 21) points out, this is impossible: many factors--such as vibration of the building, cosmic ray bombardment, and relative humidity--may vary during the experiment. But, he goes on, there is no reason in logic why these factors should be controlled. The conditional form "if C then E" carries with it the implication that C is both necessary and sufficient for the occurrence of E: other factors are said to be irrelevant. By allowing such factors to vary, we are in fact exploring a wider range of cases in search of possible refutation of the hypothesis under test.

Testability (or, in Popper's more precise terminology, falsifiability) is the hallmark of a scientific hypothesis:

> . . . no statement or set of statements T can be significantly proposed as a scientific hypothesis unless it is amenable to objective empirical test, at least "in principle." (Hempel 1966, p. 30)

T, in other words, must have test implications, as defined above. If it has none, it is said to lack empirical import.

Popper's notion of falsifiability gives greater significance to this term. To begin with,

> A theory is falsifiable . . . if the class of its potential falsifiers is not empty. (Popper 1959, p. 112)

This definition allows a comparison between theories:

> . . . if the class of potential falsifiers of one theory is "larger" than that of another, there will be more opportunities for the first theory to be refuted by experience. . . . the first theory says more about the world of experience than the second theory, for it rules out a larger class of basic statements. (Popper 1959, pp. 112-113)

Thus the relationship between falsifiability and empirical import (or content) can be derived:

> . . . the amount of empirical information conveyed by a theory, or its empirical content, increases with its degree of falsifiability. (Popper 1959, p. 113)

For Popper, therefore, there is an equivalence between empirical and logical content: to compare their empirical content is to compare their degrees of falsifiability. This results in the methodological rule

> . . . that those theories should be given preference which can be the most severely tested

being equivalent to

> favouring theories with the highest possible empirical content. (Popper 1959, p. 121)

The question which now arises is whether testing does anything more than provide refutations of unjustified theories. It seems necessary to accept the fact that, as pointed out in section 4, no matter how stringent the testing or how long it goes on, no theoretic formulation can ever in logic be conclusively proved. In practice, as Popper himself points out (Popper 1959, p. 252), although it is logically possible, it rarely happens that one day an old experiment suddenly produces new results: scientific method assumes that natural processes, and regularities directly tested by experiment, do not change. What does happen is that a new experiment falsifies an old theory; which, even when superseded, often retains its validity as a limiting case of the new theory. Furthermore, Popper accepts that we can speak of theories as better, or less well, corroborated; and he recognizes that the judgement is subjective:

> I speak of the <u>corroboration</u> of a theory; and corroboration can only be expressed as an appraisal. (Popper 1959, p. 265)

But for Popper corroboration has nothing to do with the number of favourable outcomes of a test implication of a hypothesis: it is rather a measure of the severity to which a hypothesis is subjected. The severity of the testing, or the degree of corroboration, cannot be formalized; as, for instance, a statement of the number of tests that have been carried out. Rather,

> . . . the idea was to sum up, in a short formula, a report of the manner in which a theory has passed--or not passed--its tests, including an evaluation of the severity of the tests: only tests undertaken in a critical spirit--attempted refutations--should count. By passing such tests, a theory may "prove its mettle"--its "fitness to survive." (Popper 1976, p. 103)

Thus corroboration depends upon a subjective idea:

> . . . the notion of the sincerity of the experimenter or observer and of his theoretical colleagues who ask him to perform certain observations: they must be actively and sincerely trying to test the theory in those circumstances in which they judge it least likely to succeed, as well as those cases in which success is expected. (Smart 1966, p. 198)

Both theories and non-theoretic formulations in Hempel's view, are said to be able to receive support from other sources. The relation of a hypothesis to a body of theory is held to be of significance. It can be direct support from above:

> . . . that is, from more inclusive hypotheses or theories that imply the given one and have independent evidential support. (Hempel 1966, p. 38)

This is essentially a deductive relation. Other things being equal, we will tend to favour those hypotheses which accord with what we already believe.

A second source of general support for one hypothesis as against another may come from its simplicity. Popper equates simplicity with degree of falsifiability:

> The degree of universality and precision increases with its degree of falsifiability, as we have seen. Thus we may perhaps identify the degree of strictness of a theory--the degree, as it were, to which a theory imposes the rigour of law upon nature--with its degree of falsifiability. . . (Popper 1959, p. 141)

The virtue of this notion is that it brings the nebulous idea of simplicity within the ambit of rigorous methodological demands, by linking it to the previously defined concepts of testability and empirical content:

> Simple statements, if knowledge is our object, are to be prized more highly than less simple ones because they tell us more; because their empirical content is greater; and because they are better testable. (Popper 1959, p. 149)

2.2.7 Laws and theories

Despite the fact that it is said not to be possible by logic to distinguish between a hypothesis, a law, and a theory, in practice such distinction is often, and usefully, made. A hypothesis has been defined as a generalized statement arrived at inductively, and not yet subjected to empirical test. A law can be defined as "a universal statement of unrestricted range" (Harvey 1969a, p. 101); and, in practice, a law has usually been subjected to empirical testing, and is known to hold to at least a certain degree.

Suppose that we have formulated a hypothesis, tested it, and pronounced it a law, in the manner outlined in the preceding sections. We must now ask just what we have achieved.

Central to scientific enquiry is the notion of explanation. Scientific explanation is said to be causal, and involves a prediction:

> The explanation fits the phenomena to be explained into a pattern of uniformities and shows that its occurrence was to be expected, given the specified laws and the pertinent particular circumstances. (Hempel 1966, p. 50)

This form of explanation, usually termed the deductive-nomological pattern of explanation, can be represented by the following schema (Smart 1968, p. 59):

$L_1, L_2, \ldots\ldots, L_r$	universal laws
$C_1, C_2, \ldots\ldots, C_k$	initial conditions

E	event to be explained

L_1, L_2, ..., L_r are the universal statements, hypotheses of the character of natural laws, which do not contain names of definite descriptions. C_1, C_2, ..., C_k, on the other hand, are singular statements, which apply to the specific event in particular regions of space and time. From these two sets of statements, the event can be logically (deductively) predicted. If the prediction is confirmed by testing, we may feel justified in saying that the initial conditions were the cause of the event (effect), and that we have therefore explained it.

It should be noted that even if one is to seek this form of explanation, it is not necessary to accept the philosophy of a principle of causality: that is, "the assertion that any event whatsoever <u>can</u> be causally explained--that it can be deductively predicted" (Popper 1959, p. 61). It is possible to make instead a methodological decision:

> . . . that we are not to abandon the search for universal laws and for a coherent theoretical system, not ever give up our attempts to explain causally any kind of event we can describe. (Popper 1959, p. 61)

Popper further adopts the convention that all natural laws be regarded as strictly universal, and defined as

> non-verifiable statements which can be put in the form: "Of all points in space and time (or in all regions of space and time) it is true that . . ." Statements relating only to finite regions of space and time then become "specific" or "singular" statements. (Popper 1959, p. 63)

In contrast to deductive-nomological explanation is statistical or probabilistic explanation. Two sorts of statistical explanation can be distinguished (Smart 1968, p. 65). The first is deductive-statistical: this form applies particularly in modern quantum theory, where many of the fundamental laws of nature are said to be statistical, and where explanation

therefore reduces, despite the probabilistic nature of the laws, to the deductive-nomological form.

This differs from the inductive-statistical form, which can be represented by the following schema:

If A ∈ S, it is highly probable that A ∈ T

A ∈ S
---------- [makes highly probable]

A ∈ T

(Smart 1968, p. 65)

This has the same structure as the deductive-nomological form: a general statement in conjunction with a specific statement yielding a conclusion. However, in inductive-statistical explanation, the laws are of probabilistic, rather than universal, form: and the event is predicted with only near, rather than deductive, certainty. Three schools of interpretation of probability are commonly distinguished (Popper 1959, pp. 148-149). The first is the subjective view, which regards probability as the degree of confidence that we may have in the truth of a particular proposition. On this view, probability measures certainty or uncertainty, belief or doubt, of a particular individual. The second is the logical view, which treats probability as a special kind of logical relationship between two statements, and measures the extent to which one may be said to imply the other. This is allied to the subjective view because degree of rational belief is involved.

The third is the objective view, which

> . . . treats every numerical probability statement as a statement about the relative frequency with which an event of a certain kind occurs within a sequence of occurrences. (Popper 1959, p. 149)

On this view, probability is an operational notion, which can be determined by making a long series of tests and counting the number of favourable outcomes. It is not expected that in any given point of the series the relative frequency will precisely equal the probability, but it is predicted that this difference will become progressively smaller as the tests continue.

Science has achieved many striking predicted successes with probabilistic laws. Yet how is it possible to regard our hypothesis as definitely refuted? In a finite series of tests some deviation of observed from predicted frequencies is expected. According to the relative frequency view,

> . . . probability statements will not be falsifiable. Probability hypotheses do not rule out anything observable; probability estimates cannot contradict, or be contradicted by, a basic statement; nor can they be contradicted by a conjunction of any finite number of basic statements; and accordingly not by any finite number of observations either. (Popper 1959, pp. 189-190)

Clearly, therefore, a methodological decision has to be made by which highly improbable events are ruled out; and (as Hempel would say, but Popper would not) highly probable events accepted. The decision is a matter of personal choice, and can involve standards of any degree of strictness. Popper adopts the convention that:

> If . . . we find reproducible deviations from a macro effect which has been deduced from a probability estimate . . . then we must assume that the probability estimate is falsified. (Popper 1959, p. 203)

Hempel (1966, p. 65) points out that the nature of the methodological rule chosen will depend largely on how one views the types of error it is possible to make:

	Null H true	Alternative H true
Reject null H	Type I error	Correct decision
Accept null H	Correct decision	Type II error

Table 1. Types of Error
(after Harvey 1969a, p. 252)

Obviously Popper's rule is primarily directed towards avoiding Type I error.

It is important to note that probabilistic laws differ from universal laws by virtue of their logical force and explanatory power, not of their evidential support. It could be said, for instance, that even universal laws are probabilistic, since (as pointed out above) they can always be falsified by a subsequent test. However, this has nothing to do with the fundamental distinction:

> A law of universal form is basically a statement to the effect that in all cases where conditions of kind F are realized, conditions of kind G are realized as well; a law of probabilistic form asserts basically, that under certain conditions, constituting the performance of a random experiment R, a certain kind of outcome will occur in a specified percentage of cases. (Hempel 1966, p. 66)

When a set of laws is systematically related, and has achieved a certain credibility, it can then be referred to as a theory. Hempel (1966, pp. 72-73) distinguishes two types of principles in the formulation of theory; internal, and bridge, principles:

> The former will characterize the basic entities and processes involved by the theory and the laws to which they are assumed to conform. The latter will indicate how the processes envisaged by the theory are related to the phenomena with which we are already acquainted, and which the theory may then explain, predict, or retrodict.

Bridge principles, in other words, encapsulate the explanatory power of a theory. They are often directly measurable or observable, but need not necessarily be so: they can be couched in the terms of antecedently established theories, and presuppose its principles.

Thus organized, a good theory is said to deepen understanding in three ways (Hempel 1966, pp. 75-77). Firstly, it will provide a systematic unification of apparently diverse phenomena. Secondly, it will expand the range of previously established theories, without detracting from their validity within that range. Thirdly, it will predict and explain new phenomena as they are discovered. Thus

> Theories are nets cast to catch what we call "the world": to rationalize, to explain, and to master it. We endeavour to make the mesh ever finer and finer. (Popper 1959, p. 59)

The best theories are therefore those which are most completely formalized; that is, which fulfill the requirements necessary to make them axiomatic systems. There is a good reason for this. Only by making rigorous the logical relations between the elements of the system can the results of testing be assessed with certainty:

> . . . a severe test of a system presupposes that it is at the time sufficiently definite and final in form to make it impossible for new assumptions to be smuggled in . . . they system must be formulated sufficiently clearly and definitely to make every new assumption easily recognizable for what it is: a modification and therefore a revision of the system. (Popper 1959, p. 71)

Furthermore, if the logical relationships between elements of a theoretical system are clear enough, it may be possible to regard a negative test result as falsifying only one part of the system, rather than the whole.

Finally, it should be made clear that some disciplines normally regarded as scientific, such as descriptive biology (Smart 1968, ch. 4), do not give

their statements the character of laws, nor their theories full deductive elaboration. The crucial distinction between the generalizations of natural history and laws of nature lies in the difference that a counter example makes to them. In the case of a law falsification is said to be complete with even one refutation: in the case of a generalization of natural history, a refutation simply becomes an exception to an otherwise accepted general rule. Furthermore, such generalizations are not universal:

> . . . any generalization of descriptive biology is part of the natural history of the earth, and in most cases it will not be true unless it is qualified by some explicit or implicit reference to our planet. Otherwise it is quite probably falsified by some phenomenon of extra-terrestrial natural history. (Smart 1968, p. 96)

In such a situation, if one wishes to attain a greater degree of explanatory power, it seems that two strategies are available. The first is to attempt to perceive in biological phenomena the action of physical laws that are well established: this is the process of reduction. The second is to wait until better theories about biological mechanisms arise, so that formalization can begin, and test implications be derived. Otherwise one simply has to accept that biological phenomena are not, by their very nature, amenable to scientific enquiry, and rest content with description and generalization.

2.3 Kuhn's view of scientific enquiry

It is evident that in the foregoing account of scientific enquiry there are a number of areas in which the logic of deduction is inadequate to carry the enquiry forward. To begin with, the effectiveness of the hypothetico-deductive approach espoused by Popper and Hempel is fundamentally dependent on the fertility of the hypotheses that are to be submitted to critical testing; and yet the subjective processes by which such hypotheses are developed--processes which may be, but are not necessarily, inductive and systematic--are not dealt with under this view, beyond a reference to the "dogmatic phase" (Popper 1976, p. 51), "the free creations of our own minds, the result of an almost poetic intuition" (Popper 1963, p. 192), which must of necessity precede critical enquiry.

The essential role of subjectivity in this ostensibly deductive approach does not end there. Interpretative sentences, which operationalize the hypothesis and allow for its testing, must be constructed; and that construction must proceed through the selection and use of theories and terms available outside the domain of the hypothesis itself, a selection which requires subjective judgement. Then the test itself must be devised to meet the predictions made by the hypothesis; and the results of testing must be interpreted, and clearly related to the predicted outcomes: again, both steps require the application of subjective judgement. Even more obviously, we noted the subjectivity of Popper's conception of corroboration, which is nevertheless an essential component in his notion of the rigorous testing, and selecting out, of weaker theories on the basis of their inability to withstand such testing, when compared to competing theories. Finally, the relationships

between hypothesis, law, and theory, the delineation of which is so clearly essential to any conception of the growth of scientific knowledge, are not made, in the Popperian view, on the basis of logic--their role in the deductive logic of scientific enquiry is identical--but, again, through subjective judgement.

If scientific enquiry is to cohere and to progress, in the sense of movement to more general, more simple and more powerful theories, the logical gaps that these areas represent need in some way to be secured; and since that cannot be achieved by the application of objectively demonstrable logic (objective, that is, in the sense of intersubjectively acceptable), subjective approaches may be considered. We conclude, therefore, that without a systematic and predictable account of the operation of subjectivity in these crucial areas, the Popperian claim to be able to establish a logical basis to scientific enquiry is open to serious question. If, furthermore, it could be shown that the operation of subjectivity in science can be delineated systematically; that such a delineation lies outside the domain of logic, the operation of which it frames; then the challenge would be directed not just to the details of logic that are claimed to operate in scientific enquiry, but to the relevance of logic at all. The very foundations of the Popperian view of scientific enquiry would thus be open to challenge.

Such, essentially, was the nature of the challenge launched by T. S. Kuhn in 1962 in a celebrated work (Kuhn 1970a); a challenge which, it is widely felt, has still not been adequately met. Kuhn's account of scientific enquiry is based on a close analysis of the actual historical conditions within which enquiry has taken place. This approach is termed a "historiographic revolution" in methods of approaching science which attempts "to display the

historical integrity of that science in its own time" (Kuhn 1970a, p. 3). It directly challenges the notion of an incremental process in science, and substitutes instead "incommensurable ways of seeing the world and of practicing science in it" (Kuhn 1970a, p. 4). It accepts that science must be based on observation and experience, but in addition insists that the sociology and social psychology of scientists, and even "an apparently arbitrary element, compounded of personal and historical accident" (Kuhn 1970a, p. 4), must be taken into account, with at least equal weight, if an accurate account of scientific enquiry is to be developed. Scientific research, far from being the free and creative enquiry sought by Popper, is seen instead to be

> a strenuous and devoted attempt to force nature into the conceptual boxes supplied by professional education. (Kuhn 1970a, p. 5)

Kuhn's analysis is based on the idea of normal science, defined as

> research firmly based upon one or more past scientific achievements, achievements that some particular scientific community acknowledges for a time as supplying the foundation for its further practice. (Kuhn 1970a, p. 10)

Normal science is seen to be structured by the textbooks, or classic works, which encapsulate such achievements, and to be adopted pervasively by the different arms of the scientific community, in secondary and tertiary education, in research, in publications, and in scientific societies. The scientific activity to which the normal science of a particular time and place gives rise is fundamentally related to a paradigm, a concept not precisely defined by Kuhn, but associated with the observation that

> some accepted examples of actual scientific practice--examples which include law, theory, application, and instrumentation together--provide models from which spring particular coherent traditions of scientific research. (Kuhn 1970a, p. 10)

The essence of a paradigm is, then, said to be a shared commitment to particular rules and standards for the practice of scientific enquiry. Particular paradigms are likely to be preceded by a number of competing "pre-paradigms." one of which eventually comes to dominate, as Kuhn documents through the history of early optics and of early electrical research. He emphasizes, however, that the successful paradigm gains its status not just through its apparently greater success in solving a problem or problems that have come to be recognized as acute, but through

> . . . a promise of success discoverable in selected and still incomplete examples. (Kuhn 1970a, pp. 23-24)

Normal science then becomes a "mopping-up" or "puzzle-solving" activity: it consists in

> . . . the actualization of that promise, an actualization achieved by extending the knowledge of those facts that the paradigm displays as particularly revealing, by increasing the extent of the match between those facts and paradigm's predictions, and by further articulation of the paradigm itself. (Kuhn 1970a, p. 24)

The main foci of normal science are thus said to be, firstly, those facts which the paradigm has shown to be "particularly revealing of the nature of things" (Kuhn 1970a, p. 25); secondly, predictions made by the paradigm theory which have still to be investigated; and thirdly, the empirical articulation and clarification of the paradigm theory. The activity of normal science is thus, in Kuhn's view, fundamentally directed against novelty. The paradigm defines which problems are chosen for investigation, and anticipates the outcome of such investigations: only the method of achieving such outcomes is unknown. In this sense, normal science is described as "puzzle-solving":

> Bringing a normal research problem to a conclusion is achieving the anticipated in a new way, and it requires the solution of all sorts of complex instrumental, conceptual, and mathematical puzzles. (Kuhn 1970a, p. 36)

Thus a paradigm is seen to provide a criterion for which problems are to be recognized as genuinely scientific: namely that, from the structure of explanation provided by the paradigm, they can reasonably be supposed to have solutions. Other problems

> are rejected as metaphysical, as the concern of another discipline, or sometimes as just too problematic to be worth the time. (Kuhn 1970a, p. 37)

Moreover, both the nature of the solutions and the steps by which they are obtained must conform to the established viewpoint of the paradigm. Thus normal science may dictate the paradigmatic statements of law and theory, the types and usage of instrumentation, and "quasi-metaphysical" (Kuhn 1970a, p. 41) commitments to the nature of the universe. However, it is not necessary for the effectiveness of a paridigm that the rules of research which are derived from it be either explicitly codified or explicitly recognized by the scientific community. Indeed, Kuhn argues, the nature of scientific education, in which theory is learnt by application in the context of problem-solving, ensures that the paradigm is usually able to conduct the operation of normal science without such explication, until the paradigms themselves are felt to be insecure:

> When scientists disagree about whether the fundamental problems of their field have been solved, the search for rules gains a function that it does not ordinarily possess. While paradigms remain secure, however, they can function without agreement over rationalization or without any attempted rationalization at all. (Kuhn 1970a, pp. 48-49)

Finally, in his exposition of normal science, Kuhn notes that a general paradigm, such as quantum theory, may act simultaneously, though in markedly different ways, for several groups within the scientific community, whose activities of normal science overlap without being identical: thus a change of

paradigm within one of these spheres will not necessarily be generalized to the others.

In a close textual analysis of Kuhn's work, Masterman (1970) notes that Kuhn's use of the term paradigm is multiple, embracing three categories: metaphysical paradigms, which relate to beliefs about the nature of reality, to ways of seeing the world; sociological paradigms which relate to scientific achievements, and to the judgement of the scientific community; and artefact paradigms, which relate to the accepted procedures of puzzle-solving of normal science. She goes on to point out that, seen from the second of these points of view, a paradigm is

> . . . a set of scientific habits. . . . they may be intellectual, verbal, behavioural, mechanical, technological; any or all of these; it depends on the type of problem being solved. (Masterman 1970, p. 66)

A paradigm may therefore function when no coherent theory exists. For this reason, Masterman argues, Kuhn is able to treat of

> . . . both the rise and fall of science, in the whole process of human beings trying to achieve a scientific explanation,

including not only normal science but the "crude forms and early stages of a science" (Masterman 1970, p. 68). She concludes that a paradigm may be logically described as a "crude analogy" (Masterman 1970, p. 79), an analogy which cannot be developed too far without giving rise to anomalies (the property of finite extensibility); which is not, in principle, able to be compared with any other crude analogy (the property of incommensurability); and which is extensible at all only through a process of inexact replication in which the main features of the analogy are reproduced. In his discussion of Masterman's analysis, Kuhn endorses this description of his views, emphasizing that paradigms may include

> . . . shared symbolic generalizatons . . . shared models . . . shared values . . . and concrete problem solutions. (Kuhn 1970c, pp. 271-271)

It is therefore clear that, in Kuhn's analysis, paradigms operate in a powerfully conservative manner:

> Normal science does not aim at novelties of fact or theory, and, when successful, finds none. (Kuhn 1970a, p. 52)

Nevertheless, as Kuhn goes on to observe, the history of science records the repeated uncovering of new and unpredicted phenomena, a process of such historic consistency that it may reasonably be conjectured whether it is not in the nature of normal science itself, of research within a particular paradigm, that the techniques for producing such novelties may be found. In Kuhn's view, it is just the increasing constriction of scientific activity operating within the paradigm, that gives rise to the discovery of novelties:

> . . . within those areas to which the paradigm directs the attention of the group, normal science leads to a detail of information and to a precision of the observation-theory match that would be achieved in no other way. . . . Without the special apparatus that is constructed mainly for anticipated functions, the results that lead ultimately to novelty could not occur. And even when the apparatus exists, novelty ordinarily emerges only for the man who, knowing <u>with precision</u> what he should expect, is able to recognize that something has gone wrong. (Kuhn 1970a, pp. 64-65)

Such novelties, by definition, will, if accepted, challenge the validity of the prevailing paradigm: they are therefore regarded as anomalies, things that have "gone wrong," and emerge as such only with difficulty after overcoming, by their persistence, substantial psychological resistance to potentially distracting incongruity.

When anomaly persists for some time, and challenges a sufficiently fundamental level of the paradigm, the field in question will, in Kuhn's view, be characterized by a growing awareness of crisis:

> . . . a period of pronounced professional insecurity . . . generated by the persistent failure of the puzzles of normal science to come out as they should. Failure of existing rules is the prelude to a search for new ones. (Kuhn 1970a, pp. 67-68)

However, he goes on to argue, it is only when another paradigm, which explains the anomaly, is sufficiently elaborated to be regarded as a serious alternative that the old paradigm is renounced. Anomalies, in Kuhn's view, are not treated by scientists as the counter-instances that the Popperian view of the logic science demands:

> The decision to reject one paradigm is always simultaneously the decision to accept another, and the judgement leading to that decision involves the comparison of both paradigms with nature and with each other. (Kuhn 1970a, p. 77)

Thus it is not that normal science encounters no anomalies, since the notion of normal science is predicated on the supposition that no paradigm is ever completely resolved; but that, until competing versions of the paradigm emerge, such anomalies are regarded as puzzles, not counter-instances. There are always some descrepancies in the fit between theory and observed fact:

> Even the most stubborn ones usually respond at last to normal practice. Very often scientists are willing to wait, particularly if there are many problems available in other parts of the field. (Kuhn 1970a, p. 81)

What, then, makes an anomaly sufficiently important to engage the attention of normal science, and to plunge the field into growing crisis? To this question, Kuhn gives a variety of possible answers, none fully convincing. He refers to historical examples of a challenge to some fundamental theoretical aspect of the prevailing paradigm; to a particular practical importance of its application; and to the length of time an anomaly may exist. He remarks, somewhat uncertainly,

> Presumably there are still other circumstances that can make an anomaly particularly pressing, and ordinarily several of these will combine. (Kuhn 1970a, p. 82)

Crisis in a field can therefore be resolved either by eventual incorporation of the anomaly within the prevailing paradigm; by being held in abeyance for some future solution, while allowing the activity of normal science within the discipline to proceed; or by transition to a new paradigm. The structure of the new paradigm may or may not be foreshadowed during the period of crisis; but always, in Kuhn's view, it emerges in an intuitive way:

> . . . the new paradigm, or a sufficient hint to permit later articulation, emerges all at once, sometimes in the middle of the night in the mind of a man deeply immersed in crisis. What the nature of that final stage is--how an individual invents (or finds he has invented) a new way of givng order to data now all assembled--must here remain inscrutable and may be permanently so. (Kuhn 1970a, p. 90)

Adoption by the scientific community of a new paradigm is termed by Kuhn a scientific revolution. It is a revolution, rather than evolution (Toulmin 1970) because, in Kuhn's view, a choice has to be made by the practising scientist between what are, as in political revolutions, two essentially incompatible frameworks. The choice is not made on logical grounds, but on the basis of a propensity to accept that scientific practice under the new paradigm is likely to be more attractive, for a variety of reasons. Such an argument

> . . . cannot be made logically or even probabilistically compelling for those who refuse to step into the circle. The premises and values shared by the two parties to a debate over paradigms are not sufficiently extensive for that. . . . there is no standard higher than the assent of the relevant community. (Kuhn 1970a, p. 94)

This position is critical to Kuhn's view of science. It affirms that paradigms, if not always--though sometimes--of themselves identifiable world views, are nevertheless invariably associated with such views:

> . . . when paradigms change, the world itself changes with them. Led by a new paradigm, scientists adopt new instruments and look in new places. Even more important, during revolutions scientists see new and different things when looking with familiar instruments in places they have looked before. (Kuhn 1970a, p. 111)

Kuhn appeals to gestalt psychology for evidence that such world views are prerequisite to the very fact of perception itself; as, he might have added, the results of Piaget's investigations in developmental epistemology have suggested (Piaget 1971). All scientific operations and measurements, he maintains, must be viewed from the standpoint of the particular paradigm in which the scientist performing them is embedded:

> . . . one and the same operation, when it attaches to nature through a different paradigm, can become an index to a quite different aspect of nature's regularity. . . . occasionally the old manipulation in its new role will yield different concrete results. (Kuhn 1970a, p. 131)

It is for this reason, Kuhn argues, that the debates between the protagonists of competing paradigms cannot be concluded by appeals to rational criteria. Adoption of the new paradigm must, on this view, rest on faith. It is not to the record of past achievements that the scientist must look, but to the promise of the future:

> The man who embraces a new paradigm at an early state must often do so in defiance of the evidence provided by problem-solving. He must, that is, have faith that the new paradigm will succeed with the many large problems that confront it, knowing that the older paradigm has failed with a few. A decision of that kind can be made only on faith. (Kuhn 1970a, p. 158)

In time, as the new paradigm is developed and delineated, it may be that rational criteria and unambiguous decisions can be brought to bear in justifying the choice that has been made; but, in its early stages, the adoption of a new paradigm must, Kuhn argues, resemble more religious conversion than scientific discrimination. A number of features of the new paradigm--among them the ability to account for anomalies, greater quantitative precision, the ability to predict new phenomena, or greater simplicity, or any combination of these--may provide overt grounds for the position of the

convert; but, until the new paradigm has been explored and developed to the point where its adoption can rest on rational grounds, its early supporters must of necessity take their positions on other than evidential grounds.

Centrally underlying the inability of proponents of competing paradigms to base their positions solely on rational or evidential support is the concept of incommensurability. It is this concept, too, which forces Kuhn to deny meaning to the idea of the progress of science. The assertion of incommensurability, more than any other element of Kuhn's theory, is recognized by both Kuhn and Popper fundamentally to divide them.

Kuhn derives the notion of incommensurability from an analysis of undisputed historical instances of scientific revolution, such as that which transformed Newtonian dynamics into Einsteinian dynamics. Kuhn challenges the "limiting case" view of this transformation, which asserts that, for velocities that are small compared with that of light, Newton's equations yield good approximations to measurement; and that, therefore, Newtonian dynamics may be viewed as derivable from Einstein's, given a small number of restrictive conditions. On the contrary, Kuhn argues, no such derivation is possible, since the conceptual mapping that is necessary to it cannot be logically developed; space, time, and mass may appear in both theories,

> But the physical referents of these Einsteinian concepts are by no means identical with those of Newtonian concepts that bear the same name . . . in the passage to the limit it is not only the forms of the laws that have changed. Simultaneously we have had to alter the fundamental structural elements of which the universe to which they apply is composed. (Kuhn 1970a, p. 102)

Moreover, Kuhn continues, more is involved in the transformation of one paradigm into another than the phenomena of the theory in question. Because the nature of a paradigm is to direct a particular structure of normal science,

that structure, too, and the definition of relevant scientific problems which derives from it, will change:

> . . . as the problems change, so, often, does the standard that distinguishes a real scientific solution from a more metaphysical speculation, word game, or mathementical play. The normal-scientific tradition that emerges from a scientific revolution is not only incompatible but often actually incommensurable with that which has gone before. (Kuhn 1970a, p. 103)

In summary, then, competing paradigms are incommensurable--that is, not comparable--because they are embedded within traditions of normal science whose standards and definitions of science differ; whose vocabularies and conceptual apparatuses are so divergent as to require translation, or mapping, of the terms of one to the terms of the other; and whose world views are so incompatible that the proponents of each actually see different things when looking, as it were, at the same point from the same direction. It is for these reasons that a scientific revolution does not, in Kuhn's view, find its resolution through the application of rational criteria to theory-choice. Before such application can be made, it is necessary to agree on the inherent meaning and mode of operation of the rules of inference that the criteria embody; and it is precisely these rules, functioning as values in each community of protagonists, which lie at the heart of the dispute. Before the rational criteria can be used to support one theory or the other, therefore, Kuhn argues that a conversion is necessary to one perspective of the rules or the other:

> (The) debate is about premises, and its recourse is to persuasion as a prelude to the possibility of proof. (Kuhn 1970a, p. 199)

It is thus as two language communities that the proponents of competing paradigms confront each other; and the persuasion that is to lead to the

possibility of establishing rational proof is crucially dependent on whether or not an adequate translation can be effected that will allow fruitful communication between the two communities to develop. Such translation is necessary to persuasion, but may not, Kuhn argues, be sufficient to it:

> To translate a theory or a world view into one's own language is not to make it one's own. For that one must go native, discover that one is thinking and working in, not simply translating out of, a language that was previously foreign. (Kuhn 1970a, p. 204)

The end of this reasoning is a direct challenge to the traditional view of the progress of science--of its natural, inevitable incremental growth--that the Popper-Hempel position embodies. For Kuhn, in the broad perspective of history there is no progress, only change. Taking the proposal of progress in gravitational theory as an example, he asserts that

> There are no external standards to permit a judgement of that sort. What occurred was neither a decline nor a raising of standards, but simply a change demanded by the adoption of a new paradigm. Furthermore, that change has since been reversed and could be again. (Kuhn 1970a, p. 108)

There is, he goes on, a substantial degree of circularity in ascribing the property of progress to the enterprise of science:

> To a very great extent, the term "science" is reserved for fields that do progress in obvious ways. (Kuhn 1970a, p. 160)

However, in the light of his analysis of scientific enquiry, progress is held by Kuhn to be a term which only has meaning during periods of normal science, when it relates to success in solving the remaining puzzles of the prevailing paradigm:

> . . . it is only during periods of normal science that progress seems both obvious and assured. During those periods, however, the scientific community could view the fruits of its work in no other way. (Kuhn 1970a, p. 163)

At particular times in the history of science, proponents of the prevailing paradigm may contrive to rewrite the immediate past of its discipline in order to argue an inevitable linear development culminating in the contemporary theory; but that, Kuhn argues, is a natural outcome of the operation of normal science, reinforced by the insulation of the scientific community from the community at large, and by the structure of the normal scientific education, which centres on textbooks rather than on original scientific documents. It is therefore permissible, in Kuhn's eyes, to talk of "evolution-from-what-we-do-know," but not of "evolution-toward-what-we-wish-to-know" (Kuhn 1970a, p. 171). Drawing an analogy with Darwinian theory, he argues that just as, in that theory, organisms which occurred at an earlier stage of phylogenetic development can be distinguished, on a number of descriptive criteria, from those which occurred at a later stage, without making any judgement as to the progress of that development; so too it is possible to compare paradigms on criteria such as predictive accuracy, the emphasis on esoteric or everyday phenomena, and number of different problems solved, without making judgements about possible progress achieved towards an ideal of truth:

> There is . . . no theory-independent way to reconstruct phrases like "really there"; the notion of a match between the ontology of a theory and its "real" counterpart in nature now seems to me illusive in principle. (Kuhn 1970a, p. 206)

Only thus can one compare different paradigms as instruments for puzzle-solving. In these terms Einstein's mechanics improves on Newton's, which in turn improves on Aristotle's; but for Kuhn there is no clear direction towards the true representation of nature in this improvement:

> . . . in some important respects, though by no means in all, Einstein's general theory of relativity is closer to Aristotle's than either of them is to Newton's. (Kuhn 1970a, p. 207)

There can be little doubt of the power of Kuhn's argument, or of its effectiveness in challenging the view of science which prevailed at the time of its emergence. So thorough and so penetrating was its analysis that philosophers of science, if not practitioners of scientific inquiry, widely concede their discipline to have been fundamentally and irreversibly altered by it. Indeed, it has been seen to have implications for the understanding not just of science but of intellectual endeavour in general, and even for art. In commenting on its development, Kuhn cautions against too uncritical an application:

> Though scientific development may resemble that in other fields more closely than has often been supposed, it is also strikingly different. To say, for example, that the sciences, at least after a certain point in their development, progress in a way that other fields no not, cannot have been all wrong, whatever progress itself may be. (Kuhn 1970a, p. 209)

It is not difficult to identify the elements which have made Kuhn's view of science such a success. There is, firstly, his insistence that science must be viewed in its historical context: instead of arguing from <u>a priori</u> grounds about what science ought logically to be, Kuhn looks to the actual historical practice of scientists in the pursuit of those goals to which the consensus of the scientific community of that time had allocated importance, and draws conclusions about what science has in fact been. An important consequence of this approach is the appeal of the argument to documentary evidence: here, it may be felt, Kuhn is proceeding from theory to fact, and from fact to theory, in the manner of a true empiricist in contrast to the more rationalist methodology of Popper and his predecessors.

Secondly, Kuhn's approach is in close accord with contemporary theories of the sociology of knowledge, in which the power of entrenched institutions and

elite groups are seen to determine not only the direction in which knowledge is to be pursued, but the whole framework of ideas and ideology which the society espouses. This view is shared by both Marxist and non-Marxist commentators alike (Mannheim 1952, 1955; Merton 1979). Moreover, although Kuhn looks to gestalt psychology for support in terms of individual approaches to knowledge, his view is clearly consistent with modern epistemology, such as that of Dewey and Piaget, which underpins contemporary theories of the sociology of knowledge and of education. To the inherent fertility of his own view, therefore, is added the power of these well-established bodies of theory.

Thirdly, there is the logical force of the argument of incommensurability. The subsequent history of the debate has demonstrated that it is this notion which most divides the Kuhnian and Popperian camps. It seems difficult to deny the conclusion drawn by Kuhn--indeed, as we shall see, Popper himself does not attempt to deny it, merely to diminish its force--and yet it has been shown to be inextricably bound up with the immensely difficult problems of semantics, meaning, and truth with which twentieth century philosophy, largely unsuccessfully, has attempted to grapple. It is in the context of a discussion of such issues that Langer (1967, p. 7) refers to "the pathetic muddles of modern philosophical thought." As we shall see, the philosophical difficulties have not been resolved in over twenty years of debate, and yet it is probably true to say that the concept of incommensurability now ranks with that of paradigms as the major contribution of Kuhn to the understanding of science.

There are, nevertheless, good grounds to feel uneasy about the conclusions Kuhn derives from his study of this history of science. As we have seen, one of the consequences of accepting the argument of the incommensurability of scientific theories is the denial of meaning to the notion of scientific

progress, except within particular periods of history dominated by single paradigms, or across historical epochs only in the narrow terms of puzzle-solving ability. Logical as the sequence of these ideas may be, we may perhaps question whether the development of technology, a development which can hardly be denied on any reasonable grounds, does not in itself indicate some element of progress in the scientific theories on which such technology is based; particularly since the lineage of that development can be traced back in an almost unbroken line to the beginnings of the scientific tradition, and even earlier in the predecessor of that tradition, the lore and institutions of the craft tradition (Mason 1962, ch. 10). We may, moreover, wonder why, if Kuhn's argument is valid, such a premium should be placed by modern disciplines of both the natural and social sciences on their status as a science: it would seem that, following his argument, that science has no intrinsic value as a model of intellectual endeavour, and becomes simply something of curiosity for the historian of social customs. The scepticism thus induced by wholesale acceptance of his views clearly presents a serious challenge to any discipline, including human geography, which aspires to place its conceptual structure on scientific grounds; and, if the argument of this dissertation is to proceed, it is a challenge which must be met.

Associated with this problem is the difficulty of extracting from Kuhn's argument any normative interpretation for the conduct of science: description there certainly is, but it would appear to lack implications for the practice of contemporary science. Science should be like this, Kuhn argues, because it is like this, and nothing more can be said about it. In addressing this question, he asserts that his description of science does have normative implications: it presents

> a viewpoint or theory about the nature of science, and, like other philosophies of science, the theory has consequences for the way in which scientists should behave if their enterprise is to succeed. (Kuhn 1970a, p. 207)

It is, however, clear that here Kuhn equivocates with the idea of success, which, according to the theory of normal science, is defined normatively. To be a successful scientist is just to be a normal scientist, to practice normal science: it says nothing about the status of the product of that endeavour. Such a view provides no guidance to those who seek, as this dissertation seeks, to enhance, in some non-relative sense, a tradition of research by placing it on more scientific grounds. Again, the challenge would appear to be a crucial one.

Finally, there is a special sense in which, to the degree to which Kuhn's thesis is found persuasive, to that same degree its conclusions are placed in question. This paradox results from the fact that Kuhn himself is, as has been pointed out, engaging in a kind of empirical investigation to which his own theory may be applied. That investigation is described by him as follows:

> Concerned with scientific development, the historian then appears to have two main tasks. On the one hand, he must determine by what man and at what point in time each contemporary scientific fact, law, and theory was discovered or invented. On the other, he must describe and explain the congeries of error, myth, and superstition that have inhibited the more rapid accumulation of the constituents of the modern science text. (Kuhn 1970a, p. 2)

Yet it is clear that such an investigation must be conducted within the framework of a particular world view, developed in contemporary times, and necessarily transported into the study of earlier times. Such a world view is, in Kuhn's own incisive account, comprehensive: it frames all aspects of experience, to the extent that participants of different world views are described as inhabiting "different planets" (Kuhn 1970a, p. 111). What, then,

is the basis upon which the historian of science makes his determinations of fact, and identifies those instances of error, myth, and superstition which it is his task to explain? Where is the theory-independent framework, the objective perspective essential to the success of such an enterprise? It would seem that in making claims to authenticity on the basis of historical fact, Kuhn is ignoring the implications of his own argument, implications which are, moreover, truisms of historical research:

> It used to be said that facts speak for themselves. This is, of course, untrue. The facts speak only when the historian calls on them: it is he who decides to which facts to give the floor, and in what order or context. . . . The historian is necessarily selective. The belief in a hard core of historical facts existing objectively and independently of the interpretation of the historian is a preposterous fallacy, but one which it is very hard to eradicate. (Carr 1961, pp. 11-12)

2.4 Responses to Kuhn: Popper and Feyerabend

We may now turn to a consideration of some responses to Kuhn's seminal challenge. It is indicative of the fundamental nature of this challenge that debates concerning it have continued for over two decades, and are still largely unresolved. Rather than survey the complex arguments and counter-arguments that have been canvassed in that time, we may usefully focus on three philosophers whose works represent the main streams of the continuing debate. They are Karl Popper, Paul Feyerabend, and, in the next section, Imre Lakatos.

Popper's response to Kuhn's work has been incisive and unambiguous: he is opposed to it on logical, historical, and even moral grounds. A paper delivered in 1965, on the same platform with Kuhn, Feyerabend, and Lakatos adequately summarizes these views (Popper 1970). He begins by accepting certain of Kuhn's assertions, at least to a limited extent. Scientists do, he agrees, approach their research within a framework of accepted scientific doctrines, as is implied by his long-held principle that "we approach everything in the light of a preconceived theory" (Popper 1970, p. 52). Moreover, he agrees that normal science and revolutionary science exist. However, he differs markedly from Kuhn in his evaluation of these two modes of science; or, to be more accurate, he provides an evaluation of them, which Kuhn does not attempt, nor, indeed, see as possible in principle. Normal science, in Popper's view, is bad science:

> The "normal" scientist, as described by Kuhn, has been badly taught. He has been taught in a dogmatic spirit: he is a victim of indoctrination. . . . As a consequence, he has become what may be called an <u>applied scientist</u>, in contradistinction to what I should call a <u>pure scientist</u>. He is, as Kuhn puts it, content to solve "puzzles". . . . I admit

> that his kind of attitude exists; and it exists not only among engineers, but among people trained as scientists. I can only say that I see a very great danger in it and in the possibility of its becoming normal . . . a danger to science and, indeed, to our civilization. (Popper 1970, p. 53)

This evaluation is based on some fundamental disagreements with Kuhn about historical facts. Few scientists, certainly few great scientists, asserts Popper, have been normal scientists in Kuhn's use of the word: though not consciously revolutionary, great scientists such as Darwin are seen as constantly bringing forward new conjectures to deal with new problems, rather than as acquiescing in the prevailing modes of thought. Moreover, Popper takes issue with Kuhn's model of relatively long periods of stability in science dominated by one paradigm: more often, he proposes, science has seen "ever since antiquity, constant and fruitful discussion between the competing dominant theories . . ." (Popper 1970, p. 55). In this context, Popper introduces the term "research programme," a notion used also by Lakatos.

More significantly, Popper rejects Kuhn's argument of incommensurability of scientific theories. He does not accept its epistemological basis, for one thing:

> I do admit that any moment we are prisoners caught in the framework of our theories; our expectations; our past experiences; our language. But we are prisoners in a Pickwickian sense: if we try, we can break out of our framework at any time. Admittedly, we shall find ourselves again in a framework, but it will be a better and roomier one; and we can at any moment break out of it again. (Popper 1970, p. 56)

Secondly, and more importantly, Popper does not accept that critical discussion and rational comparison of competing frameworks is not logically possible. To say that, he asserts, is just to make an impossibility out of a difficulty:

> It is just a dogma--a dangerous dogma--that the different frameworks are like mutually untranslatable languages. The fact is that even totally different languages (like English and

> Hopi, or Chinese) are not untranslatable, and that there are many Hopis or Chinese who have learnt to master English very well. (Popper 1970, p. 57)

He goes on to challenge the specific example Kuhn uses to develop his incommensurability argument: the relationship between Newtonian and Einsteinian dynamics:

> . . . there are many points of contact (such as the role of Poisson's equation) and points of comparison: it follows from Einstein's theory that Newton's theory is an excellent approximation . . . (Popper 1970, p. 57)

Popper concludes with a rather uncoordinated, though clearly deeply felt, polemic against the use of psychology and sociology in understanding science, a view which is consistent with his other writings on the subject (Popper 1957).

In summary, it may be concluded that, although the areas of difference between the two views are clearly marked out here, there are a number of areas where Popper seems to accept at least some of the theses put forward by Kuhn, while still differing as to their significance for the scientific enterprise. These areas all appear to relate to the historical nature of scientific enquiry: the context in which the problem arises; the idea of normal (or, in Popper's terminology, applied) science; and the revolutionary nature of science are all held in common, as Kuhn points out (Kuhn 1970b). The differences between the two views on these matters are in terms of emphasis (as both have remarked), each, in accordance with the historiographic principle enunciated by Carr above, selecting those historical facts which from his perspective appear to be most significant. On the other hand, the issue of incommensurability still appears to present a clear and irreconcilable difference, although one may wonder whether, by taking Kuhn's analogy of language communities to the further domain of culture, and then using that context to challenge the

original argument, Popper is not weakening his opposition to this notion: anthropological literature amply documents the difficulties involved in gaining an inside view of another culture, difficulties which language translation is unlikely completely to resolve. However, Popper, for one, seems to harbour few doubts about the validity of his position, and the essential irrelevance of Kuhn's. This response to Kuhn, perfunctory as it is, is nevertheless his most substantial: his subsequent work continues to centre on the issues identified before Kuhn's challenge as fundamental. His autobiography, first published in 1974 (Popper 1976), contains no reference to Kuhn, or to any of the issues he raised.

A more penetrating and significant critique of Kuhn's position has been developed over the course of more than a decade by Paul Feyerabend. It is not possible to deal adequately with this philosopher's work, which is both substantial and original (Feyerabend 1970, 1975, 1981a,b), in the context of this dissertation, but it is important to provide an outline of his critique and independent position.

Feyerabend begins his analysis of Kuhn's work by noting, as we have noted, Kuhn's equivocation with the prescriptive and descriptive approaches to science: he goes on to argue that if Kuhn's description of science is intended to be taken prescriptively, it presents a serious challenge to the creativity of science. Furthermore, he rejects the notion of puzzle-solving as a criterion for science, arguing that many other forms of human activity (such as, he suggests ironically, organized crime) can meet this criterion without being regarded on any reasonable grounds as science.

However, Feyerabend accepts Kuhn's identification of what he terms a "principle of tenacity" (Feyerabend, 1970, p. 203) in science. Such a

principle is reasonable, he agrees, firstly because scientific theories are always capable of improvement, and may well develop to the point where they can explain apparent anomalies which, in their undeveloped state, they could not incorporate. Moreover, he goes on, experimental results that may appear to give rise to anomalies must be regarded with some caution:

> Different experimenters are liable to commit different errors and it usually needs considerable time before all experiments are brought to a common denominator. (Feyerabend 1970, p. 204)

Thirdly, since a theory provides criteria by which the scientific enterprise is carried on (and on this point he accepts Kuhn's position), it seems to be reasonable to maintain it for as long as possible, in order "to keep the discourse rational for as long as possible" (Feyerabend 1970, p. 204). Most importantly, however, he points out that a fact is never a mere fact, and an observation never a mere observation. Always it is embedded within both the theory to which it relates as a test implication and assumed bodies of theory which are used to derive such an implication:

> Basic theories and auxiliary subjects are often "out of phase." As a result we obtain refuting instances which do not indicate that a new theory is doomed to failure, but only that it does not fit in at present with the rest of science. This being the case scientists must develop methods which permit them to retain their theories in the face of plain and unambiguously refuting facts, even if testable explanations for the clash are not immediately forthcoming. The principle of tenacity . . . is a first step in the construction of such methods. (Feyerabend 1970, p. 205)

Acceptance of a principle of tenacity in science, together with an assumption of the importance of paradigm change (an importance which, Feyerabend points out, Kuhn insists upon but does not defend), implies a "principle of proliferation" (Feyerabend 1970, p. 205), through which a theory can be replaced by other theories which underscore the difficulties the

original theory faces, while promising solutions. Certainly Kuhn regards the competition between theories as an essential part of the scientific enterprise, but, Feyerabend rightly argues, is unable to account for their emergence: "if normal science is de facto as monolithic as Kuhn makes it out to be, then where do the competing theories come from?" (Feyerabend 1970, p. 206).

Moreover, he argues, proliferation of theories, far from being a consequence of crisis in a paradigm--as outlined in section 2.2, Kuhn sees theories alternative to the prevailing paradigm emerging only when established anomaly has fundamentally challenged the credibility of the paradigm--such alternatives are always present, and it is their active interaction which leads to the downfall of the prevailing paradigm. By thus eliminating the separation between periods of proliferation and periods of monism, Feyerabend is able to argue that normal science, as described by Kuhn, does not exist. In this he amplifies the historical observation made in this context by Popper, as outlined above.

From an insistence on the value of proliferation of theories for the promotion of human happiness--one of the characteristic thrusts of Feyerabend's thinking is his refusal to separate ethical from logical or historical considerations--Feyerabend is led to support the synthesis contained in Lakatos's view of science (discussed in more detail below):

> The picture (of science which should replace Kuhn's account) is the synthesis of two discoveries. First, it contains Popper's discovery that science is advanced by a critical discussion of alternative views. Secondly, it contains Kuhn's discovery of the function of tenacity which he has expressed, mistakenly I think, by postulating tenacious periods. The synthesis consists in Lakatos's assertion (which is developed in his own comments on Kuhn) that proliferation and tenacity do not belong to successive periods of the history of science, but are always copresent. (Feyerabend 1970, p. 211)

To this account, however, Feyerabend would wish to add the concept of incommensurability. He strongly supports the logic of Kuhn's analysis of Newtonian and Einsteinian dynamics, adding that, because our experience and understanding of the world are embedded in theories of this type, the implication of Kuhn's argument may be taken even further:

> Different magnitudes based on different concepts may give identical values on their respective scales without ceasing to be different magnitudes. . . . This conceptual disparity, if taken seriously, infects even the most "ordinary" situations: the relativistic concept of a certain shape, such as a table, or of a certain temporal sequence, such as my saying "yes," will differ from the corresponding classical concept also. It is therefore vain to expect that sufficiently long derivations may eventually return us to the older ideas. . . . A comparison of content and a judgement of verisimilitude cannot be made. (Feyerabend 1970, p. 222)

To the objection that new concepts and linguistic terms must, before being employed in the formulation of theory, be framed in concepts and terms that already exist, Feyerabend points to the epistemological work of Piaget, and to the experience of anthropological field work, which suggest that human learning is inherently constructed in terms of incommensurable frames of reference; and that, in consequence, the stable, observational language sought in the traditional Popperian view is not available. Moreover, he challenges the requirement of conceptual continuity which would want to see older theories embedded as limiting cases in the new:

> Why should the relativist be concerned with the fate of classical mechanics except as part of a historical exercise? There is only <u>one</u> task we can legitimately demand of a theory and it is that it should give us a correct account of the <u>world</u>. What have the principles of explanation got to do with this demand? (Feyerabend 1970, p. 227)

Refutation, therefore, remains a central component of science, but it assumes the weaker role of operating within the confines of a particular theory: it cannot adjudicate between competing theories.

From this position, Feyerabend goes on to develop a line of thinking that has become his main theme in the course of the last decade: the inherent irrationality of science. Since the sets of rules by which science is conducted change from age to age, and since, in addition to these rules, a multitude of incidental factors--"accidents, prejudices, material conditions . . ., the vicissitudes of married life, oversight, superficiality, pride, and many other things" (Feyerabend 1970, p. 216)--must be taken into account in developing a complete picture; and, since, Feyerabend alleges, the incommensurability of scientific theories implies the necessity of introducing notions of taste, such as are used in the evaluation of poetry, science cannot be regarded as a wholly rational enterprise. Indeed, in a later work (Feyerabend 1975), he argues that science is inherently irrational: that essential to the growth of science is the complete freedom of theory and method that can only come with methodological anarchy; and that counterinductive, even apparently non-rational, characteristics often characterize what have later come to be recognized as major advances in scientific knowledge. In this work of great verve and originality, Feyerabend throws considerable light on the processes of scientific creativity, and on those early stages in the growth of a new research program singled out as crucially important by Lakatos (1970). Despite the ingenuity of this argument, however, we may reasonably feel that Lakatos manages to avoid the implication of irrationality in science; and it is to this view that, finally, we may now turn.

2.5 Lakatos on scientific research programmes

We may reasonably conclude, from the above discussion, that the critique developed by Popper and Feyerabend (and, as we shall see, by Lakatos) raises serious objections to Kuhn's view as an overarching account of the scientific enterprise. At the same time, we may feel that some of Kuhn's central insights--notably the tenacity of scientific theories, and the consequent historical character of science--are valid, and must be included in any adequate account of science. Moreover, we have not as yet come to any conclusion about the key issue of incommensurability. In this section, it is argued that Lakatos's notion of scientific research programmes successfully resolves most of these issues, and provides the best available account of science on which our analysis of the conceptual foundations of human geography is to be based.

A number of works elaborate different parts of these views (Lakatos 1970; 1978a,b), but the central argument is most systematically developed in his 1965 response to Kuhn (Lakatos 1970), and it is from that monograph that the following account is drawn.

As noted above, the Lakatosian synthesis referred to by Feyerabend is generally accepted by him, at least in his more sober moments. It should also be clear, however, that Popper's views, too, are far from limited to that more rigid insistence on falsification as the lynchpin of science outlined in section 2.2. We have noted, for instance, his acceptance of the role of dogmatism in science:

> I believe that science is essentially critical; that it consists of bold conjectures, controlled by criticism, and that it may, therefore be described as revolutionary. But I have always stressed the need for some dogmatism; the dogmatic

> scientist has an important role to play. If we give in to criticism too easily, we shall never find out where the real power of our theories lies. (Popper 1970, p. 55)

In this context, he even locates continuity in science in terms that are close to Kuhn's, and even closer to Lakatos's: he refers to that time in the history of science when it was generally felt that

> . . . the phenomenological theory of heat should become the one paradigm of all physical theories. (I am using here the word "paradigm" in a sense slightly different from Kuhn's usage: to indicate not a dominant theory, but rather a research programme--a mode of explanation which is considered so satisfactory by some scientists that they demand its general acceptance.) (Popper 1970, p. 55)

Lakatos begins his analysis with this recognition that there are a number of theories of science based on falsification: he terms these dogmatic falsification, which, Lakatos argues, is often attributed to Popper but which he never espoused; methodological falsificationism, which is the basic position of The Logic of Scientific Discovery (Popper 1959); and sophisticated falsificationism, which is found throughout his writings, especially in more recent works (Popper 1963, 1972), and which becomes the basis of Lakatos's notion of scientific research programmes. In subjecting these theories to critical analysis, Lakatos demonstrates that Popper's position is more profound than had formerly been thought, and is able to account satisfactorily for the observations and arguments that Kuhn makes.

Dogmatic falsificationism Lakatos identifies as the combination of a belief in the fallibility of all scientific theories and an insistence on the infallibility of empirical evidence. On this view, scientific honesty lies in specifying an event such that, if it is obtained, the theory has to be given up. It thus depends on a separation between theoretical and observational propositions, a separation which both Feyerabend (1975, ch. 14) and Lakatos

argue is not legitimate, and not in accord with historical fact, as illustrated by Galileo:

> It was not Galileo's--pure, untheoretical--observations that confronted Aristotelian theory but rather Galileo's "observations" in the light of his optical theory that confronted the Aristotelians' "observations" in the light of their theory of the heavens. This leaves us with two inconsistent theories, prima facie on a par. (Lakatos 1970, p. 98)

Moreover, as Popper argues, epistemologically a purely empirical account of experience is invalid, a view which Piaget's work has upheld (Piaget 1970, 1971). Thus

> . . . there are and can be no sensations unimpregnated by expectations and therefore there is no natural (i.e., psychological) demarcation between observational and theoretical propositions. (Lakatos 1970, p. 99)

Furthermore, again following Popper, Lakatos dismisses the logical basis of the notion of experimental proof:

> . . . no factual proposition can ever be proved from an experiment. Propositions can only be derived from other propositions, they cannot be derived from facts: one cannot prove statements from experiences . . . (Lakatos 1970, p. 99)

Finally, even if there were a natural demarcation between theoretical and observational propositions, Lakatos points out that any theory can be saved from refutation by simply altering the ceteris paribus ('other things being equal') clause that is implicit to all scientific theories. We are thus forced to conclude that the attempt to use dogmatic falsificationism as a demarcation criterion must inevitably collapse, since it leads to a recognition that "Scientific theories are not only equally unprovable, and equally improbable, but they are also equally undisprovable." (Lakatos 1970, p. 103)

It is recognition of these logical flaws that, as outlined in section 2.2, leads Popper to erect his theories on the basis of a more coherent notion of

falsificationism, termed by Lakatos methodological falsificationism. This approach seeks to establish a fundamental ground of objective "fact" not by natural or logical argument, but simply by convention: such an approach

> makes unfalsifiable by fiat some (spatio-temporally) singular statements which are distinguishable by the fact that there exists at the time a "relevant technique" such that "anyone who has learned it" will be able to decide that the statement is "acceptable". . . . This decision is then followed by a second kind of decision concerning the separation of the set of accepted basic statements from the rest. (Lakatos 1970, p. 106)

There is no question of insisting on the factual nature of these statements; they are simply adopted methodologically in order to further the enquiry as quickly as possible, by establishing some reasonable basis to work from, "unproblematic background knowledge 'which we accept (tentatively) as unproblematic while we are testing the theory'" (Lakatos 1970, p. 106). This leads, secondly, to the selective notion of scientific progress, together with the notion of corroboration, outlined in section 2.2. The whole operation of science is thus regarded as a relative, rather than an absolute, matter, and its ultimate truth is entirely dependent on the status of the fundamental statements adopted, whose truth in turn can never be established within the structure of science. It is in this sense that Popper remarks:

> The empirical basis of objective science has thus nothing "absolute" about it. Science does not rest upon solid bedrock. The bold structure of its theories rises, as it were, above a swamp, but not down to any natural or "given" base; and if we stop driving the piles deeper, it is not because we have reached firm ground. We simply stop when we are satisfied that the piles are firm enough to carry the structure, at least for the time being. (Popper 1959, p. 111)

In addition to the two fundamental decisions on which methodological falsificationism is based, three more decisions are identified as necessary. The first relates to the rules by which statistical evidence may be judged

inconsistent with the theory; the second to the application of a refutation either to the theory itself or to its ceteris paribus clause; and the third to the elimination of some theories which, by virtue of their logical form, preclude refutation in the sense Popper requires.

It is clear that there are considerable risks in proceeding in this way. Any conventional decision runs the risk of simply being wrong, and here it would seem, Lakatos asserts, that the decisions required are of such a fundamental nature that the whole enterprise may better be labelled "dare-devil" (Lakatos 1970, p. 112). These decisions, he goes on to argue, are too arbitrary, too risky; and, what is more, they are not in accord with historical fact: history suggests, in Lakatos's view, that tests are contests more between rival theories and experiment, than between theory and experiment; and that some of the most important and interesting experiments give rise not to refutations, but to confirmations. Rather, however, than abandon the attempt to give a rational account of the success of science and speak in terms of paradigms and social psychology--as Lakatos asserts, Kuhn does (although Kuhn (1970c, p. 261) objects strongly to his approach to science being seen as grounded on irrational psychology)--Lakatos therefore seeks to follow Popper's later thinking in reducing the element of decision in falsificationism in a more sophisticated approach, upon which can be based a more considered, and historically supported, view of the methodology and success of science.

This approach to science, which Lakatos terms sophisticated falsificationism, is, he argues, implicit in many of Popper's writings, though nowhere systematically spelt out. Rather than focus attention on the relationship between a theory and empirical evidence derived from the testing of it, sophisticated falsificationism takes as the fundamental ground of its

analysis a series of theories and empirical evidence related to it. In this view, a new theory is scientific only if it has "excess empirical content" (Lakatos 1970, p. 116) with respect to the previous theory, and if some of this excess empirical content is verified, or corroborated. Thus a theory is falsified only if another theory is proposed which has some corroborated excess empirical content by comparison. A series of theories which fulfills these conditions at each stage of its development is termed progressive: theoretically progressive if each theory contains some excess empirical content when compared to its predecessor; and empirically progressive if some of the excess empirical content is also corroborated. Lakatos summarizes these propositions as follows:

> Sophisticated falsificationism thus shifts the problem of how to appraise theories to the problem of how to appraise series of theories. Not an isolated theory, but only a series of theories can be said to be scientific or unscientific: to apply the term "scientific" to one single theory is a category mistake.
>
> The time-honoured empirical criterion for a satisfactory theory was agreement with the observed facts. Our empirical criterion for a series of theories is that it should produce new facts. The idea of growth and the concept of empirical character are soldered into one. (Lakatos 1970, p. 119)

Clearly this criterion lends to falsification an historical character, since it is not the relation between a theory and empirical evidence which is at issue, but

> a multiple relation between competing theories, the original "empirical basis," and the empirical growth resulting from the competition. (Lakatos 1970, p. 120)

Moreover, this historical character is of a different order to that identified by Kuhn. Far from waiting, until an anomaly has been recognized as a refutation, for the development of alternative theories, proliferation of theories is now the essence of the scientific enterprise: a replacement is

sought for every hypothesis in a better one, whether it is falsified or not; and now there is a criterion for what constitutes better hypothesis.

This view of science, Lakatos argues, is not only normative, but descriptive of historical fact. Taking the replacement of Newtonian dynamics by Einsteinian dynamics, the focus of the debate between Kuhn, Popper and Feyerabend, as an example, Lakatos points out that there are many known anomalies to Einstein's theory, as there are to Newton's; but that Einstein's is progressive in terms of excess corroborated empirical content, and therefore may reasonably be preferred.

Furthermore, it reduces the degree to which science is dependent on decisions. It eliminates the need for a decision about which part of a theory or the ceteris paribus clause to address falsification to:

> We may try to replace any part and only when we have hit on an explanation of the anomaly with the help of some content-increasing change (or auxiliary hypothesis), and nature corroborates it, do we move on to eliminate the "refuted" complex. (Lakatos 1970, p. 125)

Moreover, a theory is not eliminated if it clashes with a well-corroborated theory, but only if it fails to produce a progressive shift in the scientific view of the problem, and if there is a better rival theory available. Metaphysical theories may therefore be retained provided they produce new empirical content. These two reductions dispense with the need for the fourth and fifth types of decision of methodological falsificationism: the first three types, however, remain indispensable.

Lakatos points out that this view, although it elevates the clash between theories to a central position, still allows for the ultimately empirical character of science. Theoretical progress may be achieved in a succession of theories; but if science is to remain effective, from time to time empirical facts must be appealed to:

> . . . experience still remains, in an important sense, the "impartial arbiter" of scientific controversy. We cannot get rid of the problem of the "empirical basis," if we want to learn from experience: but we can make our learning less dogmatic--but also less fast and less dramatic. (Lakatos 1970, p. 131)

Finally, when a series of theories attains a certain degree of continuity, derived from the power of the original theory, it becomes, in Lakatos's terminology, a scientific research programme. Such programmes will have a "negative heuristic," a "hard core" of theory which is taken to be immune to refutation by reason of methodological decision (Lakatos 1970, p. 133). Instead, the scientist will direct his attention to the "protective belt" of "auxiliary hypotheses," which is erected on the foundation of the hard core of theory, and which is to demonstrate the characteristics of a progressive series of theories, as defined in the preceding discussion: this is the positive heuristic of scientific research programmes. An important implication of this approach is that refutations are expected from the beginning of the programme, and do not interfere with its progress:

> . . . this is why one speaks of "models" in research programmes. A "model" is a set of initial conditions (possibly together with some of the observational theories) which one knows is bound to be replaced during the further development of the programme, and one even knows, more or less, how. This shows once more how irrelevant "refutations" of any specific variant are in a research programme: their existence is fully expected, the positive heuristic is there as the strategy both for predicting (producing) and digesting them. Indeed, if the positive heuristic is clearly spelt out, the difficulties of the programme are mathematical rather than empirical. (Lakatos 1970, p. 136)

Moreover, the driving power of the research programme must be the instances which tend to confirm the original thrust of the theory: "it is the 'verifications' which keep the programme going, recalcitrant instances notwithstanding" (Lakatos 1970, p. 136). This leads Lakatos to posit the

relative autonomy of theoretical, as opposed to experimental, science: "the anomalies are listed but shoved aside in the hope that they will turn, in due course, into corroborations of the programme." (Lakatos 1970, p. 137).

In summary, Lakatos challenges the view of empirical fact as the arbiter between theories in crucial experiments. There is, in his view, no such "instant rationality" in science:

> . . . continuity in science, the tenacity of some theories, the rationality of a certain amount of dogmatism, can only be explained if we construe science as a battleground of research programmes, rather than of isolated theories. (Lakatos 1970, p. 175)

Furthermore, what is appropriate for the conduct of mature science may not be appropriate for the conduct of immature science: it may be more important to assert dogmatically the validity of some theory when it first emerges than to subject it to intense critical scrutiny, since otherwise

> we should give the theory up before we had a real opportunity of finding out its strength; and in consequence no theory would ever be able to play its role of bringing order into the world, or preparing us for future events, of drawing our attention to events we should otherwise never observe. (Popper 1963, p. 312 fn.)

Thus Lakatos sees as valid, and satisfactorily accounts for, Kuhn's identification of continuity in the scientific enterprise, and of the tenacity of scientific theories, in the context of a rejection of dogmatic falsificationism; but sees as invalid his consequent abandonment of any attempt to reconstruct rationally the growth of science, and his appeal to what Lakatos terms a "psychology of discovery," which leads to the conclusion that "scientific revolution is irrational, a matter for mob psychology" (Lakatos 1978, p. 178). The growth of science, he argues, is not a matter for psychology, but for that third world of ideas and articulated knowledge whose

existence was first demonstrated by Popper (1972c). It is Kuhn's inability to identify Popper's more sophisticated positions which, Lakatos argues, leads him to this extremity.

On the central question of incommensurability, Lakatos, rather inexplicably in view of Kuhn's and Feyerabend's insistence on the crucial importance of the concept, attempts no systematic exposition. His one reference to it is contained in a parenthesis to a footnote, and appears to follow Popper in adopting a methodological stance to the problem:

> I have not dealt with Kuhn's and Feyerabend's claim that theories cannot be eliminated on any objective grounds because of the "incommensurability" of rival theories. Incommensurable theories are neither inconsistent with each other, nor comparable for content. But we can make them, by a dictionary, inconsistent and their content comparable. If we want to eliminate a programme, we need some methodological determination. This determination is the heart of methodological falsificationism; for instance, no result of statistical sampling is ever inconsistent with a statistical theory unless we make them inconsistent with the help of Popperian rejection rules. (Lakatos 1970, p. 179 fn.)

Thus Lakatos subsumes the problem of incommensurability under the third type of decision he sees as essential to the conduct of scientific enquiry in the approaches of both methodological and sophisticated falsificationism.

On all other points than this, we may reasonably feel that Lakatos has satisfactorily answered Kuhn's challenge. His answer consists in part of rejecting, on both theoretical and historical grounds, certain propositions central to Kuhn's argument, notably that which seeks to limit the proliferation of theories to the aftermath of refutation and crisis; and in part by accepting other propositions, notably those related to the continuity and tenacity of scientific theories, which seems to be historically indisputable. The resulting synthesis seems to incorporate these important observations, and to

replace scientific enquiry in its historical, rather than just its logical, context. Moreover, it retains the logical and empirical structure of science that Popper seeks: whatever the relative autonomy of theoretical progress, it must, in the final analysis, be supported empirically, and lacking that support, must be given up. He is therefore able to avoid the appeal to psychology and sociology that Kuhn resorts to, and the irrationality that Feyerabend sees as inevitable. Science remains a rational enterprise, founded on the now inseparable concepts of growth and empirical content, while at the same time, as Popper's swamp analogy implies, rejecting those absolute epistemological foundations implicit in naive views of science.

On the remaining question of incommensurability, a final determination is not easy, and has not, in fact, been obtained in the two decades or more of debate that have followed Kuhn's publication. Recent structuralist approaches (Sneed 1979; Balzer and Sneed 1977, 1978; Stegmuller 1975, 1979), which cannot, through lack of space, be dealt with in this context, have even contended that incommensurable theories can be accepted in science without necessarily inferring non-comparability and non-reducibility of competing theories (Pearce 1982, p. 389). In terms of the debate outlined above, it appears that the logic of incommensurability is not disputed by either Popper, Feyerabend, or Lakatos: they do not follow the structuralist attempt to ameliorate the force of this logical objection, and accept instead its strict validity. However, Lakatos, following Popper, does not conclude from that acceptance to either Kuhn's or Feyerabend's position: the adoption of a 'dictionary' which allows a degree of interaction between competing or successive theories is seen substantially to vitiate the force of this objection, and to relegate it to the methodological foundations upon which all scientific activity is necessarily

based. Moreover, the epistemological position taken by Kuhn, and from which the logic of incommensurability derives, is not accepted by Lakatos or Popper in the extremes which Kuhn demands; as we have seen, Popper insists upon the possibility of breaking out of particular cognitive frameworks to more expanded ones, while conceding that some framework is necessary to all cognitive processes. Feyerabend's attempt to refute this proposition on the evidence of genetic epistemology and anthropological experience is not convincing: even if we were to grant the validity of his interpretation of this evidence--which interpretation is open to question, as section 4. suggests--the legitimacy of arguing from it to the notion of scientific paradigms is not established by Feyerabend; and, we may reasonably feel, cannot be. Thus incommensurability becomes an aspect of the methodologically relative status of scientific truth; a status which does nothing to inhibit the practice of science, or its success.

It would seem that whether we accept this rebuttal or not is, to use Feyerabend's (1970, p. 228) phrase, more "a matter of taste" than of logic. Structuralist attempts may succeed in establishing the logical thesis they currently propose, and if they do the position adopted by Lakatos and Popper will be strengthened. But, even without that support, we may reasonably feel that, taking the views of science discussed above in holistic perspective, there are good reasons for accepting Lakatos's notion of scientific research programmes as the most coherent and the best documented of them. Furthermore, its preservation of rationality; its incorporation of the historical character of science; its pragmatism and practical applicability; and, above all, its insistence on theoretical and empirical progress as the fundamental criterion of science, make it more attractive than the scepticism of Kuhn, the anarchism of Feyerabend, and the restrictiveness of Popper (in his methodological

position). Lakatos does not evade a normative interpretation of his Position--indeed, he welcomes it--and yet his view seems to be in accord with the historical facts, as far as we have them, and with our intuitive impression of current scientific practice. We may conclude that it represents a more sophisticated and valid account of the nature of scientific enquiry than those naive views--whether inductive or dogmatically falsificationist--on which the human geographic understanding, and practice, of science has rested. It is therefore this position on the nature of scientific enquiry which will be taken as a basis for the succeeding chapters of this dissertation, and which, it will be proposed, may point the way to a resolution of the problems raised in the first chapter.

CHAPTER III

EXPLANATION AND UNDERSTANDING IN THE SOCIAL SCIENCES

Outline of the Chapter

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- Geographic views of social science
  - Humanistic geography
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  - Guelke
- Positivistic approaches to the social sciences
  - Mill
  - Popper
  - Nagel
  - Rudner
- Critiques of positivism in the social sciences
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    - Language, intelligibility, and context
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- Inadequacy of positivist model
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- Methodological difficulties in the critiques
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Theory in human geography
- Characteristics of ideal theory
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- The individual in human geography

CHAPTER III

EXPLANATION AND UNDERSTANDING IN THE SOCIAL SCIENCES

3.1 The nature of social scientific inquiry

3.1.1 Geographic views of social scientific inquiry

The attempt to place the study of human affairs on a scientific foundation rests on two related conceptual contexts. The first is the nature and structure of science itself. As we have seen, this is by no means as straightforward as it may first appear, and has not, by and large, been well handled by human geographers. The second is the manner of application of this notion of science to human phenomena. This is the domain of the philosophy of science. The problems raised by it are, if anything, more complex than those of the philosophy of science, and, as we shall see, have been even less, and less successfully, dealt with by geographers. Yet it is clear that they are problems of such a fundamental order that the construction of human geography as a science cannot validly proceed until they are resolved. To the extent that geographers have failed to recognize, much less to grapple with, these problems it is clear that the whole discipline of human geography, conceived of as a science, is placed in jeopardy. In this chapter, an attempt will be made to identify and resolve the main issues raised by the philosophy of social science; and to locate in that resolution, and in the conclusions of the preceding chapter, a direction for the specific task, undertaken in the

succeeding chapters, of placing human geography on a valid and relevant scientific foundation.

The reluctance of human geographers to grapple with the central issues of the philosophy of science is perplexing, to say the least. The whole thrust of the humanist critique of positivistic human geography has its roots there, and could have derived much coherence and power from such an analysis; and yet, if one looks to seminal documents of that school (Buttimer 1974, 1976a; Relph 1970, 1976a,b; Tuan 1974, 1976b), though passing reference is made to these issues, nowhere are they given that depth of treatment which the proposed rejection of science would seem to demand. Relph (1970, p. 195), for instance, takes as a foundation for his advocacy of a phenomenological approach to human geography a rejection of the "dictatorship of scientific thought over other forms of thinking," but does not elaborate the grounds for his rejection, and vitiates his position by noting in footnote, "In its attack on scientism and positivism phenomenology is, of course, concentrating on a fairly narrow range of scientific thought" (Relph 1970, p. 200). Clearly more is required for the phenomenological approach to human geography to proceed on secure foundations. In a similar manner, Buttimer (1976a, p. 283) remarks that

> One must reject any scientific cause-effect models of subject and object, and conceptualize the relationship between bodysubject and world as reciprocally determining one another. Focusing on the relationship between body and world raises a number of philosophical and methodological questions. Much of the body's behaviour becomes unintelligible if one treats it as pure subject (i.e., separate from environment).

Buttimer here locates a central issue of the philosophy of social science and correctly frames it in terms of intelligibility: one can only regret that the "philosophical and methodological questions" referred to are not given systematic treatment, particularly as elsewhere philosophical matters have

been systematically treated by her (Buttimer 1974, pp. 37-39). Working within the same general framework, and towards the same general end--an understanding of "the way human beings respond to their physical setting--their perception of it and the value they put on it" (Tuan 1974, p. 2)--Tuan nevertheless identifies himself with the social sciences, despite the inherent difficulties of that position:

> As social scientists we have many skills but the crucial . . . problems often escape us because we lack sophisticated concepts to frame them. . . . Nonetheless, systematized findings are invaluable for they give precision to the hunches of common sense, and they sometimes challenge and overthrow mere opinion. (Tuan 1974, p. 3)

Yet this unexceptionable statement raises more questions than it answers; for if the pastiche which results from the application of this approach represents social science (Tuan 1974), clearly the word "science" is being radically redefined. A similar redefinition is implicit in Buttimer's adoption of the phenomenological view that "transcendental reflection . . . should become . . . the fundamental attitude from which all scientific enquiry should spring" (Buttimer 1976a, p. 279), a view which demands a far greater treatment of the notion of science than is here given. Such examples could be multiplied, but the point would seem to be clear: as fundamental as conceptions of science and of social science are to the rejection of scientism which underlies the humanist position, little systematic attention has been given to these conceptions, or to the grounds for rejection associated with them.

Nor is it easier to understand the reluctance of human geographers adhering to the traditional conception of social science to discuss openly these philosophical and methodological issues. The underpinnings of the geographical spirit have always been empirical and pragmatic; and yet it is

difficult to imagine that practitioners of human geography, whatever their degree of confidence in the conceptual foundations of their craft, should not have encountered, for instance, the problems of objectivity and interaction that particularly characterize the social sciences. We do not suppose that human geographers are any less intelligent, as a group, than are the practitioners of other social scientific disciplines, who have identified such problems and have attempted to come to terms with them: what, then is the cause of this reticence? Perhaps we may attribute to geographers an understandable hesitation to become lost in a seemingly unprofitable morass of philosophical argumentation: and yet the issues remain. If human geographers are serious in their endeavour to place their discipline on a valid scientific foundation, the attempt to resolve them cannot be ignored.

Of the few attempts that have been made, two may be singled out for closer attention. The first is that of Harvey (1969, ch. 5), and the second is that of Guelke (1974, 1981, 1982). Arising in the context of very different movements in the history of human geographic thought, they nevertheless represent adequately the positivist and anti-positivist positions which arise from the main debates of the philosophy of social science.

Harvey's account of the problems of explanation in the social sciences and history is, like his account of scientific explanation, far from satisfactory. He draws on the standard works--Mill(1974), Rudner (1966), Brown (1963), Hempel (1965), Winch (1958), Dray (1964), Collingwood (1948), and so on--but his method, as with his approach to science, speaks more of a superficial gleaning of notions favourable to his own position than of a systematic and uncompromising attempt to resolve these issues at a fundamental level. Ironically, he begins his analysis by castigating those who

> . . . bent on resisting the idea of "scientific" explanation (have) usually pointed to just one of the varied set of opinions extant in the natural sciences (Harvey 1969a, p. 44);

a proceeding which he himself, as we have seen, is guilty in his own approach to science. He then appeals to Carnap's (1958) distinction between the pragmatics, semantics, and syntax of a linguistic system as a conceptual framework for his discussion of the social sciences--though why these particular concepts are to be employed is given no justification--by generalizing them, as he puts it, "crudely" (Harvey 1969a, p. 45) to the categories of techniques, conceptual content and logical structure. With his own assessment of these generalizations we can only agree: it is not at all clear on what basis they are made; and the first of them, in particular--"pragmatics" to "techniques of investigation"--seems entirely to have misconstrued Carnap's conception, which explicitly refers to "the speaker of the language" (Carnap 1958, p. 78), and invites the kind of analysis of the social sciences which Winch (1958) undertakes.

Nor are matters much improved when the analysis is launched on this basis. Problems under the category of techniques of investigation are limited to a consideration of the different role of experiment in the social and natural sciences, and are summarily resolved by including a statement on the issue by Rudner (1966, p. 5), without any of the supporting argument; and by noting the obvious fact that some natural sciences, like astronomy and meteorology, take their experiments where they find them. Entirely ignored are Nagel's penetrating analysis of that issue (Nagel 1961, ch. 13), and the far more crucial matters relating to interaction between observer and observed that are fundamental to the philosophy of the social sciences, and to social scientific practice: these are relegated to the later consideration, where they are subsumed under a general discussion of objectivity.

Harvey then moves to a discussion of the conceptual content of explanations. This he characterizes as a "far more serious issue" because of the emotional nature of the controversy over the degree to which free will is unique to the human species, which controversy created, in the determinist-possibilist argument, "an intellectual legacy of great significance" (Harvey 1969a, pp. 46-47). It is not clear, we may feel, why emotion should be either necessary or sufficient to the seriousness of an issue; nor need we accept this estimate of what can equally be seen as an essentially sterile period of debate in human geography (see chapter V). If, however, the issue is to be of serious moment, it is difficult to see in Harvey's conclusion a satisfactory resolution of it:

> It is not possible to differentiate between natural and social science in terms of the concepts adopted since there is clearly as much variation within disciplines as there is between them. (Harvey 1969a, p.47)

Harvey here resorts to description, rather than to the analysis that a normative response to this issue would require. We may note, moreover, that the notion of conceptual content is inextricably linked to the logical structure of explanation, and cannot adequately be discussed in isolation from it.

Finally Harvey comes to a consideration of logical structure, which in fact includes within its ambit the two preceding sections, and the succeeding one on verification (hence the danger of <u>ad hoc</u> conceptual frameworks). He begins by appealing to the deductive-nomological model of Popper and Hempel (section 2.2 above) in its application to history: "both Popper and Hempel take the view that historians <u>must</u> use this kind of rigorous form in offering explanations" (Harvey 1969a, p. 49), he asserts; and yet, as we shall see, this is true only in the broadest sense, since Popper takes a particular view

of history as characterized by an interest in singular events, although in the context of the deductive-nomological model (Popper 1959, p. 143). It is this view which Joynt and Rescher (1961) employ to rebut the uniqueness thesis of Weber, Oakshott and Collingwood, with which rebuttal Harvey is in agreement. Yet he goes on to misinterpret entirely Popper's position as lamenting the inadequacy of social scientific generalizations; whereas Popper's whole point is that historical explanations centre on initial conditions rather on the assumed laws in conjunction with which the explanation is constructed. It is, perhaps, not surprising that Harvey, after a brief mention of rationality--the heart of the anti-positivist critique of scientific method in history--gives the argument up, with the observation it is getting too complicated (Harvey 1969a, p. 54).

On the important argument advanced by Winch (1958), that human action must be approached in terms of its context of meaning, Harvey is only a little less confused. After correctly summarizing Winch's argument, he goes on to find in it a methodological implication which conjoins "intentions, reasons, motives, and dispositions," the last two of which are emphatically not part of the argument of either Winch or Collingwood, for both of whom rational explanation is all that is either necessary or obtainable. He goes on to argue, moreover, that even were Winch's position valid, the social sciences could still not be distinguished from the natural sciences, because Kuhn's position on the latter is identical. Yet the force of this argument rests on an assimilation of Kuhn's view of science to that of Popper and Hempel; and, as we have seen, these views are fundamentally opposed. Harvey is at least consistent in his misreading of Kuhn; but misreading it he surely is. With arguments such as these, it is small wonder that Harvey finds himself compelled to appeal to the "use and effectiveness" of the scientific model in the social sciences; to see

that model as an "ultimate objective"; and to content himself with the hope that "such an ideal is surely not so unattainable?" (Harvey 1969a, p. 61). It is, we may feel, somewhat less than convincing; and since Harvey's position embraces the attempt of other geographers, such as Schaeffer, Bunge, Morrill, Berry, Garrison and Gould, to establish human geography as a social science, its inadequacy presents a serious challenge to the discipline.

Guelke (1974, 1981, 1982) presents a more coherent and, within its frame of reference, a more informed account of social explanation. His concern is to elucidate the foundations of historical geography, and to derive methodological prescriptions from that conceptual clarification. In thus restricting his inquiry, he distinguishes history from both the natural and the social sciences; but goes on to affirm its fundamental importance to the study of human societies:

> This work is based on the contention that history is an autonomous field of knowledge, quite distinct from the natural and social sciences, with objectives, concepts and procedures of its own. The term "history" is used here in its broadest meaning to include all the disciplines concerned with approaching or understanding human societies as historical creations. History is seen as the fundamental foundation of human studies. What theory is to the natural sciences, history is to the study of human society. (Guelke 1982, p. 1)

Guelke bases his analysis firmly on the ideas of Collingwood, a philosopher who expounded with great cogency and insight an idealist approach to historical studies whose maxim was "all history is the history of thought" (Collingwood 1948, p. 215). The details of this position are discussed below, and need not be recapitulated here: what is important is that Guelke follows its principles carefully and with insight, and is in consequence able to approach historical geography from a well-integrated philosophical position. In contrast, we may feel, to Harvey, Guelke has understood the deepest principles of the authority to whom he appeals.

We may note, without here pursuing it in detail, that there has developed a substantial critique of the idealist position espoused by Guelke. Watts and Watts (1978), Harrison and Livingstone (1979a,b), and Curry (1982a,b) have challenged Guelke's notion of idealist geography from a number of different perspectives. In the main, however, the inability of these critics to deal adequately with the philosophical position they are challenging has vitiated their force. Curry (1982), for instance, identifies what he feels to be four major problems in Guelke's position: the adoption of essentialism, confusion about the relationship between thought and intelligibility, difficulties over the idea of verification, and conflation of cause and reason. It seems clear, however, from this article and the subsequent debate (Curry 1982a,b; Guelke 1982b) that these criticisms arise from misunderstandings of Collingwood's central ideas, and raise points that have successfully been dealt with in the philosophic literature, as is outlined in section 3.1.3.

However, we may also be justified in feeling that Guelke has accepted these principles too uncritically. For instance, when dealing with the method of "re-thinking" or "re-enactment" that is central to Collingwood's view of historical method, Guelke notes:

> The re-thinking of thought has nothing to do with sympathy or empathy, although an appreciation of an individual's emotional state can sometimes provide clues to his thought. . . . The scholar is not interested in re-experiencing or even trying to re-experience the emotional state of historical agents. (Guelke, 1982, p. 39)

Yet it is clear that emotional factors can act powerfully and systematically in giving rise to action in the landscape; as, for example, in the nostalgia of early Australian settlers for English landscapes in an environment perceived as alien, a clearly-held, well-documented emotion that contributed in considerable measure to wholesale alteration of the Australian landscape in the settled

areas. There would seem to be no bar in principle to the "re-enactment" of such an emotion, familiar as it is to every traveller. A similar difficulty confronts Guelke's attempt to dismiss the argument that action may proceed from irrational or subconscious roots: it is simply not enough to say that, on the one hand, such things are "not the concern of the historian," while at the same time affirming that "we identify the psychological state in terms of rational actions which express it" (Guelke 1982, p. 44). There are clear difficulties here which an uncompromising search for consistency and validity cannot avoid.

More difficult to excuse is his acceptance of Collingwood's denial of historicity and rationality to "primitive societies" (Collingwood 1948, p. 227). While quick to forestall any criticism of this view as the "prejudice of a modern European" (Guelke 1982, p. 46), he nevertheless concurs with Collingwood's view that

> the historian can do little in the way of rethinking thoughts in the absence of a framework of thought within which society can develop. (Guelke 1982, p. 46)

Despite Guelke's disclaimer, this is a peculiarly ethnocentric notion: it is not clear what societies Collingwood has in mind when referring to the "instinctive life of societies in which rationality is at vanishing-point," but, as Winch (1958) so cogently argues, rationality is a concept which varies from culture to culture and cannot be evaluated from the absolute frame of reference Collingwood so casually assumes. What he ought to be asserting, as he does elsewhere (Guelke 1981, p. 138), is the difficulty of re-thinking or re-enacting structures of rationality in cultures other than his own, a difficulty which is closely allied to the problems which arise in re-thinking structures of rationality in past historical periods.

These, however, are relatively minor matters, and the fact remains that Guelke's account of Collingwood is comprehensive and convincing. A more

significant criticism of his approach arises from his treatment of scientific method. Here, in accord with the practice of most human geographers, Guelke contents himself with a discussion of the Popper-Hempel model of deductive-nomological explanation which, as we have seen, is just one of a number of approaches to science, and by no means the most sophisticated. The criticisms Guelke directs at this model--the lack of general laws of human behaviour, its assumptions of rationality, its dependence on unrealistic idealizations and on ceteris paribus clauses--are certainly, as the preceding chapter has shown, some of the arguments that can be raised against it, but are neither all, nor the most important of them. Moreover, on some matters he entirely misconceives Popper's view of science. In attempting to justify a distinction between the natural sciences and history and the social sciences on the grounds that, in the latter, theory creates bias, he remarks:

> The idea that theories could be a source of bias would be unthinkable in a discipline based on genuine scientific principles . . . (Guelke 1982, p. 61)

On the contrary, however, as we have seen, the idea that theoretical bias is an intrinsic part of scientific enquiry is central to Popper's position. These significant weaknesses--of which the most important, of course, like Harvey, is his failure to deal with Kuhn's challenge and the ensuing debate--substantially vitiate, we may feel, the force of his rejection of science, and his championship of idealism in the Collingwood mould.

In summary, there would seem to be some justification to the view that the attempt to construct human geography as a science has been doubly distorted: firstly, by inadequate views of science itself, as the preceding chapter argued; and secondly, by the attempt to apply these inadequate views to human action in the environment. Just as chapter II attempted to work systematically through the first set of issues, so this chapter now attempts a similar task

for the second set of issues. The approach taken here is similar to that of chapter 2: the traditional view of the philosophy of social science, which espouses the essential unity of the natural and the social sciences, is first outlined; the challenge to this view launched by Winch (1958), along lines similar to Kuhn's in the context of the natural sciences, and by Collingwood (1948) is then discussed; the responses to these critiques are briefly considered; and a resolution to the main issues raised is finally sought, and discussed in the light of the conclusions of chapter II.

One can sympathize, to a certain extent, with the geographer who finds such an exercise tedious, and therefore either treats it cursorily and superficially, or holds it in abeyance, or ignores it altogether in order to advance more quickly to those matters in which, as a geographer, his real interest lies; and yet, as this dissertation has consistently argued, if the geographer wishes his discipline to have the apparent benefits that scientific status confers, it is an exercise which cannot be avoided. To proceed without a satisfactory resolution of these matters is to place at risk the validity, unity, and relevance of the discipline as a whole. A primary contention of this dissertation, it is worth reminding ourselves, is that the present fragmentation and vulnerability of the discipline is due, in no small part, to just such an attitude, and to the conceptual inadequacies to which it has given rise.

3.1.2 Positivistic approaches to the social studies

The notion that the logic of science can embrace both the natural and the social sciences, and systematically give rise to the most comprehensive and fruitful explanations in both contexts, is well established within both

philosophy and the relevant disciplines. Winch (1958, p. 66) points out that it stems primarily from the work of John Stuart Mill, especially in Book VI of his System of Logic (Mill 1974) entitled "Logic of the Moral Sciences," for which sciences

> The logic is the same as that of any other science and all that has to be done is to elucidate certain difficulties arising in its application. (Winch 1958, p. 67)

Mill's view is directed towards the necessity of reductive explanations (or, as he termed it, the Inverse Deductive Method), and is discussed in some detail below (see section 3.2.2). For our purposes, it is sufficient to note here that the position stated above

> underlies the pronouncements of a large proportion of contemporary social scientists, even if they do not always make it explicit . . . (Winch 1958, p. 66)

In general terms, it is a position which later philosophers of social science, too, have agreed (Popper 1957; Nagel 1961;Rudner 1966). In Popper's language, it becomes:

> . . . a doctrine of the unity of method; that is to say, the view that all theoretical or generalizing sciences make use of the same method, whether they are natural sciences or social sciences. (Popper 1958, p. 130)

As we shall see, however, Popper treats history as a special case under this general rubric. Moreover, while adhering to Mill's conception of the unity of the natural and social sciences, Popper does not accept the terms in which Mill couches his notion of explanation, which fails to distinguish between universal laws and initial conditions; a distinction which, as we have seen, is fundamental to the Popper-Hempel notion of scientific explanation. Thus where Mill looks to explain an individual fact by pointing to its cause in the laws of which it is an instance, Popper prefers to say, more precisely:

> . . . the causal explanation of a regularity consists in the deduction of a law (containing the conditions under which the regularity asserted holds) from a set of more general laws which have been tested and confirmed independently. (Popper 1958, p. 125)

The model thus offered by Popper as the basis for explanation in the social sciences is the hypothetico-deductive form with which we are already familiar (see chapter II). It is succinctly summarized, in this context, by Popper as follows:

> The methods always consist in offering deductive causal explanations, and in testing them (by way of predictions). This has sometimes been called the hypothetical-deductive method, or more often the method of hypothesis, for it does not achieve absolute certainty for any of the scientific statements which it tests; rather, these statements always retain the character of tentative hypotheses, even though their character of tentativeness may cease to be obvious after they have passed a great number of severe tests. (Popper 1957, p. 131)

In this model, Popper goes on to point out, the notions of explanation, prediction and testing are identifiable only in terms of the different emphases placed on structure: an emphasis on laws and initial conditions gives rise to explanation; on the prognosis implied by these, to prediction; and on the assessment of laws or initial conditions in the light of the prognosis, to testing. He confirms again that nothing is asserted here about the means by which hypotheses are developed; these are, he proposes, a "private matter" in both the natural and the social sciences (Popper 1957, p. 135). The objects with which social science is concerned, he argues, are clearly

> theoretical constructions used to interpret our experience . . . the result of constructing certain <u>models</u> (especially of institutions), in order to explain certain experiences--a familiar theoretical method in the natural sciences (where we construct our models of atoms, molecules, solids, liquids, etc.). (Popper 1957, p. 135)

Popper warns that such theoretical constructions must be seen, particularly in social science, for what they are: abstract hypothesis and not concrete reality. In order to guard against this latter possibility, he espouses methodological individualism in the social sciences (see discussion below, section 3.2.2):

> . . . the task of social theory is to construct and to analyse our sociological models carefully in descriptive or nominalist terms, that is to say, <u>in terms of individuals</u>, of their attitudes, expectations, relations, etc. (Popper 1957, p. 136)

The status of our conclusions in the social sciences, he concludes, is no different from that of the natural sciences, in that it only precludes certain possibilities (the position, as we have seen, of methodological falsificationism, as defined by Lakatos). There is no reason, he goes on, to assume that social phenomena are any more complex than physical phenomena; indeed, he sees in the element of rationality, with which every human action in social situations is imbued, good reason to argue that <u>in situ</u> social situations are more amenable to systematic analysis than corresponding physical situations:

> Admittedly, human beings hardly ever act quite rationally (i.e., as they would if they could make the optimal use of all available information for the attainment of whatever ends they may have), but they act, none the less, more or less rationally; and this makes it possible to construct comparatively simple models of their actions and interactions, and to use these models as approximations. (Popper 1957, p. 141)

This is a particularly interesting position since, as we shall see, Winch (1958) infers the exact converse from the same premises. It leads Popper to support the "zero method" of modeling in the social sciences, familiar to us in human geography from central-place theory:

> By this I mean the method of constructing a model on the assumption of complete rationality (and perhaps also on the assumption of the possession of complete information) on the part of all the individuals concerned, and of estimating the deviation of the actual behaviour of people from the model behaviour, using the latter as a kind of zero-coordinate. (Popper 1957, p. 141)

Finally, Popper concedes some difference between the natural and the social sciences in the ease of applying quantitative methods, despite advances in statistical methods; and confirms the importance of resolving such difficulties, since

> . . . for without such measurement we should often not know whether or not some counteracting influences exceeded an effect calculated in merely qualitative terms. (Popper, 1957, p. 142)

The problem lies, he proposes, not so much in the number and complexity of interrelationship of the variables of social science, but in the fact that they are quickly changing, in contrast to the natural constants of the physical sciences. Again this observation, centreing on the notion of constancy, is significant, and one we shall take up in succeeding sections.

With this account, Nagel (1961, ch. 13) essentially agrees. He emphasizes, however, that explanation in the social sciences will almost always be of the deductive-statistical form: in such a schema, it will be recalled (see section 2.2), explanation is statistical by virtue of the "theories containing statistical assumptions (being) the explanatory premises for various (physical) laws" (Nagel 1961, p. 509). Rudner (1966, pp. 65-67), indeed, goes further in allowing for the use of "rational foresight" in the social sciences, since

> . . . we are sometimes justified in accepting scientific hypotheses about the future even if the evidence supporting them (i.e., statements adduced in their support) may fail to include any that deductively imply such a hypothesis. (Rudner 1966, p. 66)

It is necessary, he claims, to relax the conditions under which explanation is to be permitted in the social sciences because few true laws, defined by their claim

> . . . that given the occurrence of a certain type of characteristic, there will concomitantly occur--without exception--a characteristic of a second type (Rudner 1966, p. 67),

have been discovered there. This state of affairs is attributed by Nagel not to the commonly proposed difficulties attendant upon the greater complexity of social science subject matter, nor to the notion of the free will of human conduct, but to the inability of social scientists to produce discriminating classifications of social phenomena:

> . . . the terms employed in empirical social research frequently possess an indeterminate connotation; they codify less refined or detailed distinctions than do the terms occurring in the laws of the natural sciences; and the items subsumed under them are in consequence usually far less homogeneous in pertinent respects than are these latter terms. (Nagel 1961, p. 506)

Both Nagel and Rudner support Popper's identification of the "zero method" (Popper 1957, p. 141), or the use of idealizations, as most appropriate to the social studies. Nagel points out that the physical sciences, too, employ this method, since "the experimental evidence for the universal laws of physical science is rarely if ever in perfect agreement with them" (Nagel 1961, p. 508): all experimentation, by its very nature, yields statistical results. However, he observes, the physical sciences have been successful largely by pursuing a particular logical strategy:

> . . . it is possible in many branches of natural science to state laws as universally valid under certain "ideal" conditions and for "pure cases" of the phenomena investigated, and to account systematically for any discrepancies between what the laws assert and what observation reveals in terms of more or less well-authenticated discrepancies between those ideal conditions and the actual ones under which the observations are made. (Nagel 1961, p. 508)

Nevertheless, he suggests that this strategy has not been so successful in the social sciences, because of the paucity of theory which might give rise to idealizations that adequately account for actual conditions. Rudner supports this view, giving the example of economically rational behavior:

> We might expect that a general theory of social behavior or a general theory of social action would, were it a viable theory, have variables of behaviour such that description of . . . rational behaviour . . . could be <u>derived</u> by letting those variables take on extreme values. Such a comprehensive theory of social action would, in short, subsume economically rational behaviour (or sensible behaviour) as a special case. Unfortunately, no such comprehensive theory seems at present to be available. (Rudner 1966, p. 62)

This analysis does, however, help to explain why explanation in the social sciences has been almost exclusively statistical:

> As the history of science as well as common experience amply testify, correlations between empirical data are rarely perfect, and generalizations based exclusively on such correlations are almost inevitably bound to be statistical. (Nagel 1961, p. 509)

One of the most interesting points made by Nagel in his analysis of explanation in the social sciences is that greater deductive certainty is achievable in the social studies, but that, in moving in the direction of locating more elements which approximate natural constants (as suggested by Popper (1957, p. 143)), explanation tends to become trivial:

> It is not too difficult to state well-founded universal generalizations about social phenomena. However, such generalizations would frequently be regarded as trivial, either because they assert what is "obvious" or because they fail to make distinctions that are held to be "important." (Nagel 1961, p. 509 fn.)

This is, indeed, as we have seen in chapter I, the burden of the humanistic critique of positivistic human geography. The examples that Nagel provides in support of this proposition, however, raise some questions about the real meaning of this point:

> For example, there appear to be no exceptions to the generalization that every religion has some form of collective ritual for renewing the common sentiments of its adherents, nor to the generalization that all delinquent children are found in societies in which there is a socially structured tension between cultural goals and institutionalized means for achieving them. The first of these is perhaps a candidate for the class of "obvious" trivia, the second for the class of "unimportant" ones (since it does not distinguish between types of tensions or between kinds of goals that are commonly regarded as of the greatest practical moment. (Nagel 1961, p. 509 fn.)

It is far from clear what makes either of these generalizations either obvious or trivial; indeed, one is struck by the improbability of the social sciences' achieving such valuable insights. What is true, however, is that both generalizations raise further questions of a more fundamental order: for instance, the nature of such common religious sentiments that seem to require renewal in this manner; the nature of the tension that can be so institutionally structured; the mechanics, so to speak, of institutional structuration; and so on. Nevertheless, the fact that such questions can be raised does not, in principle, establish their triviality or unimportance: on the contrary, as we have seen, one of the chief values of establishing an enquiry in scientific terms is its ability to move systematically towards more fundamental areas of explanation. Implicit in the nature of scientific enquiry itself is the existence of such questions. We may feel, therefore, that the issue is not so much whether a generalization is trivial, obvious, or unimportant, but whether it is satisfactorily grounded in (deducible from) an acceptable body of theory. We conclude that the task is to establish such a body of theory, rather than abandon the attempt to develop a science of social inquiry on the grounds that such a science would be, <u>a priori</u>, limited to trivialization.

3.1.3 Critiques of positivism in the social sciences

The view of Popper, Hempel, Nagel, Rudner and other positivists that the natural sciences and the social sciences share in a unity of method, expressed by the hypothetico-deductive model developed in the context of the physical sciences, has been subjected to strong criticism. The strategy of this critique in the context of the social sciences has been parallel to Kuhn's approach in the context of the natural sciences: essentially it has been to deny to explanation in the social sciences any neutral frame of reference outside the social phenomena for which explanation is being developed. The challenge has been mounted from two directions: from the modern philosophic tradition itself, which finds its intellectual roots in Wittgenstein, and its affinity with traditional social theory in Weber; and from history, with its long tradition of practice and theory in understanding unique historical events, societies, and periods. Reviewing the literature of this critique, it is clear that two works have been seminal to it: Winch (1958) and Collingwood (1948). Although each of these works mounts the attack on positivism from an entirely original position--the first in philosophy, the second in history--their concerns are similar, and subsequent commentators such as Ryan (1970) have seen them as mutually supporting. They are therefore discussed in some detail. Responses to these critiques by both positivists and anti-positivists will be then be considered. Finally, an attempt will be made to draw some conclusions about the validity of the two positions in the social sciences, and to give direction to the analysis of human geography which follows in succeeding chapters.

Winch on "The Idea of a Social Science" (1958)

Winch begins his analysis by noting that philosophy is centrally concerned with the understanding of reality, and that this concern leads inevitably to an interest in language:

> Inseparably bound up with the question whether reality is intelligible . . . is the question of how language is connected with reality, of what it is to say something. (Winch 1958, p. 12)

Following Wittgenstein, he therefore denies the possibility of separating "the world" and "the language in which we try to describe the world":

> Our idea of what belongs to the realm of reality is given for us in the language that we use. The concepts we have settle for us the form of the experience we have of the world. . . . That is not to say that our concepts may not change; but when they do, that means that our concept of the world has changed too. (Winch 1958, p. 15)

It follows, then, that the notion of intelligibility is not something that can be placed on an absolute basis as it is applied to the understanding of different contexts; for instance, of science, history, and art:

> . . . it becomes apparent that the notion of intelligibility is systematically ambiguous . . . in its use in those contexts: that is, its sense varies systematically according to the particular context in which it is being used. (Winch 1958, p. 18)

The importance of this principle for social inquiry lies in the fact that the phenomena with which the social scientist is concerned are, in Winch's view, expressions of different contexts of intelligibility:

> A man's social relations with his fellows are permeated with his ideas about reality . . . social relations are expressions of ideas about reality. (Winch 1958, p. 23)

It is here that the intimate relationship between language and intelligibility becomes particularly significant. Winch argues that language can only be

developed within a context of rules; and a rule is a social construction. A rule is, he asserts, following Wittgenstein, a categorical structure through which reality is apprehended and experienced, and it can only be formed in conjunction with other people. It must be possible for other people to decide whether a particular behaviour is following the rule or not, otherwise it makes no sense to talk of a rule at all. Just as, he argues, it is not possible to conceive of a private language, developed in complete isolation, so it is not possible to conceive of a private system or rules; indeed, the one implies the other. In summary, therefore, he proposes that human behaviour is an expression of intelligibility which, in its essential nature of language and rule structures, must arise in a social setting and is, in consequence, a social product. Thus at the heart of social understanding lies epistemology, which

> . . . will try to elucidate what is involved in the notion of a form of life as such. (Winch 1958, p. 41)

Without such an elucidation, Winch argues, social phenomena cannot legitimately be approached.

A clear implication of this line of analysis is that there is no such thing as "mere" behaviour; that is, behaviour observed and described in some kind of socially neutral context, as required by positivistic approaches to the social studies. We are concerned, asserts Winch, with those forms of human activity which have "a <u>meaning</u>, a <u>symbolic</u> character," the "subjective sense" that Weber associates with all behaviour, and which is implicit in such notions as "<u>motive</u> and <u>reason</u>" (Winch 1958, p. 45). Motive, he goes on to remark, is in this conception rational motive:

> . . . a meaningful configuration of circumstances which, to the agent or observer, appears as a meaningful "reason" . . . of the behaviour in question. (Weber, quoted in Winch 1958, p. 45)

The observation and explanation of social phenomena requires, therefore, not one level of explication, as in the natural sciences (what the observer observes and understands to be true), but two: the validity of explanation rests on the fact that the concepts of meaning expressed in it are grasped not only by the observer, and those to whom he addresses his theories (vicarious observers), but also by the agents themselves. Unless it can be demonstrated that the context of meaning in which an agent carries out action is the same as that in terms of which the explanation is being couched, the explanation cannot be considered valid. All explanations of social phenomena, Winch proposes, must be of this character: even Freudian notions of rationalization rest on the meaning of concepts; and apparently motiveless action, such as traditional or undeliberated action, requires choice directed either to normative behaviour, or to commitment to future action; in both of which, Winch argues, the notion of a rule is implicit. Nor can the anarchist be excepted from this principle, since he

> . . . has reasons for acting as he does; he <u>makes a point</u> of not being governed by explicit, rigid norms. Although he retains his freedom of choice, yet they are still significant choices that he makes: they are guided by considerations, and he may have good reasons for choosing one course rather than another. And these notions, which are essential in describing the anarchist's mode of behaviour, presuppose the notion of a rule. (Winch 1958, p. 53)

Against Oakshott's proposal that the best description of human behaviour is in terms of habit or custom, Winch argues that both notions are simply codifications of rules. They imply criteria of right and wrong action that are dependent on the possibility of reflection, and on an understanding of the alternatives available, and hence can be regarded as forms of meaningful behaviour.

In approaching Mill's conception of explanation in the natural and social sciences--which, as we have seen, has its basis in an assertion of the unity of method of the two contexts, and in reduction of social regularities to the laws of individual thought--Winch is therefore able to mount a convincing attack:

> It is not a question of what empirical research may show to be the case, but of what philosophical analysis reveals about <u>what it makes sense to say</u> . . . the notion of a human society involves a scheme of concepts which is logically incompatible with the kinds of explanation offered in the natural sciences. (Winch 1958, p. 72)

It is not, he goes on, just a question of the greater complexity of human as opposed to physical phenomena (although, as we have seen, Popper has questioned this distinction, too): the primary difference is one of kind, not of degree. Moreover, attempts to locate the explanation of human action in the conventional categories of "motives," "dispositions," "purposes" and so on--whether interpreted as conscious events, following Mill, or as states of the organism, following some current psychological practice--are misconceived. Understanding an action means understanding the context of meaning in which it is undertaken, and need not refer in any sense to conscious events or to physiological states. Nor, Winch argues, is Ryle's attempt to appeal to disposition any more valuable:

> Ryle says that a statement about someone's motives is to be understood as a "law-like proposition" describing the agent's propensity to act in certain kinds of way on certain kinds of occasion. . . . But the "law-like proposition" in terms of which N's reasons must be understood concerns not N's dispositions but the accepted standards of reasonable behaviour current in his society. . . . Learning what a motive is belongs to learning the standards governing life in the society in which one lives; and that again belongs to the process of learning to live as a social being. (Winch 1958, pp. 81, 83)

Clearly this analysis places the observer who wishes to obtain an empirical and generalized account of social phenomena in an almost untenable position. Winch prefigures Kuhn in pointing out that the observer, too, is experiencing the world within a particular framework of meaning, which is developed in conjunction with his fellow observers as much as with the phenomena he investigates. In the natural sciences, the rules which embody that framework of meaning

> . . . rest on a social context of common activity. So to understand the activities of an individual scientific investigator we must take account of two sets of relations: first, his relation to the phenomena which he investigates; second, his relation to his fellow-scientists. Both of these are essential to the sense of saying that he is "detecting regularities" or "discovering uniformities"; but writers on scientific "methodology" too often concentrate on the first and overlook the importance of the second. (Winch 1958, pp. 84-85)

In the context of the investigation of social phenomena, this difficulty obviously becomes particularly acute:

> The concepts and criteria according to which the sociologist judges that, in two situations, the same thing has happened, or the same action performed, must be understood <u>in relation to the rules governing sociological investigation</u>. But . . . whereas in the case of the natural scientist we have to deal with only one set of rules, namely those governing the scientist's investigation itself, here <u>what the sociologist is studying</u>, as well as his study of it, is a human activity and is therefore carried on according to rules. And it is these rules, rather than those which govern the sociologist's investigation, which specify what is to count as "doing the same kind of thing" in relation to that kind of activity. (Winch 1958, pp. 86-87)

Clearly it is this conflict between the contexts of meaning of the observer and those of the observed which becomes particularly acute when dealing with past historical periods, or with different contemporary cultures, and which leads to the claim that Weberian verstehen or idealist re-thinking or re-enactment are

the only legitimate methods for the study of social phenomena. Moreover, it eliminates the possibility of making prediction in social inquiry, and of using the fulfillment or non-fulfillment of that prediction to bear on the validity of the generalization to which it is deductively related (as Popper requires), since, even if we feel that we have identified a rule, the question still arises of "what is involved in following that rule" (Winch 1958, p. 92): it is just the awareness of alternatives that, in Winch's view, makes a rule a rule. The format of explanation may be seen as reducing to an inductive-statistical model, which, as we have seen (see chapter II) a counter-instance neither confirms nor disconfirms; yet even this interpretation cannot be sustained. Statistical facts, according to Winch's analysis, are different in kind from the facts of meaning being sought or asserted:

> The difference is precisely analogous to that between being able to formulate statistical laws about the likely occurrences of words in a language and being able to understand what was being <u>said</u> by someone who spoke the language. . . . "Understanding" . . . is grasping the <u>point</u> or <u>meaning</u> of what is being done or said. This is a notion for removed from the world of statistics and causal laws. (Winch 1958, p. 115)

In summary, therefore, Winch is arguing that, whatever may be their external expressions, all social phenomena are internal, in the sense of having their roots in a subjectively apprehended structure of meaning. In this view, there is a close equivalence between ideas, language, intelligibility, and social relations which leads to the conclusion of the internality of social relations:

> If social relations between men exist only in and through their ideas, then, since the relations between ideas are internal relations, social relations must be a species of internal relation too. (Winch 1958, p. 123)

All human action is seen as having this character: to act is to understand the concept on which action is based:

> . . . the performance of such acts is itself the chief manifestation of their possession of those concepts. (Winch 1958, p. 125)

This places Winch squarely in opposition to Popper's espousal of methodological individualism, which denies the reality of social structures of this type:

> Popper's statement that social institutions are just explanatory models introduced by the social scientist for his own purposes is palpably untrue. The ways of thinking embodied in institutions govern the way the members of societies studied by the social scientist behave. (Winch 1958, p. 127)

On the other hand, it enables him to lend qualified support to Collingwood's notion of history as the history of thought. While expressing doubt about the validity of the method of re-enactment which Collingwood proposes, Winch nevertheless supports the idea that the empiricist approach to history is misconceived, since it underestimates the importance of ideas and theories in history. As these are constantly developing and changing, and must be understood each in its own context, they are not likely to be well explicated by generalizations of the scientific sort:

> Historical explanation is not the application of generalizations and theories to particular instances: it is the tracing of internal relations. (Winch 1958, p. 133)

The relationship between such generalization, or theories, and the "historical narrative" (a term which, as we shall see, raises problems of its own) is, he goes on, more like the relationship between theories of logic and examples of reasoning in particular languages, than like the relationship between scientific theories and experimental data: in the natural sciences,

> It is only <u>in terms of the theory</u> that one can speak of the events being thus "connected" . . . the only way to grasp the connection is to learn the theory. But the application of a logical theory to a particular piece of reasoning is not like that. One does not have to know the theory in order to appreciate the connection between

> the steps of the argument; on the contrary, it is only in so far as one can already grasp logical connections between particular statements in particular languages that one is even in a position to understand what the logical theory is all about. (Winch 1958, p. 134)

Thus, he concludes, in contrast to the natural sciences, it is not through a knowledge of historical "laws" that one can come to terms with historical phenomena: on the contrary,

> it is only in so far as one has an <u>independent</u> historical grasp of situations . . . that one is able to understand what the law amounts to at all. (Winch 1958, p. 136)

<u>Responses to Winch</u>

Responses to Winch by positivist theorists, or by theorists who wish to retain, by suitable compromise, predominantly positivist modes of thinking about the social sciences, have varied widely. Rudner (1966) is representative of the extreme positivist position which rejects out of hand Winch's arguments; Nagel (1961) presents a more reasoned and careful consideration; and Ryan (1970) not only comes to terms with the fundamental philosophical notions presented by Winch, but constructs an original defence of causal explanation which incorporates those notions. These three are now briefly considered, and we may regard them as representative of the positivist defence in the face of Winch's challenge.

Rudner (1966), while accepting that Winch's "is a complex argument" with wide ramifications, nevertheless seeks to dismiss it in a single stroke by appealing to the "reproductive fallacy." The grounds on which this rejection is based are, however, not convincing, and rest on some fundamental misconceptions about the nature of Winch's position. Rudner attempts to defend

the positivist stance by attacking Winch's notion of "understanding". Winch, he asserts, lends plausibility to his position by "equivocation" over this notion:

> There are at least two senses of "understanding" at issue, one of which warrants the application of the term only if the individual to whom it applies has had certain direct experiences of the subject matter being "understood." "Understand," in the other sense, does not have the occurrence of such experiences as a necessary condition. For example, with respect to the natural sciences, it is generally agreed that a scientific understanding or knowledge of things or events of a given kind does not necessarily presuppose direct experience of such things. . . . we have achieved the understanding appropriate in natural science when we have achieved, say, a causal explanation of the type of event being investigated. (Rudner 1966, p. 82)

Rudner supports this criticism by reference to the understanding of a natural phenomenon, such as a tornado, which does not have to be described in terms of experience to be understood scientifically. To require the kind of complete understanding which includes experiential aspects of the phenomenon is to fall into the reproductive fallacy:

> It is the function of science to describe the world, not to reproduce it. . . . the claim that the only understanding appropriate to social science is one that consists of a reproduction of the conditions or states of affairs being studied, is logically the same as the claim that the only understanding appropriate to the investigation of tornados is that gained in the direct experience of tornados. (Rudner 1966, p. 83)

Rudner concedes that some kind of knowledge or understanding is implicit in experiencing a social phenomenon, like religion, but that there is no necessity to draw from this the conclusion that this kind of understanding is the only kind appropriate to understanding and explanation in the social sciences. There is no reason, therefore, to insist upon explanation in the social sciences being any different from that in the natural sciences: methodological objectivity in both remains intact.

It is not easy to see validity in this response. To begin with, there is no "equivocation" in Winch's use of the term "understanding": equivocation carries with it the sense of deliberate ambiguity that is exploited to obscure logical flaws in the argument. Winch, on the contrary, is clear and consistent in his idea of understanding in social inquiry: his position is simply that there is a difference, not just of degree of complexity in explanation between natural and social phenomena, but of kind. There is no equivocation in this position: it may be different from the position on understanding taken by Rudner, but that does not make it deliberately ambiguous.

Moreover, Rudner's attempt to dismiss the arguments presented by Winch by appeal to the "reproductive fallacy" entirely misses the point Winch is trying to make. No one can doubt that the framing of hypotheses and the use of orthodox scientific method to test them can be applied to the social studies; indeed, it is done all the time. Winch certainly accepts that such explanatory schema can be developed. The point, however, is not whether such schema can be developed, but how useful they are, how appropriate to the phenomena of social life. Winch's argument is that they are not appropriate, that they present a description of social life that is far removed from its reality, since that reality is intimately bound up with the contexts of meaning that underlie social behaviour. The terms in which such scientific schema must be constructed are not neutral with respect to the context of meaning: there is no independent language which can be used to frame the statements that comprise them. Thus the difference between explanations in the natural sciences and the social sciences is a logical difference;

> . . . the notion of a human society involves a scheme of concepts which is logically incompatible with the kinds of explanation offered in the natural sciences. (Winch 1958, p. 72)

It is clear, therefore, that Rudner has failed to come to terms with the central thesis presented by Winch, and has addressed himself instead to a distorted and trivialized version of that thesis that can conveniently be treated under the idea of the reproductive fallacy. Winch's critique of positivism remains unaffected by such a response.

Nagel (1961), while not referring specifically to the arguments raised by Winch, nevertheless deals with matters that are closely related to them. Under an explanation of the methodological problems of the social sciences, he alludes to the notions of cultural relativity, and of the subjective nature of phenomena in the social sciences, both of which are, as we have seen, central components of Winch's position. With respect to the first issue, Nagel summarizes the difficulty raised by the idea that human social behaviour occurs within a context of culture that varies from society to society:

> Accordingly, since the forms assumed by human social behavior depend not only on the immediate occasions that call forth the behavior but also on the culturally instituted habits and interpretations of events involved in the response to the occasion, the patterns of social behavior will vary with the society in which the behavior occurs and with the character of its institutions at a given historical period. (Nagel 1961, p. 459)

It is clear that the idea of a culture, defined in this way as "habits" and "interpretations" brings it very close to Winch's conception of "rules," which are developed in a social context and which describe both the behaviour and the notions of reality that underlies it. Nagel concedes the force of this objection to the use of scientific method in approaching social phenomena, pointing out that, while some generalizable statements about social life in cross-cultural contexts may be developed, the pervasiveness of culture makes it likely that such statements will be at the relatively trivial level of basic

biological and environmental facts. Thus, although these facts cannot be considered independently of technologies and traditions of using the environment,

> The possibility must certainly be admitted that nontrivial but reliably established laws about social phenomena will always have only a narrowly restricted generality. (Nagel 1961, p. 460)

Nagel seeks to vitiate the force of this objection by referring, firstly, to an assumption often implicit in it that a true scientific theory must allow for accurate prediction some time into the future: this, he asserts, is not true of science, where the systems studied are not often sufficiently isolated from other systems to allow long-range prediction, and should not therefore be required of a social science, confronted with contexts of culture that are always growing and changing. It is difficult to see, however, however, how this observation addresses the question at issue: the assumption identified by Nagel as implicit to it seems not necessarily to be so; and objections to that assumption, however justified in its own terms (and we do not deny Nagel's point), do not bear on the principal objection, which relates not so much to the question of change as such, but to the question of differences of cultural context.

He goes on to note that the wide variety of types and patterns of social behaviour from culture to culture is, in principle, no bar to the establishment of a single theory in which all these expressions are embedded, just as in the natural sciences, the development of more comprehensive theories has consistently coordinated widely dissimilar phenomena. However, he refrains from attempting to predict the form such a theory would take, and contents himself with noting that to discount such a possibility is not justified. It is difficult, however, to see what kind of theoretical structure could fulfill

such a role if Winch's thesis is to be accepted: clearly it would have to be of such generality and power as to be able to account for different contexts of meaning. We may agree with Nagel that such a possibility cannot be logically ruled out; but before the impact of Winch's contention that the social context of meaning is the fundamental reality of social life can be reduced, it would seem to be necessary to produce a viable theoretical structure of the kind sketched by Nagel. Indeed, even after going on to note that the "historically conditioned" character of social phenomena does not in principle prevent the development of scientific understanding of them, since idealizations and variables of unspecified generality are employed in science to deal with just such variation, Nagel concludes by expressing pessimism about the possibility of establishing cross-cultural laws:

> . . . the possibility must also be recognized that in comparison with the variables employed in the past in proposed transcultural laws, the concepts required for this purpose may have to be much more "abstract," may need to be separated by a greater "logical gap" from the familiar notions used in the daily business of social life, and may necessitate a mastery of far more complicated techniques for manipulating the concepts in the analysis of actual social phenomena. (Nagel 1961, pp. 465-466)

In the context of this dissertation this is an important point, and we will return to it in section 3.1.4 and in chapter VI; here it is sufficient to note that the analysis undertaken and conjectures made by Nagel so far do not materially affect the force of Winch's contention.

The second relevant issue raised by Nagel concerns the subjective nature of social subject matter. Nagel characterizes this difficulty as follows:

> The subject matter of the social sciences is frequently identified as purposive human action, directed to attaining various ends or "values," whether with conscious intent, by force or acquired habit, or because of unwitting involvement. . . . its study is commonly said to presuppose familiarity with the motives and other psychological matters that constitute the springs

> of purposive human behaviour, as well as with the aims and values whose attainment is the explicit or implicit goal of such behaviour. (Nagel 1961, pp. 473-474)

He goes on to refer to the apparent need to experience directly these "springs of purposive human behaviour" in order to understand them, and to draw explanations from "what the human actors themselves believe about those things" (Nagel 1961, p. 474), rather from what might be observed in some notionally objective manner. Like Popper, Nagel points out that the observer, being himself involved in social life, is able to achieve this understanding by drawing on "his own subjective experience of the 'internal meanings' of social actions" (Nagel 1961, p. 475). Clearly this has much in common with Winch's insistence upon the internality of social relations consequent upon the context of meaning in which social action arises and with which it interacts, and to the nature of understanding of social phenomena which he endorses.

In response to this view, Nagel argues, firstly, that although the understanding of subjective states is usually necessary to social explanation, it may not be sufficient to it: it may be necessary to refer to matters that lie outside the framework of beliefs of the agents themselves, such as the actual actions they undertake, or insights established in other contexts of investigation which bear on the utility or effectiveness of those actions. To this, however, Winch might respond that, although the observable actions, and information brought to bear on the social context from outside it, may add to the understanding of the subjective "springs" of purposive human behaviour--in the first case, by demonstrating the integration of objective and subjective social life, in the second, by illuminating it from a different perspective--these approaches cannot function as structures of explanation of social life on their own: they must be referred to the fundamental understanding of rules and meaning in order to contribute anything meaningful at all.

As a second response to the critique of scientific method based on the subjective nature of social subject matter, Nagel forwards the claim of behaviourism to represent a legitimate objective response to the study of social phenomena. He emphasizes that behaviourism does not deny the existence of subjective states, nor their relevance to human behaviour, but takes instead a methodological stance which seeks "to base psychology upon publically observable data" (Nagel 1961, p. 477). He notes, moreover, that the more credible forms of behaviourism accept self-report as a legitimate form of investigation, and do not commit themselves to that extreme of reductionism which would seek to reduce all subjective states to statements made "in terms belonging unmistakably to the languages of physics, physiology, or general logic" (Nagel 1961, p. 479). Nevertheless, he argues, such a position is not inconsistent with the view that subjective states are regularly associated with particular complexes of externally observable behaviour, be it physiological, verbal, or active;

> . . . a behaviourist can maintain without inconsistency that there are indeed such things as private psychic states, and also that the controlled study of overt behaviour is nevertheless the only sound procedure for achieving reliable knowledge concerning individual and social action. (Nagel 1961, p. 480)

Nagel then points to a variant of behaviourism which seeks to establish explanatory relations (in the sense of the orthodox deductive model, which combines law statements, statements about initial conditions, and statements about predicted events) between observed behaviour and underlying models or unobservable states and processes. This kind of reductionist program, he remarks, has ambitions that at present range far beyond its actual achievements; but

> . . . provided that the program does not dismiss well attested forms of human conduct as in some sense "unreal"--and there is no reason inherent to the program

> why it should--it cannot be ruled out on a priori grounds as illegitimate or as intrinsically absurd. (Nagel 1961, p. 480)

Again, however, we may feel that this response fails adequately to meet Winch's challenge. If Winch is correct, the understanding of social behaviour must appeal to the socially constructed contexts of meaning within which action is carried out. Unless the systems and processes, or states, or self-reports that the behaviourist seeks to coordinate with observed behaviour address the notion of meaning, they will not, in Winch's view, bear on the true springs of human action. Indeed, he might go on, unless the terms in which such explanations are couched are constructed within the structure of meaning, they have no relation at all to the reality of the social life they are describing. And if they do include such a perspective, then they simply exemplify the kind of understanding Winch is advocating, and cease to become behaviourist in any real sense of the term.

Finally Nagel puts forward the interesting view that, even granting the fundamental importance of understanding social phenomena in terms of their subjective categories of meaning, empathy and identification are not the ways to such an understanding, which still must be constructed according to the canons of deductive logic. He argues that there is no foundation to the assumption that sympathetic identification on the basis of the observer's own experience is a reliable means of understanding the subjective states of the agent he is observing; nor can it be assumed that, unless such internal experience is available to the observer, no understanding can be achieved. He puts the argument succinctly as follows:

> . . . the fact that the social scientist, unlike the student of inanimate nature, is able to project himself by sympathetic imagination into the phenomena he is attempting to understand, is pertinent to questions concerning the origins of his explanatory hypotheses but not to questions concerning their validity. His ability

> to enter into relations of empathy with the human actors in some social process may indeed be heuristically important in his efforts to invent suitable hypotheses which will explain the process. Nevertheless, his empathic identification with those individuals does not, by itself, constitute knowledge. The fact that he achieves such identification does not annul the need for objective evidence, assessed in accordance with logical principles that are common to all controlled inquiries, to support his imputation of subjective states to those human agents. (Nagel 1961, pp. 484-485)

The difficulty is, however, that it is by no means clear what constitutes the "objective" evidence that is to be used in constructing such an explanation. Winch's point is that all facts of social life have a primary dimension of meaning that makes social relations internal; and, if this is granted, there are no such things as "mere" or "objective" social facts. All have to be interpreted in the light of the context of meaning. To make a prediction by combining theory and initial conditions in the orthodox model is, in Winch's view, to assume a context of meaning in which the statements of which the model is constructed become intelligible. It is clear, therefore, that this response of Nagel's, too, fails to meet Winch's challenge.

Ryan (1970) presents a more detailed and tightly reasoned critique of Winch's thesis than is developed by either Rudner or Nagel. He begins by noting that to accept Winch's position does not imply that "we have to stop at the level of understanding which the participants themselves enjoy" (Ryan 1970, p. 152); as Winch himself has pointed out (Winch 1958, pp. 89-90), all that he requires is that the act of reflection which is to give rise to an explanation, even if that explanation is couched in technical terms that lie outside the ambit of the context of meaning employed by the participants, should be "based on, or rooted in that (account) of the actors themselves" (Ryan 1970, p. 153). He accepts, however, that Winch's view of cultural relativity, and of the absence of objective, culturally-neutral matters of fact, cannot be denied, but

that its significance varies from one social study to another. It is critical, for instance, for anthropology (and, we might add, for cultural geography), and less so for economics (or economic geography),

> . . . where the conceptual problems are taken as solved, and the attempt then made to understand the regularities which appear in economic activity by seeing what mathematical relationships hold between crucial variables. (Ryan 1970, P. 157)

Ryan goes on to examine Winch's contention that all actions must be elucidated by reference to their internal logic. He notes that such a criterion appears to rule out such commonsense notions as doing something for "no reason," or that if his criterion is to be maintained for such actions it becomes trivial (one can elucidate the context of meaning of going for a stroll, to use MacIntyre's (1973) example, but the result may not tell us anything of importance). He affirms, moreover, that is is possible to evaluate the rationality of actions, even in their own terms:

> . . . once we have discovered what the agents themselves believe the point of their activities to be, we can certainly go on to ask whether the world is so constructed that their accounts are plausible or implausible, and hence whether their behaviour is rational or not. We can do this because ways of life overlap and compete, share assumptions and serve different purposes. . . . While it is true that there is no single God-like stance from which to evaluate these processes, it remains the case that there are various human stances from which to make the evaluation, and make it we do. (Ryan 1970, pp. 161-162)

It is far from clear, however, how Ryan finds it possible to maintain this point of view: just how it is that ways of life "overlap" is not elaborated, and in consequence the "various human stances" from which we are to make an evaluation of rationality remain obscure. It is not enough to assert that such evaluations are in fact made: it is necessary to show that they are validly made, and this Ryan fails to do. This deficiency, moreover, is all the more

striking since Ryan is here not claiming, as Nagel does, that cross-cultural generalizations are possible, despite the inherent risk of triviality. On the contrary, he directs his argument towards an understanding of what Winch asserts to be the most significant aspect of social life, its modes of rationality; and the difficulty of establishing a culturally neutral standpoint from which to evaluate these modes is here at its height.

Moreover, it is not clear why this difficulty should vary in intensity from one disciplinary context to another; why conceptual problems of this sort should be taken as solved in one context and not in another. To take Ryan's own example of econometrics, it may be that the prevailing content deals with mathematical structures and models, but it does so on the basis of assumptions about precisely the matters Winch raises. As Ryan rightly remarks, these matters are "taken as solved"; that does not mean, however, that they are solved. Indeed, we may feel that is is characteristic of social sciences like these that such problems are not even recognized, much less addressed. Ryan's response to Winch on these two issues is, therefore, far from convincing.

Ryan then goes on to deal with the difficulties that arise when we might want to disbelieve the account a participant gives of his action, and to substitute one which we feel is closer to the "reality" of the situation. He quotes as examples of such a situation accounts which are "based on the idea of ideological self-deception, and those which employ the psychoanalytic concept of repression" (Ryan 1970, p. 162). Ryan affirms that such alternative accounts of behaviour are "sufficiently logically coherent" to compete with the accounts participants themselves might give of their behaviour; and argues that, in the examples given, there are no grounds for preferring one to the other, since these contexts

> . . . depend to a large extent precisely on teaching people a new conceptual scheme within which events wear a different significance from what they did before. (Ryan 1970, p. 164)

Accordingly, it is asserted, causal explanations that lie outside the framework or meaning of the participant can legitimately be constructed, and those explanatory structures will be falsifiable:

> . . . it is true that if people endow their actions with a certain significance, and thus follow certain social rules, they will necessarily display regularities in their activities, so that the absence of these regularities would mean that our account of the significance of their actions was wrong. (Ryan 1970, p. 165)

Moreover, Ryan claims, we can develop a causal analysis for an entire context of a particular way of life, and for the entire range of internal logic that characterizes it: thus

> . . . of course we must inquire into the aesthetics of aesthetic change, but that does not mean we must not inquire into the economics and technology of it as well. (Ryan 1970, p. 166)

A similar point is made by Nagel, as outlined in the discussion above.

Now, we do not deny that such causal explanations can be developed: it is clearly possible to appeal to knowledge that has been developed about conditions that are external to, yet essentially affect, the form of life being investigated. Thus it is likely to be valuable, in approaching patterns of trade and exchange, to gain an understanding of environmental conditions, through the methods of the natural sciences, and of the social groupings through which food is obtained, practices which preserve certain vulnerable environmental elements by specific seasonal taboos, or patterns of movement and territory delineation that adjust environmental pressure. Obviously explanations which, we may feel, illuminate these phenomena, can be developed. What we must not claim, if we accept Winch's thesis, is that these explanations

are more valid than those that are given by the participants in the particular form of life themselves. Again, therefore, while accepting the development of causal explanations of this type, it does not seem possible to accept Ryan's notion of such explanations "undercutting" (Ryan 1970, p. 164) those provided by the participants, which carries with it a clear implication of priority. Ryan has not established his position, and hence Winch's thesis essentially remains intact. We still do not seem to be in a position to claim that any understanding developed outside the structure of meaning intrinsic to any form of social life, or not based on the fundamental concepts of that structure, substantially elucidates the nature of that form of life.

The more conservative point made by Ryan--that our understanding of the structure of meaning we take as underlying social life, and of the regularities to which it is assumed to give rise, is falsifiable, and therefore fulfills the criterion necessary to a deductive explanation--is, however, worthy of more attention. It recapitulates a notion developed earlier in the work, that in the social sciences the idea of a "cause" is equivalent to "having good reasons," and that the event in question is rendered intelligible by reference to that structure of reason. This is certainly in accord with the principles laid down by Winch, and it leads Ryan to argue that, seen in this light, the type of explanation developed in the social studies exhibits all the qualities we require of explanation in the natural sciences:

> The importance of deductive relationships is as great as ever; only if we know that a rule covers all of a given class of cases, and that all of a given group of persons follow that rule can we move towards deductively certified prediction of their actions; equally, the importance of consistency and inconsistency are undiminished, when we consider that neither we nor the people we study can persist in following what are seen to be inconsistent rules. And our commitment to theoretical realism is now better founded than ever, for in the case of human beings alone can we have our assertions about the rules governing the phenomena

> confirmed by the testimony of the phenomena themselves. The logical properties and ontological presuppositions made by explanations thus seem to be unchanged, even if the social sciences aim at the elucidation of rules rather than the establishment of regularities only, and at the unraveling of conceptual rather than contingent relationships. (Ryan 1970, p. 147)

Thus, Ryan concludes, Winch's analysis illuminates the content of social understanding, without requiring that we abandon the form of explanation developed in the natural sciences.

However, as we have noted, Winch addresses this point directly and in a way that Ryan here fails to come to terms with. The "deductively certified prediction" of actions that Ryan looks to requires that we observe such actions, and attempt to decide, through the use of standard statistical decision theory, whether or not our characterization of the rule (which here is taken to play the role that theory does in the natural sciences) is correct or not. And yet this is just what Winch asserts we cannot do:

> Against this, I want to insist that if a proffered interpretation is wrong, statistics, though they may suggest that that is so, are not the decisive and ultimate court of appeal for the validity of sociological interpretations in the way Weber suggests. What is then needed is a better interpretation, not something different in kind. The compatibility of an interpretation with the statistics does not prove its validity. (Winch 1958, p. 113)

It is not possible, Winch goes on to point out, to close the gap between these two notions: we may be able to make highly accurate predictions about social behaviour without any understanding of what was involved in such behaviour; just as, he asserts, one could make statistical predictions about the occurrences of words in a language without actually understanding what was being said in the language.

And yet we may feel that, in this rejoinder, which attempts to distinguish the nature of understanding natural and social phenomena, Winch is not entirely convincing; since, as we have seen, even in the deductive explanations developed in the natural sciences there are no guarantees that a particular theory, even if confirmed, in fact reflects the real workings of nature. The scientist, working in a particular research programme, simply makes the methodological decision to go on regarding it as such, until convincing evidence to the contrary accumulates. We thus arrive at the rather complicated conclusion that although Ryan's attempt to preserve a deductive structure of explanation that incorporates essential features of Winch's thesis does not address itself to fundamental objections made by Winch, Winch's rebuttal itself seems to depend on a notion of science that our analysis of chapter II has shown to be not entirely valid. On points other than this, however, it would appear that Ryan is not able to challenge effectively Winch's position. We note, nevertheless, that further investigation of Winch's conception of science is necessary before drawing a final conclusion; such an investigation is undertaken in section 3.1.4 below.

Collingwood on "The Idea of History" (1948)

Historical explanation and human geographic explanation have never been far removed from each other. Indeed, as is well known, Sauer (1941b) argued persuasively that all human geographic explanation was, by its very nature, historical. In more recent times, Guelke (1974, 1981, 1982), as we have seen, has attempted to restore an interest in the methodology of history to the discipline; although, unlike Sauer, he sees historical geography as methodologically separate from the rest of human geography. Moreover, as has been noted, many philosophers of social science have based their analyses on

ideas that have emerged in discussions about the nature of historical studies. It is therefore particularly relevant to explore central aspects of the philosophy of history in this context, particularly as Winch specifically links his thesis to issues raised there.

The most penetrating and illuminating of contemporary approaches to the philosophical and methodological problems of history is that undertaken by Collingwood. His view of historical enquiry was developed over several decades in a number of essays (Collingwood 1965), and is most fully elaborated in his celebrated work, The Idea of History (Collingwood 1948). It is to this latter work that most reference is made by philosophers of social science, and, as it represents the mature and systematic exposition of Collingwood's views, it is this work that will now briefly be examined.

The first two-thirds of the book is devoted to a survey of views of historical method expounded since the inception of the discipline as a formal body of study; and with this material we need not, in this context, concern ourselves. The final third of the book, about one hundred and thirty pages, is, however, given over to an "Epilegomena," in which the conclusions that derive from this study are systematically laid out. It stands, in fact, as a monograph in its own right, of about the same length and stature as Winch's, and presents the culmination of a lifetime's work devoted to bringing about a rapprochement between philosophy and history. Indeed, its influence has spread far wider than the audience of philosophically minded historians for whom it seems to have been intended, and has become a central document for all those who have attempted to clarify the methodological and conceptual foundations of the social sciences as a whole.

Collingwood begins by boldly affirming the necessity for a science of human nature to knowledge of any kind: without such self-knowledge, he asserts, the nature of the knowledge that we assume we have of other things is obscure. He thus affirms a principle that is of great importance for the social sciences, and for the direction of this dissertation:

> Self-knowledge is desirable and important to man, not only for its own sake, but as a condition without which no other knowledge can be critically justified and securely based. (Collingwood 1948, p. 205)

It seems difficult to dissent from such a fundamental principle. He then goes on to assert a second principle of an almost equal importance. The attempt to understand the human mind through methods developed by the natural sciences is misconceived, he argues, and can only be achieved by history:

> . . . the science of human nature was a false attempt--falsified by the analogy of natural science--to understand the mind itself, and (that), whereas the right way of investigating nature is by the methods called scientific, the right way of investigating mind is by the methods of history. (Collingwood 1948, p. 209)

Here the validity of such a proposition is not so obvious and requires an understanding of what Collingwood means by "history." This term is identified by him not so much with time and sequence, as the lay view might have it, as with "human affairs" (Collingwood 1948, p. 212). The events which comprise these affairs have in consequence a particular structure that distinguishes them from events in the natural sciences: they have an "outside and an inside"; external expressions that can be observed and described, and internal "springs" (to use Nagel's term) that can only be described in terms of thought (Collingwood 1948, pp. 212-213). Thus Collingwood, like Winch, wishes to insist on the internality of events in the social sciences, an idea which he takes as clearly distinguishing the activity of the scientist from that of the historian:

> For science, the event is discovered by perceiving it, and the further search for its cause is conducted by assigning it to its class and determining the relation between that class and others. For history, the object to be discovered is not the mere event, but the thought expressed in it. To discover that thought is already to understand it. After the historian has ascertained the facts, there is no further process of inquiring into their causes. When he knows what happened, he already knows why it happened. (Collingwood 1948, p. 214)

The historian is concerned, therefore, with only those events which are observable expressions of thoughts, and he is concerned with thoughts far more than he is concerned with events. The notion of mind itself is, in this view, historical: there are no generalizations that can be made about it that can predict future patterns of events, no laws in the accepted scientific sense. Once events have been rendered intelligible, there is no further step that a proposed "science" can take:

> The historian, when he is ready to ~~had~~ hand over such a fact to the mental scientist as a datum for generalization, has already understood it in this way from within. If he has not done so, the fact is being used as a datum for generalization before it has been properly "ascertained." But if he has done so, nothing of value is left for generalization to do. (Collingwood 1948, p. 223)

Intelligibility implies rationality: it is to the rationality of thought that Collingwood looks for historical explanation, although recognizing that human affairs are rational only by "fits and starts" and that irrational elements in the mind must be admitted to exist. Thought, however, is only of historical significance to the degree that it is rational; or, to put it another way, it is only to the degree that thought can be identified in the historical process that it is rational thought at all.

This view of history has important methodological implications. Collingwood begins his analysis of them by noting that the epistemological basis upon which historical knowledge is developed is significantly different from that upon which scientific knowledge rests:

> Historical thought is of something which can never be a this, because it is never a here and now. Its objects are events which have finished happening, and conditions no longer in existence. Only when they are no longer perceptible do they become objects for historical thought. Hence all theories of knowledge that conceive it as a transaction or relations between a subject and an object both actually existing, and confronting or compresent to one another, theories that take acquaintance as the essence of knowledge, make history impossible. (Collingwood 1948, p. 233)

He goes on to affirm what has come to be called the uniqueness thesis: that all historical events are particular and individual in their essential nature, partaking of unique combinations of circumstances and giving rise to unique kinds of events, which cannot therefore be legitimately described by the universal statements (whether methodologically based or not) that scientific method employs. Historical knowledge thus begins and ends with the historian. It is the historian who establishes the "facts" he wishes to address himself to:

> He selects from (his authorities) what he thinks important, and omits the rest; he interpolates in them things which they do not explicitly say; and he criticizes them by rejecting or amending what he regards as due to misinformation or mendacity. (Collingwood 1948, p. 235)

The question then arises of how the historian is to establish a ground of undisputed truth from which to carry out these kinds of activities. Collingwood rejects Bradley's notion that

> . . . our experience of the world teaches us that some kinds of things happen and others do not; this experience, then, is the criterion which the historian brings to bear on the statements of his authorities. (Collingwood 1948, p. 239)

Clearly such experience can tell us only what might happen, not what did happen: it is bound to the historical conditions under which it is formed, and, following the idea of uniqueness, cannot be related without considerable

risk to the conditions of some past time. Collingwood wishes to replace this criterion with what he calls "a priori imagination" (Collingwood 1948, p. 240). This kind of thinking is not only valuable to the historian, but essential. Without it, he asserts, there can be no historical understanding at all. The imagination of the historian is distinguished from that of the historical novelist, in that it must conform to the evidence provided by the documents or authorities on which the historian is basing his account. These fixed points are not given as true, but are established by "critical thinking":

> The historian's picture of his subject . . . thus appears as a web of imaginative construction stretched between certain fixed points provided by the statements of his authorities; and if these points are frequent enough and the threads spun from each to the next are constructed with due care, always by a priori imagination and never by merely arbitrary fancy, the whole picture is constantly verified by appeal to these data, and runs little risk of losing touch with the reality which it represents. (Collingwood 1948, p. 242)

Three rules of historical method are, then, implicit to this view. The account of historical phenomena developed must refer to a particular context of space and time; it must be internally consistent; and it must relate adequately to the evidence available, viewed through the perspective of historical imagination. With respect to the last of these, Collingwood amplifies his epistemological position as follows:

> The whole perceptible world, then, is potentially and in principle evidence to the historian. It becomes actual evidence in so far as he can use it. And he cannot use it unless he comes to it with the right kind of historical knowledge. . . . Evidence is evidence only when some one contemplates it historically. Otherwise it is merely perceived fact, historically dumb. It follows that historical knowledge can only grow out of historical knowledge . . . (Collingwood 1948, p. 247)

Here it would seem, however, that Collingwood is taking a position that, in its own terms, can hardly be justified. We can accept that historical knowledge can grow and develop in the way that Collingwood describes once it is in existence: by moving between his account of the historical phenomena in question and the evidence, it is not unreasonable to suppose that the account may be enriched, or even fundamentally altered. But Collingwood does not tell us how historical knowledge can come into existence in the first place: indeed, he rejects the idea of experience that might have fulfilled such a role. Even then, however, some kind of accepted authority, be it documentary or verbal, would be necessary in order that the process of enrichment can begin. All Collingwood can offer to fill this crucial gap is

> . . . the idea of history itself: the idea of an imaginary picture of the past. That idea is, in Cartesian language, innate; in Kantian language, a priori. . . . It is the idea of the historical imagination as a self-dependent, self-determining, and self-justifying form of thought. (Collingwood 1948, p. 249)

Even were we to grant the reality of this notion--and cross-cultural comparisons would seem to render it questionable, to say the least--Collingwood does not give us any idea of how the concrete accounts which the historian seeks to develop are constructed from this abstract foundation. On the face of it, it is difficult to see how such development could occur at all. It would seem to be far more sensible to say that the historian has to accept, in a methodological sense, some parts of his body of evidence as "fact," upon which he can begin the activity described by Collingwood; and which, in the light of other "facts" established through the exercise of historical imagination, he may even come to reject, or to regard as less reliable than he had thought. Such an account of historical method preserves the central notion of historical imagination while making it a practical possibility: and, although it is still

clearly different from the deductive model of scientific method that Collingwood takes as characterizing science, it is not, as we have seen, so far from the idea of science as a research programme, as outlined by Lakatos. Here, too, theory is constructed on the basis of some background information made fundamental by methodological decision, and is developed both internally, in relation to theory, and externally, in relation to evidence. We may therefore begin to feel that Collingwood, like Winch, wishes to distinguish explanation in social inquiry from explanation in a particular view of science that has itself been subject to substantial criticism and reconstruction.

In attempting to support further the particular nature of historical, as opposed to scientific, understanding, Collingwood points to a difference in the organization of the two disciplines. As a consequence of the uniqueness thesis, the historian develops his understanding of a particular phenomena not by reference to other such phenomena that occurred at different times of history, but by reference to other aspects of that period of history in which the phenomena is embedded. He accepts that historical understanding is evidential and intersubjective in much the same way as is scientific understanding (although in making this concession, he seems to be forgetting about the strictures he has already placed on the idea of evidence), but goes on to assert that inductive inference is the characteristic mode of reasoning in historical explanation:

> . . . no historical argument ever proves its conclusion with that compulsive force which is characteristic of exact science. Historical inference, the saying seems to mean, is never compulsive, it is at best permissive; or, as people sometimes rather ambiguously say, it never leads to certainty, only to probability. (Collingwood 1948, p. 262)

Here again, however, it would seem that Collingwood is taking a simplistic view of science: as we have seen in chapter II, no hypotheses, theories, or laws, indeed, no statements of any kind, can ever be proved beyond doubt, in science or any other kind of discipline--as Collingwood, with his acquaintance with Hume ought to have known; and we have seen that all modern views of science, even those which, like Popper's, would like to place science entirely on a foundation of deductive logic, are forced to allow the inductive notions associated with confirmation to occupy a central place in the structure of scientific explanation.

Finally, it is important to emphasize that, as we have noted, Collingwood, like Winch and Kuhn, and others who have challenged the method of science in various contexts, bases his argument on a particular epistemological position. However, Collingwood is far more explicit than Winch in arguing for a particular methodological approach to social phenomena, rather than simply challenging the use of scientific method in social contexts. Since the problem being approached by Collingwood is paralleled to that approached by Winch, a brief mention of Collingwood's epistemology seems appropriate.

He reaffirms, firstly, that the use of the historical imagination is an activity undertaken by the historian; that to rediscover the thought which lies at the basis of an historical event or phenomenon, "the historian must think it again for himself" (Collingwood 1948, p. 283). But, he then asks, is this experience one which only resembles the original thought, or is it identical? If the latter, how is it historical knowledge, since its reality is entirely in the present? In attempting to answer these questions, Collingwood constructs a theory of mind that is based on the self-reflexive nature of human consciousness:

> If, then, mere consciousness is a succession of states, thought is an activity by which that succession is somehow arrested so as to be apprehended in its general structure: something for which the past is not dead and gone, but can be envisaged together with the present and compared with it. Thought itself is not involved in the flow of immediate consciousness; in some sense it stands outside that flow. (Collingwood 1948, p. 287)

Thus it is not re-enactment itself that makes historical knowledge: the historian must also know that he is re-enacting it, and submit that thought to critical inspection. The act of historical imagination, which is the sole basis of historical understanding, becomes an act of self-knowledge: it is

> the critical study of one's own thought, not the mere awareness of that thought as one's own. (Collingwood 1948, p. 292)

Moreover, it is accepted that any thought arises at a present time, in a particular context which both frames it and is inextricable from it. Thus the critical judgement which the historian exercises derives in large part from his ability to recreate that context, and to view it from his own perspective:

> Thus, the mere fact that someone has expressed his thoughts in writing, and that we possess his works, does not enable us to understand his thoughts. In order that we may be able to do so, we must come to the reading of them prepared with an experience sufficiently like his own to make those thoughts organic to it. (Collingwood 1948, p. 300)

This is an interesting position because it differs from both Winch's and Kuhn's, for whom the context of meaning in which explanation and understanding arises is not something that can be subjected by the individual to any kind of objective judgement. For Collingwood, on the other hand, the idea of "critical thought" is not only possible, but part of the structure of human thinking. Here he foreshadows the "bracketing out" process developed by phenomenologists (see chapter IV below); and, ironically, seems to lend support to Popper's

rejoinder to Kuhn that, while we may function within a framework that predisposes us to view the world in a certain way, it is a framework that we can break out of at any time into a "better and roomier one" (Popper 1970, p. 56).

It is still, however, far from clear that the element of critical judgement can be developed independently of the phenomena to which it is to be applied. If experience is necessary to a proper understanding of historical phenomena, and if that experience is to be gained not from the present context in which the historian is located but from the wider context of the historical period in question, it is not clear how the historical understanding necessary to that kind of experience can be obtained, nor what critical judgement is to be directed towards it. The argument becomes an infinite regress, and we find ourselves back to the idea that historical knowledge can only come from historical knowledge; and that idea we have already subjected to criticism.

Responses to Collingwood

It is difficult to underestimate the influence that Collingwood, "the only British thinker in the present century who has made a serious contribution to the philosophy of history" (Carr 1961, p. 21), has had on the theory and philosophy of the social sciences. From his view has sprung a whole literature dealing with various aspects of the questions he raises. We will look at three responses--those of Dray (1964), Carr (1961), and Popper (1957)--that bear directly on the issues discussed above, and which may illuminate the more general issues of the philosophy of social science to which they relate.

Dray (1964) is in broad agreement with Collingwood on the general thrust of his exposition of historical understanding. He analyzes three objections that have been raised to Collingwood's theory. The first is that the kind of

rational thought which Collingwood sees as underpinning action does not allow for irrational thought, or for thought not consciously entertained; and inasmuch as it refers only to individuals, it does not provide any insight on the larger units--nations, institutions, movements or conditions" (Dray 1964, p. 12)--that historians are often concerned with. To this Dray replies that Collingwood's notion does account for at least a substantial part of the basis of individual action; and that, if one accepts the idea of methodological individualism, such an account of individual action can legitimately be taken as the basis of community or social action.

The second objection follows Hempel (1959) in asserting that the kind of re-enactment or re-thinking advocated by Collingwood can only give rise to hypotheses taking the form of psychological laws which

> . . . will assume the form of a statement of necessary connection between an agent's having a certain thought and acting in a certain way. (Dray 1964, p. 13)

However, Dray rightly notes that to take this position is to misunderstand the notion of historical understanding that Collingwood wishes to develop, which is directed not towards the relationship between acts and thought, nor towards the assertion of compelling, in the sense of deterministic, reasons for carrying out a certain action, but to the rational basis of action, to its intelligibility: thus "to resolve puzzlement it is enough to show that (the action) follows 'rationally'" (Dray 1964, p. 14).

Hempel (1959) points out that since the lack of action of an agent is not, in Collingwood's view, inconsistent with the rational basis established, something more is needed to explain why in fact the action took place; and that must be a statement to the effect that the agent in question was a rational agent, and rational agents, in such circumstances, always, or at least probably, act in a certain way. Again, however, Dray notes that Collingwood

only requires the possibility of rational action for historical understanding, and that this imposes limits on the kind of explanation that can be obtained:

> As long as we have reason to think that he <u>can</u> act so--that he is a rational agent in the sense of being able to choose what to do, and able to appreciate the force of the various considerations presented to him--then if he acts for good reason we can claim to understand his action. . . . The demand for further "completion" of the explanation appears to be a demand for what we probably cannot get, and in any case do not need. (Dray 1964, p. 15)

Dray concludes his account of historical understanding by noting that some writers have advocated limited forms of deductive explanation which may employ restricted kinds of generalizations that hold only within a particular period of time or a particular geographic area; but that these forms appear to encounter unresolvable difficulties in asserting the law-like status of such generalizations, and in constructing test structures that do not employ universal laws. Moreover, he asserts that the deductive model of explanation may be altogether inadequate in providing the kind of understanding that historians often seek, such as in explaining how something has happened, in spite of presumptions that it could not have happened; and in developing synthetic, rather than analytic, insights, often on the basis of analogy, that related the parts of history to a whole of some kind--as, for example, with the idea of the Renaissance, or the Reformation. To these tasks of the historian, which employ a method that "is no artistic ornament to historical inquiry: it is of its essence" (Dray 1964, p. 20), the deductive model of the natural sciences, he argues, can bring nothing.

Both Dray and Collingwood are philosophers writing about history, and practising historians have not in the main concurred with their view. Carr (1961), for instance, is inclined to challenge Collingwood on a number of grounds. He accepts Collingwood's insistence on the central role of the

historian himself in selecting the evidence on which he wishes to base his account, and of the experience of the historian in constructing the accounts that lead to historical understanding: this he sees as a salutary reversal of the emphasis on objective "facts" which he, like Collingwood, regards as unacceptable on theoretical grounds, and impossible to obtain in practice. But he sees danger in Collingwood's attempt to take this notion to its logical conclusion, which would seem to rule out the possibility of any objective component in history, and to be likely to give rise to a history that is whatever the historian decides to make it. Carr takes issue with what he sees as such extremism:

> It does not follow that, because interpretation plays a necessary part in establishing the facts of history, and because no existing interpretation is wholly objective, one interpretation is as good as another, and the facts of history are in principle not amenable to objective interpretation. (Carr 1961, p. 27)

Moreover, there is always the danger that the criterion of objectivity can be determined by reference to some present purpose, rather than to its own internal standards. In practice, Carr asserts, what happens is that the historian proceeds from a ground of "capital sources" to interpretation and back to the sources again, in a continuous process of interaction that develops and changes the whole structure of explanation in its course. The possibility of establishing such a starting point is exactly what Collingwood, as we have seen, denies; and yet, as we have noted, the procedure advocated by Carr is both logical and practical, and has much in common with Lakatos's view of scientific research programmes:

> The historian starts with a provisional selection of facts, and a provisional interpretation in the light of which that selection has been made--by others as well as by himself. As he works, both the interpretation and the selection and ordering of facts undergo subtle and perhaps partly unconscious changes, through the reciprocal action of one or the other. . . . The

> historian and the facts of history are necessary to one another. The historian without his facts is rootless and futile; the facts without their historian are dead and meaningless. (Carr 1961, p. 30)

Carr goes on to oppose the idea that historical understanding must be addressed to individuals, and that what is to be understood is the conscious motives of such individuals. He emphasizes that it is not the emphasis on individuals as such that is misconceived, but the attempt to draw a distinction between the individual and the society of which he is a part: history, he asserts, is "the process of inquiry into the past of man in society" (Carr 1961, p. 48). In this he does, perhaps, some injustice to Collingwood, who speaks of the necessity of understanding the "context" in which individual action arises, but it must be admitted, nevertheless, that his characterization of Collingwood's position is essentially correct. Moreover, he goes on, the actions of individuals only assume importance in relationship to some movement which is evident in the society as a whole: thus "numbers count in history" (Carr 1961, p. 50). Indeed, he argues, it is wrong to argue that the results of actions by the actors in history bears any constant relationship to the intentions with which they were undertaken:

> It defies all the evidence, to suggest that history can be written on the basis of "explanations in terms of human intentions" or of accounts of their motives given by the actors themselves, of why "in their own estimation, they acted as they did." The facts of history are indeed facts about individuals, but not about the actions of individuals performed in isolation, and not about the motives, real or imaginary, from which individuals suppose themselves to have acted. They are facts about the relations of individuals to one another in society and about the social forces which produce form the actions of individuals results often at variance with, and sometimes opposite to, the results which they themselves intended. (Carr 1961, p. 54)

Clearly, therefore, Carr is much more sympathetic to the idea of a broadly scientific approach to history than is Collingwood or Dray. He endorses the description of science as a reciprocal interaction of fact and theory (Cohen and Nagel, p. 596) as an accurate account, too, of historical method, and notes that, on this view of history and science, the hypotheses employed in both fields of enquiry have a similar status:

> Nowadays both scientists and historians entertain the more modest hope of advancing progressively from one fragmentary hypothesis to another, isolating their facts through the medium of their interpretations, and testing their interpretations by the facts; and the ways in which they go about it do not seem to me essentially different. (Carr 1961, p. 61)

He is thus led to reject the uniqueness thesis: events may, in one sense, be regarded as unique, but the historian is interested in what is general in them, and in the relationship of that generalization to the unique events, not in their uniqueness as such. As with the attempt to isolate the individual as the unit of study at the expense of his social context, Carr takes the view that the difficulty arises in the attempt to separate on the one hand individual and society, and on the other, unique and general:

> History is concerned with the relation between the unique and the general. As a historian, you can no more separate them, or give precedence to one over the other, than you can separate fact and interpretation. (Carr 1961, p. 65)

Thus there ought to be, in Carr's view, no reticence about the ability of the historian to deal in causes: indeed, he asserts, "the study of history is the study of causes" (Carr 1961, p. 87). Although we may agree that human action is undetermined to the extent that freedom of action is an attribute of human existence, we need not, he argues, agree with Popper's dictum that "everything is possible in human affairs" (Popper 1945 ii, p. 197): depending on one's perspective, all human actions are both free and determined. Nor does the idea of causality necessarily require the notion of inevitability:

> In practice, historians do not assume that events are inevitable before they have taken place. They frequently discuss alternative courses available to the actors in the story, on the assumption that the option was open, though they go on quite correctly to explain why one course was eventually chosen rather than the other. Nothing in history is inevitable, except in the formal sense that, for it to have happened otherwise, the antecedent causes would have had to be different. (Carr 1961, p. 96)

The historian is therefore able to identify a cause, and commonly several causes, to an event, and go on to assign them to an order or priority: like the scientist, Carr asserts, the historian must try

> . . . to simplify the multiplicity of his answers, to subordinate one answer to another, and to introduce some order and unity into the chaos of happenings and the chaos of specific events. (Carr 1961, p. 91)

It is interesting to note that this view of history arises chiefly from the practice of being an historian, rather than from some philosophical position, just as Lakatos's view of science directs its attention to the practice of scientists, rather to some logical or philosophical principles that may underlie such practice. What both seem to have in common is the idea of steady growth of theory, going from time to time to the evidence and then back to the theory; and the accretion of knowledge that results begins more and more to provide pragmatic answers to the philosophical problems that a more strictly logical view presents. The relationship between these two approaches to explanation in the sciences and the social studies will be further explored in the conclusion to this section.

Finally, we may briefly note some responses made by Popper (1957), in the context of his rebuttal of historicism, to the kind of position Collingwood exemplifies. Like Carr, he opposes the uniqueness thesis, but he does so on rather different grounds. He accepts that there are regularities in social life that are characteristic of certain periods of history, but argues that

this fact makes no special difficulties for social inquiry beyond those that are experienced in the natural sciences. In particular, he points out that scientific laws, in the natural sciences, cannot be regarded as universally true, because of the problem of induction identified by Hume and that their universality for the purpose of constructing deductive explanation is methodological only (see section 2.2). Moreover, he asserts, there seems to be no reason why social scientists, like natural scientists, cannot construct valid generalizations that apply to different historical periods: the simple fact of differences between these periods does not preclude the possibility of generalizations which hold in both, particularly since many of these differences are "of a comparatively superficial character (such as are differences in habits, in greeting, ritual, etc.)" (Popper 1957, p. 101). Interestingly, on this point, Nagel, as we have seen, takes a precisely opposite view: for him, it is generalization that is predisposed to superficiality, not the identification of singularities. Thus, in the study of society no less than in the study of nature, it is not only possible, but necessary, to advance in the direction of universally valid explanations through statements that are taken to be universally true until evidence to the contrary arises. We do not qualify our generalizations about societies with the remark that they apply only to specific period:

> For it is an important postulate of scientific method that we should search for laws with an unlimited realm of validity. If we were to admit laws that are themselves subject to change, change could never be explained by laws. It would be the admission that change is simply miraculous. And that would be the end of scientific progress; for if unexpected observations were made, there would be no need to revise out theories: the ad hoc hypothesis that the laws have changed would "explain" everything. (Popper 1957, p. 103)

However, it would seem that here Popper is addressing himself to a version of the uniqueness thesis that takes the idea of uniqueness to extremes. The point of the uniqueness thesis is not that no generalization valid for several historical epochs can be devised--of course they can, at some level--but that such generalizations cannot incorporate the context of rationality or intelligibility which is fundamental to the phenomena about which generalizations are being constructed. Indeed, Collingwood would no doubt respond to Popper by asserting that, given the apparent fact that the notion of rationality can only be understood in the context of its historical existence, and that rationality is the hallmark of historicity, to the extent that ~~generalization~~ generalizations are applicable to more than one historical period they are also non-historical. Thus Popper fails to address himself to the deeper implications of the uniqueness thesis, and in consequence fails to meet Collingwood's objections to the use of scientific method in history. His rebuttal stands in contrast to Carr's, which, as we have seen, is based on a rejection of Collingwood's view of history.

When Popper comes to deal directly with the nature of history, the difference between his position and Carr's becomes more marked. Having argued that valid generalizations that span historical periods can be developed, he goes on to assert that

> . . . history is characterized by its interest in actual, singular, or specific events, rather than in laws or generalization. (Popper, 1957, p. 143)

Under this view, the fundamental structure of historical explanation remains the deductive-nomological model (see section 2.2.7), but instead of directing attention to the law-like statements which frame that structure, as the natural sciences do, history, he argues, is more interested in the initial

conditions which, in coordination with the law-like statements, explain a particular event. This gives rise to a particular interpretation of historical causality:

> . . . a singular event is the cause of another singular event--which is its effect--only relative to some universal laws. (Popper 1957, p. 145)

Thus all explanations, even those in the natural sciences, have an element of historicity in them, since initial or singular events are always involved. The difference between the two is therefore one of emphasis only. The historian employs laws to construct explanations, but these laws are

> . . . so trivial, so much part of our common knowledge, that we need not mention them and rarely notice them. (Popper 1957, p. 145)

Both causal and non-causal perspectives are therefore needed in constructing an historical explanation, as an event may be viewed at one time as typical, or causally developed, and at another time as unique.

It is difficult to avoid feeling that there is some sophistry here. Popper seems, firstly, to be unable to decide what the historian is interested in, and ends by including both singular and general approaches, without attempting to reconcile the two. Secondly, to the extent that he insists on the deductive model as being most appropriate to historical understanding, his argument stands on doubtful ground; for he is obliged to alter the notion of deductive explanation so substantially--utilizing trivial laws, and placing emphasis on initial conditions--that it becomes hardly recognizable as the structure employed by the natural sciences, in the orthodox view. Thirdly, he fails to elucidate the nature of the causal connection that binds one singular event to another event: in Collingwood's view, this is clearly of fundamental importance, since such an elucidation can only be made through an appeal to the notion of rationality, or intelligibility. Popper assumes that, because a

structure of explanation can be erected that is common to both the natural and social sciences--although as we have noted, that commonality stands on shaky ground--the nature of the causality implicit in such a structure of explanation is also common; and that is just the point with which Collingwood takes issue.

3.1.4 Conclusions

Having, in the preceding chapter, examined and attempted to resolve the main contemporary views of the nature of science; and having, in the present chapter, briefly evaluated the main critiques of positivism in social inquiry, and responses to those critiques, we may now be in a position to attempt a conclusion to these issues that will allow us to proceed to an examination of the conceptual foundations of human geography. It is evident that the analysis of both chapters will necessarily be involved in developing such a conclusion, since the challenge to the use of scientific method in social inquiry has been directed towards the orthodox hypothetico-deductive model of scientific explanation, and has not taken into account developments in recent years which, as we have seen, have substantially altered the contemporary notion of science. Two broad thrusts are therefore required in developing a conclusion that incorporates all the matters that have been raised in these chapters. The first attempts to resolve the debate over the nature of explanation and understanding in social inquiry in its own terms: the second then submits that resolution to the conclusions established in chapter II. With such an approach we may fairly feel that we have covered the field, at least to the extent of not avoiding any of the main positions; and that, if a conclusion can be developed, it is one in which we may reasonably feel greater confidence in applying to the conceptual foundations of a scientific human geography than in those that have been offered by the geographic theorists examined so far.

1. It is clear, to begin with, that the logical positivist account of explanation and understanding in the social sciences, outlined in section 3.1.2, will not stand without substantial modification. Even without the critique developed by Winch and Collingwood, deficiencies internal to this account place its validity in question, and point the way to alternatives. It is important to emphasize, however, the distinction between the idea of a unity of method in both the natural sciences and social sciences, and the idea of applying the hypothetico-deductive model developed in the natural sciences to social phenomena. These are not the same although, significantly, they have been treated as such by both positivist and anti-positivist commentators alike. It is clearly possible to maintain the idea of a unity of method on the basis of a mode of explanation other than that presented in the hypothetico-deductive model: that will, in fact, be the strategy attempted in this conclusion. Here it is sufficient, before examining the inadequacies identified in the use of deductive model in the social studies by proponents of that approach, to note that rejecting the applicability of the deductive model to social inquiry does not in principle preclude the idea of a possible unity of method.

With this caveat, we may proceed to a summary of problems associated with the deductive model in the social studies, as outlined in section 3.1.2. It is significant that Popper, Nagel and Rudner, while asserting that there seems to be no logical bar to the use of the deductive model in the social studies, all nevertheless concede the lack of success of this approach, when compared to the natural sciences. Various factors are examined in order to account for this: greater complexity is discounted, with Popper even asserting that the element of rationality in human affairs ought to make the formation of good hypotheses about them a simpler matter; but the variability of social phenomena, particularly the speed of change, rather than the mere fact or degree of

change, is admitted to be a problem, and to require the application of sophisticated statistical techniques. Nagel notes the difficulties that arise in attempting the classification of complex social phenomena. All these problems, however, are agreed to have a common basis in the lack of comprehensive theory. The success of the models and idealizations that the social studies employ is clearly seen to depend on the comprehensiveness of the theory on which such models are based: they do not give rise to reliable predictions just because they do not adequately model the springs of human action. The response of these commentators is that, while the lack of success of this approach is disappointing, it is not a permanent state of affairs, and improvement simply awaits the development of better theory. How that improvement is to be effected is not discussed, since the development of hypotheses is regarded as a "private matter" (Popper 1957, p. 135), and therefore not subject to systematic elaboration.

The inadequacy of social theory begins to assume a more precise form if we look to the kind of theory envisioned. Popper (1957, p. 136), as we have seen, specifies models that are analyzable "in terms of individuals, of their attitudes, expectations, relations, etc." Nagel (1961) bases his pessimism about the possibility of obtaining universal social laws on the observation that

> . . . it would be necessary to classify social phenomena part by reference to minutely differentiated physical and physiological traits of human participants in those phenomena, and in part on the basis of detailed data concerning the culturally acquired habits and beliefs each participant possesses. (Nagel 1961, p. 507)

Rudner (1966, p. 62) looks to establish a "comprehensive social theory" which would contain all the variables necessary to derive deductively an account of social action. It is clear, therefore, that all three commentators recognize

that the effectiveness of the deductive model in the social studies rests on the ability of social theory to encompass comprehensively the nature of human functioning, in both its observable and its unobservable dimensions. Since "attitudes, expectations" and "beliefs" must, it is held, be incorporated in the construction of such theory, this conclusion seems to point towards the kind of approach to human affairs developed by Winch and Collingwood. Of the three commentators, only Nagel, as we have seen, accepts this implication and attempts to deal with it--in his case by a modified form of behaviourism which is by no means convincing. On these considerations, an approach to the internality of social relations begins to appear an unavoidable necessity.

This conclusion was strengthened by our consideration of Nagel's argument that triviality is a necessary consequence of generalization in the social studies. Popper, it should be noted, is contradictory on this question: on the one hand, he asserts that only the differences between social contexts are trivial, not the generalizations that they may have in common; on the other, in his discussion of historical studies, he sees the laws that frame explanation as trivial, and the singular initial conditions as significant. This contradiction can only be resolved by separating social from historical studies, and this Popper fails clearly to do. Discounting Popper, then, we may agree that the social studies are likely to be particularly vulnerable to trivial generalizations (about, for instance, the universal facts of human biology and physiology), without accepting the logical necessity for such trivialization. The conclusion drawn was rather that, if trivialization is to be avoided, substantial social theory that deals with non-trivial aspects of human functioning--such as the 'attitudes, expectations and beliefs' identified above--must again be developed.

With the clear recognition of this need in positivist accounts of explanation in the social studies, and the conspicuous lack of success in meeting it, it is difficult to avoid the conclusion that positivist social theorists have been looking in the wrong place. Since the emphasis of the deductive model of explanation must be on refutability of theory, and on intersubjective reliability, the observation of regularities in social life has been, in the main, the sole domain of theory in the social studies: indeed, it follows from the attempt to model the structure of explanation as closely as possible on the natural sciences. The considerations outlined above--considerations which, it must be emphasized, have been developed entirely within the positivist account of the nature of social study--lead, however, to the conclusion that observable regularities do not constitute the essential nature of human affairs; and that social theory must, if it is to be at all successful, deal with the internal things that lie at the heart of social life. Here the positivist analysis ends: about how these things are to be apprehended, and how theory concerning them is to be framed, it has nothing to say. But it is worth noting that the need is recognized; and thus in attacking what they conceive to be positivist insistence on the adequacy of their theory, critiques of positivism are to that extent misconceived.

2. The critique of positivist approaches to the social studies, developed by Winch and Collingwood, places at its centre the idea that the springs of social action lie in thought. This idea--that all social events are the external expressions of structures of thought, and that it is to these structures that, for substantive explanation of such events, appeal should be made--represents a significant advance in the understanding of the social studies. For it offers something which, as we have seen, positivist accounts

cannot lay claim to: insight into the internal, but undeniably central, areas that influence social life as it is actually lived and experienced. Now, there is no reason why, in principle, positivist structures of explanation could not deal with such matters, if a sufficiently comprehensive theory were available. But the critique developed by Winch and Collingwood takes the view that, if such a theory were to be constructed, the act of construction itself would give rise to the requisite understanding: there is nothing to hand over to a deductive model of explanation, nothing more to be gained. Deductive explanation is possible, but redundant. Moreover, since the principles on which action is based are recognized not to be deterministic, a counter-instance does not tell us anything; as it would, under the orthodox view, in the context of the natural sciences.

Thus, for both Winch and Collingwood, one speaks not so much of explanation as of understanding; and what is to be understood is the structure of thought that underlies action. But it is important to note that, for both, thought means rational thought, roughly equivalent to the "attitudes, expectations, habits and beliefs" identified by Popper and Nagel: emotional and other kinds of "thought" (taking the term in the broad sense) are specifically excluded. Winch is, as we have seen, explicit about the nature of such rational thought, but says little about the way in which it is to be apprehended; in contrast to Collingwood, who says little about its nature, but much about its apprehension. For Winch, thought arises in a particular social context of meaning: and meaning is inextricably bound up with the structures of language and of intelligibility that both determine and express how the world is viewed and interpreted. A central consequence of this position is that the understanding of social phenomena is 'systematically ambiguous' from context to context, and can only be developed in terms of the rules, or

principles of a particular form of life. Since rules, like language, can only, in Winch's view, be constructed in a social context, they express the forms of social relations that arise in a particular society. Hence to understand social relations is to understand the the structure of meaning to which they give rise, and with which they continually interact. Collingwood, too, although he is not explicit about it, must have in mind something similar, since he refers to the context in which thought arises at a particular time, and to the need to experience that context in order to understand the thought.

Now, this all follows quite logically and persuasively, but it is important to note that its validity depends on establishing its epistemological foundations; and this, we may justifiably feel, Winch fails to do. He provides no warrant for the notion that the structures of human thought are social in their origin and development, beyond a discussion of the idea of private languages, which he attempts, unconvincingly, to refute. There is in fact, good evidence to the contrary, and it is contemporary evidence of which Winch should have been aware. Piaget (1970, 1971), whose published studies and theories have been available for half a century or more, has provided strong evidence for the view that it is the structure and function of human physiology that forms the framework on which cognitive schemas, in interaction with the physical and social environment, develop; and that such development occurs through predictable stages which seem to occur similarly in a great many social contexts (see chapter IV). Thus although it is clear that social relations do play an important role in developing structures of thought, it does not seem to be true that they are sufficient to that development; or, indeed, that they have priority among the different factors that Piaget and others have identified as being relevant. In consequence, the idea that understanding in social inquiry must be undertaken exclusively within the context of meaning

of the social context in question loses its restricted epistemological foundation; and the possibility is opened up for generalizations that are common to different forms of life.

We do not deny the value of the understanding that is constructed within the structure of meaning associated with a particular form of life: such understanding will always have much to tell us about the internal dimension of that form of life, about the way it is experienced and lived. What we do wish to resist, however, is the notion that this is the only kind of understanding which can, in principle, be gained about human affairs; that there is a logical incompatibility between the concepts involved in the idea of a human society and the deductive model of explanation developed in the context of the natural sciences. To this proposition of Winch's we would now wish to oppose the view that the epistemological foundation of such a logical incompatibility cannot be established; and, indeed, in the light of the contemporary understanding of cognitive development, seems likely to be wrong. However, a positive assertion of the value of the deductive model of explanation to understanding in the social sciences requires further steps that this position does not undertake. Here it is simply established that the value of such a model is not ruled out a priori.

3. We have seen that the critique developed by Winch and Collingwood embraces not only a particular view of social life and the nature of the understanding of it that can be obtained, but a particular view of the methodology that is to be employed in gaining that understanding. Winch brings his analysis to the point of noting that two levels of analysis seem to be involved in understanding human societies, and that these levels relate to the contexts of the observer and of those he is observing; but he seems to find it

difficult to go any further than this general notion, and is unable to specify what kind of methodology should be employed. He insists that it is the internality, the context of meaning, of observed action that must be apprehended; and, while apparently endorsing Weber's concept of verstehen--intuition--and sympathetic identification, rejects the idea that the conclusions derived from this method can legitimately be verified or refuted by observation of "externalizations." For Winch, verification is not external, but internal: the meaning of an action, as codified in the rules according to which it is being carried out, is open to scrutiny by other members of the society, who are familiar with the rules, but is not open to an observer external to that framework. Just how the investigator of social phenomena--and Winch does not seem to be disputing the idea of such investigation--ought therefore to proceed is not clear. Winch does not extend his epistemological position to an attempt to specify how knowledge of a form of life can arise. He is content simply to challenge the deductive model, which rests on the concept of objectively determinable fact, and to reject the methodology associated with it.

Collingwood, on the other hand, expounds at length his view of appropriate methodology, and develops his epistemological position in that discussion, rather than in his consideration of the nature of social life, as Winch does. As we have seen, his view of the nature of historical knowledge presents some fundamental problems: he is unable to reconcile his belief that "historical knowledge can only grow out of historical knowledge" (Collingwood 1948, p. 247) with a recognition that certain "fixed points," related to the "authorities" the historian draws on, are necessary to avoid the "risk of losing touch with the reality . . ." (Collingwood 1948, p. 242). He attempts to bridge this gap by appealing to the idea of "critical" thinking, which the historian brings to

bear on his documents, and which provides the stable framework from which the valid assessment of interpretation can be made. Yet the possibility of such critical thinking rests on an epistemological position that appears to be in conflict with his basic premise, that action is the expression of rational thought; for he is forced to posit the idea of thinking about thought, a self-reflexive process in which, through the natural structure of human thought, the historian somehow stands outside his own thought process in order to assess it. As evidence for such a view of human thought, Collingwood is content to provide us with an account of his own thought processes, presumably gained by the kind of reflexive act he is attempting to establish, and therefore assuming the very thing he is setting out to demonstrate. Moreover, if it is granted that such is the nature of thought in the historian as a human being, it follows that those whose form of life he is attempting to elucidate also think in this way; and that therefore to understand the "rational" thought behind an action is far from simple, since it requires the understanding of the thought behind the thought, for which the historian would seem to need a thought behind the thought behind the thought, and so on, in an infinite regress. Finally, we noted that understanding of the historical period as a whole cannot be posited as the stable ground from which critical judgement is to operate, since that understanding too has to be gained by the same process, and a third kind of infinite regress results. It is clear, therefore, that Collingwood's attempt to construct a methodology in terms of specific contexts of thought, while at the same time preserving the ideas of reliability and critical judgement, fails, though it indicates what might be needed for a less rigid view to succeed.

That view, we may feel, is provided by Carr, who cuts through the logical regresses created by Collingwood by simply asserting the possibility of

identifying a selection of facts which are taken as a provisional starting point, in the light of a provisional interpretation. It is clearly implied in Carr's view that this interpretation is not a product of some Kantian, a priori process, but of the whole understanding of human affairs that the historian has built up by virtue of his participation in a society, and in a specialized academic community. If we want to pursue this notion to its foundations, we find a Piagetian approach to cognitive development that places historical understanding within the ambit of understanding as a whole. In consequence, historical knowledge is seen to grow as all human knowledge grows; by accretion, through the repeated comparison of fact and interpretation, and the steady modification of the structure of understanding. Such an account, we may reasonably feel, is more logical, better founded and more practical than Collingwood's somewhat rarefied views.

Moreover, Carr presents what seems to be an undeniable argument that directly challenges the value of the kind of historical re-thinking advocated by Collingwood: it is the simple proposition that "numbers count in history" (Carr 1961, p. 50), together with the inference that the understanding of society is not to be gained by investigating individuals, however prominent they may appear to be or to have been. Indeed, Collingwood's view of historical understanding, which addresses itself to re-thinking the historical situation of particular individuals in their historical context, hardly follows from his own premises; since, if history is to be the study of human affairs, and if the events that constitute such affairs are expressions of thought, it follows that what ought to be of concern to the historian is not the thought of an individual, but the thought behind the human affairs, the thought of society at large. In seeking that kind of insight, the historian is certainly entitled to gather data about the patterns of social life, to the extent that they are

recorded; and to pursue an understanding of that form of life which preserves its holistic dignity, and does not demean the individuals of that society by proceeding on the assumption that the majority moves at the whim of the few. Thus, in Carr's view, the individual and the society, the unique and the general, the fact and the interpretation, and the subject and the object are all aspects of the way knowledge about human affairs grows, as they are aspects of the way any knowledge grows. While we do not assert the possibility of separating these conjugates so as to establish an independent, neutral ground on which interpretation and theory may rest, neither need we adopt a counsel of pessimism which makes the study of human affairs whatever the investigator wishes it to be. Carr's approach seems to demonstrate that, at least for the historical study of human affairs, it is possible to move in the direction of theory and interpretation that is at least reasonable and affirmed by consensus, if not established beyond dispute.

We may note that the kind of history Carr bases his views on is, in the main, restricted to the study of the historian's own culture. Where the history of another culture is attempted, problems similar to those encountered in the attempt to approach another contemporary culture arise. Even in the first circumstance, these problems are not absent, since there can be no guarantee that the structures of meaning and intelligibility of former periods of one's own society bear a close relationship to contemporary structures. It is clear, however, that the approach advocated by Carr is intended to incorporate a solution to these problems. The framing of interpretations will of necessity include notions of meaning and intelligibility, which are likely to be somewhat crude in their initial form, but which, through the process of submitting interpretation to selected facts in the manner outlined by Carr, ought to become more and more sensitive to the nuances of the culture, and more

and more accurate in representing them. This is, in broad terms, the process of developing a "dictionary" which allows for movement from one cultural contest to another; and, as Popper and Lakatos have noted in the context of the language communities associated with scientific theories, it is a difficult procedure, but not impossible. We may accept Winch's argument that there are no legitimate grounds on which such an externally constructed view of a culture may be held to be more objective, or more valid, than the internal view of the participants in the form of life themselves. But we need not deny the value of an approach modelled on Carr's methodology: it provides an insight which, though overlapping internal structures of explanation, has its own particular value and provides its own particular understanding, in combining internal and external accounts under one integrated perspective.

4. Some of the difficulties associated with the views of Winch and Collingwood can be attributed to inadequacies in their notions of science. Since both want to maintain the position that understanding in social contexts is different in kind from understanding in natural contexts, it is essential that they have such a notion, and we are in a position to assess these notions from the perspective of the analysis of scientific method undertaken in chapter II.

Winch is far from consistent in his view of science. On the one hand, as we have noted, he associates science with an appeal to facts, and the statistics that represent them, as the final court of appeal, as the orthodox hypothetico-deductive model would have it; and he talks both of demonstrating the falsity of a hypothesis and of verifying it. Now this amalgamation of concepts raises many questions, as has been shown in chapter II. To begin with, if "facts" are to be regarded as a final court of appeal, we have seen that such certainty cannot apply to verification, as Popper has pointed out: if we

want to talk about verification, it must be in the kind of context outlined by Lakatos, in which the progressive momentum of science is maintained both by shifts in theory and by empirical substantiation. In such a view of science, however, facts are not the final court of appeal: counter-instances may be disregarded if the momentum of theory is strong. Moreover, "facts" are regarded as empirical or objective only in a methodological sense: the role of decision and interpretation in selecting "facts" is explicitly recognized to be unavoidable in mounting the whole scientific enterprise; and there cannot be, in consequence, the kind of certainty of verification that Winch here appears to be referring to. His rejection of science in terms of facts therefore rests on a misconception about the nature of science itself, and cannot be upheld.

On the other hand, Winch seems to be well aware of the nature of scientific activity, which he describes in terms that are almost identical to Kuhn's. Thus he notes that a scientist is only able to observe phenomena and relate his observations to his concepts because he shares in an established form of activity with his fellow-scientists:

> What is important is that they are all taking part in the same general kind of activity, which they have all <u>learned</u> in similar ways; that they are, therefore, <u>capable</u> of communicating with other about what they are doing; that what any one of them is doing is in principle intelligible to the others. (Winch 1958, p. 86)

To this view we would want to oppose the whole discussion of Kuhn's position developed in chapter II, and to point to the questionable epistemological basis on which it is founded, and to the various ways of mitigating the force of his objections we have alluded to. In particular, a notion of science that takes account of the importance of theory in science, and of the embedding of that theory in an historical context, while retaining the traditional elements of progress and empirical standing is, as Lakatos has shown, not impossible to

develop, provided one does not insist on the necessity for absolute, rather than methodological, philosophical foundations.

Collingwood's account of science is less sophisticated even than Winch's. For him, science is that objective, empirical enterprise, leading from observed facts to generalizations and laws, on which the idea of science in the common wisdom is based:

> For science, the event is discovered by perceiving it, and the further search for its cause is conducted by assigning it to its class and determining the relation between that class and others. (Collingwood 1948, p. 214)

This notion of science is subject to the full range of criticism outlined in chapter II, from Popper's insistence on a priori concepts and rejection of inductive logic, to Kuhn's critique of objectivity, to Lakatos's attempt to resolve these opposed views. To the extent, therefore, that Collingwood attempts to establish the particularity of his idea of historical method for the study of human affairs by contrasting it with the methods of logic and methods of science, his thesis is misconceived. Indeed, it is an ongoing misconception which leads Collingwood into a number of related errors, such as attempting to distinguish between science and history because of the role of the historian in selecting the matters to which he wishes to give his attention. Thus his remark that "Evidence is evidence only when some one contemplates it historically" (Collingwood 1948, p. 247) is equally true of science, in all the views outlined in chapter II, if one substitutes "theoretically" for "historically." Similar problems arise in his treatment of generalization, which he regards as confined to particular historical periods, and inference, which he regards as exclusively inductive; when, as we have seen, in the views of Kuhn and Lakatos similar notions about science can be identified.

We therefore conclude that the attempt by both Winch and Collingwood to establish the specificity of understanding and explanation in social contexts, with respect to the nature of understanding and explanation in the natural sciences, is ill founded, since in both cases it is based on inadequate conceptions of science. Moreover, if the argument of chapter II is accepted to be a correct view of science, and Lakatos's notion of scientific research programmes taken to represent the best account of the scientific enterprise so far developed, it is possible to go further and assert the possibility of some fundamental similarities between explanation and understanding in the natural sciences and in the study of human affairs. For we have seen that the epistemological and methodological difficulties associated with the positions Winch and Collingwood take are so considerable as to preclude effectively their application to the practice of investigating human forms of life; and that, in order to develop an approach to the social studies that does not founder in such fundamental conceptual problems, and yields an effective and practical methodology, it is necessary to look to the kind of approach that Carr offers. Between Carr and Lakatos there are many parallels. Both look to a provisional, or methodological, basis of fact, or background information, on which the enterprise can be mounted. Both assert the importance of establishing a relationship between fact and theory, or interpretation. Both see as central the ability to develop theory and interpretation by considerations internal to it, and by repeated exposure of theory to fact. Both look to establish laws, or law-like generalizations, and to develop ideas about causes. Both reject the extreme relativistic notions on which Kuhn and Winch base their analyses, while recognizing the role of the investigator, embedded in his own theoretical or historical or social context, in selecting facts and creating explanations. Both accept that, for this reason, enquiry cannot have an absolute basis of

indisputable truth, but that it can, nevertheless, proceed in a systematic and practical way towards explanation and understanding that is, to a considerable degree, subject to intersubjective assessment, and therefore constructed in terms of concensus. Both reject ideals of uniqueness or incommensurability which would preclude explanation outside historical contexts, constrain the generalization of the understanding achieved, and prohibit a priori any absolute claim to understanding of the universe, or human kind, as a whole. Both base their views on the practice, as well as on the theory, of investigation--in contrast, for instance, to Kuhn, who looks almost exclusively to the practice of scientists, and to Popper, who looks almost exclusively to the logic of science--and thus both achieve a more balanced view which gives rise to achievable implementation based on sound theory.

Certainly we would expect to see some differences in the way this shared approach is handled in the two contexts: it is likely, for instance, given the relatively poorly developed theory of the social sciences, that there the development of the enquiry would make more frequent appeal to the facts, and would be less directed by purely theoretical considerations; as is possible, for instance, in a discipline like physics. But the general strategy for developing explanation and understanding, and the nature and status of that understanding, can, on the considerations outlined above, be held to be the same for both the natural sciences and the social sciences, without violating the requirements either of logic or of practice. We may thus feel with some confidence that it is to this model of scientific enquiry that appeal can best be made in the attempt to place any discipline, whether more closely allied with the natural sciences or with the social sciences, or whether some hybrid of the two, on a scientific basis. It is this model, therefore, that, we may

conclude, can best underpin the analysis of the conceptual foundations of human geography undertaken in the succeeding chapters.

5. Finally, before embarking on such an analysis, it is worth taking note of an observation made by Nagel in his attempt to specify the nature of generalizations in the social studies. Having drawn from the very real difficulties presented by cultural relativism the inference that any generalization which refers to cross-cultural contexts is likely to be restricted to a narrow, even trivial, range, he makes an important qualification: that if valid and useful transcultural laws are to be developed, they may need to employ concepts that are

> more "abstract," . . . separated by a greater "logical gap" from the familiar notions used in the daily business of social life. (Nagel 1961, pp. 465-466)

Although it is not entirely clear precisely what "abstract," or "logical gap," means, one way of interpreting this notion is that scientific explanation of social phenomena will of necessity be more concerned with the generic functioning of human societies, and with concepts that relate to human functioning per se. Nagel recognized part of this implication in referring to

> . . . the fact that all human actions involve physical and physiological processes whose laws of operation are invariant in all societies. (Nagel 1961, p. 460)

But it seems reasonable to question whether such generic laws must inherently be concerned with only "physical and physiological" processes, and therefore, from the holistic perspective of the form of life as it is lived, be restricted to relatively trivial concerns. Certainly Winch and Collingwood would be in sympathy with this view, since the non-trivial aspects excluded here--matters relating to attitudes, beliefs, expectations, and the whole range of subjective

experience--are taken by them to arise and develop entirely within a specific context. But, as we have seen, the epistemological notions on which this kind of position is based is difficult to sustain without substantial qualification; and it therefore seems legitimate to ask whether there may be aspects of these subjective matters that are, indeed, subject to a generalized and more systematic understanding. If such an understanding could be developed, we might reasonably expect it to deal at just those more "abstract" levels of human functioning, more "separated" from the specific details of daily life, that Nagel foreshadows. Following the conclusions established above, we do not deny that understanding of a form of life can be developed in terms that are exclusively internal to it; nor do we wish to claim priority for a more generalized understanding. We simply assert that if a generalized understanding, more in accord with the notions of scientific enquiry we have developed, is to be sought, this may be a fruitful strategy to adopt. More significantly, from the perspective of this dissertation, such explanation and understanding, functioning within a broadly scientific framework, might go some way towards reconciling the different views of human geography outlined in chapter I. On the basis of the considerations outlined in this chapter, and the preceding one, it would seem at least to be a reasonable direction to take, and a reasonable task to attempt, in approaching the conceptual foundations of the discipline from the perspective of the model of scientific enquiry we have now established.

3.2 Human geography as a social science

3.2.1 Theory in human geography

We are now in a position to examine the notion of theory, as the term is employed in this dissertation. The main conclusion of the foregoing discussion is that it is a primary task of human geography to seek a set of statements that can constitute a valid and useful body of theory. The ideal characteristics of such a body of theory have been outlined in section 2.7. Following Lakatos's view of science it is to be required that the laws or theoretical statements which constitute the theory be systematically interrelated; and that they have already attained, through testing, a degree of credibility (or corroboration). There must be bridge principles that relate the fundamental terms of the theory to existing bodies of accepted knowledge. The best theory will be able to unify diverse phenomena, and to expand the range of previous theories through its ability to predict and explain new phenomena. It will further be as fully formalized--that is, the logical relationships between elements of the theory will be as rigorously developed--as possible, so that test implications may be derived, and the results of testing may be clearly assessed: only in this way will that dialectic of theory and evidence, which, in Lakatos's view, is to give rise to the growth of knowledge in the discipline, be effectively generated.

In addition to its role in the development of human geographic knowledge, theory is held to be essential to the formulation of effective strategies of intervention that might better the man-environment relationship. The applied nature of twentieth-century human geography has always been one of its most

widely supported values; and the contemporary thrust of the discipline towards a prescriptive, or committed, geography (see, for instance, Bunge (1973a), Ley (1978a), Stoddart (1981a)) has brought it to a position of unprecendented eminence. With this trend the author is in complete sympathy: the problems facing modern man are too urgent and too critical to allow much undirected theorizing, though some pure research would seem to be an essential part of the formulation of future effective interventions. However, it is not accepted that, as Harvey (1973) and others have argued, a scientifically based human geography has failed to address itself to real and substantial human problems primarily because of its adherence to scientific method, and must be replaced by ideological commitment and analysis. Rather it is held that what is needed is better science: more comprehensively human theory, more penetrating methodologies of investigation, and, in consequence, more effective strategies of practical intervention.

Taking as a whole the positions outlined so far--on scientific method, on social science, and on theory--it is evident that theory occupies a fundamental position both in the growth of disciplinary knowledge and in the formulation of effective strategies of intervention. In Lakatos's view, the momentum of a scientific research programme is largely theoretical, with empirical evidence serving largely to support its direction. There is no such thing as mere fact: it is only in the context of theory that factual evidence assumes significance. On the process by which theory is formulated, we place no logical restriction; and we accept that contact with the real world is inseparable from that process. Ultimately, it may be necessary to take a genetic view of concept formation, as suggested by Piaget (1970, 1971). Explanation, however, in a scientific sense, begins with theory, and then proceeds to evidence.

The limited aim of chapters IV, V, and VI is therefore to begin to establish a coherent theoretical basis on which the development of such a research programme might proceed. That is, it attempts to derive a set of statements, or principles, which has a degree of corroborative status through empirical test, and which exhibits as many as possible of the central qualities of an ideal body of theory, as outlined above. This can be described as an exercise in philosophical analysis, and the procedures by which such analysis may be carried out have been outlined in chapter I. We may, however, here define more formally its nature, and its position in the logical structure of explanation.

The ideal, with respect to formalization, to which a scientific theory aspires is, as outlined in section 2.2, a deductive or axiomatic system. It is not expected that the social sciences will be in a position to develop formal axiomatization, in the technical sense of complete abstraction of the theory from its content (Stoll 1974, p. 161); and, indeed, such a development is neither necessary for its scientific status, nor likely to be of much value in the understanding of most human phenomena. It may, however, be possible to develop a body of theory in the spirit of informal axiomatization, as outlined by Stoll (1974, pp. 138-142). In such a system, certain basic notions (or primitive terms) are combined with certain assertions (or axioms) about them to derive statements (or theorems) which constitute the body of theory. If the procedure has been carried out validly, such theory will successively yield specific test implications, and the process of growth of theory can proceed:

> The initial steps in such a formulation are critical. The first is to list what are judged to be the basic notions discussed by the theory, together with what is judged to be a basic set of true statements about these notions. (Stoll 1974, p. 141)

Having then decided what theories are to be assumed (for instance, the theory of logic) it becomes possible to define more precisely the properties of the fundamental notions and to develop a calculus through which axiomatization can proceed. What is important here is that the success of the entire procedure rests on the validity and precision with which the fundamental concepts, often intuitive, are identified and described. In particular, if the theorems developed are to be subject to empirical test, the primitives of the system must be embedded in the real world, and themselves yield test implications.

Let us be quite clear about what is being proposed here. It is not being asserted that human geography, as a discipline, ought to aim at formal axiomatization through the use of symbols and appropriate languages of manipulation; though specific areas of the discipline, notably in the development of spatial and behavioural theory, have employed such a methodology. Rather, adoption of the broad structure of a deductive system is being urged as a way of clarifying both the conceptual roots of the discipline, and the higher-order theories that have been developed from them. On this basis, reorientation of the discipline, in the light of new bodies of theory and research, can validly proceed.

Thus the first and most critical step is to identify the primitive concepts of human geographic theory, many of which are implicit or unrecognized; and then to examine them critically, in the light of internal logic and consistency, of other relevant bodies of theory and empirical evidence, and even of common experience. The aim of this procedure ought to be to establish these notions as validly and as comprehensively as possible in the real world, since only on that basis can the criteria which, as outlined above, characterize the best theory, be met. We may now turn to a discussion of the general nature of such fundamental concepts.

3.2.2 Primitive concepts in human geography

It has been proposed that the domain of the human geographic enterprise principally centres on the notion of man and environment; and that the development of understanding about this domain may validly and usefully proceed on scientific principles. The question which next arises is at what level of resolution the analysis is to take place. The phrase "man and environment" is, perhaps, widely understood to relate to macroscopic systems; to human populations, and to the environmental systems with which they interact. Yet it is clear that such a specification is not a logical requirement, since "man" can include all combinations from individual to global, and "environment" an equivalent range. What, then, is to be the focus of the enquiry?

Harvey (1969a, ch. 24) has discussed this issue in the concluding portions of his methodological treatise. He points out that there are three interrelated decisions which have to be made a priori to geographic investigation; about which, in other words, it is necessary to adopt methodological conventions. They are decisions about the nature of geographic individuals (units of analysis); about the nature of geographic populations; and about scale. He takes the position that the geographer

> . . . is not concerned with the spatial patterning of crystals in a snowflake (the resolution level is too high) nor is he concerned with the spatial patterning of stars in the universe (the resolution level is too low). . . . Geographers tend to pick a resolution level that lies somewhere between these two. Characteristically geographers tend to work with human and physical differentiation at the "regional" level although it is difficult to pin this down with any precision. . . . Any phenomenon that exhibits significant variation at that resolution level is likely to be the subject of investigation by the geographer. (Harvey 1969a, pp. 484-485)

Thus, in human geography, the community, the neighbourhood, the city, the region, an ethnic or racial group, a culture, are all held to be examples of the scale at which the geographer focuses his investigations. We may adopt the term collectivities to describe them. By and large, despite the current fragmentation of the discipline, a survey of contemporary literature seems to indicate that Harvey's proposition would receive widespread support. It is therefore proposed to adopt it as a reasonable statement of the scale of resolution for which the human geographer aims to develop understanding and explanation. Having adopted this as a convention, we may now ask whether anything can be derived on logical grounds about the way in which such understanding and explanation is to be achieved.

The historical development of the methodology of social science suggests two apparently separate approaches to the study of collectivities. One, identified with the main protagonists of social science, such as Durkheim, asserts both the validity and the necessity of an holistic view of social collectivities. An hierarchical view of physical and social systems is adopted, in which at each scale (or, to employ Harvey's terminology, level of resolution) there exist characteristic explanatory schematas:

> Life cannot be thus divided; it is one, and consequently cannot be based on anything other than the living substance in its totality. It is in the whole, not in the parts. . . . We must, then, explain phenomena that are the product of the whole by the characteristic properties of the whole, the complex by the complex, social facts by society, vital and mental facts by the sui generis combinations from which they result. (Durkheim 1974, p. 29)

Durkheim therefore opposes what has come to be termed explanation by reduction: the attempt to derive empirical predictions about collectivities from corroborated theory about the singularities of which they are comprised. In social science, this notion of reduction has been described as methodological individualism, as espoused by Popper (1957), Watkins (1953), and others.

Before proceeding to a closer consideration of this concept, however, it may be of value to review briefly Nagel's (1961, chs. 11 and 12) discussion of the logic of reduction. He identifies a number of formal and non-formal conditions for reduction to be valid (Nagel 1961, pp. 345-366). The first include explicit formulation of the primitives, axioms, and laws of the respective theories; and conditions of connectability and derivability which link the terms and the laws of the two theories. The second include the conditions that the primary theory, or science, be adequately corroborated; demonstrate a wide range of application, fertility of implication for the secondary science; and provide a unified explanation of various experimental laws. These are formidable criteria to meet, and constitute a salutary caution as to the likelihood of achieving any real success in attempting a valid reduction.

In applying these general criteria to the analysis of collectivities--"wholes" and "sums"--Nagel points out that there is considerable ambiguity both in the distinction between whole and part, and in the notion of addition inherent in the idea of a sum. In particular, organic, or functional, wholes, which "exhibit a mode or organization often claimed to be incapable of analysis in terms of an additive point of view" (Nagel 1961, p. 391), are found to be difficult to distinguish from summative systems, since the analysis of each is likely to require assumptions both about the elements of the system and about the organization of those elements within the whole. Nagel concludes that, while an additive analysis can be adequate for some functional wholes, for others, notably living organisms, no such analysis has yet been successfully achieved.

We are now in a position to take up again the question of how collectivities in the social science may best be analyzed. Methodological individualism, as noted earlier in this chapter, originated with Mill's (1974) attempt to derive a logic of investigation for both the natural sciences and the social sciences. He sought

> the science of man in society: of the actions of collective masses of mankind, and the various phenomena which constitute social life. (Mill 1974, p. 875)

His methodologic stance on the investigation of society is simple and unequivocal:

> All phenomena of society are phenomena of human nature, generated by the action of outward circumstances upon masses of human beings: and if, therefore, the phenomena of human thought, feeling, and action, are subject to fixed laws, the phenomena of society cannot but conform to fixed laws, the consequence of the preceding. (Mill 1974, p. 877)

The laws of human nature are seen to comprise psychological and ethological (or situational) laws, and to act reciprocally on the environment:

> The circumstances in which mankind is placed, operating according to their own laws and to the laws of human nature, form the characters of human beings; but the human beings in their turn, mould and shape the circumstances. (Mill 1974, p. 913)

The laws of social collectivities can only be deduced from the laws relating to individual functioning: and they generate no laws of nature, but only empirical laws; or, in Popper's (1957) terminology, trends.

Popper (1957, 1963b), while a strong advocate of methodological individualism, is opposed to Mill's form of it, which he dubs "psychologism" (Popper 1957, p. 158), and which he sees an imprecise and tautological: what is meant by human nature will vary, he claims, with different social institutions. He substitutes a rather different view of methodological individualism in which

> the social sciences are largely concerned with the unintended consequences, or repercussions, of human actions. (Popper 1957, p. 158)

This requires of social scientists a focus on actual individuals, not on the theoretical collectivities to which they are said to give rise:

> . . . the task of social theory is to construct and to analyze our sociological models carefully in descriptive or nominalist terms, that is to say, in terms of individuals, or their attitudes, expectations, relations, etc. (Popper 1957, p. 136)

This is seen to be consistent with the method of the natural sciences, in which it is asserted that holism gives rise to essentially trivial hypotheses about the inevitable relations between things, and where the approach to understanding is always through specific elements.

The holism-individualism debate has been varied and wide ranging, but has not, in essence, moved far from these central concerns. As we have seen, Winch sees in the rules and structures of meaning that guide behaviour in a society the imprint of the social relations in which they arise, and which they therefore express: and all individual action is seen to take place within the confines of these structures. Collingwood, while addressing his analysis primarily to the individual, concedes the necessity for locating that individual within his historical context. Carr is strongly opposed to any approach to explanation in the social studies which seeks to treat of any individual outside the society of which he is a part; or, indeed, which sees particular individuals as more important in understanding the patterns of a society than the mass of people of which the society is composed. On the other hand, Homans (1964, p. 815), for instance, largely follows Mill in seeking a psychological understanding of the process by which normative rules are internalized: in explaining social phenomena and relationships, one examines "not the needs of society . . . but the needs of men." Watkins (1973)

advocates an approach through ideal individual types, obtained by inspecting the situation of actual individuals, and abstracting from them a simplified view of them comprising "the form . . . of the actors' dispositions, the state of their information, and their relationships." Lukes (1968), however, argues that facts about the individual can be placed along a continuum of least to greatest social relevance, and that only the latter can develop valid explanations of social phenomena; as, he points out, Popper, following Mill, concedes by admitting "situations" and "interrelations" into his explanation. Nagel (1961, pp. 535-546) identifies a number of difficulties associated with the individualistic viewpoint, including those of defining individual terms separately from collective, of translating collective into individual terms, and of realizing the formal and informal conditions of valid reduction in seeking psychological explanations. He concludes that

> . . . although methodological individualism and interpretative social science rightly emphasize that social phenomena are constituted out of interactions between purposive human agents, neither of these essentially similar approaches to social inquiry possesses the unqualifiedly pre-eminent merits that are claimed for it. (Nagel 1961, p. 546)

Ryan's (1970) accommodation of the two points of view is based on the simple observation that there is no logical need to eliminate either approach: they can be seen as complementary ways of coming to an understanding of the nature of social collectivities as they actually occur. He provides the useful analogy of a play, which can be studied and described both as a whole, and through the individual parts that constitute it, the two perspectives providing mutual illumination. Thus

> the argument between holists and individualists is no more to the point in principle than is a debate about whether in principle to study the play or its parts: they are two sides of the same activity. (Ryan 1970, p. 179)

The difficulty, which Nagel identifies, of defining either individual or collective terms independently of each other lends supports to this position. It is, indeed, simple common sense that, on the one hand, the propositions of what Lukes (1968) refers to as "Truistic Social Atomism," and, on the other, those of holism, should both be valid in their own contexts, as opposite views of a single phenomenon. It may even be concluded that, although each perspective is necessary to a full understanding of social collectivities, neither on its own is sufficient. Furthermore, it may with some justification be asserted that even when mutually complementary they are not sufficient; and we may foreshadow the possibility of developing a more general theory of human functioning--perhaps along the lines of Mill's laws of human nature--in which may be developed explanations of both individual and social phenomena, and of the milieu in which they arise.

Thus taking first one side of the reciprocal equation, we may conclude that fundamental to the analysis of human collectivities are notions about the individuals of which they are comprised; and we may, in the light of the definition adopted above, assert this to be as true of human geography as of any other social science. We may further note that such notions are likely to take the form of idealizations or models of functioning of the human individual, as suggested by Popper (1957, pp. 141-142), Watkins (1953), Rudner (1966, section 11), and Harvey (1969a, pp. 92-96), rather than of an empirical investigation of each separate individual in the group. The focus will thus be on the individual, rather than on individuals. It is not necessary to adopt the view that from such models law-like statements about the collectivity must be deductively derived: we simply state that fundamental to the analysis of human collectivities are assumptions about the functioning of the individuals which comprise it.

In analyzing the collectivities of human geography--collectivities which, according to the view of human geography developed in chapter I, include the natural milieu in which the life of human groups arises--it is therefore convenient to begin with an understanding of the human individual. We may, indeed, predict that each main school of human geography has based its approach on a particular model of, or set of assumptions about, individual functioning. These models may not be explicitly formulated, or even recognized: they do seem, however, invariably to be present, and to structure in a fundamental way the theory of collectivities that is developed. Adopting the terminology of the preceding section, we may identify them with the primitive concepts of human geography.

A detailed analysis of such assumptions in selected areas of twentieth-century geography is contained in chapter IV, and will not, therefore, be here rehearsed. Preliminary support for this proposition can, however, be obtained by selecting almost any of the works representative of human geography of the last two decades, and seeking their underlying assumptions. Thus Haggett (1965) bases his analysis of locational theory on the notion of "bounded rationality," and on its legitimate translation into stochastic models. Realizing the need to embed this approach in the real world, he remarks,

> This is of course the world which as individuals we know: a world which is neither wholly rational nor wholly chaotic, but a probabilistic amalgam of choice, calculation, and chance. (Haggett 1965, p. 27)

Fundamental epistemological assumptions are made here; and it is argued in this dissertation that the "of course" with which they are adopted is not necessarily justified. Haggett himself, later in the work, questions the relevance of the approach (Haggett 1965, p. 182).

Similarly Smith (1977), in a work which has become a fundamental source of prescriptive theory in the liberal tradition, outlines a theory of "Human Being: Needs and Wants," which begins with the following:

> What is it to be human? What is human <u>being</u>? These may seem to be philosophical questions quite beyond the traditional realm of geography, but if human geography is to be truly concerned with individual or group well-being, then the nature of human being is a necessary starting point. Theory in human geography must be rooted in the reality of human existence. (Smith 1977, p. 27)

To this postion, it should now be clear, the author lends unqualified support; and we may perhaps go on to wonder whether, in recognizing the multi-dimensional nature of human well-being, and yet in persisting with an overwhelmingly economic view of human well-being, Smith does not abandon as too difficult the task he identifies as fundamental.

Likewise, in a recent essay in the humanist analysis of place, Buttimer (1980) finds no difficulty in beginning:

> It appears that people's sense of both personal and cultural identity is intimately bound up with place identity. . . . When the fundamental values associated with . . . these levels of experience are threatened, then protest about the meaning of place may erupt. Whether all these values are <u>consciously</u> articulated in legal or behavioural terms does not seem to be the crucial point. In fact, they are often not brought to consciousness until they are threatened: normally, they are part of the fabric of everyday life and its taken-for-granted routines. (Buttimer 1980, p. 167)

It is clear from the key terms employed here--identity, values, experience, meaning, consciousness, and so on--that a specific view of individual human functioning underlies these statements. In Buttimer's case, these assumptions, though not detailed in the work quoted here, are discussed elsewhere (Buttimer 1974, 1977).

These three examples, chosen almost at random, can easily be multiplied, and underscore the central proposition of this section. Stated simply, it is that any effective systematic approach to collectivities in human geography can only proceed on the basis of an understanding of individual human functioning. Theories about the individual are implicit in every human geographic theory. We may choose to ignore them, and insist, as geographers, on restricting our domain of investigation to the collective level of resolution; but we will, nevertheless, have to employ such theories in our investigation, whether we acknowledge them or not. It would therefore seem to be wiser to attempt to obtain the best such theory, either by adoption of existing approaches, or by construction of new ones. An analysis of this approach to theory in human geography is undertaken in chapter IV.

At the same time, it is accepted that such theories cannot be developed independently of theories that relate the individual to the society of which he is part, and to the man-environment milieu which is the special province of human geography. Any approach which seeks to do so will of necessity find itself required to make <u>a priori</u> assumptions about these matters in order to formulate such theories of the individual at all. Moreover, we have established that understanding of collectivities can be approached equally as well from the perspective of holism as from that of individualism. We would therefore expect to be able to locate theories whose primitive concepts relate specifically to the phenomena of collective life; in the case of human geography, to aspects of the relationship between the human group and its environment. At this "level of resolution," to employ Harvey's phrase, it ought to be possible to develop statements which express the reality of collective life in its own right, and which, in combination with each other, can give rise to genuine theory which is able to interact with empirical

evidence in the way required by the Lakatosian model of science. This has, indeed, been the approach of traditional human geography in the twentieth century; and it is still the dominant mode of analysis in the discipline, particularly in spatial system and sociological perspectives. An analysis of this approach to theory in human geography is undertaken in chapter V.

It seems reasonable to argue, however, that of the two approaches the first is the more fundamental. Ultimately, however accurate the account of collective forms of life may be, we are not likely to feel that we have understood and explained that form of life until its patterns and movements have been related to the reality of human life as it is lived by the individuals who comprise the group in question. An important part of the succeeding analysis will therefore be to see whether the theory of the individual developed in chapter IV is sufficient to underpin the theory and insights of chapter V. It is interesting to note that, in arranging perspectives on human geographic theory in this way, an historical pattern emerges: broadly speaking, collective approaches, though they have persisted in contemporary times, are more characteristic of human geography in the first part of the twentieth century; individualistic approaches, though certainly present throughout the twentieth century, have become prominent only in recent times. If we can hold that the discipline has been making progress, in Lakatos's terms, during this time, there seems here to be an implication that moving to underpin collective theory by individual is to move towards more fundamental kinds of explanation and understanding.

We do not wish to deny the force of that argument which insists on the value of accounts of collective forms of life at their own level and in their own terms: systems theory, for instance, is predicated on this principle, which strongly opposes the reductionism and atomism it sees as characteristic

of modern science (Lazlo 1972); and systems approaches are a well established part of contemporary human geography. But if we seek, as this dissertation seeks, an accommodation between traditional, quantitative, and humanistic approaches to the discipline, it seems necessary to make the theory of the individual a priority. If the humanist demand that an account of human experience must be a central part of human geography--a demand whose validity is supported by the considerations of the preceding section--is to be met, an adequate theory of the geographic individual would appear to be of the first importance. It is therefore to this theory that we now turn.

CHAPTER IV

APPROACHES TO THE INDIVIDUAL IN HUMAN GEOGRAPHY

Outline of the Chapter

4.1 Introduction

- The generic individual
- Levels of individual functioning
- Traditional insights
- The positivist approach
- The behavioural approach
- The humanist approach
- A proposed integraged model
- Proposed evaluation of the model

4.2 Approaches to the individual in human geography

- Behaviour and decision-making
  - Rationality and optimizing
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    - Traditional perspectives
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    - Perception
    - Cognitive structures
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    - Affective values
    - Parallelism

CHAPTER IV

APPROACHES TO THE INDIVIDUAL IN HUMAN GEOGRAPHY

4.1 Introduction

The preceding two chapters have presented the view that there is an approach to scientific inquiry--the Lakatosian model--apparently capable of resisting challenges of theory and practice in both the natural and the social domains. It was concluded that therefore there seems to be no reason in principle why human geography should not be framed as a science; nor any reason in principle why, so framed, it should lose the ability to deal at a fundamental level with the realities of human experience. It was noted, however, that such a conclusion carried with it certain implications about the nature of the theory that is to be developed, if it is to avoid triviality: specifically, that such a theory may best be sought in the more abstract, less concrete areas of human living, perhaps quite removed from the concerns of ordinary daily life. In this chapter, and the next, an attempt is made to apply these conclusions to the discipline of human geography: to see whether a scientific theory of the kind foreshadowed can be established; whether, if it can, it gives rise to insights sufficiently fundamental to underpin a true scientific research programme, in the sense advocated by Lakatos and Carr, and adopted here as the best of the available models of scientific inquiry; and whether such a theory shows promise of meeting the humanist demands for relevance and realism outlined in the first chapter.

It has been proposed that, although, in the approach to environed man that characterizes human geography, both individual and collective approaches are legitimate, and have been adopted in practice, nevertheless consideration of the geographic individual seems to be logically prior to consideration of the geographic collectivity. Primitive concepts, in other words, exist at both individual and collective levels; and yet there seems to be a sense in which those associated with the first are more primitive than those associated with the second. There is, after all, the simple observation that it is possible to conceive of, and discuss, the relationship of an individual to the environment without reference to the social group of which the individual is a part; though many human geographers would agree with Winch (1958), Carr (1961), and other social theorists (including Marxist ones) that such a consideration is an artificial idealization at best, and fundamentally misleading at worst. On the other hand, it is not at all possible to talk about the relationship of a group to its environment without reference to the individuals of which the group is comprised: the idea of the 'group' would in that case lack any definition.

This common-sense observation is made formally legitimate by elementary notions of logic. As Langer (1966, ch. V) points out, the formation of any class proceeds by taking in extension a defining form, which attributes certain properties to the generic individual. Thus the class "politician" is formed by taking in extension the defining form "x practices politics", a statement which itself is comprised of class concepts formed in the same way. The direction of conceptual formation is thus inherently from individual, or element, to group, or class. In our context, we seek to construct an understanding of the geographic group in some fundamental way, though not exclusively, in terms of the geographic individual.

This conclusion, however, raises a number of further questions. Chief among these is the difficulty of deciding what is meant by the term "geographic individual". For "geographic" we may, as we have decided in chapter I, substitute "environed"; but how is "individual" to be taken?

We may begin to provide an answer by noting that, if the theory to be constructed is to be at all substantial, the term "individual" must be taken generically. The understanding that we seek is not to be limited to one particular context of group and environment: such a 'theory', formulated amongst unique circumstances, cannot be regarded as sufficient to give rise to a Lakatosian scientific research program of human geography. Although it may be capable of a degree of theoretical and empirical advance, it is constrained to operate within one particular context. Such theory may give rise to a natural history of human geography, but not to a science. A science is inseparable from the establishment of laws; and a true law, as we have seen, locates its law-like status, at least methodologically, in its universality. Any idea of the geographic individual which seeks to find expression in scientific theory must therefore transcend particular circumstances of space and time and treat of generic concepts that may be associated with any environed individual in any place, at any time.

Having established that, it is still necessary to address the question of what aspects of the geographic individual ought to be included in the analysis to be undertaken. It is evident that the actual range of functioning of the environed individual extends well beyond the confines of that domain which human geographers might normally regard as theirs. Clearly the individual interacts with the environment at a physiological level through the senses, and through participation in the physical ecology of the natural environment.

Equally clearly, the individual acts and behaves overtly in the environment, both in responding to it, and in modifying it; and creates, in so acting, particular spatial patterns. These are both aspects of the environed individual which have traditionally been central areas of investigation for human geographers, and in which substantial analysis and understanding have been achieved. Any theory of the individual that claims to underpin human geographic theory must necessarily include them.

It is, however, a matter of common experience that action is not undertaken in a vacuum: before acting, it is necessary to decide to act, and to act in a particular way. The context in which that decision is taken is the whole range of the subjective life of the individual, an analysis of which might include such terms as ideas, feelings, attitudes, beliefs, ideology, intuitions, prejudices and so on. Even for the human geographer, whose concern traditionally has been with the observable aspects of the interaction of man and environment, there seems to be no getting away from the subjective basis of human action; and although human geography need not, on this analysis, necessarily become a branch of psychology, or philosophy, or phenomenology, it must, it seems, deal with the same areas of experience, and have regard for the same kinds of understanding, that are approached and developed in these disciplines. Work in cognitive psychology, as exemplified by Piaget (1970; 1971) has even suggested that sensory interaction with the environment is mediated by cognitive structures, and cannot be considered independently of them. If, therefore, the basis of observable human interaction with the environment is to be located in the subjective aspects of human experience, it is not easy to see on what grounds such aspects are to be excluded from the ambit of human geography. One may appeal to the complexity of subjective

experience, and point to the merits of specialization in penetrating to fundamental levels of understanding (as suggested by Kuhn, see section 2.3 above); and such pragmatic considerations have, it must be admitted, some validity. But even if such specialized accounts can be developed (and, as we shall see, by and large they have not been convincing), the above considerations suggest that a complete account of environed man would require the integration and coordination of these accounts into a unified, holistic theory; and by whom is such an integration more fittingly to be effected than the human geographer, whose tradition is pre-eminently a synthetic one? We may therefore adopt the view that, in considering the geographic individual, the whole range of individual life and experience is relevant to--indeed inseparable from--geographic understanding; and that human geography shares with all the social sciences and humanities, as its primary focus, the task of delineating the nature of human experience, and the springs of human action.

Traditionally, human geographers have not been unaware of this idea. As the discussion of the next section will indicate, the identification of subjective factors at the basis of human action in the environment has been a part of human geography since its inception. It has long been recognized that, if the patterns of collective life are to be satisfactorily accounted for, the context of culture--of ideas, beliefs, and attitudes held and developed in common--in which these patterns are embedded, must be located and described. Such analyses have, however, generally been limited to the identification and description of cultural traits, and of their relationship with specific aspects of collective activity in the environment. They have not attempted to derive the analysis systematically from an understanding of the nature of the individual, and of the nature of the interaction between individual and group.

And yet to say that, for instance, a particular form of utilization of the environment derives from a particular system of priorities that expresses fundamental cultural values is to raise, perhaps, more questions than are answered. How can the culture in question be best described and defined? How are its values developed and maintained by the individuals of the group? How do these priorities find expression in a particular pattern of utilization?--and so on. These are not questions that can be satisfactorily answered without reference to the individuals of which the group is comprised; or, perhaps it may more accurately be said, without reference to the typical individual of the group, to a model or idealization of such an individual. Without such theoretical underpinning, it is difficult to feel that an explanation couched exclusively in collective terms has really explained anything: we want to know how these notions relate to the lived reality of human life; and that knowledge can only be had at the level of the generic individual.

Even this limited approach to subjectivity, which characterized human geography in the first half of the twentieth century, was abandoned with the onset of the quantitative revolution, in which, as we have seen, individual, and therefore collective, life was taken to be the product of irreducible uncertainty and randomness. This, as we have suggested in section 1.2 above, represents the nadir of the understanding of human life developed in human geography. To deny that the springs of human action have a structure of order that can be approached and understood at least partially from a rational perspective; to attribute regularities to collective thought and behaviour alone, and to locate them solely in the domain of mathematical statistics, is to debase unacceptably the idea of human life. Behind the facade of scientific method, this early approach of the quantitative school essentially abandoned

the attempt to provide a convincing explanatory account of human action in the environment, and contented itself with simple description of the collective dimension of that action. The theoretical appeal here was to patterns of movement in, and utilization of, space, and to the geometrical structures that seemed best to describe them. Little remained of human life and living.

The emergence of the behavioural school, in the decade following the quantitative revolution, represents, at least in part, an attempt to restore to human geographic theory a greater measure of human authenticity, within the scientific model. Thus the roots of behaviour in the environment were sought in the process of decision-making, a process which, it was felt, could be modelled and could give rise to testable predictions. Since decision-making is an internal process, this approach appeared to restore subjectivity to a central place in the model of the individual utilized in human geographic theory; and yet, clearly, it is a limited account of individual functioning, since it largely ignored the aspects of ideas, attitudes, feelings and beliefs which are fundamental to the making of decisions. To have affirmed, however, that outer processes are guided by inner, and to have attempted to treat these inner processes within a systematic, scientific framework, were significant developments in human geographic theory.

The humanist school, emerging slightly later than the behavioural school, took up the challenge of constructing a theory of the geographic individual that could include the whole range of individual functioning. However, as outlined in section 1.2, scientific methods of inquiry and structures of understanding were here seen to be incapable of dealing with the deeper aspects of human experience, and were therefore abandoned in favour of more subjective modes of understanding, which alone, it was argued, are capable of delineating

such apparently extra-rational domains of human experience. This contention, as chapters II and III suggested, is open to serious question; but the great value of the humanist approach in demanding a treatment of the whole geographic individual, of the whole range of human experience, is difficult to deny.

It is therefore clear that an approach to modelling the individual which includes both external aspects of physical and behavioural interaction, and internal aspects of subjective experience, can be located in the theory of twentieth century human geography. The delineation of such a model, however, can only be undertaken by viewing the discipline as an integrated whole; and, it may be argued, this is a perspective which the evident fragmentation of the last two decades does not support. Such an objection is not easily answered, particularly when proponents of each school have been at such pains to dissociate their own views from those of other schools. And yet if one attempts to view twentieth century human geography as a whole, it is possible to discern an integrating pattern. For each school identifies an element, or level, of individual functioning it takes to be of fundamental significance, whether it be the relationship between the outer environment and inner experience, or some other perspective on environed man. Since priority, rather than mutual exclusiveness, is in each case asserted, all perspectives can be taken together to yield a reasonably coherent and integrated model of the geographic individual. A primary assumption of this approach to the theory of human geography is thus that each perspective on the geographic individual has validity within the range of its own development; and a corollary to it is that no one perspective can claim to be complete. On this view, therefore, it is only in such an integrated account of human geographic theory that primitive concepts, adequate to the development of the kind of scientific understanding foreshadowed in the preceding chapters, are likely to be identified.

The integrated account of the geographic individual attempted in the succeeding section is thus not framed along historical lines, although such an approach might be thought to be a natural way of approaching the idea. Instead, an attempt is made to locate the contribution of each main school within an integrated model of the geographic individual. The structure of this account is thus not primarily historical, but theoretical. Since a central movement of twentieth century human geographic theory has been, as outlined above, from objective towards subjective aspects of the individual--from the concrete expression of action and behaviour, to the more abstract 'springs of action' located in inner experience--this vertical perspective on the nature of the geographic individual has been adopted as a framework for the discussion (see Figure 3). Thus the analysis of action and behaviour, and their derivation from decision-making, is first dealt with, as representing that level of individual functioning which is most concrete, and most accessible to objective methods. This analysis, however, indicates that decision-making is not the entirely rational procedure it may first appear to be: the context in which it is undertaken is itself a function of the more abstract, experiential structures associated with cognition, which then becomes the second level of individual functioning to be identified and analyzed. It is here that the widest historical spectrum of twentieth century human geographic theory, and the most fruitful interaction with a cognate discipline--in the cognitive psychology of Piaget--can be located. The fundamental ground of cognitive structures is identity, which therefore emerges in its own right as a major level of individual functioning. Beyond that, it seems necessary to employ such terms as 'consciousness' or 'awareness', which represent the most abstract level of individual functioning identified in the theory of twentieth century

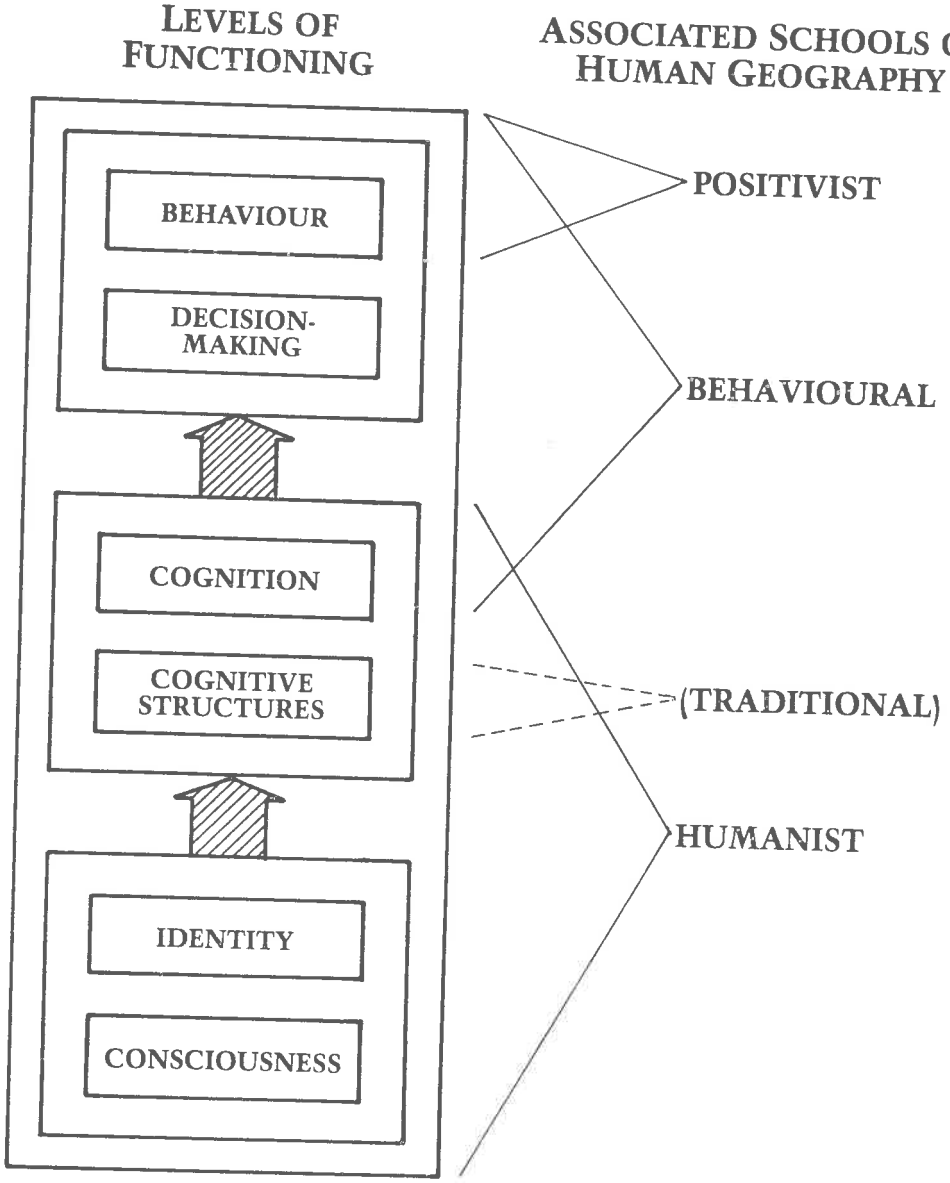

FIGURE 3:

Proposed Integrated Model of the Geographic Individual and Associated Schools of Human Geography

human geography. These are areas that have been the particular concern of contemporary humanist geography: they centre on the notion of experience, and look to subjective, rather than objective, methods of investigation.

Having surveyed the geographic literature from this perspective, there arises then the question of whether the integrated account of the geographic individual thus developed adequately fulfills the requirements for scientific theory established in the two preceding chapters. Three principal areas of consideration present themselves:

1. Can the account be characterized as a theory? Consideration of this question evidently requires an analysis of the formal structure of the integrated account, and an assessment from the perspective of the position established in sections 2.2.7 and 3.2.

2. If it is a theory, to what extent is it a scientific theory? Here it is necessary to go beyond internal structure to the historical context. It will be recalled that chapters II and III concluded, in both the natural and human domains, that the idea of science is best viewed in Lakatosian terms; and the essence of Lakatos's notion of science is the scientific research program. The question is, therefore, to what extent the integrated account developed here is capable of generating and sustaining such a program; and whether its potential in this respect appears to promise more than the actual historical performance of its constituent approaches, taken each on its own. In conformity with Lakatos's approach, both logical and historical evidence are here considered.

3. If it is a scientific theory, to what degree is it relevant to human experience? It has been proposed, in chapter I, that the fragmentation of contemporary human geography can only be met by a reconciliation of scientific

and humanist programs. It is therefore not sufficient to establish this integrated account of the geographic individual as scientific: it must, in addition, meet the broad humanist demand that theory deal effectively at a fundamental level of human experience.

These three areas of consideration appear to provide a reasonable assessment of this integrated theory of the geographic individual in its own terms. The further central question, of the degree to which the theory is capable of underpinning traditional theory at the collective level, is addressed in chapter V.

4.2 Approaches to the individual in human geography

4.2.1 Behaviour and decision-making

The analysis of human action has, more than any other single theme, been the backbone of twentieth-century human geography. On the one hand, that analysis has concerned itself with the action in the physical environment; on the other, with the action in space. Geographers, it seems, have always felt most at home with the observable, graspable world. Indeed, when human geographers showed disturbing signs of abandoning that safe and well-trodden shore for the uncharted, shadowy depths that surround it and give it form, the protest was immediate and sustained: even a geographer of such contemporary eminence as Peter Gould can recently express his reluctance to embark upon such an apparently perilous journey (Gould 1976). Yet, if the observable, measurable world of behaviour has been left, it has always been with the intention of reconstructing it: a well-known review of such studies, for instance, refers to

> the deliberate attempt of the behavioural approach to unpack and identify behavioural elements and to examine their specific impact on spatial locations and activities. (Golledge, Brown, and Williamson 1972,p.59)

Here the emphasis is on action as producing behaviours and locations; and that will be the theme of this section. The notion of man as an actor in the environment will be discussed more fully in chapter V.

For most geographers, the starting point of the analysis of action has been location. Location of human activity and its results, in space or on the earth's surface, has been, in fact, widely adopted as the criterion which

demarcates the domain of human geography. The locational perspective has centered on the geometric analysis of large-scale patterns, and has sought to establish a science of spatial relations. It has adopted a positivist, empirical approach to these spatial phenomena, seeking to define precisely primitive terms and concepts, to formulate theory, to subject theory to test in the real world, and to establish general laws. This was predominantly the work of the 1960's, as represented by Haggett (1965) and Berry (1967), and it culminated in the lucid exposition by Harvey (1969a) of its positivist underpinnings, and in the now famous avowal of faith, "By our theories you shall know us" (Harvey 1969a,p.486).

This is not to say that such approaches ignored the role of individual action. Clearly locational patterns of human geographic phenomena are the aggregate result of individual decisions, and a complete model of locational process would seem to demand models of individual decision-making. The most common approach has been to postulate an idealized individual, whose attributes can, in the empiricist tradition, be treated mathematically. The emphasis here is on reducing randomness by attempting to predict aggregate, rather than individual, behaviour:

> The idealization of rational economic man leads us to the normative location models such as those of Weber, von Thunen, Losch, and their academic descendents. Empirical evidence suggests that it is possible to conceptualize behaviour as a stochastic decision process and use probability distributions to discuss spatial behaviour . . . .Descriptive mathematical functions may then be used as the foundation for a stochastic location theory. . .(Harvey 1969b,p.35)

More sophisticated versions of this approach have stressed the interdependence of geographic patterns and human decisions: thus Cox (1972,pp.10-11) pointed out that, although patterns of movement are the result

of decisions, those decisions are themselves influenced by the locational context in which they are made.

Central to this approach is the notion of optimizing. This is a term originally taken into geography from economics in particular situations of economic location or movement. The classic statement is that of Simon (1957, p.241):

> Traditional economic theory postulates an "economic man", who in the course of being "economic" is also "rational". This man is assumed to have knowledge of the relevant aspects of his environment which, if not absolutely complete, is at least impressively clear and voluminous. He is assumed also to have a well-organized and stable system of preferences, and a skill in computation that enables him to calculate, for the alternative courses of action that are available to him, which of these will permit him to reach the highest attainable point on his preference scale.

This principle has been extended to the whole field of human geography, firstly, in the nearness problem, which

> takes the form of finding the spatial arrangement of interacting objects, often of different dimensions, and placing these objects as near to each other on the earth's surface as possible. (Bunge 1962,p.211);

and then as a field of study in which the underlying principle of order is seen to be the attempt

> to organize space efficiently, to locate activities and use land in the "best" way. (Morrill 1970,p.20)

From this approach have been developed such terms as profit maximization, perfect competition, marginal behaviour, and so on; and, in the eyes of some eminent commentators, has given rise to a rich and fruitful theory (Harvey 1969b,p.44).

It is clear, however, that such idealizations are rarely, if ever, found in the real world. For instance, individuals rarely seem to have all the

relevant information they need to make a "perfectly rational" decision; nor, often, the mathematical skills necessary to derive such a decision. Their preferences may be specific to one moment in time, and may include priorities derived from culturally conditioned attitudes, or simply from individual roles or habits (Golledge, Brown, and Williamson 1972,p.63). It is therefore recognized that sub-optimal behaviour is the norm, rather than the exception. Haggett (1965,p.181), for instance, after a lengthy discussion of models of locational structure, admits that "the spectre of sub-optimal behaviour dogs all discussion of the classic normative models discussed in the preceding pages". Gould (1976,p.84), however, cautions that to denigrate normative models of economic man on this account is to misunderstand their function, which is to reduce complexity, to establish means of assessing actual behaviour against ideal, and to identify and explain deviations from the predicted behaviour. Curry (1972,p.348) supports this view:

> It would be absurd to believe that economic man is really representative of any society but equally absurd to deny that considerable insights can be obtained by using him.

Such considerations have therefore given rise to the notion of 'satisficing', rather than optimizing, behavioural models, as developed in economic theory by Simon (1957). In these models, the courses of action that are selected will be related to a set of needs, among which maximum economic utility may not be the prime consideration (Haggett 1965,p.26). This may be thought of as a form of optimizing behaviour in which the criteria employed are non-economic; or, to put it more generally,

> it is recognized that all actors in a decision-making situation need not be homogeneous with regard to their utility function and so more attention is given to determining the types of utility functions by which different actors operate. (Golledge, Brown, and Williamson 1972,p.64)

However, Harvey (1969b,p.45) has pointed out that some behaviour may well not fall readily into any form of optimizing analysis, in which case other theoretical concepts will have to be sought: the notion of behaviour in social groupings as primarily rule-governed (Winch 1958) would seem to be an example of such a concept.

One way in which the notion of subjective utility has been explored in human geography is through the concepts of action space and activity space. All the places in which an individual potentially operates, and to which he has assigned specific utilities, are said to comprise his action space, and delimit the places of his potential operations (Wolpert 1972). Physical space is distinguished from life space, the first being actual, and the second cognitive: together they construct action space (Golledge 1969,p.106). Activity space, on the other hand, is defined by the actual movements an individual undertakes in the course of his everyday life, and is said to be closely related to perceived or actual territorial boundaries, and to the social roles an individual adopts:

> Both the temporal and the spatial aspects of his activity space (when and where he moves) are the product of his defining a set of activities in which he desires to participate. (Jakle, Brunn, and Roseman 1976,p.95)

Thus decision-making emerges as the essential intervening variable between cognition and action. To put it another way, if the phenomena of human geography--location, spatial structure and process, ecological relations, and so on--are seen to be the direct result of human action, and action is based on decision making,

> a knowledge of the factors affecting such decision, therefore, is an indispensable tool in interpreting the resulting location arrangements. (Cox 1972,p.374)

Jakle, Brunn, and Roseman (1976,p.298) analyze the process as an interlocking, circular relation between environmental structure and cognitive structuring of the environment. Spatial activity is seen to derive from geographical preferences, which are the expressions of decisions which arise from the interaction between perceptual and cognitive structures and the object environment:

> Desire messages based on the cognized environment enable the individual to identify a limited set of geographical preferences, or alternatives for spatial action. (Jakle, Brunn, and Roseman 1976,p.299)

Central aspects in the formation of geographical preferences are said to include the grounds of evaluation, the patterns of preferences across populations and between groups, and the relationship of decision making to behaviour settings.

A more sophisticated approach is that adopted by Golledge, Brown, and Williamson (1972,p.62), who point out that geographical decisions may have several components. The three basic decisions are said to be psychological (in deciding the direction of desiring), economic (in deciding the feasible degree of desiring), and geographic (in deciding to act). It is on the first two of these areas that emphasis is placed, through a model of the unmotivated individual, described by personal structural variables--such as age and education--and existence variables--such as location and orientation--who is motivated to a decision and an action by a conscious or unconscious drive, and is then able to assess the rewards of such action. In an earlier account along similar lines, Golledge (1969) emphasizes the role of habit. Once a response is established, behaviour will tend to follow that course because of the reduced demands on the individual of such decision-making:

> Routine behaviour . . . often follows a path of minimum effort, it serves to reduce uncertainty in the decision process, and reduces consideration of alternative courses of action. In other words it is the behaviour most used to cope with the contingencies of everyday living. (Golledge 1969,pp.103-104)

This notion of habitual decision making would seem to vitiate the conventional model of satisficing decision as expressed, for instance, by Rushton (1969),p.198:

> Spatial behaviour implies a search among alternatives . . . . the process of spatial choice . . . is one in which a person compares each alternative with every other one and selects that which he expects will give him the greatest satisfaction.

Here again it seems we have the model of idealized individuals, in possession of all necessary information about the external world, aware of their own preferences and priorities, and able to establish for themselves in a reasonable way the best of the available alternatives of action. Such concepts have been recently recapitulated by Parkes and Thrift (1980,pp.209-231). Experience suggests, however, that some, even most, action is not initiated in this ideal way; and there is reason to doubt, even taking Gould's (1976) view of the function of such models, that it clarifies more than it obscures of decision-making in the real world.

The difficulties associated with such models are well illustrated in studies of migration. In seminal studies of migration behaviour, Wolpert (1964, 1970, 1972) emphasized a number of central concepts. The individual is seen to be 'boundedly', or 'intendedly', rational; although limited to finite ability

> to perceive, calculate, and predict and to an otherwise imperfect knowledge of the environment, [he] still differentiates between alternative courses of action according to their relative utility or expected utility. (Wolpert 1972,p.372)

Aspiration is further seen to be regulated by experience:

> The process is self-adjusting because aspirations tend to adjust to the attainable. (Wolpert 1972,p.372)

Differentiation of possible courses of action in space gives rise to place utility; and the higher the evaluated place utility, the more likely is said to be the act of migration.

In establishing such utilities, however, the individual is seen to undertake a particular search procedure. In terms of the ideal satisficing model, the individual ought to have access to all information in the environment that is relevant to the making of the best decision. In fact, a sampling procedure is said to be adopted, the characteristics of which "are determined by the individual's needs, drives, and abilities" (Wolpert 1972, p.373), and can be the consequences of a particular stage in the cycle of life. Roseman (1971,p.592) has represented this decision-making process as one in which levels of satisfaction of aspirations between general areas, and between sites within a general area, are compared to each other and, when a threshold of dissatisfaction is reached, initiate a move.

To all such models of migration decision-making the role of information appears to be paramount. A full discussion of information spread and transmission goes well beyond the scope of this dissertation, and has been examined in many texts (Golledge, Brown, and Williamson 1972,pp.64-71; Gould 1976,p.91; Olsson and Gale 1972,p.354; Jakle, Brunn, and Roseman 1976,ch's.VI and VII; Cox 1972,ch.5; Cox and Colledge 1969,pp.2-3; Harvey 1969b,p.48). Here it is sufficient to note that migration behaviour does not seem to depend upon a finite body of information in the physical and social environment, which is transmitted to a greater or lesser degree, and upon which a more or less adequate decision is made. On the contrary, it is the individual, through cognitive structures, and social and physical location, who determines in large part the information acquired: the familiar phenomenon of chain migration is a

clear example of the operation of such factors. Thus the decision to migrate is as much, if not more, a function of the individual as a function of the milieu. These considerations suggest that studies of information flow and diffusion, such as those initiated by the pioneering work of Hagerstrand (1952; 1967), need to be supplemented by a comprehensive understanding of the way in which individuals select, are influenced by, or are resistant to relevant information.

For nearly two decades, the analysis of action in human geography has been dominated by the use of probability models. Two factors seem to have led to this preponderance. One is the obvious difficulty of obtaining deterministic accounts of human behaviour. The other is the emphasis geographers have traditionally placed on populations and groups, rather than on individuals. Where the aggregate effect of human decisions is sought, probability models seem to be particularly appropriate:

> Probability theory, especially when given a relative frequency interpretation, provides a set of extraordinarily effective models for dealing with aggregate effects of decisions that are rather repetitive in form over time and space. (Harvey 1969b, p.39)

It is a premise of this approach that the world in which the individual operates is fundamentally uncertain, and requires the sort of strategies which probability theory best reflects:

> This is of course the world which as individuals we know: a world which is neither wholly rational nor wholly chaotic, but a probabilistic amalgam of choice, calculation and chance. (Haggett 1965, p.27)

The phrase "of course" in this sentence is a revealing one, implying that the proposition is self-evident and indisputable; yet it is a premise that can be challenged.

This use of classical probability theory to predict action is typified by studies which utilize game theory, as developed by Von Neumann and Morgenstern (1944). Gould (1963,p.297) points out that

> The work of Man is all around us upon the face of our earth, and is the result of men perceiving a variety of alternatives, subsequently limiting the range of choices according to their idea of what is useful and good, and deciding upon certain strategies to gain those ends.

Anthropological studies have supported the value of this approach (Davenport 1961). In criticism, however, it has been pointed out that such use of classical probability theory may not provide the best model of decision-making. In particular, it does not allow the incorporation in the model of subjective beliefs which may both initiate and direct the course of action, and themselves be amended by consideration of the results of action in relation to the aspirations held. Such a schemata is said to be best modeled by Bayesian probability (Curry 1972,p.350; Harvey 1969a,pp.240-242). An alternative approach to modeling the apparent complexity of decision-making in the real world is that of Markov chain models (Olsson and Gale 1972). Pipkin (1978) and Gale (1972) have further pointed out that sources of inexactness other than undertainty are components of spatial choice. These include vagueness in perceiving the characteristics of the alternative sites or modes of movement; and ambiguity in the subjective values or preferences associated with sites. Fuzzy set logic is seen to provide an appropriate language for incorporating these factors into the model of decision-making.

Both Haggett (1965,p.26) and Olsson (1969,p.24) have related the adoption of probabilistic explanations in geography to the revolution in human thinking effected by quantum theory, in which statistical regularities in aggregated systems assume the status of laws (Hempel 1966,pp.59-69). Olsson notes that,

at first sight, such a conception of reality must be considered paradoxical, since the probabilities of events are said to be underlain by some notion of causality; but that the paradox can be seen to be the result of infinitely regressive explanation:

> . . . causal laws at one level of aggregation normally result from averages of statistical behaviour at a deeper level, which in turn can be explained by deeper causal behaviour, and so on indefinitely. (Olsson 1969, p.24)

The resolution of this paradox has been a source of much contention in the history of the development of quantum theory; and is clearly of equal importance in any reductive approach to the theory of behaviour in human geography.

4.2.2 Cognition and cognitive structures

Cognition in human geography. Despite the vigour and fertility of the approach to decision-making undertaken by the behavioural school, the feeling has persisted, as the foregoing section has indicated, that ultimately decision-making must be embedded in a more comprehensive understanding of human experience if it is to account at all adequately for observed activity. Many terms--including perception, learning, memory, intuition, emotion, imagination, images, representations, beliefs, attitudes, ideology, and values--have been associated with such an understanding. These are notions which, though ill-defined, and often employed in geographic literature in widely differing senses, can be subsumed under the single concept of "cognition", or "cognitive structure".

The importance of cognitive structure in the relationship between man and milieu has received constant emphasis in the work of twentieth century geographers. One of the first, Jean Brunhes, referred to

> . . . an instinct, a thought, a fear--psychological elements which vary from individual to individual, from group to group, and especially from epoch to epoch . . . the necessary intermediary between man and nature. (Brunhes 1920,pp.518-519)

Thirty years later, Carl Sauer asserted that

> . . . habitat is revalued or reinterpreted with every change in habit. Habit or culture involves attitudes or preferences, which have been invented or acquired. (Sauer 1941b,p.8)

John Kirtland Wright, in his lectures and writings, emphasized the centrality of subjective factors in geographic understanding. To these factors, imaginative processes of three types were held to be of fundamental importance: promotional imagining, which is related to personal bias or partiality; intuitive imagining, the subjective use of facts selected according to personal impression; and more importantly, aesthetic imagining, the selection and emphasis of those aspects of external reality that are subjectively felt to be distinctive or characteristic (Wright 1947,pp.74-75). The last is characterized primarily by artistic and poetic impulses, and in Wright's view has a specific purpose in geographic understanding:

> The functional purpose of aesthetic objectivity is to heighten the effect by increasing the clarity and vividness of the conceptions that we seek to transmit to reader or hearer. It enables us to share with him the impressions that place or circumstance have made upon us--to bring him down to earth from the lofty observation point of the objective and make him see and feel through our eyes and feelings. (Wright 1947,p.8)

Wright concludes that, in addition to the formal scientific studies of geography, there exists a peripheral zone, "that of the subjective geographical conceptions of the world about them which exists in the minds of countless

ordinary folk" (Wright 1947,p.81). The methodological implication of such a view, he goes on, is that conversation and imagination may do as much to elicit the true nature of a farmer's view of impending change in the weather as formal interview or questionnaires.

In a seminal article, Spoehr (1956) pointed out that the evaluation of the environment in terms of resources is not just an objective appraisal of physical facts but is largely dependent on the underlying cognitive structure:

> What is necessary is an examination, not merely of culturally conditioned attitudes towards natural resources, but of how various peoples have come to regard their relationship with their respective habitats (of which resources are but a part) and indeed with the entire physical universe in which they exist. (Spoehr 1956,p.97)

In more recent times, this notion has been subsumed under the term "environmental perception". The basic tenets on which this study is based were succinctly stated by Sonnenfeld, in his account of adaptation of northern Alaskan populations to their environment:

> Individuals and populations tend to differ in their responses to any environment. Some achieve more in environment; some achieve less; some adjust easily to environmental extremes, others adjust only with difficulty. Different responses may be a function of different abilities to respond to environment, or of different perceptions of environment. Understanding of the sources of variance in environmental perception is essential to an understanding in variation in man's environmental behaviour. (Sonnenfeld 1967,p.42)

The attempt to gain some insight into the environmental perception of these populations was based on formal questionnaires, supplemented by visual and verbal psychological tests.

A similar approach was adopted by Saarinen (1966) in his study of the drought hazard perception of farmers on the Great Plains of the U.S.A. Although he did not define precisely the view of perception he employed, it

appeared to include environmental stimuli, sensory processes, awareness, learning through problem-solving, past experience, motivation, personality, values, social norms, and cultural appraisal. It is, perhaps somewhat odd that such an apparently diffuse, non-rigorous concept should be approached through the relatively formal methodology of questionnaire and response to visual stimulus. In common with most of the writers quoted so far, Saarinen seems not to have been concerned about the imprecision of the term 'perception': evidently it, like 'culture', is one of those words which everyone understands without further explication. This confusion perhaps accounts for the unremarkable nature of the conclusions reached by Saarinen in his study: that 'perception' of drought hazard varies with the degree of aridity in the area, the amount of drought experienced, and certain 'personality' characteristics, such as willingness to be innovative, and belief in dominance over nature. In later work, Saarinen appears to have abandoned altogether the attempt to define the components of what he regarded as perception, adopting the "cognitive behaviouralism" of Sprout and Sprout (1965), and opposing the "objective or geographic environment" to the "subjective environment or psycho-milieu, which depends on what is perceived by the person" (Saarinen 1973, pp.29-30).

Lowenthal (1961; 1967; 1979; 1982) and Lowenthal and Prince (1976) have presented similar views in a more systematic fashion. In an influential article, Lowenthal (1961) takes Wright's approach as a starting point, and goes on to explore the notion of "world view":

> We inevitably see the universe from a human point of view and communicate in terms shaped by the exigencies of human life. (Lowenthal 1961, p.246)

Though there is a shared world view, there is for every individual a personal world view, complex, inaccessible to ordinary methods of investigation, and often transcendent in nature:

> These transcendent provinces of meaning, by contrast with the everyday world of work, are in varying degrees incommensurable, inconsistent, and unrepeatable. They have therefore habitually been dismissed or ignored by social science. Yet one must come to terms with these nebulous realms, for they are not segregated from ordinary existence, but infuse it continually. (Lowenthal and Prince 1976,p.120)

In defining more precisely the "nebulous realms", Lowenthal identifies unusual states of consciousness, and quotes Huxley's extended geographical metaphor, which refers to personal consciousness, personal subconscious, soul, archetypes, and visionary experience (Huxley 1959,pp.71-72). Elsewhere he makes the same point more succinctly, and less picturesquely:

> Until recently, geographers have been content to explore ... what they consider the real world. Yet in daily practice, we all subordinate reality to the world we perceive, experience, and act in. We respond to and affect the environment not directly, but through the medium of a personally apprehended milieu. This milieu differs for each of us according to his personal history; and for each of us it varies also with mood, with purpose, and with attentiveness. What we see, what we study, and the way we shape and build in the landscape is selected and structured for each of us by custom, culture, desire, and faith. (Lowenthal 1967,p.1)

The subjective apprehension of the world by an individual is thus seen to include uniquely personal aspects (past experience, memory, feeling) and shared aspects (language, culture); and some structures are transient (mood, purpose, attention), while others are stable (stereotypes, custom, faith). All these facets of subjective experience can be subsumed under four, mutually interacting processes:

> Perception itself is never unalloyed: sensing, thinking, feeling, and believing are simultaneous, interdependent processes. (Lowenthal 1961,p.251)

This theory appears to give rise to two methodologies of investigation. The first, following the view of Wright outlined above, looks to the works of artists who enter the 'nebulous realms' (Lowenthal and Prince 1976,pp.124-126).

The painter and the writer are seen to be more sensitive to and articulate about their own environmental awareness:

> We have only the sketchiest notion of feelings and preoccupations about landscape and milieu, even for those whom we know best. Largely inarticulate about such matters, most people express reactions only in unusual circumstances. But painting and literature overtly disclose a wealth of response; apprehensions of environment, tacit and acknowledged, suffuse the canvas and the page. The environments of art are not only more vivid and more memorable than most of those elicited by observation and questionnaires, they are also more meaningful. (Lowenthal and Prince 1976,p.125)

The second methodology, seen to be most appropriate to the investigation of the environmental awareness of a whole nation or culture, is based on intuition and familiarity:

> Such a method will forego statistical demonstration, interpretive emphasis, hard proof for empathy and analogy, and will seek to stimulate and entertain while suggesting fruitful ways of ordering ideas. (Lowenthal and Prince 1976,p.127)

In their studies of the English and American landscapes (Lowenthal and Prince 1964, 1965; Lowenthal 1968), empathy--imaginative identification--is complemented by literary and historical texts, superimposed on a background of fundamental geographical categories. The result, though interesting and elegant, nevertheless seems to probe only the initial levels of the complete structure of environmental perception that is foreshadowed in the theory:

> Essential perception of the world, in short, embraces every way of looking at it: conscious and unconscious, blurred and distinct, objective and subjective, inadvertent and deliberate, literal and schematic. (Lowenthal 1961,p.251)

Tuan (1974; 1975; 1976a; 1976b; 1978; 1979b; 1979c) has presented a number of wide-ranging accounts of the role of perception and cognitive structure in human geography. Though rightly respected for the breadth of his studies and for the stylistic elegance of his writing, his views are nevertheless not easy

to specify, and appear deliberately to avoid the kind of conceptual rigour that we are here seeking.

Perception is, for Tuan, focused on the operation of the senses. In this, he emphasizes the common features shared by members of the human species:

> . . . however diverse our perceptions of the environment, as members of the same species we are constrained to see things in a certain way. All human beings share common perceptions, a common world, by virtue of possessing similar organs. (Tuan 1974,p.5)

He observes, however, that perception is never pure perception:

> Perception is both the response of the senses to external stimuli, and purposeful activity in which certain phenomena are clearly registered while others recede in the shade or are blocked out. (Tuan 1974,p.4)

Clearly it is through perception that the individual develops relationships with the environment; and all the senses are involved. The visual sense, however, is seen to be unique in its importance and in its abstractness:

> Of the traditional five senses man is consciously dependent on sight to make his way in the world more than on the other senses. (Tuan 1974,p.6)

This consideration leads Tuan to examine in a later article (Tuan 1979b) the consequences of modern visual media for geographic understanding through visual perception.

The "purposeful activity" (Tuan 1974,p.4) which alters perception corresponds to the notion of cognitive structure. In his early work Tuan defined it largely in terms of attitudes (derived from culture) and a world view ("conceptualized experience", embodying both attitudes and belief system). To these were added the structuralist notions (Levi-Strauss 1962; 1963; 1966, 1967) of binary opposition and resolution:

> The human mind appears to be disposed to organise phenomena not only into segments but to arrange them in opposite pairs. . . . Opposites are often mediated by a third term. (Tuan 1974,p.16)

In later work, however, this somewhat eclectic and imprecise approach is given a more orderly form. In a discussion of the theory of mental maps (Tuan 1975), Tuan distinguishes between schemata, which are equated with somatic intelligence built up by learning and experience, and which allow appropriate responses to changing patterns in the environment; and images, which are "something we see when the environmental stimuli do not appear to justify it" (Tuan 1975,p.208). The first is described as pre-attentive; that is, it is not the product of focal attention, but of preconscious experience and habit. The second, be it the result of imagination or of memory, is seen always to involve a high degree of focal attention.

To these notions Tuan, in emphasizing the value of literary sources to the human geographer, adds the term feeling:

> Experience may be defined as the sum of means through which we know reality and construct a world. Experience includes sensation, feeling, perception, and conception or cognition. Feeling is a way of knowing. (Tuan 1976a,p.260)

The significance of this view of individual functioning in human geography is given broader expression in a second article of the same year, where the focus of the humanistic geographer is said to be

> . . . the nature and range of human experience and thought, the quality and intensity of an emotion, the ambivalence and ambiguity of values and attitudes, the nature and power of the symbol, and the character of human events, intentions and aspirations. (Tuan 1976b,p.274)

One is reminded of Lowenthal's (1961,p.251) all-embracing demand for an understanding of "every way of looking at" the world.

Tuan describes himself as a "literary humanist", a position which he justifies on the grounds that one can

> . . . regard verbalised attitudes, particularly those canonized in literature, as sufficient evidence of what people actually do. (Tuan 1976b,p.273)

Elsewhere, more evocatively, he asserts:

> Art articulates experience. Inchoate feelings are transposed into significant forms that can be seen . . . . literary works, like other modalities of art, do not explain; what they do is enable us to recognize, with the immediacy of a revaluation, the multivalent character of experience. (Tuan 1976a, p.273)

In addition, he asserts, one looks to the facts and techniques of physical geography, animal ethology, and social science for an understanding of environmental experience; and to epistemological theory in philosophy and psychology, from which is to be derived the essential unified conceptual standpoint. And yet, if one surveys the ambiguity and vagueness of the fundamental concepts outlined above, concepts which inform and structure nearly all of Tuan's work, it is just such a unified conceptual viewpoint that would seem to be absent. Ironically, he himself sees the consequences of this confusion for the humanist geographer:

> Without a fundamental viewpoint his work tends to become disjoint esoterica. (Tuan 1976b, p.274)

Both Lowenthal and Tuan, therefore, though they are the most persuasive and respected advocates of the importance of cognitive structure in human geography, appear, on closer analysis, simply to be affirming the primacy of the subjective world in the man-milieu relationship. The affirmation is a valuable one, though not new; and yet the components of that world, their functions, interactions, and limits, do not appear to have been dealt with by either in a fully systematic of fully convincing way. And although both espouse empathy and imagination as the best methodology for gaining insight into the subjective world, it seems that the result is intended to be systematic and scientific:

> One of the humanist geographer's roles is that of an intellectual middle man: he takes these nuggets of experience captured in art and decomposes them into

> simpler themes that can be systematically ordered. Once experience is simplified and given an explicit structure, its components may yield to scientific explanation. (Tuan 1976b,p.274)

A similar approach is that of Sack (1980), who sees an insistence on the priority of subjective experience of the environment as not inconsistent with scientific analysis:

> Feelings about shapes, patterns and forms affect our responses to the landscape and our designs upon it. If we are to understand these relationships scientifically, we must unpack the complex links between feelings and form, and determine clear connections among them within the mould of hypothesis testing. . . . If successful, this course would enrich the social science relationships we have traced and extend the domain of science and objectivity into the realm of subjectivity. (Sack 1980,p.116)

That the result does not match the expectation would seem to indicate both the complexity of the subject matter--the subjective world--and the limitations of the methodology used to approach it. Yet the insistence on understanding the subjective world is, in the end, persuasive, and gives rise to an implicit demand for more precise theories of that world, and more effective methods of investigating it.

In recent times, systematic models of cognitive structure have been attempted by human geographers. Jakle, Brunn, and Roseman (1976,p.65) define cognition in terms that are close to Tuan's:

> A landscape is nothing but a "to whom it may concern" message. Implicit in its forms are messages that cue behaviour. Yet the study of environmental cognition concerns not only the nature of these cues, but necessitates, as well, concern with the sensing organs that people use to receive them. Social geographers must also be concerned with memory, as stored experiences with which signals from the environment are interpreted, and with personality, as the motivational force which predisposes people towards particular kinds of environmental interpretations.

Thus a survey of human sensory and kinetic functioning, including an interesting account of discrimination and acuity in a changing environment, is

followed by a discussion of the development of spatial concepts in the child, as outlined in the work of Piaget and Inhelder (1966; 1967). The growth from concrete sensori-motor space to symbolic representative space is seen here to be central, as is the stability of the structures thus developed:

> Once an individual has discovered a successful mental response to a particular kind of spatial problem, he tends to develop it as a distinctive cognitive pattern. These patterns combine to give him a comprehensive picture of the world as a place of spatial events. (Jakle, Brunn, and Roseman 1976,p.74)

The authors go on to emphasize the role of spatial expectations in developing spatial behaviour. They point out that first experiences of place give rise to a framework, into which later information is assimilated. This procedure of searching space in order to validate expectations is seen to involve scanning and organized probing. The success or failure of the operation, which depends on a number of factors, including the motivation brought to the search and the complexity of the environment, is seen to feed back to the individual's sense of identity and self-esteem (Jakle, Brunn, and Roseman 1976,p.78). This approach is consistent with the account given by Paiget (1971,pp.4-6,10-13) of the two mechanisms of formation of cognitive structure, "assimilation" and "autoregulation" (see discussion below).

Finally, the authors affirm the value of a place as a "behaviour setting":

> . . . --a space, time, thing, and activity "milieu" with structured meanings for its participants. (Jakle, Brunn, and Roseman 1976,p.86)

Particular environmental settings are thus seen as likely to cue particular behavioural patterns, and may carry behaviour over into different settings. Such a view is again consistent with Piaget's principles of "action schemata" and "stimulus-response pattern" (Piaget 1971,pp.6-10), as outlined later in this chapter.

A similar, though less convincing, approach is taken by Fielding (1974). He distinguishes between perception--"the ability of the mind to apprehend objects through the senses"--and cognition--"the thinking about the perceptions in terms of an individual's previous experience" (Fielding 1974,p.282)--in much the same way as Tuan (1974) and Jakle, Brunn and Roseman (1976). Perceptual information is seen to be "filtered" by an individual's knowledge (itself dependent on memory), ability, and value system to create an image, which then becomes the basis of decision-making. The values are related to "consistency of needs and desires within groups and societies" (Fielding 1974,p.290), a definition which seems to raise more questions than answers. Through this process, the individual is said to be able to organize his external world into viable patterns, which are then applied to particular environments, such as the city. Fielding emphasizes that the image thus created may be distorted (though in relation to what it is not made clear), a view supported by Haggett (1972,pp.530-531):

> The evidence . . . suggests that we retain primitive, twisted, and biased pictures of our local "world" that may be far removed from the "real" world shown by the satellite sensor.

Naturally, the humanist geographer convinced of the primacy of the subjective world would want to ask the proponents of such a view what the criteria of "real" are, and on what principles such criteria are based.

Meinig (1979) is equally reluctant to engage in a systematic approach to these matters and attempts instead to substitute a common-sense view of them:

> Thus we confront the central problem: any landscape is composed not only of what lies before our eyes but what lies within our heads . . . . Recognition of that fact brings us to the brink of some formidably complex matters. But it is not necessary to plunge into the technical thickets of optics, psychology, epistemology, or culture to converse intelligently about the topic . . . . It deserves the broad attention that only ordinary language allows . . . . We are

> concerned not with the elements but with the essence, with the organizing ideas we use to make sense out of what we see. (Meinig 1979,p.34)

We do not deny the value of dealing descriptively with landscape cognition and perception, particularly for the purpose of developing hypotheses that can be more scrupulously examined; but it is far from clear that the 'essence' of the problem alluded to in this passage can only be approached by avoiding the kind of systematic, rigorous enquiry that the disciplines here ruled out represent. It is just this kind of intellectual trepidation that, as was proposed in chapter I, has limited the ability of human geography to deal effectively with its central theoretical dilemmas.

A more systematic attempt to define the role of cognition in human geographic theory has been made by both geographers and psychologists working at the interface of the two disciplines. Representative of this approach are works by Moore and Golledge (1976a), Golledge and Rushton (1976), Ittelson (1973a), Wapner, Cohen, and Kaplan (1976), and Cox and Golledge (1981). Common to these studies is a view of perception as a subset of cognition. Perception is seen to be closely linked to incoming stimuli and to immediately produced behavioural responses, while cognition is said to refer to

> . . . the various means of awareness or knowing that intervene between external energy impingements in the present and the past and the entire gamut of human behavioural responses, present and future. Cognition thus subsumes the more specific concepts and substages of sensation, perception, imagery, retention and recall, reasoning and problem solving, and judgment and evaluation. As such, it includes the various processes by which visual, linguistic, semantic, and behavioural information is selected, encoded, reduced and elaborated, stored, retrieved, decoded and used. (Moore and Golledge 1976b,p.6)

Such omnibus definitions may be thought to be more confusing than clarifying: the one above simply regards cognition as the "black box" between stimulus and response, a not very helpful view.

Other views, however, have adopted the Piagetian notion of the partial dependence of perceptual processes on existing cognitive structures:

> . . . sensitivity to stimuli . . . is a function of such assimilation schemata as are available to the subject. (Piaget 1971, p.22)

This approach has led to an emphasis on a cluster of terms--representation, construct, image--which are either directly observable and external (symbolic); or not directly observable, but related to internal knowledge and thought (Moore and Golledge 1976b, p.8). The latter notion is often identified with the idea of a personal construct (Downs 1976; Honikman 1976)--or in Piaget's terminology, schemata (Piaget 1971, pp.4-6)--and is seen to give rise to external categories, such as the map, or literary description. It is further widely agreed that cognitive structures interact continuously with the environment; that they differ between individuals and groups; and that they exist in some "configuration of psychological space" (Moore and Golledge 1976b, p.10). Golledge and Zannaras (1973) have emphasized the importance of stimulus-response and incremental learning processes in the construction of cognitive structures, on which that decison-making which gives rise to spatial behaviour is said to be based. More complex models involving multi-dimensional scaling and measurement have also been employed (Harman and Betak 1976; Sonnenfeld 1976; Golledge, Rivizzigni, and Spector 1976).

In examining, through empirical studies, the specific cognitive structures that underlie environmental experience, Ittelson, Franck, and O'Hanlon (1976, pp.199-206) have identified four characteristics of the process which gives rise to such structures: orientation, to objective aspects of physical places, and to people in places of emotional significance; development of a taxonomy for the environment; the identification of predictable sequences and

causal connections of events; and feedback through action in the environment. These structures are seen to give rise to, and to be influenced by, a number of primary modes of experiencing the environment.

First, environment is experienced as external to the self, the objectification of the world, as outlined in existentialist theory (discussed below under sections 4.2.3 and 4.2.4).

Second, the environment is experienced as self, the relationship between self-identity and environmental setting. The authors refer to a particular form of this experience in which

> . . . a total merging of self and environment is accomplished, in which the strict boundary vanishes, and environment becomes self, and self, environment. (Ittelson, Franck and O'Hanlon 1976,p.202)

Although here such an experience is attributed to the "mystic", and inaccessible to the scientific mode of explanation, it will be suggested, in the later discussion of geographic holism, that such a conclusion is open to question.

Third, the environment is experienced as social system, in terms of relationships with other people.

Fourth, the environment is experienced as emotional territory, in terms of feelings and associations.

Fifth, the environment is experienced as a setting for action, carried out in pursuit of specific goals, and giving rise to specific varieties of experience.

There have, however, been criticisms of such psychological approaches as these. Bunting and Guelke (1979), for instance, in advocating a return to the study of overt behaviour as the primary concern of human geographers, question

the validity of two assumptions they see to be fundamental to these approaches. The first is

> . . . that identifiable environmental images exist that can be measured accurately. (Bunting and Guelke 1979,p.453)

The authors challenge the validity of this assumption on the grounds that such images are complex, holistic, and essentially non-measurable:

> Although we can accept that researchers perform competently within the limitations of their basic methodological frameworks, there are strong grounds for questioning whether it is possible to extract an individual's actual thoughts from revealed preferences and images as derived in current research. (Bunting and Guelke 1979,p.453)

Thus it is not questioned that environmental images exist but that they can be measured accurately; and certainly, with respect to some of the cruder studies, the validity of this criticism must be admitted. There have, however, been other approaches to cognitive structures; it would seem to be unreasonable to dismiss, for instance, the half-century of empirical studies carried out by Piaget on the genesis of cognitive structures and their relationship, through organic structures, to the physical environment, as "research carried out . . . in very narrowly defined and often controlled or clinical settings" (Bunting and Guelke 1979,p.453).

The second assumption challenged is that

> A strong relationship exists between cognitive or mental images and actual behaviour. (Bunting and Guelke 1979,p.455)

This is, on the face of it, a criticism which seems to go straight to the heart of the research and theory of cognitive structures and their relevance to action in the environment. It emerges, however, that the authors are not questioning the strength of the relationship, but the absence of evidence about

the way in which behaviour and cognition are linked, and the consequent imbalance, from a traditional geographic viewpoint, of emphasis on cognitive rather than behavioural patterns. Again, however, the fact that a phenomenon is complex is not a valid reason for not studying it; nor is the fact that in the past geographers have had different priorities, since every vital discipline naturally finds such changes in the course of its development. It would well be argued that the findings of traditional geography (as defined in chapter I) led to the realization that, in human functioning, behaviour and structures of knowledge are inseparable; and that, therefore, the attempt to gain some understanding of the latter is a logical, even imperative, task. The desirability of not losing sight of action in the search for this understanding is, however, a point well taken; though it needs to be emphasized, as indicated earlier in this section, that twentieth century geography has never been exclusively behavioural, but has supported a strong tradition which recognizes the importance of cognitive structures in human action in the environment.

Burnett (1976) undertakes a critique of cognitive behaviouralism on philosophical grounds. By isolating the belief sets of both traditional and behavioural geographers, she demonstrates in a convincing manner the greater plausibility of the latter:

> The fact remains that behavioural geographers ascribe to individuals such mental events, states, and processes as human beings normally ascribe to each other in ordinary discourse . . . it remains possible that the behavioural geographers' constructs will yield better descriptions and predictions of observable spatial behaviours and processes than classical theories . . . . (Burnett 1976,p.41)

However, she emphasizes that such explanatory models will remain unsatisfactory until philosophical controversies about the nature of the mind, of mental events, states and processes are resolved. Any model of the mind, she

concludes, that is to be successful in predicting behaviour in the environment must be specifically based on an internally coherent philosophy of mind. The "black box" approach of so many human geographers is seen to be wholly unsatisfactory in fulfilling this requirement.

One well-established field of human geography which is based upon assumptions about cognitive structures is that of mental or cognitive mapping. This is one of a number of projective techniques said to be employed by geographers because of "their utility in tapping the subjective world . . ." (Saarinen 1973,p.51). Gould and White (1974,pp.17-18), for instance, refer to "images", which influence behaviour, and which can, they assert, be derived from the "space preferences" people express when asked. The images are seen to be largely shared by particular groups, to change with the passage of time, and to depend centrally on the operation of "perceptual filters":

> Human behaviour is affected only by that portion of the environment that is actually perceived. We cannot absorb and retain the virtually infinite amount of information that impinges upon us daily. Rather, we devise perceptual filters that screen out most information in a highly selective fashion. Our memory, far from holding every sensory impression from our environment, selects and retains only a small portion. Our views of the world, and about people and places in it, are formed from a highly filtered set of impressions, and our images are strongly affected by the information we receive through our filters. (Gould and White 1974,p.48)

This is a view close to that of Fielding (1974), and may be held to typify the more simplistic approach of a number of human geographers. The use of the term "filter", and its implied optical analogy, is not an explanation, in any sense, of the cognitive process, and may, indeed, serve only to obscure understanding. The dangers of such superficial modelling have been well analyzed by Harvey (1969a,ch.10). Some of the obvious questions that are neither asked nor

answered are: What is a filter? How is it constructed? How does it operate? Is it innate or acquired? Is it subject to change?; and so on. It would seem to be necessary to approach these questions through hard psychophysiological data (Hochberg 1964; Young 1978; O'Keefe and Nadel 1978; Bateson 1979) rather than through casual speculation.

Tuan (1975), in his discussion of the nature of images and their relationship to mental maps, hardly improves on Gould and White's attempt. Having assumed, with rather breathtaking insouciance, an equation between "construct", "image", and "percept", he goes on to assert:

> A percept is not only the registering of current environmental stimuli but also an imaginative effort produced under the needs of the moment . . . . An image is doubly a construct: it originates as a percept, and then suffers further transformation under the pressure of the occasion that prompts its recall. (Tuan 1975,p.209)

Again such a statement fairly bristles with unanswered questions about the exact nature of the forms and processes, and seems to hold little promise either of systematic understanding or of the possibility of explanation and prediction. The specification which follows of ways in which mental maps can be used--for communication, for rehearsal, as mnemonic devices, to structure and store knowledge, and as imaginary worlds--is a remarkable example of the sort of tautological trivia that such careless thinking can lead to. Taken together, they simply recapitulate intuitive ideas of the terms "mental" and "map", without forwarding the analysis in any way. For that, again, psychophysiological evidence is required.

A more coherent account of cognitive mapping is that of Downs and Stea (1977) and Downs (1981). They emphasize, in addition to the selectivity of the organism to environmental inputs, the necessity of recognizing the synthesizing

information from past experiences in order to apply such information to future circumstances. Cognitive organization is seen in terms of "the effort after or the search for meaning" (Downs and Stea 1977,p.83). The result of this process is seen to be the formation of cognitive representations, which, drawing on the work of the psychologist J.S. Bruner, are equated with categories of identity or equivalence. The authors go on to tackle the very question avoided in the two preceding accounts: ". . . What form do categories take? What do they look like?" (Downs and Stea 1977,p.85). Having candidly acknowledged the difficulty of providing the answers to these questions by inference from verbal or spatial behaviour or in terms of physiological brain processes, they centre the analysis on two modes of expression of categories, visual images and verbal descriptions, thus leading to a discussion of frames of reference, analogue models, stereotypes, and symbols. Their approach has the merit of being systematic, empirically based, and non-trivial in its applications, despite the unanswered questions identified by them, and to which one could add others: for instance, if categories are formed by logico-mathematical processes (to employ Piaget's (1971) term), how are responses on the level of emotion and feeling internalized and applied?

It should be emphasized that running through much of the work of human geographers on cognition is a recognition of the action of collective life in developing and maintaining its ~~morphlogy~~ morphology. Cognitive structures are seen to be not simply individual, but shared. The culturally based concerns of Sauer (1941b) and Spoehr (1956) have already been noted. This line of thought underlies much of the work carried out within the discipline of cultural geography, in which interaction in societies brings about a collective structuring in conceptual categories. Culture thus becomes

> . . . a vocabulary of perception. It consists of a large but finite set of agreed categories which identify, interpret, and relate the phenomena of experience . . . (Wagner 1960,p.34)

On this cognitive foundation are seen to arise the concrete phenomena with which cultural geographers deal in the field:

> The ascription of meanings, inherent in culture, guides action (whether seen as symbolic or utilitarian) and results thereby in such concrete expressions as systems of belief, social institutions, and material possessions. (Wagner and Mikesell 1962,p.3)

In a similar way, Lowenthal (1961,p.253) emphasizes the "consensual" elements in the individual's world view, elements which become particularly significant when related to cultures which order them:

> . . . it is not merely observed phenomena that vary with culture. but whole categories of experience. A simple percept here may be a complex abstraction there. Groupings of supreme importance in one culture may have no relevance in another.

Tuan (1974,p.59), in attempting to build on his analysis of cognitive structure, refers to the necessity of understanding

> group attitudes and preferences . . . a group's cultural history and experience in the context of its physical setting.

Both Jakle, Brunn, and Roseman (1976,ch.11) and Fielding (1974,p.292), as already noted, emphasize the importance of socially-structured values in the development of both identity and cognitive structure.

Studies seeking to clarify the nature of collective elements of cognitive structures have tended to adopt either sociological or anthropological conceptual bases. The first is derived from such classical concepts as Durkheim's "collective representations" (Durkheim 1974, pp.1-35; Aron 1967, pp.65-66), and Weber's identification of links between ways of thinking and attitudes to action in the world (Aron 1967,pp.233-237); which concepts have given rise to the modern fields of role-theory, the sociology of knowledge, and

reference-group theory (Berger 1963,ch.5), on the philosophical underpinning of notions of rules, meaning and value (Winch 1958; Lee 1949). In human geographic theory, these notions have emerged in an expanded definition of "behaviour setting":

> . . . social reference systems within which people conduct behaviour mold in considerable measure their organized perspectives on place. (Buttimer 1976)

Symbolic interaction. life-style, social organization, and value orientations are all seen to be elements contributing to the formation of such perspectives.

The second approach is cross-cultural, and was pioneered by Sonnenfeld (1967). It is based on concepts which can be found in almost any anthropological work:

> No man ever looks at the world with pristine eyes. He sees it edited by a definite set of customs and instructions and ways of thinking. Even in his philosophical probings, he cannot go behind these stereotypes; his very concepts of true and false will still have reference to his particular traditional customs. (Benedict 1935,p.2)

> . . . people of other cultures than our own not only act differently, but they have a different basis for their behaviour. They act upon different premises; they perceive reality differently, and codify it differently. (Lee 1949,p.401)

> We know how difficult it is for us to see those facts which would be upsetting to our deep-set beliefs. Covert culture controls perception. because it sets attitudes and beliefs. (Hoebel 1971,pp.216-217)

> The habits of any culture fit the people who learn to use them like well-worn gloves. This fit goes very deep, for their ideas of right and wrong. their selection of human desires and passions, are part and parcel of their whole version of culture. (Benedict 1971,p.228)

Thus, for instance, Bohannan (1963,ch.2) identifies habituation and education as the main formative processes of the cultural personality of the individual, operating through the medium of the role. This conforms to the sociological

view outlined above. Rappaport (1976) terms the approach "cognitive anthropology", which centres on the notion of "meaning":

> . . . the anthropological view takes the position that cognitive processes are concerned with making the world meaningful and that there are different ways in which meaning can be given to the world. (Rappaport 1976,pp.220-221)

Meaning is seen to be assigned to the world through the development of categories, in much the same way as suggested by Downs and Stea (1977):

> . . . cognition is a taxonomic process: the world is made meaningful by classifying, naming, attaching importance, and generally ordering it by means of a conceptual system. The world is humanized and made habitable, as it were, by having a meaningful structure imposed upon it. (Rappaport 1976,p.222)

Summary. In attempting to summarize the view of cognition and cognitive structures taken in human geography--as represented in the selected views outlined above--one is struck not just by the diversity of views, but by their lack of precision. Part of the reason for this state of affairs may lie in an application of Piaget's blunt remark, concerning the vitalist:

> . . . he is persuaded that he knows about mental life through the direct experience of, and introspection about his own body . . . . It does not occur to him to make any epistemological criticism of these . . . notions, since they appear axiomatic to him . . . everyone fancies himself a psychologist. (Piaget 1971,pp.43-44)

Broadly speaking, however, three "schools" or "traditions" of thought relating to the morphology of the subjective world in human geography may be identified. The first is the traditional school, developed within the notion of possibilism, as represented by geographers such as Bruhnes, Sauer, and Wright. The second is the modern humanist school, as represented by geographers such as Lowenthal and Tuan; and the third is the modern school of cognitive behaviouralism, as represented by geographers such as Golledge, and

environmental psychologists such as Moore and Ittelson. To these must be added work concerned with cognitive mapping, as represented by Gould and White and Downs and Stea, and with sociological and cultural aspects of cognition, as represented by Buttimer and Rappaport. Taking these groups together, it may be that the following statements can be tentatively identified as jointly developed principles:

1. "Perception" can be distinguished from "cognition" as relating primarily to the processing of stimuli by the sensory apparatus. Such processing will, however, be selective, in consequence of the inherent limitations of sense organs, and of the role played by attention. Attention is in turn controlled in part by cognitive structures. Perception is thus best seen as a subset of cognition, with a feedback function continually modifying the two structures.

2. That which constructs "experience" (or "knowledge") from perception can be defined as the process of "cognition". Cognition seems to be carried out by certain structures of organization, which provide a reference system for evaluating incoming environmental stimuli. Cognitive structures are not directly observable, since they relate to "transcendent provinces of meaning" (Lowenthal and Prince 1976,p.120), represented by such terms as images, attitudes, values, and feelings. Cognitive structures relating the individual to the environment appear to exist in some kind of 'psychological space', which may be expressed in mental maps. Despite their abstractness and difficulty of definition, there appears to be little doubt about the significance of cognitive structures in individual life, a significance which is of unlimited scope of application (as represented by the term "world view"), and is intimately related to actual behaviour.

3. Cognitive structures can be both personal and collective, both stable and mutable. Within the individual, cognitive structures appear to function uniformly and to give rise to predictable behaviour for substantial periods of time, particularly once adulthood is reached; at the same time, however, they do develop and change both with passing moods and feelings and--more systematically--throughout childhood and adolescence, and to some extent even in later life. Likewise, in particular social groupings individual members develop common cognitive structures which remain stable and direct particular behaviours in the group for some time, but which are subject to development, fluctuation. and even relatively sudden change. At any one time, however, different groups are likely to be identified with different cognitive structures; hence the notions of 'stereotypes' and 'value systems'.

4. Cognitive structures are formed by interaction with the environment. Part of such interaction can be described as sequential learning. which may involve discovering particular orientations in order to operate in the environment; developing taxonomies or classificatory categories that order the environment; and identifying causal links to predict events in the environment. Action in the environment, based on existing cognitive structures at any given time, will feed back to, and modify, the structures. The environment is further seen to include not just the physical but the social environment. By adopting certain socially given roles, by identifying with particular individuals and social groups. and by participating in the processes of habituation and education through which a social group inculcates its rules and priorities, the cognitive structures of an individual are modified. This is the process of

socialization. It is recognized, however, that the individual is not just a passive receptor of social inputs, but a purposeful actor in the social milieu, and thereby in part responsible for the development of his own cognitive structures.

5. Cognitive structures will thus give rise to widely differing modes of experiencing the environment. These include both the separation of the self from the environment and identification with it, the first being a process of increasing objective value, the second of increasing subjective value. The environment may also be experienced primarily in affective or emotional terms, in social terms, or in a practical sense as displaying a setting for action. From one point of view, this variability may seem to confirm the proposition that cognitive structures 'distort' or 'bias' perception; though it is at the same time evident that 'reality' is a term which has meaning in all these subjective modes of experience. The role of attention in selecting environmental inputs, and in creating, through pre-attentional or focal operation, structures of differing type and power (schemata, image), is a further factor which contributes to such variability.

6. Methodologies employed in attempting to delimit cognitive structures may be formal or empathetic. The first, in which the techniques of psychological testing--such as visual stimuli--or of structured interview are preferred, is based on the proposition that the morphology of cognitive structures can be inferred from the behaviour to which they systematically give rise. The second, which includes both analysis of artistic or literary works, and empathy or intuition through extended personal experience, regards the transformation of cognitive structure into action as incomplete at best, and positively

misleading at worst; and seeks therefore to gain understanding on the level of the structures themselves--that is, through feeling, emotion, and intuitive insight.

Piaget on cognition. It is more than a little surprising that so few human geographers, particulary those working in epistemological fields, seem to be aware of the breadth and significance of the work of Jean Piaget. Some have alluded in passing to a particular facet of it, or have used terminology derived from it (Tuan 1975); others have used quite extensively findings in areas of clear geographical interest, such as conceptions of space (Jakle, Brunn, and Roseman 1976), but have ignored the wider implications of the research and theory. Yet Piaget's work contains arguably the most complete empirical investigation of the genesis of cognitive structure, together with the most systematic and convincing theoretical account of it, available at the time of writing. Certainly there have been trenchant criticisms of both methodology and theory from other psychologists working in the same field; but even such critics have admitted that "his theory dominates the landscape" (Siegal and Brainerd 1978,p.xi). It would therefore seem to be sensible--even essential--for the geographic epistemologist to become familiar with this work.

If we take the conclusions outlined above to be a reasonable integration of epistemological approaches in human geography, it appears that substantial support for conclusions such as these is available from Piaget's empirical studies and theoretical interpretations. Indeed, it is the sheer volume of these empirical studies, relative to the paucity of empirical work within human geography, that makes the consideration of Piaget's work valuable. Clearly it would not be appropriate here to attempt to summarize forty-five years of

experimental work and vast range of theoretical writings: the following remarks will therefore be directed to each specific conclusion above, with further comments on key areas identified by Piaget, but not appearing in those conclusions. This account of Piaget is based on both general accounts, such as Piaget and Inhelder (1966), and Piaget (1970; 1971), and specific accounts, such as Piaget (1965a,b) and Piaget and Inhelder (1967). Only quotations are therefore referenced in detail.

1. "Perception" is, in Piaget's view, a subset of the cognitive function, and represents a transitional stage from innate to fully cognitive structures. Initially it is embedded within the co-ordination of action established by sensori-motor structures in their figurative, as opposed to operative, aspect. It then develops progressively with age, until it comes under the orientation of logico-mathematical intelligence. Perception, therefore, is never simply the registration of pure sensory input: at all stages, it is oriented and controlled, first by the global action of sensori-motor schemes, later by full operative intelligence. The key element of this orientation is its regulatory character, through assimilation and organization. This it shares with all other cognitive functions.

2. Knowledge therefore arises from the exercise of the cognitive function. The latter ranges from innate structures to operative intelligence, and includes all the intermediate stages. Three forms of knowledge are said to result: hereditary knowledge, related to innate structures (as, for instance, the perception of colours or dimensions in space); acquired knowledge, derived from the experience of physical objects in the external environment, and abstraction

from it; and logico-mathematical knowledge, arising from the general co-ordination of actions, which the subject undertakes in actively establishing relationships with the objects of his environment. These forms of knowledge are collectively termed cognitive, and can be distinguished from value co-ordinations, which reflect social and cultural life, and which are therefore embedded in the relationships which the subject establishes with adults and peers through affective and socializing activity. Cognitive functioning gives rise to cognitive organization and structure, as reflected in such characteristics as conservation (continuity), cyclic closure, inclusions (classifications), and seriations (ordering). There is an intimate unity of functioning between cognitive, affective, social, and moral aspects of the individual: "each stage of development is characterized by an overall structure in terms of which the main behaviour patterns can be explained" (Piaget and Inhelder 1966,p.153). Such structures can therefore be directly inferred from observed behaviour.

3. The notion of development lies at the heart of Piagetian theory. Cognitive structures are seen not only to change, but to develop:

> Our problem, from the point of view of psychology and from the point of view of genetic epistemology, is to explain how the transition is made from a lower level of knowledge to a level that is judged to be higher. The nature of these transitions is a factual question. The transitions are historical or psychological or sometimes even biological. . . (Piaget 1970,p.13)

Further, there is an identifiable sequence of transformation: the order of succession of cognitive structures is invariant from individual to individual, though the speed at which they appear may vary considerably.

Three main stages of development have been identified by Piaget in the construction of full adult cognitive structures. During the first--the

sensori-motor level, from infancy to about twelve months--there is a progression through six sub-stages from simple biological reflexes, to habits, to instrumental acts. Objects assume some permanence, the framework of space and time is laid down, and a degree of causality is recognized in the environment. Decentering and differentiation are typical of both intelligence and feeling. Most importantly, the means by which cognitive structures are constructured begin to emerge right from the beginning. In Piaget's view, knowledge is essentially active: the activity of subject in directing attention, selecting, manipulating, groping and abstracting is paramount, and results in systems of "transformation" that correspond more or less adequately to reality, and of which the fit becomes progressively better (thus giving some definition to the notion of "lower" and "higher" levels of knowledge). These systems of transformation are termed "schemata" or "schema", and the process by which they are formed "assimilation". The latter simply involves the retroactive effect of gradual connections through feedback interaction with the environment: it is summarized as "equilibration by self-regulation" (Piaget and Inhelder 1969,p.159).

The second stage--the semiotic or symbolic level--appears at about twelve to eighteen months onward, and is centred on the construction and manipulation of signs or symbols. It appears in such activities as deferred imitation, symbolic play, and drawing: all these involve the formation of mental images, which Piaget emphasizes are formed not by association, but by assimilation through internalized imitation, and which therefore represent the figurative aspect of cognitive functioning. Language develops during this stage, and is seen to be structured by the general co-ordination of actions at the sensori-motor level. This co-ordination therefore constitutes the pre-existing

schema into which the first gropings in the direction of language are assimilated.

The third stage--the operational level--appears from seven or eight years on, and completes the transition from concrete to abstract thought. This transition is embodied in the distinction between action, which is tied to the objects of the physical environment; and operation, in which the form of cognitive structures is liberated from their content. Operations are general schema, fully reversible, not isolated from the rest of the environment, common to all individuals at the same level, and entering into all cognitive exchange and reasoning. They are always directed towards conservation, towards the maintenance of equilibrium and continuity. It is scientific thought, in the formal philosophical sense, dependent on the dissociation of factors from objects, and resulting in the induction of general laws. Thus at this stage, the structuring of space and time, and the recognition of causality and chance in the environment, dominate intellectual growth.

In summary, therefore, Piaget sees four main factors underlying the development of cognitive structures. Organic growth is fundamental, providing new possibilities to the individual through an increasingly more complex nervous system and endocrine system. On this basis, exercise and acquired experience--whether of a physical or a mental nature--give rise to progressively more adequate cognitive schemata, through the mechanisms associated with assimilation. Social interaction and transmission proceed through similar mechanisms, which emphasize the primacy of the subject's action, in developing the affective and moral life. Self-regulation is the key to all three processes, and therefore constitutes the fourth: the pattern of active groping in the environment, and correction through feedback, to new,

more accurate action, infuse every process and every stage. "Equilibration by self-regulation" thus creates a unity between the development of both affective and cognitive structures. The result is to allow the individual to gain more and more complete control over a greater and greater area of the environment; but this control shifts in the course of development from behavioural to cognitive mechanisms.

This picture of cognitive development is in clear agreement with the conclusions that can be extracted from the collective work of geographic epistemologists. It has the advantage, however, of providing clear definitions of the key processes identified by them; and this clarity is only possible because of the coherence of the underlying theory. The possibility therefore arises that the work of Piaget and his collaborators may provide the theoretical basis that geographers have sought, in order to analyze concepts of concern to them, such as "space", "place", "home", and so on. If it is objected to this proposition that Piaget dealt only with children, whereas the geographer is concerned primarily with adults, we can turn to Piaget's own response to this question:

> The fundamental hypothesis of genetic epistemology is that there is a parallelism between the progress made in the logical and rational organization of knowledge and the corresponding formative psychological processes . . . . Of course the most fruitful, the most obvious field of study would be reconstituting human history--the history of human thinking in prehistoric man. Unfortunately, we are not very well informed about the psychology of Neanderthal man . . . . Since this field of biogenesis is not available to us, we shall do as biologists do and turn to ontogenesis. Nothing could be more accessible to study than the ontogenesis of these notions. There are children all around us. (Piaget 1970,p.13)

It is, indeed, an approach with which geographers should feel at home, since genetic explanation has long been integral to the understanding of geographic phenomena (Harvey 1969a,ch.21).

4. Methodologically, therefore, Piaget's studies are characterized by an essentially behaviourist approach: given the hypothesized functional unity of behaviour, cognitive, and affective structures, internal functioning is said to be inferable from external evidence. Yet clearly such a methodology cannot, and is not designed to, make available the nature of the experience that accompanies the cognitive function. Piagetian theory would not, therefore, satisfy geographers such as Lowenthal, Tuan or Buttimer who insist on the primacy of experience in individual functioning. It is an open question, however, whether the deduction of Piagetian structures makes it any the less possible to predict behaviour: in fact, when compared with the limited experimental implications of humanist theory, the contrary seems to hold, perhaps due as much to the inadequacies of humanist methodologies of examining the subjective world of experience as to inadequacies of theory. The clear fact is that Piaget deals with the logic and structure of experience, rather than with its intrinsic nature. His aim is to provide an empirically-based theory, not an intuitive description; and if the latter is insisted upon, Piagetian theory will have to be supplemented.

5. This is not to say that Piaget ignores the world of feeling and values. His research in that area is less comprehensive than in the development of intelligence, and, being in part untranslated, is less well-known. In a particularly fine article (Piaget 1965b), he locates the source of values in the exchange between individuals, and emphasizes their purely qualitative nature. It is thus inter-individual action, leading to satisfaction and obligation, that leads to respect or "valorization". In the context of the

moral development of the child (Piaget 1965a), the values of justice and equity are first of all constructed on the basis of unilateral respect, of adult constraint, leading to a kind of moral realism, in which the moral order is seen as essentially independent from the intentions and relationships to which it is applied. This heteronomy is transformed through the co-operation demanded in relationships with peers into autonomy, based on reciprocal respect, and gives rise to notions of distributive rather than retributive justice. The transformation occurs through the same mechanisms of assimilation and equilibration that are seen to operate in the development of intelligence; and this parallelism of intellectual and moral development confirms the notion of functional unity in developmental stages. Thus values, in Piagetian theory, spontaneously and inevitably arise during the course of interactions between members of a group, and are internalized in each member of the group. Such a view, is, of course, entirely consistent with notions of cultural relativity, and with the necessity for emphathetic identification in investigating the value systems of any distinctive grouping; but it does not explain cross-cultural invariants in value systems. Certainly it emphasizes values as constructed rather than as innate; and even though most of the empirical work was carried out on values identified as "moral" (justice, equity, right, wrong, punishment, and so on), the theory is capable of substantial formalization and generalization to other types of values. Taking the notion of "exchange" as central, for instance, it can be adapted to include values that relate to the environment, as in the notion of "home".

6. Finally, it should be noted that the idea of parallelism is a fundamental part of Piagetian theory. The relationship between biological and cognitive

structures is seen to be so close as to give rise to isomorphisms: identities of functioning which can convincingly be described by sophisticated cybernetic models, and perhaps by other qualitative mathematical languages, such as that developed by Thom (1972). Biological structures are seen to lay down a general system of co-ordination of actions through the endocrine and nervous systems, upon which cognitive structures build, and by which they are even directed. Organization, assimilation, and adaptation are common to both: both are dependent on equilibration by self-regulation. Above all, both biological and cognitive functions are integrated in the individual with affective, and moral functions. For Piaget, the individual is a whole, however precisely the parts which comprise individual functioning can be identified.

4.2.3 Identity and consciousness

It is an interesting, if surprising, fact that cognitive theorists have not attempted to deal systematically with the notion of identity, let alone with the more abstract idea of consciousness itself. Piaget, for instance, though aware of dimensions of cognitive development that bear on identity, specifically rules it out of his consideration. In dealing with the nature of cognitive truth, he concedes that if that truth is to be equated with organization. the question arises of the nature of the subject responsible for such organization. Thus he comments:

> If this subject is merely a human one, then we shall be in danger of extending egocentrism into a sort of anthropo- even socio-centrism, with minimal gain. As a result, all philosophers in search of an absolute have had recourse to some transcendental subject, something on a higher plane than man and much higher than "nature", so that truth, for them, is to be found way beyond any spatio-temporal and physical contingencies, and nature becomes intelligible in an

> intemporal or eternal perspective . . . the whole trouble has been. . . that this transcendental subject has been changing its appearance all the time but with no improvements other than those due to the progress of science--the progress of the real model rather than the transcendental one. (Piaget 1971,p.362)

The task of the cognitive theorist, in this view, is therefore to move towards an understanding of subjectivity by a deeper and more profound understanding of the cognitive organization associated with it:

> . . . if the true is an organization of the real, then we first need to know how such an organization is organized, which is a biological question. To put it another way, as the epistemological problem is to know how science is possible, then what we must do, before having recourse to a transcendental organization, is to fathom all the resources of the immanent organization. (Piaget 1971,p.362)

Thus from time to time, in the exposition of his cognitive theory and experimentation, Piaget notes the implications of his finding for the development of identity. He refers, for instance, to

> . . . a kind of Copernican revolution, or, more simply, a kind of decentering process whereby the child eventually comes to regard himself as an; object among others in a universe that is made up of permanent objects (that is, structured in a spatio-temporal manner) and in which there is at work a causality that is both localized in space and objectified in things. (Piaget and Inhelder 1966,p.13)

He goes on to locate this development not in biological operations, but in higher-order cognitive structures:

> . . . the purely perceptual point of view is always egocentric. This means that it is both unaware of itself and incomplete, distorting reality to the extent that it remains so. As against this, to discover one's own viewpoint is to relate it to other viewpoints, to distinguish it from and co-ordinate it with them. Now perception is quite unsuited to this task, for to become conscious of one's own viewpoint is really to liberate oneself from it. To do this requires a system of true mental operations, that is, operations which are reversible and capable of being linked together. (Piaget and Inhelder 1967,p.193)

But, on the whole, Piaget accepts that identity and consciousness, though fundamental to the structure and function of cognitive structures--"the very basis of the formal systems on which our comprehension of matter depends" (Piaget 1971,p.49)--and approachable through both neurophysiological and psychological experimentation, lie outside the central framework of cognitive theory. Ultimately, he seems to be asserting, one must have "recourse to a transcendental organization" (Piaget 1971,p.362) if one is to come to a full understanding of the subject, the individual; and such "transcendental organization" can only mean, it would appear, that it is apprehensible not by objective, but by subjective, means of investigation. In other words, the construction of identity, and the operation of consciousness, may be traced through objective means of investigation; but their reality is primarily a matter of experience.

Human geographers, where they have ventured to deal with these notions at all--and it is worth noting that only in the last decade have they been matters of concern within the discipline--have generally followed this two-fold approach. Both notions have been central to the emerging school of humanist geography, the approach to their construction being seen most clearly in the existentialist perspective, and the approach to their nature as experienced in the phenomenological perspective. We now deal briefly with the approach taken in human geography to identity and consciousness from each of these perspectives.

The constructivist account of identity in human geography is based on the assertion that the primary fact of human existence is the objectification of the world, and the entering into relationship with it. The self, the ego, of the individual is then defined by the relationships so established. Jakle,

Brunn and Roseman (1976), for instance, base their account of the construction of identity on this notion of 'entering into relationship'. They see human identity as the product of interactions with other people and with the physical environment, with the milieu at large. To the first, symbolic interaction is seen to be fundamental:

> Who you are depends upon what other people say you are as expressed in a mutually shared system of symbols. Vital is the human ability to empathize whereby people simulate the thought processes of others by considering what they would think and do if they were in another's shoes. As the individual abstracts to other people's thoughts or to what he feels are the aggregate thoughts of people in general, he is able to view himself as a social object. (Jakle, Brunn, and Roseman 1976,p.11)

Following the theory of sociologists such as Berger and Luckman (1967), most of the construction of identity is seen to occur in the context of primary relationships within intimate family and peer groups; the more formal, impersonal relationships developed within voluntary associations and the bureaucracies of business and government, are said to be of secondary importance. Identity is thus equated with the various roles and sub-roles that the individual assumes within these groups. On a wider scale, stereotyping by ethnic group, social class, and life style are found to cut across both primary and secondary identity formation:

> Stereotypes are seen to pervade an individual's complete identity . . . stereotyping lies at the very heart of human perception and cognition. (Jakle, Brunn, and Roseman 1976,p.12)

From this notion of stereotyping the authors derive the geographical aspects of human identity. As human groups are stereotyped, so are geographic places:

> Places serve as symbols of expected satisfaction or stress and are stereotyped according to the people who usually occupy and control them, the activities that usually occur in them, and the nonhuman objects that are associated with these activities. Most important in stereotyping places are the

> recurring patterns of behaviour that occur in them. (Jakle, Brunn, and Roseman 1976,p.37)

On the basis of the work of Piaget and Inhelder (1967), who showed the primacy of spatial relationships during the first years of life, this process is seen to be important in the construction of identity. Thus territoriality is seen to be a key element of identity:

> Most important to the self-concept are those places that people possess and defend as belonging to them personally. (Jakle, Brunn, and Roseman 1976,p.39)

Territoriality extends from personal to group space, and can interact with social space; as in the case of racial discrimination and segregation.

The construction of identity affected by place may be positive or negative. Human geographers have dealt more with the former than with the latter, chiefly within the framework outlined by Tuan's (1974,p.93) use of the term "topophilia":

> The word "topophilia" is a neologism, useful in that it can be defined broadly to include all of the human being's affective ties with the material environment. These differ greatly in intensity, subtlety, and mode of expression. The response to environment may be primarily aesthetic: it may then vary from the fleeting pleasure one gets from a view to the equally fleeting but far more intense sense of beauty that is suddenly revealed. The response may be tactile, a delight in the feel of air, water, earth. More permanent and less easy to express are feelings that one has toward a place because it is home, the locus of memories, and the means of gaining a livelihood.

Tuan locates a number of dimensions to this term, some of which are particularly relevant to the idea of identity. There is, to begin with, the simple fact of physical contact with the environment, particularly for those who, like farmers, depend on such contact to gain a livelihood:

> For the laboring farmer, nature has entered . . . the entry of nature is no mere metaphor. Muscles and scars bear witness to the physical intimacy of the contact. The farmer's topophilia is compounded of this physical intimacy, of material dependence and the fact that the land is a repository of memory and sustains hope. (Tuan 1974,pp.96-97)

Thus physical realities are instinct with significance by virtue of their relationship to the subjective aspects of individual life, particularly at the level of value, feeling and identity. It is, in Tuan's view, the development of familiarity and attachment that structures identity:

> . . . a person in the process of time invests bits of his emotional life in his home, and beyond the home in his neighborhood. To be forcibly evicted from one's home and neighborhood is to be stripped of a sheathing, which in its familiarity protects the human being from the bewilderments of the outside world. (Tuan 1974,pp.99)

To familiarity and attachment, a sense of the past, of tradition and history is seen to be essential (Tuan 1974,pp.99-102). This is an aspect of human experience that has been sensitively examined by Lowenthal (1975; 1979; 1982). He firmly supports Tuan's view of the past as integral to the construction of identity:

> Awareness of the past is essential to the maintenance of purpose in life. Without it we would lack all sense of continuity, all apprehension of causality, all knowledge of our own identity. (Lowenthal 1979,p.103)

Thus awareness of the past is itself, at least in part, a construction that provides continuity in the face of our own identity. (Lowenthal 1979,p.103)

> Life is more than separate events; it incorporates the quality of duration, of passage through time. Buffeted by change, we retain traces of our past to be sure of our enduring identity. We ourselves also change: we grow up, mature, and age. Our journey through these states of being, like that through the changing environment, is a voyage into the unknown, guided by our assurance of continuity. (Lowenthal 1975,pp.9-10)

Yet Lowenthal notes that the past is not the absolute, immovable repository of fixed events that this view of it as a sheet anchor to present uncertainties and difficulties would suggest it to be. He follows the idea, presented in chapter II, in our examination of the view of Collingwood and Carr, that the idea of history is as much a product of the present as it is of the past:

> Efforts to recapture or relive the past remain fictional and visionary for good reason: the achievement is impossible. The past has no doubt taken place; we ourselves stem from it. But the past we know or experience is not what actually happened; it is contingent on our own views, our own perspectives, our own present. Just as we are products of the past, so is the known past partly an artifact of our own. (Lowenthal 1981,p.91)

Thus the construction of identity that recourse to past environments effects rests on as precarious a foundation as it does when constructed entirely within the framework of a present environment.

These themes developed by Lowenthal are succinctly summarized by Jakle, Brunn and Roseman (1976,p.63):

> People tend to be attracted by what they cognize to be safe, exciting, or interesting places and repelled by dangerous and unattractive ones. The place that one regularly occupies must be congruent with one's sense of self. Places that do not reinforce self-images are stressful and encourage conflict with others.

This latter is a notion analyzed in terms of "insideness and outsideness" by Relph (1976a,pp.49-55). Essentially these two notions are the extremes of a continuum of distance, or objectification. Thus existential outsideness is "the rejection of an individual by a place . . . an awareness of meaning withheld and of the inability to participate in those meanings" (Relph 1976a,p.51). Less drastic is objective outsideness, as represented in the approach* the background condition of everyday life. On the other side is existential insideness, "in which a place is experienced without deliberate and selfconscious reflection yet is full with significances" (Relph 1976a,p.55). Less complete are empathetic insideness, or <u>verstehen</u>; behavioral insideness, or attention to the environing attributes of a place; and vicarious insideness, by identification with reported or recreated experiences of others.

Wagner (1972,p.50) follows this view in emphasizing that identity and place are inextricably linked:

* word omitted.

> The self derives its identity out of particular rhythms, places, actions, and appearances. No person is real without his plausible setting, his manner and style, his schedules, itineraries, and associations. The need for a place, for a rootedness is much more than a psychological quirk. Place, person, time, and act form an indivisible unity. To be oneself, one has to be somewhere definite, do certain things, at appropriate times.

It is this bond which gives significance to the concept of "home". If home is that setting in which one feels most completely oneself, to leave home for any extended period is a threat to the stability of identity:

> Real "aliens" are lost souls. Alienation leaves one feeling unperceived, spiritually homeless, bereft of motivation. But the exile remembers home values and lives in their light. (Wagner 1972,p.52)

Porteous (1976,p.390) sees home as the focus of territoriality, and finds in it essential contributions to identity, as well as to security and stimulation:

> Home is the space-group-time complex in which individuals spend the greater part of their lives. It is preferred space, and it provides a fixed end point of reference around which the individual may personally structure his or her spatial reality . . . . Home is a stable refuge for the individual.

Buttimer adopts a similar view:

> It appears that people's sense of both personal and cultural identity is intimately bound up with place identity. Loss of home or 'losing one's place' may often trigger an identity crisis. (Buttimer 1980,p.167)

Home provides the stable locus of personal existence from which excursions into the environment may be safety negotiated:

> . . . like breathing in and out, most life forms need a <u>home</u> and <u>horizons of reach</u> outward from that home. The lived reciprocity of rest and movement, territory and range, security and adventure, housekeeping and husbandry, community building and social organization--these experiences may be universal among the inhabitants of Planet Earth . . . one could speak of <u>centredness</u> and hypothesize that one's sense of place is a function of how well it provides for one's life interests. (Buttimer 1980,p.171)

In this reciprocity can be located the interface between personal and geographical identity:

> Personal identity and health require an ongoing process of centering--a reciprocity between dwelling and reaching--which can find its external symbolic expression in the sense of place or regional identity. (Buttimer 1980,p.186)

Sopher (1979), in a comparative view of the idea of home, perceptively remarks that the inherent power of home to shape and order human life rests not so much in the physical details of home and neighbourhood as in their human associations:

> The primary content of home, from what people say, is not material landscape but people. When one is absent, recollection of home is primarily of the human beings there. Without the continuing presence of the sustaining group, the place would no longer be home. It is one's relations with this nurturing and sheltering group as they are associated with the landscape that give it meaning as the landscape of home. At one scale, then, this landscape can be in large part that of the remembered field of familiar experience, within which particular places stand out as the loci of memorable personal events. (Sopher 1979,p.136)

He notes, too, an important distinction between "private images" of the landscape of home, to which the above analysis is addressed, and "public, consensual symbols" of that landscape, which function at a less fundamental, but still important, level of personal identity for all the individuals of a group (Sopher 1979,p.139).

In summary, stability and order are the principles common to these geographic notions of identity. The construction of identity is seen to be effected in the face of a chaotic, threatening world, dominated by alienation and uncertainty. Identity is the bulwark which is to keep at bay the hostile tides of meaninglessness and randomness. What the structure of identity is does not matter, since it can only be constructed from the environmental flotsom near at hand; as long as it exists, and does its job:

> People search for patterns with which to anchor existence. If life's events appear to be entirely random, and the world to display little consistency, then life would be very difficult to face. Stresses would build as people became unsure of facing one another . . . [we] would constantly have to expect the worst . . . deviation must be kept within bounds. (Jakle, Brunn and Roseman 1976,p.12)

Reduction of uncertainty, of complexity, is thus the overriding concern. How is it to be achieved, and at what cost, is not relevant, as long as it is achieved. The stereotyping which defines identity may possibly, if not certainly, be inaccurate; and the social relationships which derive from it may well be destructive: but these consequences are apparently unavoidable. In the end, one is forced by this argument to the existentialist view that man is compelled, for the sake of survival, to choose; and in the consequences of that choice is said to lie the tragedy of the human condition.

Consciousness has not until recently been a matter of great concern to human geographers. Its importance arose with the recognition of the role of human decision-making in creating spatial patterns, which, as we have seen, implied the necessity of uncovering the roots of that decision-making in perception and cognition (as in Golledge and Rushton 1976; Moore and Golledge 1976a; Ittelson 1973a; Wapner, Cohen, and Kaplan 1976). In this approach, consciousness was a given and assumed notion not subject to further analysis. However, the later approach, which emphasized the primacy of the lived experience of the geographical individual, required human geographic theory to look directly at consciousness: this was the school which has come to be known as humanist geography (Ley and Samuels 1978; Tuan 1976b, 1979a). It is the contribution of the approach which is now examined.

Humanist geographers have not, in the main, distinguished clearly between identity and consciousness. Their view seems to have been that the processes

which structure identity simultaneously structure consciousness. To be human is to know oneself as a human being: consciousness is consciousness of the individual self. Alternatively, humanist geographers have followed Piaget in defining consciousness in terms of the cognitive structures through which the individual arranges the objects of the world about him, and comes to understand their significance:

> . . . consciousness sets up a system of interpretations whose basic notions are designation and "implication" as between meanings . . . . That is why we say consciousness cannot be left out of our account, since it is the very basis of the formal systems on which our comprehension of matter depends. (Piaget 1971,p.49)

There is, therefore, no unanimity among humanist geographers about the meaning of the term 'consciousness'. Thus Tuan refers both to "consciousness" and "self-consciousness":

> All academic work extends the field of consciousness. Humanist studies contribute, in addition, towards self-consciousness, towards man's increasing awareness of the sources of his knowledge. (Tuan 1979a,p.388)

The latter is then identified, in the study of space from a humanistic perspective, with

> the study of people's spatial feelings and ideas in the stream of experience. Experience is the totality of means by which we come to know the world: we know the world through sensation (feeling), perception, and conception. (Tuan 1979a,p.388)

This stand is the basis of Tuan's earlier systematic exposition (Tuan 1974), which erects a theory of environmental perception, attitudes and values, on an understanding of sensory perception, and of generalized psychological structures, such as rationalization, and the resolution of contradictions. It seems, then, for Tuan, that consciousness is something that can be approached through the imaginative self-understanding of individual experience. How this

is to be achieved is not clear, though there is a reference to "the hunches of commonsense" (Tuan 1974,p.3). A reading of his works seems to indicate, however, that this methodology is a variety of literary verstehen, buttressed at appropriate points by psychological or anthropological notions.

A more systematic view of consciousness is that taken by humanist geographers who have turned to phenomenological theory for an understanding of the geographic individual. Phenomenology seeks to develop

> a philosophical method which would allow the philosopher to get "back to things themselves". That is, a method is sought which would allow one to isolate the essential aspects of objects of consciousness. (Entrikin 1976,p.617)

The objects of consciousness are thus pure essences, similar to those of Plato (Ross 1951), generalized forms or ideas of an abstract or transcendental nature, which are seen to be quite separate from the common-sense or scientific world, and which yet endow that world with meaning and value:

> Concepts are the universal terms which have specific instances just as essences are the general "forms" or "ideas" which have factual instances. Essences are not concepts, however, because they precede the development of concepts; essences are discovered only in man's transcendental realm. (Entrikin 1976,p.618)

The process by which one is to achieve such insight is termed "bracketing out": an active suspension of one's belief in common-sense or scientific explanations of the world, and of one's own biases and prejudices, in order to achieve a state of "uninvolved self-awareness", in which a phenomenon can be experienced in its essential, real nature (Relph 1976b; Entrikin 1976,p.618). This then becomes the true scientific method:

> Such transcendental reflection should probe to the foundations of all scientific enquiry; it should become, in fact, the fundamental attitude from which all scientific enquiry should spring. (Buttimer ~~1977~~ 1976a,p.279)

Consciousness is thus seen as essentially intentional, interposing between act and object the whole structure of meaning and value that is peculiar to the human species. Inextricably meshing with the objective act in objective external conditions is a world of subjective significance, or embodied intentionality:

> Intentionality is man's striving towards a structuring of his world through caring, hoping, conceiving, feeling, and meaning. (Buttimer 1974,p.38)

It is on this compound of the objective and the subjective that the foundation of geographic experience is to be laid:

> . . . the varied and even contradictory experiences we have of spaces, landscapes, and places combine the qualities and appearances of these with our moods and attitudes, and these experiences are fused in "geographicality", the preconscious and preconcepted foundation of Geography. (Relph 1976b,p.1)

Thus the phenomenological approach seeks to lay bare the a priori, the necessary, from which the life-world springs. Three points in particular emerge from this approach. Firstly, it is evident that, for the phenomenologist, consciousness has content; namely, the "pure essences", which appear to exist on the level of undefined feeling. Although the forms are referred to as "transcendental" or "preconscious", it is clear that they are so only in relation to rational or intellectual levels of thinking. That is, the individual is seen usually to be aware of the common-sense, everyday level of his thinking, the conscious mind; and since the essences appear to underlie that level, and in some sense to structure it, they are termed preconscious, or transcendental (Lowenthal and Prince 1976,pp.119-121).

Secondly, the method employed to experience the essences--bracketing out--is one of conscious reflection:

> One endeavours to peel off successive layers of a priori judgment and to transcend all preconceptions in order to arrive at a consciousness of pure essences. (Buttimer 1976a, ~~1977~~,p.279)

Clearly, then, bracketing out is an active, even arduous, process, requiring the individual by conscious thought to analyze and strip away taken-for granted experiences until he arrives at the preconscious, pre-rational forms:

> . . . phenomenology is not merely introspective, for what is involved is not a casual consideration of how I feel, but a systematic reflection on the character of my own experience. (Relph 1981b,p.106)

Thirdly, the relevance of this approach for geography lies in a recognition of the experiential foundation of geographical knowledge:

> Geography as a formal body of knowledge presupposes our geographical experiences of the world. In other words, geography has an experiential or phenomenological foundation . . . Concepts of space, landscape, city, region, have meaning for us because we can refer them to our direct experiences of these phenomena. (Relph 1981b,p.109)

Relph sees the application of phenemenology to geography to lie not so much in the experiences as in the phenomena of the geographical life-world:

> . . . cultural meanings and attitudes are made manifest in landscapes and . . . a phenomenological investigation of landscapes can reveal the character of the underlying geographicality. (Relph 1981b,p.111)

In a fine account of the roots of environmental thinking, he goes on to seek a heightened sense of environmental awareness:

> The development of a way of seeing that is based in compassionate intelligence opens the path for guardianship. Environmental humility requires a deep wondering at and respect for landscapes both natural and man-made. It simply means caring for, protecting and appropriating places. (Relph 1981a,p.189)

Buttimer expresses a similar view:

> In everyday life, one does not reflect upon, or critically examine, such horizons: the notion of life-world connotes essentially the prereflective, taken-for granted dimensions

> of experience, the unquestioned meanings, and routinized determinants of behaviour. To bring this precognitive "givens" into consciousness would elicit a heightened self-awareness and identity and enable one to empathize with the worlds of other people. (Buttimer 1977,p.281)

Thus the life-world, within which consciousness is to be experienced, is not only individual but collective, "the all-encompassing horizon of our individual and collective lives" (Buttimer 1977,p.281). In laying it bare to individual awareness, one puts oneself in the position to appreciate the subjective experiences of other people and of society taken as a whole. This, it may be noted, brings the analysis close to the Weberian verstehen, by virtue of which the social scientist, in the light of his own subjective experiences of the internal meanings of social actions, is able to identify imaginatively with the motives, values, and commitments of those whom he investigates (Nagel 1961,pp.474-475).

Some humanist geographers have turned to existentialism for an understanding of human consciousness. Existentialism opposes the idealism of phenomenology: it rejects the notion of pre-existing essences, and insists on the concrete reality of human existence (Entrikin 1976,p.623). Thus the fundamental principle of human life is not subjective but objective: the objectification of the world; and the development of relationships with it. Under this view, spatiality--the allocation of place to object--is the sine qua non of human consciousness:

> Spatiality is more than a necessary condition of human consciousness. It is the beginning of human consciousness. Spatiality is meaningful here precisely because it constitutes a minimum definition of man as the only historic life form to emerge with the capacity for detachment. (Samuels 1978,p.27)

Despite the absence of evidence for the latter part of this assertion, such detachment is seen as primal estrangement, as alienation, the basic human

condition. The individual is then under a compulsion to choose the manner in which he enters into relation with the world, in order to reduce or overcome estrangement; or he may choose not to do so, and live with the primal fact of alienation:

> Whether logically correct or absurd, man continually endeavours to bridge or fill his distances with relationships . . . . The existential "fact" of spatiality means that man pursues relationships with a vengeance, even though all the distances--by definition--cannot be crossed. If all the distances were crossed . . . man would cease to be man. If none of the distances were crossed, the human situation would be meaningless. (Samuels 1978,p.29)

Existential space is thus the construction of two components: partial space, which "is accomplished by focusing or noticing, which is to say by ordering the world" (Samuels 1981,p.120); and situations of reference, which are "the historic conditions in which assignments arise" (Samuels 1981,p.120). It is the 'partiality' that people project into their total life-worlds which then becomes the subject of geographic investigation:

> . . . 'existential geography' is a study in the biography of landscape. The two most important aspects of that geography are (1) that it begins with the subjective or with the issue of authorship in order (2) to discover the relations individuals and specific groups have with their environments as objects of their concern . . . an 'existential geography' is a type of historical geography that endeavours to reconstruct a landscape in the eyes of its occupants, users, explorers and students in the light of historical situations that condition, modify, or change relationships. (Samuels 1981,p.129)

It may be noted that in both the existential and the phenomenological approaches to human geography, the nature of the process by which consciousness, in the particular terms of the theory, is transformed into the observable activity of environed man which is of consequence for the geographer is not made clear. It is perhaps for this reason that the descriptions of the

matter and method of human geography from the standpoint of both schools seem remarkably similar; though, of course, as Buttimer (1974, Appendix) has indicated, there are close links between both philosophic perspectives.

4.3 The geographic account of the individual as a scientific theory

We may now move to an assessment of the view of the geographic individual, theory and enquiry already established (see Figure 4). There are grounds, it would seem, for attributing to twentieth century human geography a consistent concern with the way in which human individuals interact with their environment: to this concern, viewed historically as a whole, we have so far given the conservative and general term "integrated account". It is now necessary to establish to what extent this account may be described more formally as a scientific theory, in the sense developed in chapters II and III. In the course of this analysis, we may hope to begin to illuminate that issue of central concern to this dissertation identified in the first chapter; namely, the apparent irreconcilability of the demands of scientific rigour and human relevance that seems to lie at the heart of the fragmentation of the discipline.

First, however, we need to be assured that we are examining the credentials of no straw man, thrown together from the very different theoretical strands that have characterized approaches to the individual in twentieth century human geography; and having, on close examination, no reality beyond that of its components, no unified, coherent face. For it is an obvious response to the direction taken in this chapter that there is no integrated account of the individual in twentieth century human geography; that at no time in this century could one have pointed to such an account; and that no contemporary human geographer looks to such an account in the foreseeable future of the discipline, since the current trend is clearly towards continued fragmentation, rather than towards integration. If we are to feel confident in the value of continuing our enquiry, this response must be firmly met.

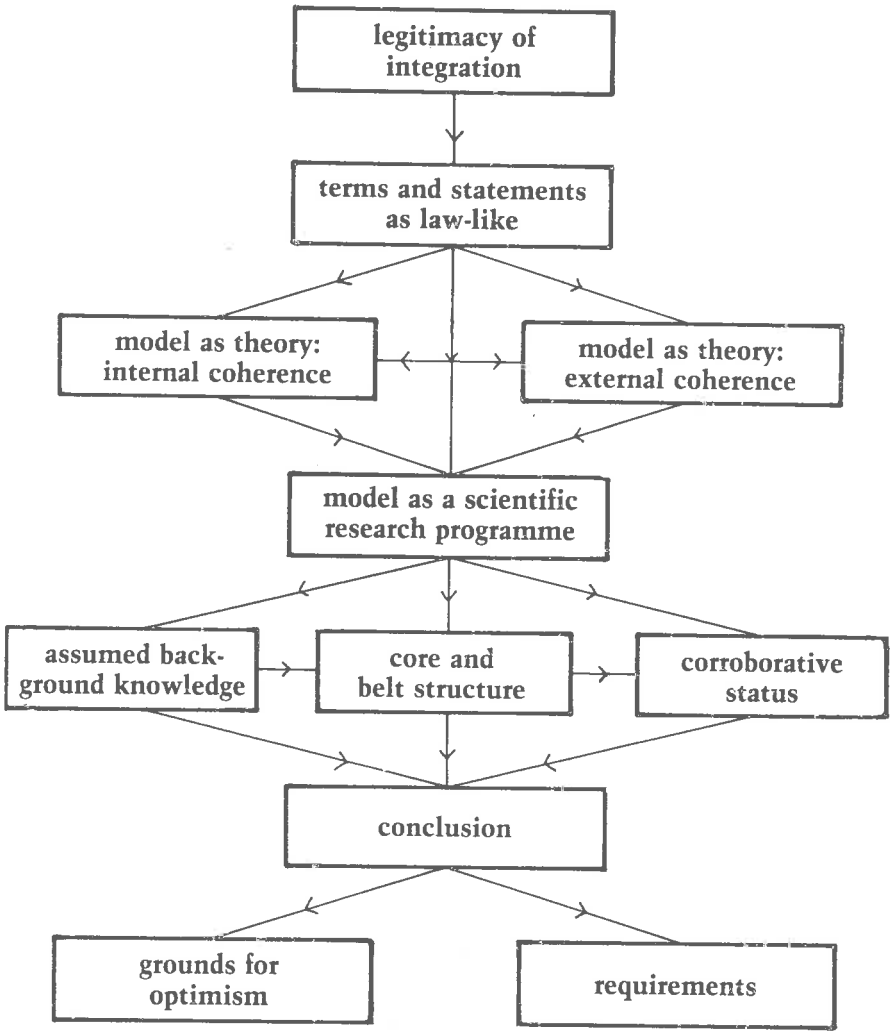

FIGURE 4:

Assessment I of the Proposed Integrated Model: The Perspective of Science

Something on this score has been said earlier in this chapter. We have noted that the perspectives of the various schools of twentieth century human geography each seem to deal with important, but separate, aspects of the environed individual; and that these perspectives, taken together and placed in hierarchical relation, do seem to point towards the kind of holistic model of the individual that humanist geographers have in recent years demanded, and which we have accepted to be an essential part of a relevant contemporary discipline. To these arguments we may now, on the basis of the preceding review of geographic theory, add two more. The first is the apparent difficulty of maintaining the independence of the different perspectives on the environed individual adopted by human geographers. Thus, as we have seen, the analysis of behaviour and decision-making seems to demand, through considerations internal to theory at that level, understanding of the cognitive processes which structure these aspects of individual functioning. Similarly, cognitive processes and structures are themselves, it would appear, reflective of the more fundamental operation of identity and consciousness. Not only, in other words, are these aspects of the environed individual hierarchical, but inseparable. We may choose, as both geographic and psychological investigators have chosen, to rank them according to the criteria of objectivity, or of concreteness and abstractness; and then to direct our attention to one particular level, and seek comprehensive explanation solely within that domain: but we find that such explanation slides inevitably towards the next lower level, and seeks to bring all levels into a functional unity. In summary, the attempt to understand the functioning of the environed individual at different levels has itself established that these levels are conceptual only, and are not found separated in the actual life of the individual.

Such a conclusion, developed mainly from the perspective of the objective approach to the environed individual, lends support to a second consideration which derives from the subjective approach. That is the simple observation that no one lives life in anything other than an holistic way. It is a rare person indeed--and not, perhaps, psychologically a healthy one--who is aware moment to moment of the processes by which he thinks, and feels, and acts; who consciously identifies decision-making or cognitive functions as they come into play. It is, one feels, safe to assert that every ordinary individual, in every society and environment, lives life spontaneously: the nature of human experience is of an undivided whole, a continuum in which the elements of feeling, thought and act are recognized to co-exist, but to be inseparably interfused. It is this reality that the inability of separate perspectives to deal adequately with the environed individual reflects: we may, indeed, feel it to be a mark of the authenticity of such perspectives that they have located both their own limits, and the direction in which those limits are to be transcended.

We conclude, therefore, that whatever else can be said against the proposed integrated account of individual functioning in human geography presented in this chapter, its integrated nature at least can resist challenge. We now move to an assessment of its scientific status.

The first question to be considered is whether it is anything more than a broadly descriptive account of the environed individual; whether it is, in the sense developed in sections 2.2.7 and 3.2, a scientific theory. It may be useful to begin by briefly recapitulating that discussion. There the notion of a theory was found to depend on the idea of a law: a theory was defined as a systematic inter-relation of laws. A law, in turn, was distinguished from mere

generalization by a methodological insistence on its unrestricted range. A theory was seen to be structured by internal principles, related to the nature of the interrelation of laws; and by bridge principles, relating it to other bodies of theory already established or accepted by the scientific community. A good theory, it was asserted. systematically unifies diverse phenomena; expands the range of previously established theories; and can predict and explain new phenomena. Central to the power of a scientific theory was seen to be the nature and degree of its formalization, particularly of its primitive base, whose grounding in the nature of things, in the way things really are, is largely responsible for the scope and capacity of the theory developed on it. Let us begin by considering to what extent the statements made about behaviour and decision-making, about cognition and cognitive structures, and about identity and consciousness in the human geographic analysis of the environed individual, can be considered laws, or at least law-like. What can we say, for instance, of Gollege and Williamson's (1972,p.62) assertion that three components--psychological, economic, and geographic--are comprised in any geographical decision? Or of Fielding's (1974) notion that perceptual information is filtered by an individual's knowledge, ability, and value system to create an image, which then becomes the basis of decision-making and action? Or of Jakle, Brunn and Roseman's (1976,p.11) pithy remark that "Who you are depends upon what other people say you are as expressed in a mutually shared system of symbols"? To what extent do we feel comfortable in regarding statements such as these as legitimate examples of scientific laws, able to support legitimate scientific theory?

It seems clear that what we are seeking, as the methodological equivalents in the social sciences of the universal statements that qualify as laws in the

natural sciences, are generic statements. A law in human geography, as in all the social sciences, will be a statement that is true for the human species as a whole, regardless of place, or time, or circumstances. As in the natural sciences, this kind of universality is methodological, rather than philosophical. Since human functioning is certainly dependent on physiological processes, and since these ~~principle~~ principal processes themselves operate on the basis of physical laws, it is clearly not possible to assert a greater degree of universality than that which obtains in physical laws. Nevertheless, such arcane speculations are not likely to lessen our satisfaction in such a law of human functioning, should it prove possible to obtain.

Having established so much, however, we now run squarely up against the theoretical difficulties examined in chapter III, where the notion of generic laws was challenged by ideas of cultural and social relativity, of contexts of meaning and intelligibility. We need not recapitulate the various arguments presented and analyzed there: let us simply recall our conclusion, that, although formidable methodological difficulties present themselves, there seems to be in principle no reason to assume that laws about human functioning cannot be obtained within the structure of an ongoing research programme along Lakatosian lines.

Coming back to human geographic perspectives, we now want to ask whether the kind of statements made by human geographers about the nature of the environed individual are examples of the generic laws we are seeking. The question is therefore not whether different structures of cognition, identity and consciousness, underpinning different modes of behaviour, are developed in different ~~milieu~~ milieus--or in different times, or different cultures--but whether the processes of formation and operation of these structures are, to recall Winch's

(1958,p.18) phrase, systematically ambiguous. In particular, to what degree are these processes peculiar to Western contexts, and developed according to Weatern concepts and priorities?

In part, the answer to these questions differs according to the level of functioning of the environed individual towards which they are directed. At the level of behaviour and decision-making, for instance, there is good reason to feel that not only the structures located by human geographers, but the processes of their formation and operation, are most relevant to one particular cultural and historical ~~contest~~ context: that of contemporary Western rural and urban society. Most of the factors identified in that analysis, and the models that are derived from them--rationality, optimizing, sub-optimizing, satisficing, habit, information, utility, and so on--were developed and tested exclusively within that context, and appear to be predicated on the maintenance of social and economic environments peculiar to it. That in itself does not conclusively demonstrate that the notions developed there are not also applicable to other cultures and contexts; but if they are to be so applied, they will have to be framed in more general terms. True scientific hypotheses, in other words, will have to be derived from what ought to be regarded as initial data and then applied to widely differing contexts. Such a procedure would be in accordance with the orthodox methodology of the social sciences, as outlined by Nagel (1961,p.456-459), which commonly seeks <u>in situ</u> experimental situations.

At the level of cognitive processes and structures, a similar challenge can be mounted: a frequent criticism, for instance, of Piagetian cognitive theory is that it rests almost entirely on an experimental basis that is limited to European children. There is, however, a crucial difference between this kind of theory and that developed at the level of behaviour and

decision-making; and that is simply that, while its experimental base may be admitted to be narrow, its theory is not limited in this way. It is specifically framed in generic terms, and directed towards an understanding of human, rather than of European, functioning. The experimental findings therefore stand in a genuinely scientific relationship to theory, in that they serve to corroborate theory through specific test implications; and the momentum they give rise to is just that of a scientific research programme. Their limitation, in terms of cultural context, is thus a reflection on the corroborative status of the theory--wider testing in more cultural contexts would provide greater corroboration--but not on the scientific status of the theory. In contrast, the above account of behaviour and decision-making is itself couched in terms that are specific to a particular culture.

Moreover, there is a widespread concensus among cognitive theorists that this theory is widely, perhaps even, generically applicable. Cole et al. (1971,p.233) remark:

> . . . cultural differences in cognition reside more in the situations to which particular cognitive processes are applied than in the existence of a process in one cultural group and its absence in another.

Cross-cultural surveys of cognitive theory, such as Kearney and McElwain (1976), have supported this view. If we look to the structure of the cognitive theory employed and developed by human geographers, as summarized at the conclusion to section 4.2.2, we can see why this may be a reasonable position to adopt. We noted, in particular, that it seems difficult to separate the operation of cognitive structures from biological structures. Perception, we saw, was integral to the formation and to the operation of cognitive structures, and required an understanding of the nature of sensory processes.

Moreover, in the Piagetian approach to cognition that human geographers have adopted, cognitive structures are seen to be intimately related to biological structures of organization, which change systematically in the course of maturation, and produce the predictable changes in cognitive style that Piaget and his co-workers have been able to delineate. Piaget has explored the theoretical implications of this parallelism of structure and function in his most substantial work (Piaget 1971). Here, it would seem, we have located just that invariant ground of human functioning on which a legitimate and scientific theory of the human individual might be based, as foreshadowed by Nagel (1961,p.460):

> . . . all human actions involve physical and physiological processes whose laws of operation are invariant in all societies.

The survey of human geographic approaches to cognitive structures and processes undertaken in the preceding section suggests, therefore, that such laws need not have, as Nagel goes on to infer, a "narrowly restricted generality" (Nagel 1961,p.460), but may indeed represent the beginnings of a scientific theory of the environed individual that can be applied to cross-cultural contexts, and thus represent a basis for the generic account here sought.

At the level of identity and consciousness, the situation is much more difficult to analyze, chiefly because the methodology of analysis employed there in human geography has been almost entirely subjective. Thus, while much has been written about the nature of individual structures of identity with respect, for instance, to the idea of 'home', there has been little attempt to underpin these descriptive or anecdotal accounts by any kind of rigorous theory, since the mode by which such theory could be tested, even assuming that adequate test implication could be extracted, and operationalized, is

altogether obscure. Certainly, however, it seems to be a primary assumption of the phenomenological perspective on human geography that, at this fundamental level of functioning of the environed individual, the processes of formation and operation are invariant between cultural and historical contexts; that, if the procedure of 'bracketing out' can be successfully undertaken, the 'precognitive givens' which remain are characteristic of the human mode of awareness of the environment, and are therefore the basis of a true science of man. There is reason to doubt, however, despite the logic of this approach, that the 'precognitive givens', the 'essences' of phenomenological theory, in fact represent that generic level of human functioning: the geographic literature in which these notions are applied continue to emphasize their dependence on a particular location, a particular time, and a particular cultural and social context; or various combinations of all these. Sopher (1979, pp. 132-133), indeed, argues that the idea of home, for instance, is a peculiarly male experience, and not available to female experience, in most societies, to nearly the same degree. For such a process, which is said to be relevant to only half of the human population, and to some societies, but not others, it is difficult to claim the title of a scientific law.

Such a conclusion is, however, profoundly unsatisfying. Might we not expect that it is precisely at these more fundamental levels of human functioning that the true laws sought by human geographers might lie? It is clear that the understanding of cognitive structures and processes has proceeded successfully along scientific lines; and the concensus of theorists working in this area is that cognition is based on the more fundamental structures and processes related to identity and consciousness. It seems reasonable to expect, therefore, that the order located at more superficial

levels, as represented by the law-like statements of cognitive theory, is embedded in a similar kind or order at the more fundamental levels of identity and consciousness. Piaget, for instance, is unequivocal about the existence of such order, and optimistic about its delineation through traditional objective approaches, through a combination of inductive inference from, and deductive testing of, overt behaviour, and of psycho-physiological correlates of particular behaviours (Piaget 1971,pp.47-49). It should be noted, moreover, that all the human geographers whose writing about identity and consciousness were reviewed in the preceding section, clearly believe that such order exists, but see no other available methodology than the subjective for elucidating it.

We therefore have the paradoxical situation that the scientific status of the human geographic account of the environed individual becomes less scientific, more speculative, and more introspective as it approaches levels which are accepted to be fundamental to the understanding of all the levels of cognition and behaviour to which scientific methods are generally applied. Obviously this paradox has its roots in the inherent difficulties in which the social sciences have always found themselves in dealing with subjective phenomena: and more will be said on this issue in chapter VI, when we come to review the conclusions of this chapter and the next, and attempt to indicate the general direction in which theory must move if it is to resolve difficulties such as these.

We may now move from a consideration of the law-like status of the statements employed by human geographers in the formulation of their understanding of the environed individual to a consideration of the status of the theory to which these statements, in combination, give rise. Here we are concerned with the nature of the 'systematic interrelationship' obtaining among

these statements, of the 'bridge principles' that maintain the internal coherence of the account, and of the 'systematic unification' effected by the entire structure, taken as a whole.

It is clear, to begin with, that the analysis of the statements of human geographic perspectives undertaken above will have direct implications for the status of the theory they comprise. It was concluded there that, on the one hand, statements relating to cognition appear to have more of scientific status, by virtue of their greater cross-cultural invariance and reliability, than those relating to behaviour, at the most concrete level, and to identity and consciousness, at the most abstract. On the other hand, however, it was established that a hierarchical relationship exists in which behaviour was seen to be embedded in cognition, and cognition in identity and consciousness. Taken together, these two conclusions present real internal difficulties for the structural coherence of the proposed theory of the environed individual. If we want to preserve scientific rigour, at least as far as possible, it seems we are to be restricted to a Piagetian approach to cognitive functioning alone: if we want to preserve an holistic view of the environed individual, it seems we must relinquish our demand for the rigour and benefits of science. The nature of the systematization that is to frame the proposed theory is therefore dependent upon the decision about which priority is to be upheld, and has no inherently logical foundation.

We have noted, too, that there exist in human geography many different approaches to central concepts at each level of analysis. Even within particular schools, concensus has proved entirely elusive. Thus, for instance, the idea of optimizing behaviour has been modified, by recognizing certain constraints of information-flow and decision-making, to produce sub-optimal or

satisficing models, which nevertheless insist on retaining the idea of optimizing in their structure. The concept of optimization--which is, in fact a good example of a primitive concept in human geographic theory--thus becomes altogether ambiguous, and compromised for its effectiveness in the construction of legitimate scientific theory. In a similar fashion, we saw that concepts such as cognition and cognitive structure, though seen generally in much the same light in the different perspectives of human geography, themselves lack sufficient definition to prevent difficulties of vagueness, ambiguity, and underidentification (to use Harvey's term) from arising. The consequence, on grounds both of ambiguity and imprecision, is that these human geographic theories cannot be satisfactorily formalized, and therefore lack the predictive power and the clear identification of test implications that, as we have seen, are fundamental to a true scientific theory. Indeed, one might go on to remark that, since between theory and precision of primitive concepts there is an intimate relation, weakness of theory and weakness of conceptual strength negatively reinforce each other, and create further deterioration in both. A reversal of this loop can only be effected by introducing more profound theory in which primitive concepts can be more precisely framed, or by embedding these concepts in other bodies of well-established theory or in a clearer and more widely accepted ground of common experience. The greater clarity primitive concepts gain by such associations begins then to support and strengthen the theory developed from them.

We may further note that testability of theory is not the same thing as predictive power, though they are obviously closely related concepts. It seems, for instance, in the account of human geographic theories of the individual given in the preceding section, that the theories of behaviour and

decision-making developed by human geographers are certainly testable, but that their predictive power is limited. The first property relates to the clarity and ease of operationalizing their terms, and the second to the degree to which the primitive base is sufficiently rooted in the nature of things to allow for the wide application of the theories in question. Thus theories based on the idea of rational economic man yield ready test predictions, but do not, as the research emerging from it has demonstrated, provide for much predictive success. From this it can be inferred that a more comprehensive primitive base needs to be developed, which attempts to describe the more significant levels of individual functioning in which the operation of rational decision-making is apparently embedded. This, it will be recalled, is Rudner's position:

> We might expect that a general theory of social behaviour or a general theory of social action would, were it a viable theory, have variables of behaviour such that descriptions of . . . rational behaviour . . . could be <u>derived</u> by letting those variables take on extreme values. Such a comprehensive theory of social action would, in short, subsume economically rational behaviour (or sensible behaviour) as a special case. Unfortunately, no such comprehensive theory seems at present to be available. (Rudner 1966,p.62)

As these levels become more and more abstract, however, although there is no difficulty in making the primitive base more powerful, the problems associated with operationalizing primitive terms and of developing test implications, become more and more substantial. At the level of identity, and consciousness the extreme of both trends, as we have seen, is reached; and herein can be located further roots of the paradox alluded to above.

Another difficulty associated with the internal structure of our integrated account is the difficulty of predicting new phenomena, which is a necessary consequence of abandoning scientific modes of theory formulation. A generalized understanding, couched in fundamental terms, such as has been

developed in human geography in approaching the notions of identity and consciousness, deals with the existing domain of phenomena; in this case, with the ground of common subjective experience associated with these levels of human functioning. But if such an account is not framed along the systematic lines of a scientific theory--as these human geographic accounts are explicitly not so framed--it becomes impossible to predict new phenomena, just because there is no systematic procedure for elaborating such theory, nor any route by which elaboration could proceed. What this conclusion underscores is that the accounts of identity and consciousness provided by human geographers are just accounts, and no more: descriptions which attempt to effect a degree of organization of these levels of the common experience, but not in terms that even approximate--or, indeed, try to approximate--the formal or quasi-formal theories with which scientific inquiry can legitimately deal. They represent, in fact, that level of non-formal theory construction which Harvey describes as

> . . . verbal explanations which cannot be even partially formalized without a substantial modification of the concepts used and clarification of the deductive relationships. (Harvey 1969a,p.99)

The human geographic accounts of identity and consciousness do not, of course, propose any such modification; and this, we have argued, is not in the best interests of the discipline. If such accounts are to move in this kind of direction, certainly the first steps will be, as we have indicated above, along the lines Harvey here identifies. Only through such an approach is it possible to predict new phenomena, however difficult it may be in this case to imagine what new phenomena of subjective human experience--phenomena, one infers, that lie outside the bounds of what is accepted to be the common human experience of subjective life--might be.

So much for the difficulties associated with the internal structure of our integrated account of human geographic approaches to the individual, viewed as a scientific theory; difficulties which relate to "the basic entities and processes invoked by the theory and the laws to which they are assumed to conform" (Hempel 1966,p.72). We now note, before assessing the integrated account in terms of its ability to generate and participate in a Lakatosian scientific research program, some important difficulties associated with external or 'bridge' principles. Such principles, it will be remembered, are seen by Hempel to relate a scientific theory to familiar empirical phenomena and

> may well be characterized in terms of previously established theories and their observation and measurement may presuppose the principles of those theories. (Hempel 1966,p.74)

Hempel goes on to make the important point that it is just the weight of established meaning and intelligibility provided by the context of these theories which allocates both explanatory power and testability to the theory in question:

> . . . the implications that permit a test of those theoretical principles will have to be expressed in terms of things and occurrences with which we are antecedently acquainted, which we already know how to observe, to measure, and to describe. (Hempel 1966,p.74)

Here we find additional reason for concern about the status of our integrated theory. As we have seen, the theories which act to give the account a contextual foundation are many, and differ widely in their scientific status, with respect both to the adequacy of their internal structure, and to the degree of corroborative status they have attained. Thus at the level of behaviour and decision-making, in addition to the ideas of rationality and optimization alluded to above, mention is made of psychological, economic, and

geographical factors, of the role of habit; of information flow; of utilities; and of cognitive structures. If we were to take seriously the task of systematically analyzing all these concepts and the bodies of theory to which they are related, that in itself, even without bringing them all to bear on the human geographic phenomena to which they are said to be related, would be a life-time's work; and may not, given the enormous range and independent status of these theories, be at all achievable. It is clear that human geographers have not considered such an analysis necessary, and have been content to assume the validity of these contextual theories.

Much the same is true of the approach to cognition and cognitive structure, although here the appeal to Piagetian theory is fortunately more convincing, by virtue of that theory's greater internal elaboration and corroborative status. Nevertheless, we find in the human geographic account of cognition reference to psychophysiology; to notions of meaning; to psychological space; to world views, both individual and collective; to stereotypes and value systems; to learning; to socialization; and to modes of subjective experience. Just to enumerate the list is to give eloquent expression to the difficulties outlined above.

The theoretical fog deepens still further on considering the level of identity and consciousness, where almost every term--attachment, insideness and outsideness, intentionality, geographicality, and so on--makes specific appeal to one of the two theoretical structures of existentialism and phenomenology, and sometimes to both. Here the difficulties are redoubled, since neither of these approaches is structured scientifically, nor has, in consequence, corroborative status. Again we see, as we have consistently seen wherever theoretical concepts external to the discipline are involved--in the notions of

science and scientific inquiry, or social science, or of the specific theories of individual functioning dealt with above--that human geographers, although adopting such theories and concepts with alacrity, seem particularly loathe to become embroiled in the critical controversies that are inseparable from them; and the theory of human geography is, in consequence, more confused and less credible. If we add to all these considerations the fact that, as has been noted, the main approaches which comprise this integrated account appear to be structured hierarchically, and that the difficulties here found to be associated with each individual approach are therefore magnified by interaction, the prospect of being able to maintain the internal integrity and scientific status of our account begins to look bleak indeed.

We now turn to a consideration of the second question posed in the introduction to this chapter; namely, to what extent the integrated account proposed here is capable of generating a true scientific research programme, in the sense developed by Lakatos in the context of the natural sciences, and by Carr in the context of the social sciences. A subordinate, but still important, question associated with the perspective is whether the proposed integrated account is more capable of generating such a programme than is any of its constituent approaches, and therefore is a better scientific theory, in the sense of the definition of that term developed earlier in this dissertation.

Again it may be useful briefly to recapitulate the fundamental concepts associated with the idea of a scientific research programme. It will be recalled that it finds its methodological and philosophical basis in the sophisticated falsificationism developed by Popper. This position of Popper's

rests on the necessity for making methodological decisions of three kinds: the identification of background knowledge that is to be regarded as unproblematic; the establishment of criteria for the corroboration of theory; and the selection of appropriate statistical decision rules. On this methodological and philosophical foundation, the structure of scientific inquiry takes a particular form, in which a series of theories, rather than a single theory, is the principal focus of investigation. The concept of scientific progress is defined in terms of excess empirical content. Theoretical progress proceeds according to its own internal dynamic, but is linked intermittently to empirical findings. In an actual scientific research programme, these principles give rise to a core of theory held to be established beyond criticism, and to a belt of theory, to which the demands of scientific inquiry may legitimately be directed. The theoretical development of such a programme is almost independent of experimentation, except that periodic verification is essential to maintain the theoretical momentum. Moreover, less stringent criteria of verification are applied to immature as against mature, research programmes. In the context of the social sciences, the methodological decisions are much the same, though more obtrusive: the selective role of the social scientist, and the subjectivity associated with the identification of objective fact have long been recognized to be an irreducible part of social scientific inquiry. The move from theory to 'fact', though likely to be more frequent than in the natural sciences, runs along similar lines; interpretation and insight are able to develop through internal considerations, but are subject to verification from the sources or data that have been taken to constitute the empirical basis of the inquiry.

What, then, is the standing of the proposed integrated account of the individual in human geography, from this perspective? To begin with, we may

note again that substantial bodies of theory and observation are assumed at every level of consideration by the proposed account. The fact of this assumption does not present any difficulty, since every scientific research programme is held to be developed from background knowledge of just this kind. However, the sheer extent of this assumed theory, and the real internal difficulties associated with most of them, presents, as we have seen, a formidable challenge to the security of the conceptual foundations of the integrated account. Background knowledge it may be: unproblematic it certainly is not. In consequence, the methodological decisions that need to be made about which bodies of theory to assume as a foundation for this account are neither simple, nor a function of rules or procedures that can be commonly agreed upon.

The attempt to identify 'core' and 'belt' aspects of the theory is no easier. At the different levels of approach, some general shape of the dichotomy can be discerned. Thus, for instance, the notion that decision-making is a necessary prelude to behaviour in the environment, and that decision-making depends crucially, amongst other things, on subjective priorities as well as on access to information, may tentatively be identified as a grouping of core concepts. It is not, as we have seen, sufficiently well-articulated to be regarded as a core theory, in the formal sense; but it does represent the kind of initial intepretative or conceptual position which has been identified following Carr, as an appropriate mode of inquiry for the social sciences. It has, in fact, provided a sufficiently secure centre of gravity for a substantial tradition of research in the school of behavioural geography (Golledge, Brown and Williamson 1972)—much of it conceptual, as we have seen--to develop. Techniques of experimentation, and therefore empirical

data, have, on the other hand, been difficult to obtain, and appear to await resolution of the substantial conceptual problems that still pertain to this level of analysis.

At the next level, however, the idea of cognitive structures, of their relationship, through perception, to the environment, and of their role in structuring behaviour is, largely due to the weight of Piagetian theory and experimentation, a well-established approach of sufficient structure and credibility to be regarded as a core theory. In consequence, the belt areas of research are more apparent: the development of the theory of psychological space, for instance, and its associated experimentation through cognitive mapping is, it would appear, just that kind of systematic inquiry which a scientific research programme supports.

Of the level of identity and consciousness it has already been remarked that human geographers have employed theoretical approaches that are specifically not structured as scientific theories; and in consequence neither the idea of core nor of belt areas of theory is here appropriate. It is clear that, as philosophies, existentialism and phenomenology are well developed, and may have implications for geographic thinking. As we have seen, some tentative beginnings have been made by writers such as Relph (1981b) and Samuels (1981) to outline what a geography based on these positions might be like. But it is equally clear that it is not possible to speak about these philosophic positions, as they stand, as core theory without distorting the description to an unacceptable extent. For that they must first be framed as scientific theories.

Directing the notions of belt and core to the integrated account rests, therefore, at least in part, on their applicability to each of these levels, in

the manner suggested. There is a sense in which the core of the integrated account might be considered to be composed of the combination of core theories identified here; and to that extent, it inherits their problems. But, from a more holistic perspective, a more fruitful view of its core is the hierarchical relation between different levels. That behaviour is largely a function of cognitive structure, which is in turn framed by the structure of identity and, ultimately, consciousness, is a fundamental premise of the attempt to construct an integrated account. As we have seen, it is supported by the conclusions of the analysis undertaken at each level. It is at least certain that if the integrated account proposed here is to succeed as a legitimate scientific theory, in the context of a scientific research programme, it is to this hierarchical relation that it must look for its conceptual anchor, for its core; and some of the requirements for that to come about have already been explored. If the hierarchical relationship could adequately be secured, we may cautiously predict that research may be directed to at least three belt areas. The first is the functioning of the individual at each level, something along present lines (except in the case of identity and consciousness), but strengthened and enhanced by the conceptual richness brought about by establishment of such a core. The second is the detailed operation of the junction points, the precise way in which each level of individual functioning relates to the next. The third is the inevitable concentration of attention at fundamental levels of the hierarchy, in the same spirit as contemporary humanist geographers, though along very different lines. Beyond this it is difficult to speculate; and it is important to emphasize again that before such research directions could begin to be pursued, there are serious conceptual difficulties in the construction of the theoretical core which need first to be resolved.

The corroborative status of the integrated account can only, at this stage of its development, be approached through the corroborative status of its constituent approaches, since it has not, as an holistic theory, yet been analyzed to yield test implications, much less subjected to the scrutiny of empirical testing. A similar pattern among the different approaches emerges with respect to corroboration as it has with respect to the law-like status of their assertions, their structure as traditional scientific theory, and their structure as theory in a scientific research programme. Indeed, we might reasonably predict such similarity, since, as we have seen, verification is made possible, and advances, largely in proportion to the degree of conceptual development of theory. Corroboration is therefore greatest for cognitive approaches, somewhat less for behavioural approaches, and, for all practical purposes, vacuous for approaches that emphasize identity and consciousness.

This last deficiency is particularly damaging to the prospect of constructing an integrated theory, because of the peculiar position of these terms in the hierarchical structure of explanation proposed in our integrated account. The simple fact is that, despite the characterization of phenomenology as a "philosophical method" (Entrikin 1976, p. 617), which is said to involve "braketing out" (Relph 1976b; Entrikin 1976, p. 618), "transcendental reflection" (Buttimer 1977, p. 279), "systematic reflection on the character of my own experience" (Relph 1981b, p. 106); and despite the projected application of this technique to geographical experience to "reveal the character of the underlying geographicality" (Relph 1981b, p. 111), to create "environmental humility" (Relph 1981a, p. 189), and to lay open to awareness "the all-encompassing horizon of our individual and collective lives (Buttimer 1977, p. 281), the paucity of actual work derived from these notions

is all too apparent. It is one thing to propose a philosophical method: it is another to practise it. Even in the phenomenological literature outside human geography, and particularly in attempts to put phenomenological method into practice in understanding societies and cultures (one thinks, for instance, of Elaide's work on religion), systematic reports of the successful and fruitful adoption of this technique are, for a major philosophical movement of now substantial continuity, remarkably few. Similarly, existential human geographers may project an "existential geography" (Samuels 1981) and a "biography of landscape" (Samuels 1979) deriving from that philosophical position; but not only is the conceptual link between the philosophy and the geography exceedingly tenuous--even in the philosophical literature itself, the idea of existentialist method is not well elaborated---but the actual products of such an approach are few and unconvincing. It is abundantly clear, therefore, that even granting the considerable latitude necessary to locate statements about identity and consciousness that are generalizable, if not entirely law-like, and a theoretical structure that can be characterized as possessing some semblance of core and belt, human geographers have not been able, in practice, to derive either test implications, or empirical support for such philosophy. Its corroborative status is therefore almost entirely lacking. This is inevitable, given the deficiencies of conceptual development and theoretical structure already identified, but it is nevertheless a conclusion that further, and perhaps most forcefully, damages the claim of the integrated theory to legitimate and fruitful scientific status.

In the face of such gloom there are at least two grounds for optimism which it is important not to lose sight of. The first is the potential fertility of the integrated account, the promise it holds of dealing

comprehensively with the environed individual, and of therefore providing powerful and effective strategies of intervention to enhance the quality of human life. This was, indeed, the very reason for raising the possibility of constructing an integrated theory in the first instance. As Kuhn pointed out, and Lakatos confirmed, new theories are adopted not because they are comprehensively supported by empirical evidence, but because they offer the promise of greater support than preceding or rival theories. Certainly some empirical support must be available for some of its key notions; yet the primary impetus for its adoption is not empirical, but theoretical.

Fundamental to such impetus is likely to be an intuitive conviction about the nature of things. Thus Einstein was moved to propose the general theory of relativity not through logical inference from the special theory, but because

> . . . every intellect which strives after generalization must feel the temptation to venture the step towards (it). (Einstein 1961,p.61).

In a similar manner, we are proceeding in the direction of an integrated theory from a conviction of the holistic nature of human life and experience, and from a perception of the promise which such an approach appears to hold both for the theory and for the domain of application of human geography. Absence of empirical verification is not, therefore, so much an indication that the integrated theory is wrong as that theoretical development needs to occur before such corroboration can be accumulated.

We recall, secondly, in support of this mitigating argument, that, in the model of a Lakatosian research programme, more flexible standards are applied to immature than to mature programmes. If, Lakatos argues, the demand for empirical support were to be applied with equal strictness to every point in a research programme, new theories would not have the breathing space they need

to establish coherent conceptual foundations. From this perspective, it is worth noting that the approach to the individual is a relatively recent phenomenon in twentieth century human geography, beginning effectively with Lowenthal (1961), and not well established until the work of Golledge, Tuan, Buttimer and other in the late 1960's and 1970's. The latitude accorded to an emerging research programme seems entirely appropriate, therefore, to the enterprise attempted in this chapter. It strongly suggests, moreover, that the primary task facing the construction of a scientific approach to the individual in human geography is not empirical, but theoretical. As we have consistently maintained throughout this dissertation, ideas and insights are prior, both logically and practically, to the identification and collection of facts.

Moreover, there are good grounds, as we have seen, for maintaining the inability of the separate approaches to behaviour and to cognition to function entirely within their own domains, without being forced to seek more secure foundations in other levels of individual functioning. That fact alone is almost sufficient to give precedence to the integrated account; and it may help to explain why the approach to behaviour, in particular, has been unable to develop a true research programme, without feeling the need to clarify at every step its conceptual foundations. The approach to cognition has been more fortunate, in benefiting from Piagetian theory and research, with its well-defined and pragmatic handling of the problem of its underlying conceptual foundations (Piaget 1971,pp.47-49). But even there, as we have seen, in the application of Piagetian theory to human geography--in cognitive mapping, for instance--unresolved conceptual problems relating to the definition of the primitive terms, and of their underpinning by notions like consciousness, have cramped their advance. There seem to be good reasons, therefore, for

attempting to construct an integrated theory in which these difficulties might find resolution, whatever the conceptual and empirical obstacles that might now exist, rather than continue to accept the restrictions that clearly attach to existing approaches, taken separately.

In conclusion, although we want to insist upon the value and legitimacy of pursuing an integrated theory of the individual in human geography, it is nevertheless clear that, in the form identified here, there are serious obstacles to accepting it as a scientific theory. We have seen that the statements employed to express assertions at each level of consideration vary in their ability to represent a generic account of functioning, with the result that as more fundamental levels of human functioning are located, the more difficult it seems to be to frame statements that are able to participate in the development of scientific explanation. These differences of invariance and reliability are compounded by the hierarchical relationship that obtains between the different levels, to create real difficulties for the internal coherence of the theory. Ambiguity and vagueness of primitive concepts, and the consequent deficiencies of formalization, substantially vitiate its predictive power; and again the problems are most acute at more fundamental levels, where the potentially greater power of the primitive base is effectively nullified by difficulties of definition and operationalization. The ability to predict and account for new phenomena, particularly at more subjective levels, is thereby seriously weakened.

Moreover, the extensive reliance on assumed bodies of theory seems to go far beyond what we would judge to be acceptable, in a scientific theory, and includes bodies of theory which are themselves lacking in scientific credibility. Even the assumption of such a substantial body of theory,

moreover, does not seem to make it possible to develop an account which has identifiable core and belt areas of theory, other than that which obtains superficially at the level of behaviour, and more credibly at the level of cognition. Finally, and inevitably, given the above conclusions, the corroborative status of the proposed integrated account seems to be almost entirely derived from Piagetian experimentation, to which at least some contribution has been made by human geographers, and is notably deficient at levels that are held to underlie human functioning as a whole.

Nevertheless, as our analysis has proceeded it has become more and more clear that the task of establishing the proposed integrated theory on a sound scientific base may not be as difficult as, at first sight, it may appear. We have found, as we have worked our way systematically through the requirements for scientific theory as they apply to the proposed theory, that the various conclusions derived from each step of the analysis seem to converge on one area of theory: that of identity and consciousness. The idea of an hierarchical relation between the different levels of human functioning, and consequently of the fundamental role of identity and consciousness in structuring the other levels, in itself directs attention to this area; but we have also seen that there are considerations peculiar to this level that single it out still further. We have noted, for instance, that the ambiguity and imprecision of primitive terms is greatest at this level; that here formalization is least well developed; that therefore the predictive power and testability, though potentially greatest here, are relatively weak; and that the philosophies from which the geographic approaches are derived, are the least well-structured as scientific theories. We have seen, furthermore, that core and belt areas of theory are most difficult to identify at this level, in consequence of these

structural deficiencies, and that the corroborative status not only of the geographic derivatives but of the philosophic theories in their original form is remarkably poor.

If, therefore, the integrated account proposed here is to achieve the status and credibility of a scientific theory in the framework of a genuine and fruitful scientific research programme--and we have argued that there are, in the context of human geography as a discipline, good reasons for seeking such a theory--we may suggest that the greatest single step towards it may be the development of a scientific approach to the structure and function of identity and consciousness in the human individual. Though it is not clear, at this stage, where such an approach might be located, we can at least infer some general things about it. To begin with, it seems likely not to be derived from a more intensive investigation of phenomenology and existentialism: the principles and methodologies of both these philosophic positions have been widely canvassed over some decades without producing, as we have noted, the kind of theory or corroboration required in a genuine scientific research programme. A thorough-going recasting of the principles along more formal lines may be possible (though the need for it has not anywhere in the discipline been foreshadowed), but the difficulties and unproductiveness of the methodologies of investigation suggest it is not likely to be worthwhile. By way of corollary, we may project that a successful scientific approach to identity and consciousness will find its rigour and degree of formalization, and hence its predictive power, in its ability to render these abstract areas of human experience subject to systematic investigation. Only then can the relationship between theory and fact, essential to the success of a scientific research programme, be established. Finally, it hardly seems necessary to

repeat, it will be a theory whose constituent statements are not restricted to a particular cultural, spatial or historical context, but to human kind as a whole. All these inferences are supported, it will be remembered, by Nagel's remark that

> . . . in comparison with the variables employed in the past in proposed transcultural laws, the concepts required for this purpose may have to be more "abstract", may need to be separated by a greater "logical gap" from the familiar notions used in the daily business of social life, and may necessitate a mastery of far more complicated techniques for manipulating the concepts in the analysis of actual social data. (Nagel 1961,pp.465-466)

That the construction of such a theory presents formidable challenges is not in question. Nevertheless, there seems to be no good reason to assume it to be unachievable. A number of contemporary and traditional approaches to these abstract levels of human functioning present themselves as possible candidates; and some of the most prominent of them are considered in the final chapter of this dissertation.

Before undertaking this analysis, however, we want to be assured that if our proposed integrated account of individual functioning could be realized, on the basis of a valid and fruitful scientific approach to identity and consciousness. it will uphold and further illumine the insights developed in the more traditional approach to the collective level of human functioning in the environment. This was established in the first chapter as a main objective of this dissertation; and we ought now, in the light of the foregoing analysis, to have the conceptual tools to undertake it. It is therefore to this task that we now turn.

CHAPTER V

TRADITIONAL TWENTIETH-CENTURY PERSPECTIVES ON HUMAN FUNCTIONING IN THE ENVIRONMENT

Outline of the Chapter

5.1 Geography as relational

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- The holistic tradition
- Hartshorne's discussion
  - Holistic and relational views
  - Science and complementarity
  - Unity and diversity

5.2 Determinism and possibilism

- The Darwinian foundaton
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- Reciprocity and holism

5.3 The Vidalian tradition

- The main principles
- Regional monographs
- Sorre; Demangeon; Vallaux
- Genres de vie
  - Vidal; Sorre
- Personality
  - Vidal; Sorre; Sauer
- Brunhes; Demangeon; Sorre
- Summary of principles
  - Terrestrial unity
  - Reciprocity of man-environment relationship
  - Landscape
  - Genres de vie
  - Personality

5.4 Human ecology

- Barrows
- Sauer
- The Chicago school
- Models of the individual
- Ecosystem approaches
- Effect of man on environment
- Critique of human ecology
- Summary

5.5 Summary and conclusions

- Feasibility of reductive approach to human geography
  - The idea of macroscopic theory
  - Rebuttal
- Summary of main themes of traditional geography
  - Model of the individual
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- Physical and biological functioning
  - Addition to the integrated model
  - Hierarchy well established
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- Holism
  - Connectedness of man and nature
  - Cybernetic mechanism
  - Application to the model
  - Priority of identity and consciousness
  - Subjective-objective integration

CHAPTER V

TRADITIONAL TWENTIETH-CENTURY PERSPECTIVES ON HUMAN FUNCTIONING IN THE ENVIRONMENT

5.1 Geography as relational

Speculation about the nature of the relationships between man and the environment, between culture and nature,is presumably as old as human thought itself. Certainly an investigation of pre-literature cultures would appear to bear out this proposition: in Australian Aboriginal culture, for instance, the relationship is evidently the cornerstone on which the whole cultural edifice is constructed and maintained (Maddock 1972; Elkin 1974). Glacken (1967) has amply demonstrated that ideas of the influence of the environment upon man, and of the effect of man in modifying the environment, can be consistently traced throughout the development of Western thought, from classical to modern times. And although, in the latter tradition, this concern has not been confined to geographic thought, geographers have nevertheless consistently played a central role in formulating and codifying theories of man and the environment.

A cursory examination of any reputable review of geographic thought, such as James (1972), confirms that, perhaps more than any other single theme, this notion of relationship has provided a framework for geographic, and particularly human geographic, enquiry. Even twentieth century geographic thought with all its vagaries and turbulence, seems to testify to the

centrality of this concern. Thus William Morris Davis, the father of modern American geography, in a famous essay (Davis 1909,p.8), remarked that

> . . . any statement is of geographical quality if it contains a reasonable relation between some inorganic element of the earth on which we live, acting as a control, and some element of the existence or growth or behaviour or distribution of the earth's organic inhabitants, serving as a response; more briefly some relation between an element of inorganic control and one of organic response.

Man, it is clear, is included as an "organic inhabitant" of the earth. The essential terms of this view, the relationship of man and environment, is, as Buttimer (1971) has shown, maintained in the French school, though in a more holistic and less causal manner. A classic statement is that of Sorre's (1961,p.3):

> The first problem of human geography is the elucidation of the relationships between man and milieu, considered from a spatial viewpoint. Those relationships are reciprocal, since our technologies both modify the natural environment and must adapt to its demands. . . . In large measure human geography presents itself as an ecology of man.*

On the other side of the Atlantic, Carl Sauer, although he changed his mind about the central concern of geography several times in the course of his long career, in the end affirmed that

> . . . we need to know much more of the impact of human cultures on plant cover, of man's disturbance of soil and surface, of his relation to the spread or shrinkings of individual species, of human agency in the dispersal and modification of plants. . . . We cannot fail to be concerned with man as a steadily increasing dominant of the living world . . . (Sauer 1956a,p.399)

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* Where standard translations of French authors are available, they have been used. Where they are not, as here, the translation is the author's.

It is evident, however, from the above quotations, that although it is widely agreed that "It is this regard for the interaction between human activity and natural circumstances that has epitomised so much that is challenging in geographical studies" (Eyre and Jones 1966,p.6), the precise nature of such interaction is a matter of much dispute. A central question is to what extent man (or human activity, or culture) can be separated from his environment (or natural circumstances, or nature); and therefore to what extent interaction, or relationship, are appropriate terms. Historically, the foundation of geographical thought has been laid within an emphatically holistic view of the world: Humboldt, for instance, asserted that

> Nature considered <u>rationally</u>, that is to say, submitted to the process of thought, is a unity in diversity of phenomena; a harmony, blending together all created things, however dissimilar in forms and attributes; one great whole . . . animated by the breath of life. (Humboldt 1849,pp.2-3)

This notion, though shared with contemporary philosophers and poets, was seen by Humboldt as accessible to an empirical investigation of the world, through direct observation and measurement of data (Bowen 1970,p.222). Yet so quickly did intellectual fashion change, following the death of Humboldt in 1859, that Marsh could write by 1864 of <u>Man and Nature</u>: <u>Physical Geography as Modified by Human Action</u>, though the fundamental interconnectedness of things is an idea that survives in his footnotes (Marsh 1864,pp.548-549). Bowen notes that

> Professional geographers of this period had little sympathy with Humboldt's philosophy of science and his concern for synthesis. They accepted his regional studies and empirical methodology but were inclined to dismiss the rest of his work as outdated romanticism. (Bowen 1970,p.225)

In the twentieth century, however, the concept of a unified world flourished in the twin concepts of a real association and region, based on

Hettner's view of geography as a chorological science (Dickinson 1969, pp.116-118). The tradition of French geography was strongly imbued with this belief, following Vidal's dictum that

> The dominant idea in all geographical progress is that of terrestrial unity. The conception of the earth as a whole, whose parts are coordinated, where phenomena follow a definite sequence and obey general laws to which particular laws are related. . . (Vidal 1926,p.7)

This conception is borne out in the famous regional monographs to which it gave rise, and in the notion of genres de vie (see discussion later in the section). The view of man as inseparable from his environment has come to be accepted in a number of recent works that have attempted to deal in general terms with modern geographic thought (for example Wooldridge and East 1951,p.25; Jones 1965,pp.16-17; Minshull 1970,pp111-112). The use of biological models, such as the ecosystem, or of systems theory, have (as will be shown) strengthened this position: Ackerman (1963,p.435), for instance, identifies geography's overriding problem as

> . . . nothing less than an understanding of the vast, interacting system comprising all humanity and its natural environment on the surface of the earth.

An important discussion of this controversy was undertaken by Richard Hartshorne (1939; 1960) in two influential books concerning geographical methodology. In the first, The Nature of Geography, he begins by conceding that the study of the relationship between man and nature has been "for half a century the dominating concept in in American geography" (Hartshorne 1939,p.120); but distinguishes between a study which focuses on the links between the phenomena,and one which focuses on the phenomena taken as a sum total, interconnected in an area. He goes on to argue that the latter is the truly geographic view, the former appearing in periods of temporary aberration,

then to be followed by a restoration of the "balanced view". In support of this assertion he points to the work of Ratzel and his successors--particularly Davis, Semple, Huntington, and Taylor in the U.S.A., and Bruhnes and Vallaux in France--as representing the aberrant view, and carrying on studies "in areas that must at least be regarded as transition zones, if not definitely parts of other fields" (Hartshorne 1939,p.125). The mainstream is located in the chorological emphasis of Hettner, the regional approach of the Vidalian school, and the human ecology of Barrows. It is perhaps of some significance that the greater part of Hartshorne's opposition to the notion of relationships therefore seems to rest on the somewhat sterile grounds that it is not really geography, and that it has led to extreme forms of environmental determinism, and therefore to ridicule from commentators in other disciplines--concerns whose damaging effect on the discipline have been cogently argued by Sauer (1956a). The one substantive criticism of the relational view is that

> a definition of a field of science in terms of causal relationships , robs him of his impartiality, and easily leads to dogma. (Hartshorne 1939,p.126.)

An associated problem is seen to be the lack of specification of concrete phenomena, and the consequent lack of distinctive method implied.

In this Hartshorne is drawing heavily on Sauer (1931), and it is not easy to see the logic of the argument. Certainly contemporary philosophy of science does not rule out <u>a priori</u> notions in scientific enquiry: on the contrary, it is widely agreed as we have seen, that only by having such notions to submit to empirical test that science can proceed deductively (Popper 1959; Nagel 1961; Hempel 1966). A phenomenon must be presumed to exist, to be subject to empirical investigation, before it can be investigated. Moreover, criticism on the grounds of intangibility is contrary to Hartshorne's own later conclusion that

> . . . the limitation of geographic study to things observable by the senses is founded neither in the logic nor in the history of the subject and does not provide a basis for restricting even a central core of the field which would be unified and complete in itself. (Hartshorne 1939,p.218)

This view is, furthermore, contrary to his previous acceptance of the idea that both relational and holistic approaches to man and nature exist, and that the difference is a matter of differences in the focus of attention; and finally to his later statement that

> since geography is a science developed by and for men . . . the relations between the world of man and the non-human world are of the greatest concern in geography. (Hartshorne 1939,p.299)

A more cogent attack is mounted in the second work, Perspective on the Nature of Geography (1960,pp.50-64). Hartshorne here argues that the "naively given factors" with which geography is concerned are "earth features", which are complexes whose composite nature can be analyzed into human and natural elements only in an arbitrary way. Such an attempt is seen inevitably to lead to a tendency to overlook or simplify important aspects of human society, and hence to conceptual confusion, by placing "cultural factors" on the causal side of the equation; a confusion which extending the definition of the term "environment" to include sociological or ideational notions is seen rather to exacerbate than to alleviate. Moreover, it is argued that the process of assigning elements to cultural or natural categories must be complete before explanation can yield sound conclusions; and since such completion is admitted to be a theoretical idea, and not in practice attainable, Hartshorne concludes that relational explanation "can yield only hypotheses of low reliability", and will prevent the cumulative progress of such studies towards greater accuracy and certainty, since "one knew that later still more thorough research might

appear to demonstrate entirely different conclusions" (Hartshorne 1960, pp.53-54). After locating in the determinist-possibilist debate the central problem of "environmentalism", Hartshorne reaffirms his belief in the priority of "the total character of areas", and clarifies his view of relationships within that framework:

> Geography, in seeking to analyze the complexity of integrated phenomena in reality, is concerned to examine relationships among phenomena, of whatever kinds, which are found to be significant in the total integration. In many cases such relationships may be those between human and non-human phenomena, in others between animate (whether human or non-human) and inanimate phenomena, or between visible and invisible, or between material and non-material. But no one of these dichotomies is logically of any more significance to geography than any other; in every case it is the particular nature of the phenomena which determines the relationships. (Hartshorne 1960,p.64)

And finally:

> In describing and analyzing individual features and elements, we are free to utilize whatever categories of classification are empirically significant to the study of their interrelationships, without concern for the abstract distinction between those of human origin and those of natural origin. (Hartshorne 1960,p.64)

A number of things can be said about this point of view, despite some obscurity in the manner of its expression. Firstly, it now becomes clear that Hartshorne is not denying that interaction between man and the natural environment exists, but that it is unilaterally causal,from nature to man, and that it is pre-eminent among the different types of relationships, taken together.

Secondly, although in theory there is an infinite variety of relationships, those deemed to be of importance are those which are "significant in the total integration", "empirically significant". It is difficult to extract any real meaning from the second of these phrases, but if

it is intended to equate with the first, then it is clear that Hartshorne is here advocating a systems view of interactions of man and environment; though such a view does not accord well with his emphasis on "the total character of areas".

Thirdly, it is manifestly incorrect to characterize explanation in terms of geographical relationships as unscientific on the grounds of incompleteness, of probability, or openness to future refutation. All are factors which, as we have seen, contemporary philosophy of science recognizes as an essential part of scientific explanation. To insist on any one of them as a criterion which science is compelled to meet would render unscientific nearly all existing scientific explanations.

Fourthly, it seems that Hartshorne is caught between two views of the real world, which, though apparently irreconcilable, can, as was shown in section 3.2.2, be seen as complementary. On the one hand, inasmuch as science deals with phenomena, and phenomena are admitted never to exist in isolation, relationships are real and important in accounting for the dynamics of the real world. On the other, the world appears to display, if not a total unity, at least degrees of holism in which elements are presented as indivisible wholes. The task of geography is then to approach these wholes. Yet it can only do so through their elements and their relationships; and it can only establish which of these are of relevance by reference to the whole in which they are embedded.

We may conclude, therefore, that Hartshore's discussion reflects most of the ambiguities and uncertainties that have attended the discussion of relationships in geographic thought; but it does something to clarify, though little to resolve, the central issues. In what follows, it will be argued that this confusion derives from a genuine logical paradox in the notions of unity

and diversity; and that in this paradox are further implications for that integrated theory of the environed individual which we seek to underpin the theory of human geography.

5.2 Determinism and Possibilism

The intellectual milieu within which twentieth century views of man-environment relationships developed was essentially the work of Charles Darwin. Darwin saw himself as the intellectual successor to Humboldt, whom he claimed as his mentor and inspiration. Like Humboldt, he saw a fundamental unity in the world, a unity expressed in natural laws, and found at every point and at every scale of the natural world. In the famous passage which concludes The Origin of the Species, he was moved almost to poetry on the subject (Darwin 1859,p.374) Yet Darwin's unity is fundamentally different to that of Humboldt's Kosmos. For Humboldt, unity was achieved by virtue of the humanizing of the environment: nature was made in man's image imbued with all the spiritual qualities that were evident in the real nature of man. For Darwin, on the other hand, man was a biological being, whose apparently higher powers conformed to the same laws that governed the operation and progress of all organic life. This, above all, is the substance of the watershed which Darwin's thought has, in retrospect, come to represent.

Stoddart (1966) has identified four major contributions that Darwin has made to geographic thought. They are the notions of time and evolution, particularly within geomorphology and biogeography, though extending to the analysis of cultures and landscapes; of organization and ecology, particularly in considering man's place in "the web of life", and extending to organismic views of man-environment complexes in specific areal associations; of selection and struggle, particularly in determinist approaches to the nature of the man-environment interaction; and of randomness and chance, in combination with the operation of natural laws, which notion Stoddart felt had been neglected in

geography (but which surely has been the central element in the attempt of the last two decades to employ stochastic models in the analysis of human spatial behaviour). In essence, every major movement in the progress of twentieth-century geography is here.

Furthermore, it is clear that Darwin's view of man in nature was unflinchingly holistic, and extended the scope of his enquiry to encyclopaediac dimensions. Thus he dealt with man as individual, constituted of all the attributes that are commonly held to distinguish human from non-human; with man as a social being, relying crucially on his relationships with other men for support and approval; with man as a collective entity, a population subject to the same laws of check and balance as are all organic beings; and with man as a species, evolving over time, and even now giving rise to new races, and perhaps new species. The questions raised by each of these domains is now a discipline in itself: Darwin wanted answers to all of them.

One can trace these concerns from his earliest to his last writings. Thus in his journal of the voyage of the Beagle (Darwin 1839), his observation of indigenous peoples led to an hierarchical mode of thought concerning the organic world, in which man was already seen as inextricably embedded, and to speculations about the relationship of cultural habit and the environment in which it arises, and possible biological mechanisms by which such habits might be transmitted. The M and N notebooks of 1838-1856 (Gruber 1974) were specifically set aside to record an astonishingly rich variety of hypotheses concerning the place of man in nature: the mechanisms of heredity; the nature of emotions in animals and in men; the nature of habit; instincts in man, in animals, and in plants; language in man and animal; the nature of human intelligence and morality; the relative attributes of different races; and the

possibility of developing mental powers, were all examined. These themes were partially formalized in two seminal essays of 1842 and 1844 (Darwin 1909) in which the newly established principles of variation, inheritance, and selection were applied to habits, to instinct, to mental faculties in man and in animals (and here we may see the foundations of sociobiology); and to particular aspects of human physiology.

In The Origin of Species (Darwin 1859), these concerns are contracted into a focus on the extent to which man is subject to the great laws of organic life. Man and nature are treated as one, and laws established in the human domain are freely applied to the non-human: it was, after all, Malthus's treatise on the growth of the human population that crystallized Darwin's thought into the principle of natural selection. The view which Darwin here takes of man is indicated by the phrase "slow-breeding man" (Darwin 1859,p.53), which, when placed in the context of the reproductive capacities of plants and elephants, serves perfectly to illustrate the overriding picture of organic man in the organic environment. Stoddart (1966,p.688) observes in this "the idea of the interrelationships between all living things and their environment", an idea which, following Haeckel, is seen as giving rise to the twentieth-century science of ecology.

However, in the Descent of Man (Darwin 1871) the breadth of hypotheses and associated evidence concerning man that existed prior to the publication of The Origin of Species is fully and systematically explored. Darwin always insisted that the two books should be read together; and it is clear that it was only once the formalization of the first was complete that the second could be undertaken. An explanation of the apparently special place of man in nature depended on the establishment of the connections which united them. In a very

real sense, as Darwin was fully aware, the unique properties of the human species, such as cerebration, socialization, and civilization, constituted the most formidable of all challenges: his theory, if it was to justify the fundamental position he demanded for it, must be able to provide an explanation of the entire range of human characteristics; and he did not shrink from putting it to such a test. Thus not only was human physiology, anatomy, and structure, but human instinct, intelligence, technological knowledge, the ability to abstract, the notion of individuality, emotion and its expressions, language, the sense of beauty, religious belief, social morality, and nature of civilization, all brought within the ambit of the theory. To Stoddart (1966,p.689), this work was an attempt to treat "modern man on the level of all living beings"; and that is true, but only if modern man is understood to include everything that makes man what he is: his cities, his arts, and his sciences, as well as the whole of his psychophysiological structure. It was Darwin's profound belief that all these attributes were fundamentally biological, and hence subject to the same laws that appeared to direct the progress of all organic life. It is easy, in retrospect, to find much to ridicule in this view; and yet it is as well to recall that the most recent attempt to demonstrate its truth (an attempt, it may be noted, which proceeded along conceptual lines identical to Darwin's) occurred within the last decade (Wilson 1975; 1978), and, although hotly debated, has at least been treated as a serious attempt to account for human nature, human behaviour, and man's social and cultural productions.

Darwin's view of man in nature is commonly seen to enter geographic thought through the determinist school. Identified with this school are Davis, Semple, and Huntington, and its roots are invariably traced to Friedrich

Ratzel. Ratzel was trained as a biologist, and his dissertation dealt with the significance of Darwin's ideas; and yet he later came to be critical of Darwin's mechanistic, evolutionary approach, and to move closer to philosophy than to biology (James 1972,p.221; Dickinson 1969,p.66). It is perhaps understandable that there has been considerable confusion about the precise position Ratzel took on determinism, since, like another great geographer and teacher, Carl Sauer, he published an enormous amount during his long career, and changed many of his fundamental views a number of times. Moreover, some parts of his work were far better known than others: it was Sauer who brought to the notice of his colleagues the "unknown Ratzel" of the second volume of Anthropogeographie, where cultural mechanisms, particularly of diffusion and migration, are specifically dealt with (Sauer 1941b,p.356).

The focus of those who regard Ratzel as more determinist than anything else has been his account of lebenbsraum. Central to this notion was the idea of the political state as an "earth-bound organism", "a piece of humanity and a portion of the earth" (Dickinson 1969,p.69). Association of a population with area, expansion with increase of population, and natural or human checks on expansion were the implications that followed. Although Ratzel himself emphasized that this was an illuminating analogy, and not a scientific hypothesis, later geographers attempted to take it in the latter sense, which made it deterministic: in fact it was the holistic aspect of the state, its natural and human ecology, that Ratzel was concerned to emphasize. He was, moreover, firmly of the view that the social characteristics of a society were relatively autonomous, a view that Durkheim, in a famous debate, pressed upon him, in the mistaken belief that his apparent determinism was all embracing. If he could affirm that

> We see in soil...the source of every servitude....It regulates the destinies of people with a blind brutality

he could at the same time state quite unequivocally that

> In vain men have sought for characteristics in the rocks of the earth and in the composition of the air by which one land may be distinguished from another. The idea of great, lasting, conclusive, quantitative variations in different parts of the earth is mythical...(in Buttimer 1971,pp.36-37)

The apparent contradictions are not logically irreconcilable; and one suspects that the rhetorical nature of early twentieth century polemic has not been sufficiently allowed for.

A similar deficiency of critical insight has bedevilled a more famous work of apparently deterministic thinking, Ellen Semple's Influences of Geographic Environment (1911). Semple was a devoted disciple of Ratzel's, and wrote her book with the express intent of providing "a simplified paraphrase or restatement of the principles embodied in Friedrich Ratzel's Anthropo-geographie" (Semple 1911,p.v.). Buttimer (1971,p.29), amongst others, has pointed out, however, that although many British and American geographers took this aim as an expression of achievement, in reality Influences was based primarily upon volume 1 of Ratzel's work, thus placing his thought far more completely in the determinist mould than a balanced reading, as later advocated by Sauer, would suggest is justified. Moreover, Semple approached the relationship of man and the environment from an entirely different discipline to that of Ratzel: her historical perspective is in significant contrast to the former's biological cast of thought, and must, in the end, be seen as truly original.

The importance of Influences in twentieth century geography is emphasized in Wright's (1962) amusing and affectionate account of its "bibliobiography". Not the least perceptive of his insights is a recommendation to the reader that

Semple's true position must be sought beneath the apparently absolute categories she employs. And, indeed, nothing could be more circumspect, or clearer, than this introductory statement:

> The eternal flux of Nature runs through anthropogeography, and warns against precipitate or rigid conclusions....For this reason the writer speaks of geographic factors and influences, shuns the word geographic determinant, and speaks with extreme caution of control. (Semple 1911,p.vii)

Moreover, she specifically rejects Ratzel's organismic analogy of society and state, in its more extreme, Spencerian forms, retaining simply "the life-giving connection between land and people" (Semple 1911,p.vii). The much-quoted opening of the book is invariably taken to be an archetype of determinist modes of thought, when it is nothing of the sort: it is above all a poetic metaphor, intended to compress into one striking image the sweeping horizon of her theme; and to take it as a statement of scientific theory is entirely to misread it. It is, moreover, immediately followed by an example of just such a scientific assertion:

> Man's relations to his environment are infinitely more numerous and complex than those of the most highly organized plant or animal. (Semple 1911,p.2)

She then goes on to point out that it is not her intention to deny the effect of man on nature, simply to restore the balance by examining the reciprocal influence in particular.

The point needs to be laboured because, if one were to be guided by the recognized reviews of twentieth century geographic thought, one might come to believe that Semple's entire work and thought is encapsulated in the first paragraph of Influences. As an image, there is some truth in this view; but as systematic conceptual exploration it represents her real achievement hardly at all. In the exposition which follows it, the complexity of the geographic

environment and the extent of interaction of its constituent elements are constantly emphasized. Economic and social life are seen to intervene between the environment and its cultural products. Even the conception of environment itself is widened to include the history and culture of a people, which are said to "embody the effects of previous habitats and of their final environment. . . . It involves influences emanating from far beyond the borders" (Semple 1911,p.29). Above all, the earth and the human population which inhabits it is to be viewed as a whole:

> Each country or sea is physically and historically intelligible only as a portion of that whole....As the earth is one, so is humanity. Its unity of species points to some degree of communication through a long prehistoric past. (Semple 1911,p.30)

Clearly, if this is determinism, it is a most unusual view of it, in placing historical, cultural, and physical factors on an equal footing, and in seeking the nexus between them. The fact is that Semple came to a consideration of man and environment as an historian cast in the mould of Toynbee, and reflecting in her language the fashion of her time: no one who has read other works of hers (Semple 1903; 1931)--or, indeed, who has taken the trouble to finish Influences itself--could find much justification for the extreme determinist label she received from later commentators.

William Morris Davis, on the other hand, is unrepentently determinist in his view of man and nature. The quotation given at the beginning of this section is said to be typical of his position, and to derive from Darwin and his much-abused interpreter, Herbert Spencer. The environment, or, more precisely, the physiographic environment, presents its demands, and man responds. And yet man is not alone: Davis is careful to include man only in an overall category of organic life, as he emphasized in another essay. In a complete physical geography,

> ....it is extremely desirable that plants, animals, and man should receive frequent mention...in illustration of the organic consequences that follow from controls exerted by physical environment or organic opportunity. (Davis 1909,p.144)

Moreover, it is clear that Davis's notion of response is a broad one: it includes cultural diffusion, the geography of languages, and the nature of regional differences of human groups, all of which are themes normally associated with opponents of determinism. Finally, it must be noted that Davis, for all his ascription to a deterministic view, made very few remarks about man: he is, in fact, strong in his view of physical geography as excluding "organic forms". In the context of the total body of his work, these much-quoted statements are simply a few unsystematic, unresearched, off-hand remarks about an area of geography in which he claimed no expertise. One suspects he would have been surprised to see them taken so seriously.

It would appear, then, that the vehemence with which these early writers have been attacked as determinist may be largely misplaced. In retrospect it seems that they were exploring a model of man-environment interaction, in which they held as constants the effect of man on the environment, and the role of history, culture, and sociology in human society, and focused simply on the influence of the environment. None of the leading writers of this school denied the operation of these other factors: indeed, Huntington, who came to be regarded as the arch-determinist, because of his pronouncements on the effect of climate on human populations (Huntington 1915), in his later work emphasized the importance of intervening cultural factors, such as diet, in providing the energy base for human activity (Huntington 1945,p.417).

The reaction to determinism, or rather, the reaction to determinist excesses, was widespread, and laid the foundation for a tradition which has

since dominated geographical thinking. Arguments presented by Sauer (1925; 1931) are typical of this reaction. Sauer opposed what he saw as the mechanistic philosophy of determinism, the notion that the environment exerts some force over man, which threatened to make geography "part of biophysics". The effect of the environment on man was not denied, but was seen to be purely "somatic"; or, to use the modern terminology, physiological. It was therefore outside the domain of the geographer whose task it was rather to elucidate the cultural structures ("tabu or totemism") or individual actions (of "his own will") that gave rise to a particular pattern of use of the environment (Sauer 1925,pp.348-349).

What Sauer particularly objected to was the a priori nature of determinism:

> Geography under the banner of environmentalism represents a dogma, the assertion of a faith that brings rest to a spirit vexed by the riddle of the Universe. (Sauer 1925,p.348)

The consequences of such a faith were, in Sauer's view, a number of insoluble methodological problems (Sauer 1931,p.31). No science, he claimed, is characterized by a particular causal relation. Furthermore, no science saw its domain as relations, rather than phenomena: determinism therefore lacks not only objects of inquiry, but a characteristic method. Finally, any determinist study cannot avoid "special pleading", in the attempt to demonstrate environmental adjustment. The modern philosophy and methodology of science, with its emphasis on a priori theories and deductive logic as its foundation, would not support the view that the charges laid by Sauer need necessarily make determinism an unscientific hypothesis; but there is no doubt that, in its "excesses", special pleading was real and laid these studies open to such trenchant criticism.

It is interesting to note that geography was not the only discipline of the time which moved away from deterministic to probabilistic explanations. Anthropology, for instance, gained ground on the general acceptance of the primacy of cultural factors in the man-environment interaction: Boas, Kroeber, and Forde, among mainstream anthropologists, were in the forefront of this movement; and, chiefly through the work of Vayda and Rappaport, the new bridging discipline of cultural ecology was developed (Grossman 1977, pp. 127-128). Kroeber, indeed, became Sauer's collaborator. Moreover, the entire shift of the centre of gravity, from environment to man, and consequently from certainty to probability, took place in concert with a ferment in the physical sciences in which, through Heisenberg, Schrodinger, and Bohr, it appeared that the fundamental laws governing the physical environment were probabilistic, and lacked deductive certainty (Jammer 1974); how much more so, then, with the study of man in the environment. At about the same time, Godel demonstrated that no axiomatic system can provide a complete picture of reality (Jeffrey 1967, ch. 10; Hofstadter 1979, pp. 15-19). Darwin had already demonstrated the operation of chance in natural selection, an element which had been largely ignored within geographic determinism (Stoddart 1966, pp. 695-697). The time, it is clear, was ripe for such a change. As Ackerman (1963, pp. 430-431) notes, in geography this movement gave rise not only to an alternative school of thought, that of possibilism, but ultimately to the burgeoning of statistical techniques and stochastic models of recent years, an application which is still central to contemporary human geographic paradigms.

Although Vidal (whose work is considered separately in the next section) is usually credited with the formulation of possibilism as a concept opposed to that of determinism (Sauer 1931, p. 31), it was Febvre, an historian by

training, who provided its most incisive polemic. In his view, "natural regions are simply regions of possibilities for human groups": such possibilities vary in both quality and number from region to region (Febvre 1925, pp. 181-182). All the elements in the life of a people are seen to be relevant in determining the degree to which possibilities are used; and, if "civilizations" in similar areas (such as mountains) are comparable, that is simply a reflection of the fact that sets of possibilities have been used in similar ways, rather than a demonstration of the determinist thesis (Febvre 1925, p. 200). More and more, he argued, man was becoming the dominant agent of change in the environment: "the grip of man on the earth" was seen to be stronger than in the past, though the historical continuity of this effect was fully recognized (Febvre 1925, pp. 354-356). Thus man and the environment were to be approached as acting reciprocally on each other, and not as cause and effect; the focus was on the nature of the interaction, not on influences:

> "Relations" is a sane word and its past is not wrapped in fog and obscurity or steeped in occultism. (Febvre 1925, p. 361)

Febvre looks specifically to the Darwinian theory of chance for support; and, using the rather un-Darwinian (though now orthodox enough (Piaget 1971, pp. 195-201)) analogy of preadaptation, goes on to argue the primacy of cultural social habits in determining how the environment is to be used:

> We must not say blindly that "such and such a region necessarily constrains its inhabitants to adopt such and such a way of life," but rather that under the powerful action of organized and systematic habits "gradually deepening their ruts, and thus imposing themselves with greater and greater strength on successive generations, stamping their mark on minds, and giving a definite trend to all progressive forces" the aspect of a country may be completely transformed. (Febvre 1925, p. 368)

One might be excused for seeing in this a new determinism, this time cultural or social, rather than environmental. Tatham (1951), however, in a

discussion of the main principles of possibilism, emphasizes that this is not so. On the contrary, it is the notion of terrestrial wholes, of unity, that is fundamental: this is an idea developed to its greatest extent by the Vidalian school, and is examined in the later discussion relating to it. Beyond that, Tatham with Febvre identifies man as the starting point of the analysis of such wholes; and, in particular, man's activity in modifying both inorganic and organic components of the unified complex. He confirms, too, the importance within possibilism of habit, and particularly of habitual "modes of thought, long-cherished ideas" (Tatham 1951, p. 158), which may detract from, or contribute to, utilization of the environment. He stresses, however, that in no way does the possibilist position deny the power of environmental conditions in limiting the opportunities from which man is free to select; though these limitations are seen to derive both from the nature of the particular environment and from the historical development of the technology and culture that seeks to make use of it. Thus every action of man, chosen freely and derived often from historical or cultural contexts, elicits from the environment a reaction: indeed, the increasing power of man to modify the earth is seen in the evidence that "The more imposing and glorious man's conquest, the more cruel the revenge of the thwarted facts" (Bruhnes 1920, p. 611).

The determinist-possibilist debate was long and bitter, and, in retrospect, essentially sterile. On both sides, the argument reached the height of absurdity. Thus Platt (1948, p. 128), for instance, in his attack on determinism, affirmed that "full explanation, in fact, involves the whole world and everything that has happened in it," and that an acceptance of the philosophical doctrine of individual free will implies an acceptance of the view that

> . . . for the future [these] decisions are not yet made, and there is no indication that we shall ever be able to make them in advance for our own future lives, to say nothing of our inability to make them in advance for the future lives of everyone else in the world.

Both statements are as much a travesty of the determinist position as of the nature of social scientific explanation. Martin (1951) in a similar, though opposite, vein insisted that the search for laws implied a cause-and-effect analysis and made determinism a logical necessity. Reviewing contributions to the debate (such as Barrows 1923; Montefiore and Williams 1955; Spate 195/; Jones 1956; Taylor 1941; Lewthwaite 1966) one is forced to conclude that the original concern--the nature of the relationship between a human population and its environment--became lost in the philosophic maze of a free will-determinist argument, and caught up in methodological arguments associated with the post-World War II movement to establish geography as a science; neither of which disputes was a necessary or a logical component of the original one. The wonder is that the confusion lasted for so long.

The simple fact is that the positions were in many important respects identical and were certainly not irreconcilable. As this brief review of some of the actual works of principal protagonists has, it is hoped, demonstrated, the determinist did not deny the importance of cultural, social, and historical factors, nor the possibilist the power of environmental factors, in the constitution of actual relationships. Eyre and Jones (1966, p. 17) are inclined to lay the blame for this apparently mutual myopia on

> the scope and perspective of many of their studies. . . . their minds tended to flit too readily to great abstractions. . . . They drew lines on maps of frighteningly small scale, and they drew their inferences from the same source.

As Barrows attempted to point out in 1923 (Barrows 1923, p. 3), and many others (such as Eyre and Jones 1966, pp. 16-17) subsequently, it is on the reciprocity

of the relationship that the focus must inevitably lie. This view is naturally associated with that more ancient and, at the same time, more contemporary notion of the earth as structured in unified wholes, in complex systems of human, biological, and physical elements; a view which, in recent times, has given rise to ecological and systems approaches. It is the genesis of this view in twentieth-century geographic thought that will now be examined.

5.3 The Vidalian tradition

Although many national schools of geography, among them the American, the British, the German, and the Russian, made fundamental contributions in the twentieth century to the theory and the research of man-environment relationships, it can be argued that the French tradition uniquely embodied its fundamental principles and practice. As Buttimer (1971, p. 1) observed,

> Unlike other geography schools of the twentieth century, which tended to treat man individualistically or as the pawn of economic law, the French maintained an Aristotelian vision of collective man as zoon politikon, organized into spatially recognizable social groupings.

And although, as a discipline, geography has had an unusually large number of eminent individuals whose thought has dominated the discipline for substantial lengths of time (one thinks of Ratzel, of Davis, of Sauer), none has had a more profound effect, through his own thought or through illustrious students, than Paul Vidal de la Bla che.

Vidal did not set down a systematic account of his thought, as did Brunhes (one of his students), but in several long articles, and in the posthumous collection edited by Emile de Martonne, there is sufficient to outline his basic views. Dominating them was an irresistible conviction of terrestrial unity, a conviction that is spelt out in both articles and lectures (Vidal 1913, p.290; 1926, p.7) and which Church (1951, p.73) notes is equated with "the interdependence and interconnexions of physical factors." Such unity chiefly displayed itself in discrete units:

> Every region is a domain where many dissimilar beings, artificially brought together, have subsequently adapted themselves to a common existence. (Vidal 1926, p.10)

This is clearly a view that echoes Ratzel's, and it has come to be regarded as synonomous with Vidal's use of the term "milieu":

> . . . the organically integrated physical and biotic infrastructure of human life on earth: "A composite . . . capable of holding together heterogeneous beings in mutual vital relationships." (Buttimer 1971, p.45)

> . . . the geographical bond which unites these individuals is strong enough to make them cohere, even to make of them a unit, by virtue of their dependence on one another. (Vidal 1926, p.165)

In this relationship, the influence of the physical environment is "sovereign" (Vidal 1926, p.17), and, following the fundamentally Darwinian cast of the contemporary geographic thought, can best be seen as progressively more inventive adaptation to environmental conditions:

> . . . man also depends on what chiefly distinguishes him, namely, his mind. His effort has been directed toward . . . whatever had the charm of a new invention. . . . There is an inherent element of progress in all struggles which result from the compulsion of environmental necessity. (Vidal 1926, pp.171-172)

Thus man is both active and passive: in conformity with the possibilist position, man exercises his freedom of choice--influenced, certainly, by a "social factor"--to adapt the environment to his needs. Each human group leaves its visible imprint on the landscape, giving rise to a distinctive "physiognomy of landscapes," which, it now becomes clear, is the dynamic product of an interplay between the conditions of the natural environment and the sociocultural characteristics of the group occupying it:

> Man utilizes not only inorganic agencies in his work of transformation. . . . he further collaborates with all living forces grouped together by environmental conditions. He joins in nature's game. (Vidal 1926, p.20)

Human geography was thus seen to centre on three foci: population, technologies and civilizations that arose in interaction with specific environments. Though not formalized in any single systematic book, these conceptions were nevertheless extremely fertile, and gave rise to a tradition

of geographic work that spanned almost half a century. The showpieces of this tradition were the great regional monographs that were produced largely by Vidal's students, often under his direct supervision. One such was Sorre's Les Pyrenees, published in 1922 by Armand Colin, the publishing house that became so intimately associated with the French school of geography. This work typifies the genre. Faithful to Vidal's dictum of terrestrial unity, it begins with a "vue d'ensemble," an overview; and proceeds through accounts of relief, of climate, of vegetation and fauna, and of rivers and streams to an exposition of the modes of living ("genres de vie") characteristic of the region. The last, which is a notion more fully discussed later in this chapter, embraces agriculture, husbandry, clearance of woodlands, settlement forms, social and political life, and movements of peoples. This pattern of approaching the region as a whole then becomes the model for description of the various sub-regions and localities.

A similar structure is evident in the many other monographs that appeared. Demangeon (1927), for instance, embarks on an account of Belgium and Holland from an initial overview of these regions as a unique area within the fabric of Western Europe, and of their common struggle against the sea derived from the fundamental patterns of relief. The more detailed work on localities and sub-regions follows Sorre's model, though coloured by Demangeon's own view of the importance of the humanized landscape--"field, house, village, route, town and state" (Dickinson 1969,p.212)--as a fourth focus for human geography. The approach is not significantly different from his early account of Picardy (Demangeon 1905), and is followed by many of Vidal's other pupils (as, for instance, de Martonne (1930)).

Despite the criticisms to which these regional studies were later subjected, there is no doubt that they were extraordinarily successful. They

seem to embody so much of what, in later decades, was felt to be missing in geographic work: insight and imagination, for instance (Sauer 1956a,pp.400-401; Wright 1947); and vigorous, subtle expression (Wright 1947,p.79). Reading them today, one is further struck by the breadth of expertise they must have required of the geographer: the best are in no sense superficial description, but derive their power from an intricate interlacing of accurate observation, significant statistical and cartographic facts, and what one can only call a "feel" for the country and its inhabitants. Indeed, the intent that underlies them is strictly empirical: they are seen as constituting the first step of inductive Baconian methodology, though the notion of uniqueness of localities and regions dominates the methodological assumptions, and limits the potential for generalization. There is, despite this intent, some irony in the success of the monographs, which fully reflects the strength of the a priori concepts. Informing the whole body of work is a conviction--one might better say, perhaps, an experience--of a fundamental principle:

> The dominant idea in all geographical progress is that of terrestrial unity. The conception of the earth as a whole, whose parts are coordinated, where phenomena follow a definite sequence and obey general laws to which particular cases are related. . . . (Vidal 1926,p.7)

It is difficult to think of any body of geographic work which has more faithfully or successfully shaped its contours to such a fundamental mould.

A crucial element in the success of this tradition was its coalescence around a central notion, that of genres de vie. It was, perhaps, the expressed version of the principle of terrestrial unity: in modern jargon, we might say that it operationalized the fundamental principle. It was given its first and fullest expression in Vidal's (1911) two-part article. As developed there, genre de vie is a concept of considerable power and subtlety. Vidal emphasizes

the effect that man has always had on his environment, particularly in recent times; and asserts this effect to be reciprocal, through the basic conditions of life:

> ....the action of man on nature, or of nature on man, operates principally through the intermediary of the vegetative and animal world, that is to say, of that infinitely supple and tenacious thing which is called life. (Vidal 1911,p.195)

The relationships developed are mutually beneficial: in any given area, the inhabitants, both vegetative and animal, are bound together by "a bond of reciprocal interest" (Vidal 1911,p.196), an arrangement to which Vidal, after Haeckel, applies the term "l'Oecologie". The role of man in such a system is to isolate those plants and animals that are of potential use to him and to insulate them from the selective pressures of the others. It is by modifying this pre-existing equilibrium, by becoming both a creator and a destroyer, that man acquires independence from the apparent random chance of natural conditions, and gives rise to a particular genre de vie:

> In order to constitute modes of life which render him independent of the fluctuations of daily nutrition, man has had to destroy certain associations of living beings in order to form others. He has had to group, by means of elements gathered from every side, his clientele of animals and plants, thus to become both destroyer and creator; that is to say, to accomplish simultaneously the two acts which encapsulate the notion of life. (Vidal 1911,p.200)

In documenting these conceptions in different environments--tropical, dry, woodland clearings, mountains--Vidal, in accordance with Darwinian principles, locates variety as centrally important in furnishing man with "multiple possibilities of intervention and of opportunities for initiative" (Vidal 1911,p.212). Moreover, genres de vie are not static: they change, grow, develop. The particular form they take is seen to depend on the biological foundations of life that are established: the plants selected, propagated, and

cultivated; the sites and habitats chosen for settlement; the pastoral activities developed; and the means of transport adopted. Genres de vie are therefore, in summary (Vidal 1911,p.304), unique to places and peoples: they have "an autonomy which attaches itself to, and follows, human nature", and they are the product of cultures, of habitual modes of thought and of transmitted practices, since "man is more a being of habits than of initiative". Above all, they are powerful agents of social formation, fundamentally structuring social groupings:

> They create and maintain among men, often in the same country, those social differences which, in that state of mixing into which, more and more, the civilized nations are subsiding, balance and will, in the end, dominate ethnic differences. (Vidal 1911,p.304)

Although genre de vie was a concept widely employed by geographers in the first half of the twentieth century, it was not until 1948 that a substantial evaluation of it appeared. Perhaps that fact testifies to the completeness of Vidal's original formulation: certainly Sorre's (1962a) discussion is firmly based on the main principles Vidal laid down. His original contributions centre on the modes of change of genres de vie. He recognizes that internal mechanisms will cause a particular genre de vie to develop through various stages to maturity (much as succession of plant associations has been modelled in biogeographic theory); that transportation of a particular genre de vie to a new environment will demand adaptive changes; and that new elements may be introduced by historical diffusion of cultural elements. As he emphasized in a later work, every genre de vie is further subject to the fluctuations of the socioeconomic and cultural milieu in which it is embedded:

> The description of the rural activity of a human group is part of a socio-economic complex--including its psychological elements. . . . The regime as a whole must be evoked to explain it, and not just an isolated characteristic of climate and soil. (Sorre 1961,p.85)

He goes on to analyze genres de vie in terms of the Vidalian concept of "circulation", which he views as "an essential condition of their existence and an agent, in some degree, of their stabilization" (Sorre 1962a,p.409). He attempts to answer the pertinent question of whether what is pre-eminently a rural concept is appropriate to the main geographic condition of mid-century life--urbanization--and concludes that the concept can usefully be extended in this way. Finally he notes the clear trend towards standardization of genres de vie as the isolation of regions is broken down on a global scale:

> The world seems to be losing some of its wealth and variety, and from our vantage point it seems that man has perhaps also suffered a profound loss. (Sorre 1962a,p.415)

The concept of genre de vie allowed the Vidalian school to come to terms with an experience that is embedded in many folk traditions as well as in the conventional wisdom of contemporary industrialized societies: that of the unique personality of a locality or region. Vidal employed the term ("La personnalite geographique de la France") to describe the national genre de vie ("la vie generale") that was constructed from the regional and local genres de vie ("la vie locale"). Yet the concept becomes, in Vidal's hands, a subtle one, going well beyond the simple notion of aggregation that the above terms imply. In a passage of remarkable clarity and fertility, he asserts:

> A geographic individuality does not result simply from geological and climatic conditions. It is not something delivered complete from the hand of Nature. The idea we must begin with is this: a country is a store-house of dormant energies, laid up in the germ by Nature but depending for employment upon man. It is man who reveals a country's individuality by moulding it to his own use. He establishes a connection between unrelated features, substituting for the random effects of local circumstances a systematic cooperation of forces. Only then does a country acquire a specific character differentiating it from others, till at length it becomes, as it were, a medal struck in the likeness of a people. (Vidal 1928,p.14)

It is therefore through interaction between man and the environment that personality emerges. In Vidal's conception, the human species, by virtue of "what chiefly distinguishes him, namely, his mind" (Vidal 1926,p.171), plays a crucial role in making manifest that structure which is unmanifest, latent, in Nature. It is particularly noteworthy that this structure has a unifying effect; though the uniqueness of the different elements that constitute national life remain, they are embedded in a flow of wholeness:

> An all-pervading atmosphere, instilling ways of feeling, methods of expression, tricks of speech and a particular kind of sociability, has enveloped the various populations whom fate has drawn together on the soil of France. Nothing has done more to draw the different elements into one. (Vidal 1928,p.72)

Evidently this "atmosphere", whatever its precise nature, operates at a fundamental level of the environment, and is able to exert its power of unification on every element of which the national genre de vie is constructed. It is, perhaps, too easy to dismiss such a conception as a poetic flight of fancy, rather than an attempt to put into words a clearly experienced, but abstract, phenomenon. Vidal is certainly unequivocal about its existence:

> There is, then, a beneficent power, a genius loci, which has rendered a national existence possible for France, and which imparts to it an element of wholesomeness--something indefinable that rises superior to territorial divergences. It balances them and combines them into a single whole: yet the variations persist; they still have to be reckoned with...(Vidal 1928,p.72)

It is the phenomenon that Hartshorne later grappled with, as discussed in the first part of this section, and which, we shall propose, carries particular implications for the integrated theory of the environed individual that we are seeking to develop.

More than forty years later, Sorre (1961) explored the structure of personality in a geographic area. Personality, he pointed out, was inseparable

from individuality of the landscape; and this in turn was related to a type of organization of the different geographic elements. Yet the real questions concern the things that give rise to organization:

> What sentiments, what modes of thought determine it? How is it modified in the course of a long contact with adverse factors? (Sorre 1961,p.269)

It is evident, he goes on, that different human groups have constructed different solutions to problems that appear, on the face of it, to be identical. This has meant that no two groups have evolved in the same way, since it is apparent that different milieux have produced different behaviour patterns. Such an analysis seems to demand a geography that is sensitive to the collective modes of thinking of human groups:

> Ethnography and collective psychology here come to the aid of geography, whose investigation finally opens out into a sort of psychological geography. (Sorre 1961,p.269)

The landscape; is, in sum, the "material and spiritual expression of a culture" (Sorre 1961,p. 269). The outline of a geography based on such principles was indeed attempted in 1939 by George Hardy; and, though severely criticized by traditional geographers like Demangeon, it is a remarkably fertile work.

It was through the concept of landscape that the notion of personality was applied in substantive geographic work. In a seminal article, Sauer (1925) explored in some depth the nature of landscape. Here the natural environment is seen to be acted upon by human cultural groups to produce a new artefact, the cultural landscape:

> The cultural landscape is fashioned from the natural landscape by a culture group. Culture is the agent, the natural area is the medium, the cultural landscape the result...(Sauer 1925,p.47)

Through the process of adaptation--in the case of men, as opposed to animals, a "largely subconscious" and "imitative" process--is derived

> the feeling of harmony between the human habitation and the landscape into which it so fittingly blends. (Sauer 1925,p.47)

All this is strictly in accord with Vidal's ideas, as Sauer himself often pointed out: for him, as for Vidal, and for Sorre, the mainspring of the interaction between man and Nature is to be located not in external circumstances, but in "the mind of man" (Sauer 1925,p.47). Later in his career, as is well known, Sauer came to emphasize more the role of historical processes in the formation of the cultural landscape (Sauer 1941b; 1974). Nevertheless, the notion of personality remained intact. In introducing his account of "The personality of Mexico" (Sauer 1941a,p.353), he remarked:

> The designation "personality" applied to a particular part of the earth embraces the whole dynamic relation of life and land. It does not deal with land and life as separate things, but with a given land as lived in by a succession of peoples, who have appraised its resources for their times in terms of their capacities and needs, who have spread themselves through it as best suited their ends, and who have filled it with the works that expressed their particular way of life.

The geographers of the French school were remarkably faithful to Vidal's ideas for nearly half a century. Though many new approaches to the relationship of man and nature emerged, the essential framework of Vidal's thought was retained almost entire. The development of the French school has been traced in detail by Buttimer (1971), and need not be recapitulated here; but the work of three eminent geographers may be briefly mentioned to establish the point.

Jean Brunhes was one of Vidal's first followers. His most significant contribution to the theory of man and nature was his 1910 exposition of the principles of human geography (Brunhes 1920). In this work he attempted to

systematize the analysis of man-environment relationships, and located at their basis the idea of livelihood, based in turn on what he saw to be the fundamental needs of the human physiology. The nature of the organized work of a community determines its social and political forms. The role of historical factors is explicitly recognized (Brunhes 1920,pp.590-592). Most importantly, a "psychological factor" is seen to intervene between the natural environment and the form of livelihood that arises within it:

> The human psychological element is, then, at the origin of the geographical fact, the necessary intermediary between man and nature, and might be called..."the direction of attention:...(Brunhes 1920,p.599)

The landscape created by man is further seen to be a construction of collective consciousness:

> The city is preeminently the "projection" of a collective mass of human wills. (Brunhes 1920,p.600)

Between man and nature there is a reciprocal relation: man is both passive and active, and natural elements assume significance only in interaction with the forms of human thought:

> the psychological influence of geographical causes upon the human being, in proportion to his own appetites, needs, or whims--this is the subtle and complex factor that must prevail; in every study of human geography...(Brunhes 1920,p.606)

Nevertheless, there are always limits to man's creativity:

> ....he does not suppress the natural facts--he modifies them, shapes them, interprets them. (Brunhes 1920,p.607)

Thus there would seem to be little ground for Buttimer's (1971,p.65) criticism of what she sees as "a more subtle determinism of livelihood" and a "obvious truncation" of Vidal's ideas. Rather it would appear that Brunhes is here attempting to analyze synchronically Vidal's essentially holistic

understanding of man-environment relationships, and thus allow its greater application to the contemporary world. Such an attempt must, by its very nature, lose something of the original inspiration: but much, too, of rigour, of clarity, and of precision, is gained.

Albert Demangeon centred his work on Vidal's concept of genres de vie with a particular emphasis on the rise of technologies, and added to them a concern for the distribution, density, and forms of human settlement. In his 1921 analysis of human settlements (Demangeon 1962), the natural environment is seen as providing the basic mould for settlement forms, through surface configuration, soil, and water resources. He recognizes, however, that attributes of human societies--such as cultural traditions, the need for security, and the agrarian regime--will have their own dynamic, independent of natural conditions. In particular, the type of agricultural economy, developed at the interface of the natural and cultural systems, will be a major determinant of settlement form. It is clear that Demangeon is much closer than Brunhes to a "subtle determinism of livelihood"; and certainly, if his review (Demangeon 1940) of Hardy's Géographie Psychologique (1939) is anything to go by, there is little doubt of his scepticism about the intangible factors of human thought that underlie the approaches of both Vidal and Brunhes, and his determination to find in the material conditions of life the roots of geographic expressions in the landscape.

The work of Max Sorre, as has been noted already, was firmly rooted in the classical Vidalian tradition. His range of interests was wide. In earlier works, an ecological viewpoint predominates, but he moved in his later career to a more humanistic position, in which the relationship between geography and sociology is paramount. The first of these phases is, for our purposes, the

more important. Thus, as Buttimer (1971,p.125) notes, his monograph Les Pyrénées (1922) clearly locates the genres de vie characteristic of the region in the physical and biological systems that comprise the natural environment: indeed the original dissertation on which it was based is sub-titled "An essay in biological geography". This concern was systematized in the first volume of his great work on the fundamentals of human geography (Sorre 1943), and is typified by a subsequent (1952) article outlining the geography of diet and human nutrition (Sorre 1962b). In the latter, diet is seen to be not only a result of the natural conditions--through climate, which determines energy requirements, and through the local ecology, which determines the availability of particular foods--but an expression of the belief system of the group, its socioeconomic structure, and its changing genre de vie (for example, during the process of urbanization). Thus diet is seen to be inseparable from "the other chapters of human geography" (Sorre 1962b,p.452).

In his last work (Sorre 1961) it is the classical Vidalian concept of interrelated unity that prevails. Man is "Man of relationships and groupings" (Sorre 1961,p.2); he interacts reciprocally with the environment:

> ....our technologies modify the natural environment at the same time as they have to adapt to its demands....the image of the milieu...is humanized by an interplay of factors....In large measure human geography presents itself as an ecology of man. (Sorre 1961,p.3)

We are urged never to lose sight of

> the profound unity of man, of man entire in each of his acts, and even in his contradictions. (Sorre 1961,p.3)

Yet the idea of the geographic individual is seen to be fundamental to the understanding of the relationship between groups and their milieux:

> ....the ecology of the group (synecology) is composed of the individual ecologies of its members. (Sorre 1961,p.48)

This is, of course, a proposition that underlies the argument of this dissertation. Above all, Sorre asks us to approach the oekoumene with "a rejuvenated imagination": there are no limits to our enquiry, since the oekoumene "extends into the Cosmos" (Sorre 1961,p.6). Only there do we find the true perspective on human action in the environment:

> The flowering of the oekoumene has occupied a tiny fraction of time....The reign of man will pass; he will be reabsorbed in the Cosmos. He will at least have been the consciousness of the Universe; a flash of light amidst the bottomless shades. (Sorre 1961,p.340)

In summary, then, it is possible to identify the following themes in the approach of the Vidalian school to the interaction of man and the environment:

1. The inorganic and organic forms of a given area are interrelated in such a way as to present the essential unity of that area. Thus relief, climate, vegetation, fauna, and human genres de vie are best considered to be inseparable components of an holistic entity. This is the principle of terrestrial unity. Approached with sufficient imagination, this principle may imply an oekoumene whose borders are those of the Universe itself.

2. Within this unified whole, man plays both an active and a passive role. His relationship with the environment is reciprocal: he affects nature and is affected by it, and the interaction can be mutually beneficial. The environment has its most significant effect on man through the basic biological needs of the human physiology, and particularly through diet and nutrition. Man has considerable choice in selecting the precise form of his adaptation to natural conditions; but those conditions do impose limitations on the modes of

adaptation available. The effect of man on the environment is generally a property of human groups, rather than of individuals: an understanding of how the individual relates to the environment is nevertheless essential to an understanding of the relationships that communities and societies develop with the milieux in which they arise. It is accepted that such groups have their own internal dynamics of culture and economy that will directly affect the interactions with their respective milieux.

3. The effect of man as an active agent is to produce a visible imprint on the earth's surface: the landscape. A particular landscape is unique to a particular group in a particular environment: it can be, to some extent, empirically described by statistics, but many of its elements require for their complete description an insight and empathy that only comes about through direct experience. The form the landscape takes is determined by the forms of social, political and economic structures that develop from the fundamental interaction between the human organism and the environment. On occasions the physiographic conditions of the environment will give rise directly to particular landscape forms, such as settlements.

4. The geographical impact of man can be best described by the term genre de vie (mode of life). Genres de vie arise in particular areas as a result of the selective activity of human groups among the pre-existing variety of organic life: the biological foundations are paramount. Genres de vie are subject to internal change and, in certain cases, to orderly evolution, and are further influenced by the milieu, especially the socio-economic milieu, in which they are embedded. Central to the formation and maintenance of a genre de vie is

its pattern of circulation: that is, the rhythms of transport, communication, and migration that characterize the way in which life is conducted in the area. Though a concept that initially arises from the consideration of rural localities and regions, it can be fruitfully generalized to the modern urban milieux within which the majority of people live. The uniqueness of genres de vie has, in recent times, been eroded, due primarily to the greater ease of global transport and communication: this trend to standardization is not held to be a positive one, since the variety and richness of human interactions with the environment is reduced; and, by genetic analogy, lessening the options for human action in the environment.

5. The operation of a particular genre de vie in a specific environment gives rise to a unique personality of that area. Personality is something latent in nature: it is revealed by the action of man. It bears a close relation to the individuality of the landscape, and is therefore in part an artefact of the culture of the group inhabiting the area. Ultimately it can be traced to the collective modes of thinking of the group, to its collective consciousness. Personality thus arises as a result of an adaptive process; but it is a process in which the mind of man is intimately involved. The passage of time is of the greatest importance in the emergence of personality: not only will historical processes influence its development, but the interaction of man and environment that gives rise to a unique personality of that area ~~Personality is something latent in nature: it is revealed by the action of man. It bears a close relation to the individuality of the landscape, and is therefore in part an artefact of the culture of the group inhabiting the area. Ultimately it can be traced to the collective modes of thinking of the group, to its collective~~

~~consciousness. Personality thus arises as a result of an adaptive process; but it is a process in which the mind of man is intimately involved. The passage of time is of the greatest importance in the emergence of personality: not only will historical processes influence its development, but the interaction of man and environment that gives rise to~~ will have the opportunity to become more and more complete. Personality is the expression of wholeness of an area: it unites the disparate elements of inorganic form, or organic life, and human activity that are found there into a living expression of harmony.

As a post-script to this summary, we may note the degree to which these themes of the Vidalian school are comprehensive of the development of traditional Anglo-American human geography in the twentieth century, at least until the onset of the quantitative movement. To begin with, the principle of terrestrial unity, given such persuasive and penetrating form in the regional monographs, laid the framework on which the concept of the region was subsequently elaborated. Contemporaneous with Vidal himself, we may point, for instance, to Geddes (1924), with his construction of the social sciences in terms of Place, Work, and Folk; and Herbertson's (1905; 1915; 1963; 1965) concept of a natural region, in which the environmental basis, the active and passive nature of man in that environment, the holistic status of the region, and the idea of "regional consciousness," "a spirit of place" accessible to "a loving familiarity with the region" (Herbertson 1915, p. 153), all find a place. We see Davis (1903; 1906; 1015; 1924) upholding regional description and explanation, in which human and natural factors are to receive "homologous" treatment (Davis 1915), though fundamentally underpinned by an understanding of the natural regional forms; and Salisbury (1907; 1912) urging an understanding

of the way in which different human groups and communities have adapted their modes of living to different environments. Barrows (1923; 1962), though chiefly known for his advocacy of human ecology as the central theme of geography, enunciated this principle within the overarching concept of the region, and was instrumental in having integrated regional studies adopted in the context of national resource management. Roxby (1926), following the idea of a natural region espoused by Herbertson, and, much earlier by Marshall (1788), saw the region as the centre of geographical investigation, and adopted an analysis of it by organic analogy, in which physical, biological, and human elements were seen to be so well synthesized as to give rise to real unity. In the hey-day of the French regional monograph, Newbigin (1932) applied both Vidalian theory and practice to a masterly regional account of southern Europe. Atwood (1935, 1940) promoted and developed the regional basis of geographic education; and Bowman (1916; 1924; 1931; 1934), though better known for his settlement studies, undertook his first field work in the Andes of Peru within the regional framework, and became a strong advocate of regional synthesis as the geographer's first and characteristic concern.

For some decades after the full flowering of the Vidalian school, the influence of the principle of terrestrial unity, expressed in the concept of the region, remained dominant among geographers. Platt (1928; 1935; 1959) developed sophisticated field-work methods based on an approach to the areal pattern of human occupance produced by the functional organization of human activity--a more complicated way of saying what was so compactly expressed by Vidal's notion of <u>circulation</u>. Whittlesey (1925; 1929; 1936; 1954) worked for over three decades within the framework of regional theory, and made notable contributions to it, both within the field of political geography, and

directly, at the national and international scales, and in theoretical approaches to the concept. Wright (1933a,b; 1936; 1937), though better known for his uniquely imaginative historical studies, made significant forays into the field of regional geography, and developed cartographic and statistical techniques for regional analysis. James (1929; 1949; 1952), beginning with an early regional study, maintained his commitment to the regional concept for over four decades, and took up its cause when the quantitative critique was launched against it. Most notably, Stamp (1950) made geography in the U.K. virtually synomous with the regional approach to environed man: his "Land of Britain" surveys, and the monumental work that resulted from them, were based on an unequivocal conception of the natural region, delineated by natural features of topography, structure, climate, and vegetation, and giving rise to the characteristics of human activity in the region that are always secondary, no matter what effect they have had upon the landscape.

Even in contemporary times, this theme can be traced, despite the swing away from regional analysis. Thus Berry (1964), one of the main proponents of quantitative and theoretical geography, attempted to reconstruct regional geography in terms of a geographical data-matrix, from which, according to the axis chosen, regional, systematic, or historical geography emerged. Dickinson (1976), in reviewing the development of the regional concept, declared himself an ardent advocate of regionalism as the core of a 'true' geography, defined in terms of the areal association of physical, biotic, and human phenomena, and resisting more abstract, interregional generalization. Moreover, as we have partly seen, other themes developed in the Vidalian school on the basis of the principle of terrestrial unity can also be located in modern times. The notion of reciprocal exchange, of the active and passive nature of human relationships

with the environment, has emerged through writers such as Stevens (1939) and Stoddart (1965; 1956) as the ecological approach to geographic phenomena (examined in more detail in the next section). The idea of landscape, as we have noted, was developed from its beginnings in Cornish (1918; 1931; 1935) primarily by Sauer (1925; 1941b), and can be traced through the writings of people like Wooldridge (1949; 1951; 1956) to the modern landscape school, as represented by Appleton (1975). Even the elusive idea of personality has retained its credibility, from Herbertson (1915), through Sauer (1941a), and Roxby et al. (1937), and more recently, as we have seen, as a primary conceptual foundation of the humanist school. It therefore seems that the insights of the Vidalian may be considered profound, and powerfully framed, since the geographic tradition of theory and research with which they have been associated was, for some five decades, the core of the discipline; and even now, it would appear, attracts both traditional and experimental interpretations. In making this assertion, we do not dissent from Buttimer's (1971, p.58) assessment of Vidal's thought, which emphasizes "insight," "artistic finesse," "power to suggest . . . to evoke . . . to open new horizons," rather than systematic exposition of theory; but we do emphasize that such "power" is power of theory, and derives its fertility from the precision of its primitive concepts, and the degree to which they are rooted in the way things really are. We may conclude, then, that any theoretical approach to the environed individual which claims to underpin the discipline must be capable of systematic elaboration that comprehensively upholds and illumines Vidalian insights: indeed, it might reasonably be said, the degree to which it can do so is a real measure of the success of the entire theoretical enterprise.

5.4 Human ecology

Although geographers working within the Vidalian tradition have continued working until recent years (Sorre wrote his last work in 1961, Sauer in 1974), it is generally agreed that the theory of man-environment relations in geography and associated disciplines has been substantially contracted in scope. This can be demonstrated by a brief review of work that has been carried out under the general heading of human ecology. Barrows (1923) is usually credited with establishing the theme in geography. He disputes the utility of studies of distribution of phenomena on the surface of the earth, and of regional approaches, and advocates instead an ecological approach, based on the belief

> . . . that those relationships between man and the earth which result from his efforts to get a living are in general the most direct and the most intimate; that most other relationships are established through these. (Barrows 1923, p.13)

This approach is firmly equated with "man's adjustment to the environment" and with "the relationships existing between natural environments and the distribution and activities of man" (Barrows 1923, p.3). Clearly this is a fundamentally materialistic position, which, in its insistence on the primacy of livelihood in structuring geographic phenomena, bears a close resemblance to Demangeon's. The paradigm within which such a view operates is in essence biological: psychological and cultural factors are secondary.

Although, as has been indicated, Sauer's early work was carried out within the Vidalian tradition, much of his best work, particularly later in his career, was carried out along the lines suggested by Barrows. In his account of the origins and development of agriculture (Sauer 1952), for instance, he

begins by looking at man as an organism within the "primordial habitat," and deals in particular with fire as an element of the ecology, and, as analyzed by Vidal (1911), the selective effect of man on natural communities. This approach was summarized in a famous paper (Sauer 1956b). perhaps his finest, which is based on "the biologic nature of man," especially as it can be seen to structure social organization and culture, and concerns itself in greater detail with fire, agriculture, and the contemporary ecological crisis. In other works, Sauer elaborated the framework erected there. An account of the human ecology of tropical America (Sauer 1958), for instance, places man firmly within an ecological system, and assigns to him a particular niche in the habitat. Again there is a concern with alteration of the environment through settlement, agriculture, and firing, a theme which is further explored in another paper with particular emphasis on early man (Sauer 1963a). These emphases account for his insistence on the value of biological understanding to geography, which he considers to be of such importance that it should be acquired by whatever means necessary, even if it means moving outside the discipline (Sauer 1956a, p.399). Their influence can even be discerned in an earlier work (Sauer 1941b, p.14) of which the prime concern is history and culture: they are the foundation for the remark that "the traits of making a living are for us the dominant thing to observe"; and for the fact that the first two themes suggested for investigation by geographers centre on the reciprocal relationship of man and "physical geography." The notion of ecological balance underlies a belief in what Leighly (1963, p.7) terms "a humane use of the earth"; and, as is discussed later in this section, was the motivating force behind his concern about the destructive effects of man and the environment.

The value of the application of ecological concepts to the geographic study of man-environment relationships has been noted by many commentators. Jones (1956, p.376) sees it as the only way out of the determinist-possibilist maze; to Sprout and Sprout (1965), it represents a frame of reference and mode of analysis with which to approach human affairs; Harvey (1969a, p.115) identifies it as the "essential focus" of man-environment studies, one of the five main themes of geographic research. Grossman (1977) regards it as the backbone of the conceptual framework shared by modern geography and anthropology, and a number of works (such as Watson and Watson 1969; Dice 1955) have elaborated theoretical systems which view cultural forms as part of the total human ecosystem. In the tradition of Sauer and the Berkeley school, Wagner (1960, p.36) constructs an approach to culture and environment that is based on the idea that "man is a natural phenomenon." Bates (1961) begins his well-known account of man and nature with the sentence, "Man is clearly an animal" (Bates 1961, p.1); and although he concedes that culture is that which distinguishes man from animal, and that one is always dealing with "the man-culture-environment complex" (Bates 1916, p.5), it is on the biological nature of man that the work centres.

Bates alludes in passing to a school of sociology which arose in Chicago during the 1930s, and gave itself the title of "human ecology"; and the approach of the Chicago school (as it came to be called) is worthy of some attention in this context, if only to clarify the confusion that their adoption of the "human ecology" label has led to. As developed by Park (1936) and McKenzie (1926), human ecology in this sense centred on an analogy between the urban community and the biological community, and involved the extended use of biological terminology: competition, dominance, succession, symbiosis,

invasion, structure, and so on. Entrikin (1980) convincingly established that the analogy was not a naive one, but was based on a particular philosophy of science. It has, nevertheless, been trenchantly criticized as being altogether inadequate to the analysis of human communities, as it does not deal with those aspects which are characteristic to human, as opposed to animal, communities; namely "sentiments. value systems, and other ideational concepts" and "all the many ramifications of human interrelationships" (Hawley 1944,p.402). Recently this approach has been further attacked on ideological grounds (Harvey 1973). At all events, it is clear that the principles that underlay the Chicago school were essentially sociological and not biological: they are therefore to be clearly separated from the main tradition of human ecology which, as has been seen, takes biological relationships as fundamental to human communities; and in which cultural and social institutions, while operating according to their own internal dynamic, are in large measure seen to be structured by those fundamental relationships. The human ecology of the Chicago school need not, therefore, concern us further.

Let us return, then, to the postulates that underlie the theory of man-environment relationships in the main tradition of geographic human ecology. These postulates are primarily concerned with developing a model of the individual in interaction with the environment. Wagner (1960,ch.1), for instance, bases his account of the functioning of human societies in the environment on an essentially biological model of the individual: this includes the fundamental biological exchanges with the environment that man is part of (such as sensory operations, influences affecting the physiology, environmental catastrophes, and biotic effects. all mediated through the physiological requirements of the human individual); man as a component of natural

communities, in ecological relationship with its other members; and man's creation of "mutualistic relationships" (Wagner 1960,p.19) with the environment, that is, his ability to rearrange the conditions of the environment and thus extend the range of living, within the basic physiological constraints. This is summarized in the first of three postulates that are seen to underlie human geography:

> Man is a natural phenomenon, that is, that his body is a physical object and that some of his actions are physical and biophysical events. The actions of man have concrete results in the material world: and, in turn, the material conditions under which action takes place influence its results. (Wagner 1960,p.360)

Although the other two postulates refer to the development of culture in particular human groups, the first is given as the primary one: the way in which culture is subsequently analyzed emphasizes the point, as it centres on "certain idiocratic [sic] features of the human species" together with "some general mammalian characteristics" as "foundations of societies in man as expressed geographically" (Wagner 1960,p.37).

Hewitt and Hare (1973), in a more exploratory account of the same theme, construct their analysis in a similar way. It is based on a model of the human individual as an organism in exchange with the non-human environment. These exchanges are seen to be

> grounded in what are essentially <u>translation</u> procedures. The success of failure of human adjustments depends upon our ability to translate environmental stimuli into human meanings and actions or to transform resource materials into humanized substances. (Hewitt and Hare 1973,p.8)

Clearly this is both a biological and a cultural notion, as reflected in the particular exchanges identified: metabolic, biomedical, and sensing (the biological exchanges); and communications, emotional and sexual, and work and

mobility (the cultural exchanges). These categories are seen as "interpenetrating", not distinct; and neither primarily biological nor primarily cultural elements are seen to be fundamental to the other. What emerges is the notion of a "bio-cultural system" (Hewitt and Hare 1973,p.9) whose main function in the individual is to facilitate adaptability to both the social and the physical environment (Hewitt and Hare 1973,pp.18-22): and it further insisted that

> In man, all transactions with environment must ultimately be channeled to scales of exchange that can be managed by the various pathways that can be described for the individual human. (Hewitt and Hare 1973,p.21)

Order and organization are seen to be the main features of such systems: in the process of transmitting matter and energy

> ....they manage to maintain and locally <u>increase</u> the degree of natural order in the organically retained parts of transfer. They create or recreate higher organization. (Hewitt and Hare 1973,p.21)

In the geographic analysis of man-environment relationships, the two concepts of organization and system have been best developed within the theory of the ecosystem, whose exposition dominated much geographic writing during the 1960's. Most of this work is firmly biological; although Schnore (1961), one of the earliest, adopts a view of human ecology close to that of Park and the Chicago school in seeking correlations between spatial patterns and social organization.

Stoddart (1965, 1967), on the other hand, places human ecology unequivocally within the biological paradigm. He identifies four main properties of ecosystem. The first is monism: the ecosystem

> brings together environment, man, and the plant and animal worlds within a single framework, within which the interaction between the components can be analyzed. (Stoddart 1965,p.243)

This is specifically contrasted (incorrectly) with the holism of the regional monographs of the French school where the unity is seen to be primarily aesthetic, rather than functional, and therefore not susceptible of definition. The second property is structure: again it is emphasized that what is involved here is empirical fact and not so-called "transcendental properties of the earth and its regions as organisms or organic wholes" (Stoddart 1965,p.244). The third property is functioning, involving transfer of matter and energy; and the fourth is its character as a self-regulating system. There is a clear contraction here in the scope of the theory outlined in the traditional geographic approach of the French school, and typifies what may be called a purely biological approach to human ecology, where human attributes, such as attitudes and values, being not directly measurable, are specifically excluded. This is emphasized by the discussion of "complex ecosystems in which man may play some part" (Stoddart 1965,p.247), in which man is seen to intervene in the functioning of the ecosystem through various biological mechanisms, despite the fact that these mechanisms may be underlain by specific cultural changes, such as changes in diet. It is, perhaps, of some significance that the best studies of this sort are seen to be those relating to primate and "primitive" groups. Accepting for the moment what is clearly a doubtful proposition--that primitive (by which is meant, presumably, traditional) groups present less cultural complexity and therefore greater biological simplicity--there is here at least an implicit recognition that the ecosystem approach outlined is inadequate for the full range of human interactions with the environment. In a later article (Stoddart 1967), the analysis is not much changed: the only cultural attributes examined are those relating to technologies of cultivation, and to some economic aspects of democratic societies which are seen to be subject to

"normal ecological feedback mechanisms" (Stoddart 1967,p.530). The emphasis is squarely on biological aspects of man-environment relationships: the value of systems analysis is seen to lie in bringing geography "back into the realm of the natural sciences" (Stoddart 1967,p.534) and to its particularly successful methodology.

A number of writers have strongly supported this position. Morgan and Moss (1965) look to a "geography of living things" through the concept of the community to provide a framework for the study of man-environment relationships. They espouse both ways of looking at human communities, biological and sociological, but do not question that the first is the most important perspective: into the distinct units that comprise the biosphere

> man comes both as a component and as a factor. As a component he fills a number of distinct niches....As a factor man is a profound modifier of natural balances, a source of energy, and a final determinant in interspecific competition, both of plants and animals. (Morgan and Moss 1965,p.345)

In a given biological area, man is not an external influence, but an essential component: this analysis

> ....provides the major link between man and land, and between land and man. Few other functional relationships can be so convincingly demonstrated. (Morgan and Moss 1965,p.349)

Eyre (1964) and Eyre and Jones (1966), though essentially in agreement with this view of human ecology, widen its scope:

> It is this great interaction between the natural and the psychological, between the blind forces of nature and self-conscious activities of man, that is envisaged here when the term "human ecology" is used. (Eyre and Jones 1966,p.7)

Thus a study of human ecology in urban areas must take into account the "territorial resources of the community, as well as its social and economic needs" (Eyre and Jones 1966,p.13); equally, a study of the evolution of

grassland vegetation must take into account "the whole history of land utilization by man" (Eyre 1964,p.372). The latter clearly places man in the ecosystem as a biological component, and centres on his effects as an organism on other organisms, and their effects on him. The value of this approach for studies of human land use is emphasized by Simmons (1966, 1970), who points out that different land use systems can be viewed as ecosystems in different degrees of transformation of the undisturbed situation.

Most of the geographic accounts of man-environment relationships outlined in this section adopt positions which are essentially the same as those taken in biological or biogeographical texts. Watt(1973), for instance, basis his account of environmental science (which is directed towards developing a broadly-based understanding of plant, animal, and human ecology) on fourteen principles which he feels constitute a deep underlying core of these distinct disciplines: all of these principles are biological; in both the terms they employ and the statements they assert. Similarly, Krebs (1978,p.608) concludes a well-known account of ecological theory with the statement that

> Man's effect on ecosystems can be viewed as large-scale perturbation experiments, and ecologists who have adopted the experimental approach can use man's activities to gain critical insights into the behaviour of perturbed ecosystems.

It should be clear from the preceding section that an essential component of the Vidalian view of man-environment interaction was the effect of man on the environment. It has, in fact, been a major theme of all ecological approaches in geography, both before and after Vidal. Marsh (1864) is now recognized, largely through the advocacy of Sauer, as one of the first to detail these effects. Though Marsh concedes that the influence of man has from time to time been constructive, he emphasizes destructive aspects:

> . . . the organic and the inorganic world are, as I have remarked, bound together by such mutual relations and adaptations as secure, if not the absolute permanence and equilibrium of both, a long continuance of the established conditions of each at any given time and place, or at least, a very slow and gradual succession of changes in those conditions. But man is everywhere a disturbing agent. Wherever he plants his foot, the harmonies of nature are turned to discords. The proportions and accommodations which insured the stability of existing arrangements are overthrown. (Marsh 1864,pp.35-36)

His case studies centre on man as an agent of geographical dispersal of flora and fauna, on the clearing of woodland, and on the reorganization of drainage systems: in each of them, as the above quotation would indicate, man is seen as intervening in a pre-existing ecological harmony (or, in modern terminology, balance), and he does so as part of nature; acting, that is, as a natural agent. It is, however, worthy of notice that Marsh distinguishes man from nature by virtue of his capacity for destruction (a separation that was contrasted to Humboldt's holism earlier in this discussion).

His theme, however, and even his specific concerns, appear to have laid the foundations for a great many studies that have subsequently attempted to detail the multitude of effects that man has on the environment. Thus for Sauer (1944; 1952; 1956; 1958), the power of human communities to transform the natural environment by direct or indirect means became a focus of study over forty years. He was particularly concerned with effects that have persisted for a very long time. One such is fire, with its close link to the formation of grasslands and the modification of vegetative structure; another is the biological selection of flora and fauna in domestication, and the dispersal of these favoured organisms; yet another the effect on forests of clearing, and of the establishment of settlements. In all these, though man is certainly a cultural animal, and therefore influenced in the particular form of action by

non-biological factors, the actual interaction with the environment centres on direct or indirect physical exchange, within a fundamentally ecological scheme. As Leighly (1963,p.4) remarked,

> More than anything else, his appreciation of simple people living in close contact with inorganic nature and in symbiosis with plants and animals distinguishes Sauer's writing about man on the earth.

"Simple people", Sauer argued, lived most closely in balance with their environment: it was, Leighly (1963,p.7) goes on to assert, at the centre of all his geographic thinking:

> There is such a thing as the humane use of the earth; the simpler cultures are less destructive of the terrestrial basis of man's existence than is our present technology; ;and the possessors of modern technology may find in the past experiences of man on the earth guidance toward a balance of the capacities of the land with the requirements that gives some promise of permanence.

This conviction, which Sauer documented so persuasively, led in 1956, at Princeton, New Jersey, to one of the most significant symposia on the theme this century has seen. Entitled "Man's Role in Changing the Face of the Earth", and chaired by Sauer, Bates, and Mumford, it was dedicated to Marsh and his work, and remains a monument of its kind. Amongst other things, it resoundingly affirmed the view of man as an element of the natural world, subject to the same laws as the other elements, and influencing the natural world primarily through direct interaction or interchange. The main areas of concern outlined by Marsh continued to receive much attention (Stewart 1956; Darby 1956; Clark 1956; Anderson 1956; Bates 1956), as well as contemporary areas, such as wastes and urban-industrial environments. The theme of man as a cultural animal is given relatively little attention: Mumford's (1956) conclusion is the outstanding exception, a fact that is not without

significance since he is here attempting to deal with ways of ameliorating the situation so comprehensively documented in the other papers. The success of this symposium gave a new emphasis to the analysis of man-environment interaction in geography, and can be seen to have contributed directly to the popularity of ecosystem approaches during the 1960's. Its influence is evident in neighbouring disciplines (for instance, Evans 1975; Ball 1975; Simmons 1975; Limbrey 1978), and it clearly contributed to the growing international awareness of global ecological crisis. Significantly, the analysis of this crisis has been almost exclusively within a biological framework of man-environment interaction (Haggett 1972; Ehrlich and Ehrlich 1972; Watt 1973), though some more comprehensive views have asserted that parameters of the collective consciousness of human groups such as "hubris" (Bateson 1972,p.499) are essential to both the analysis and the formulation of solutions to the crisis.

Finally, it should be noted that although human ecology has been put forward with much enthusiasm as a basic framework for the study of man-environment relationships in geography (Brookfield 1964; Eyre and Jones 1966), there has been resistance to the concept. Chorley (1973) takes the view that the self-regulatory character of the ecosystem, with its emphasis on balance and stability, is inappropriate to human communities, in which matters dependent on time, and therefore on planning, often dominate. He further notes that the ecosystem concept is relevant only to the extent that man can be considered a part of nature; and yet it is clear that the natural environment has been replaced by

> ....a largely man-made environment which is subordinated to the socio-economic environment to a much greater extent than other organic life forms are able to control their environments. (Chorley 1973,p.160)

He therefore follows Ackerman (1963) in advocating a systems approach to man-environment interaction in which the cybernetic structure "man-machine", complete with positive and negative feedback loops that mutually modify the interacting elements, replaces the traditional man-nature dichotomy. Man is seen to be liberating himself from the biological milieu by "seizing control of his terrestrial environment" (Chorley 1973,p.167) through technological and planning strategies. Only such an approach as this, Chorley argues, is capable of modelling the complexity of the real world in which interaction takes place.

Taking the approach of human ecology as a whole, it is clear that it represents both a more contracted and a more specialized view of man-environment interaction than that developed in the Vidalian tradition. Each of the themes summarized here--man as a natural phenomenon, man as an ecological component of natural communities, and the effect of man on the environment--can be derived from the basic tenets of the Vidalian school as formulated in the preceding section. The first appears in the discussions of man as both active and passive in the environment, and of genre de vie; the second in the notion of terrestrial unity; and the third in the identification of the landscape as the physical imprint of particular groups, and of the role of environmental modification in creating genres de vie. Significantly, there is little attempt in the theory of human ecology to incorporate social, economic, or cultural factors, as there is, for instance, in the analysis of genre de vie. In consequence, there is no attempt to approach the idea of personality, as developed by Vidal; indeed, as we have seen, such notions are held to be incapable of empirical investigation, and thus outside the domain of serious research. It is therefore clear, that, taken in broad perspective, the

principles of man-environment interaction that underlie the theory of human ecology are essentially a subset of those developed by Vidal and his followers.

There is, nevertheless, a deepening of the analysis in particular areas, as a result of the greater specialization engendered by the overall contraction. The adoption and development of the ecosystem approach, with its emphasis on measurable exchange, has done a great deal to clarify the biological foundations of the man-environment interaction. In particular, as man's ability to utilize more and more fundamental laws of nature--and hence to produce larger and larger effects--in this interaction has increased, the emphasis has moved from the capacity to modify the environment to the capacity to degrade it; and here the ecological concepts of homeostasis, self-regulation, and self-sustenance have been of fundamental importance. A recognition of the essential interconnectedness of activity in the environment has given rise to a new central concept of the theory of human ecology: the order and organization of living environmental systems.

In large measure, the contraction in the domain of the inquiry evident here is a consequence of a shift in methodological emphasis. For Vidal and his followers, the understanding of an area was both an empirical and an intuitive, or aesthetic, task: for the human ecologists, empirical investigation has been the only legitimate approach. The relevant factors are observable, material, and either measurable or objectively describable. This shift brought a greater degree of rigour to the analysis; but at some cost to the scope of the theory, and hence to its application in the real world. In purely intellectual terms, the contraction is justifiable: all scientific disciplines tend to limit their domains of inquiry, for entirely legitimate methodological reasons. Yet if one is to accept that the best theory gives rise to the most effective praxis, one

must be led to doubt an approach to man-environment interaction that treats man in purely biological terms, and denies him those attributes that are most characteristically human. The simple fact is that man is not just an organic component of the ecosystem, involved in interchange with other components on the same terms as they interact with each other; and any theory which does not take account of this must, it would seem, give rise to inadequate prescriptive strategies for improving the quality of the relationship between man and environment. Central to this dissertation is the idea that an understanding not only of human characteristics such as culture and society, but of the collective consciousness that is said to structure them, is necessary in order to develop fully effective and stable strategies of such intervention.

5.5 Summary and conclusions

1. Although the foregoing account is necessarily selective, it is, we may feel, sufficiently representative to allow reasonable conclusions about the conceptual foundations of traditional human geography in the twentieth century to be drawn. In particular, we want to establish whether the integrated approach to the individual developed in the preceding chapter is an adequate theoretical basis on which to derive systematically--and hence, in some sense, account for--the central insights of that tradition. To the extent that such a derivation proves possible, the integrated theory may gain in credibility: to the extent that it falls short of traditional insights, further directions in the development of theory may be implied (see Figure 5).

The feasibility of a reductive approach to human geographic theory was suggested in section 3.2. It was concluded there that, although geographic collectivities can be legitimately analyzed at their own level, and in their own terms, there is a sense in which explanation and understanding of collective human geographic phenomena is preeminently reductive. That conclusion, in fact, encouraged us to embark upon the enterprise of this chapter. The foregoing survey of traditional human geographic theory may well, however, give us pause; for it is abundantly clear that, whatever the attractiveness and legitimacy of reductive explanation, human geographers have widely rejected it, and have sought instead to frame their theories in terms that deal directly with the macroscopic domain of their concern. Indeed, we might conclude, with Harvey (1969,p.484), that insistence upon this "scale of resolution" for observation and theory construction is the only continuity that can be traced in traditional twentieth century human geography, and best

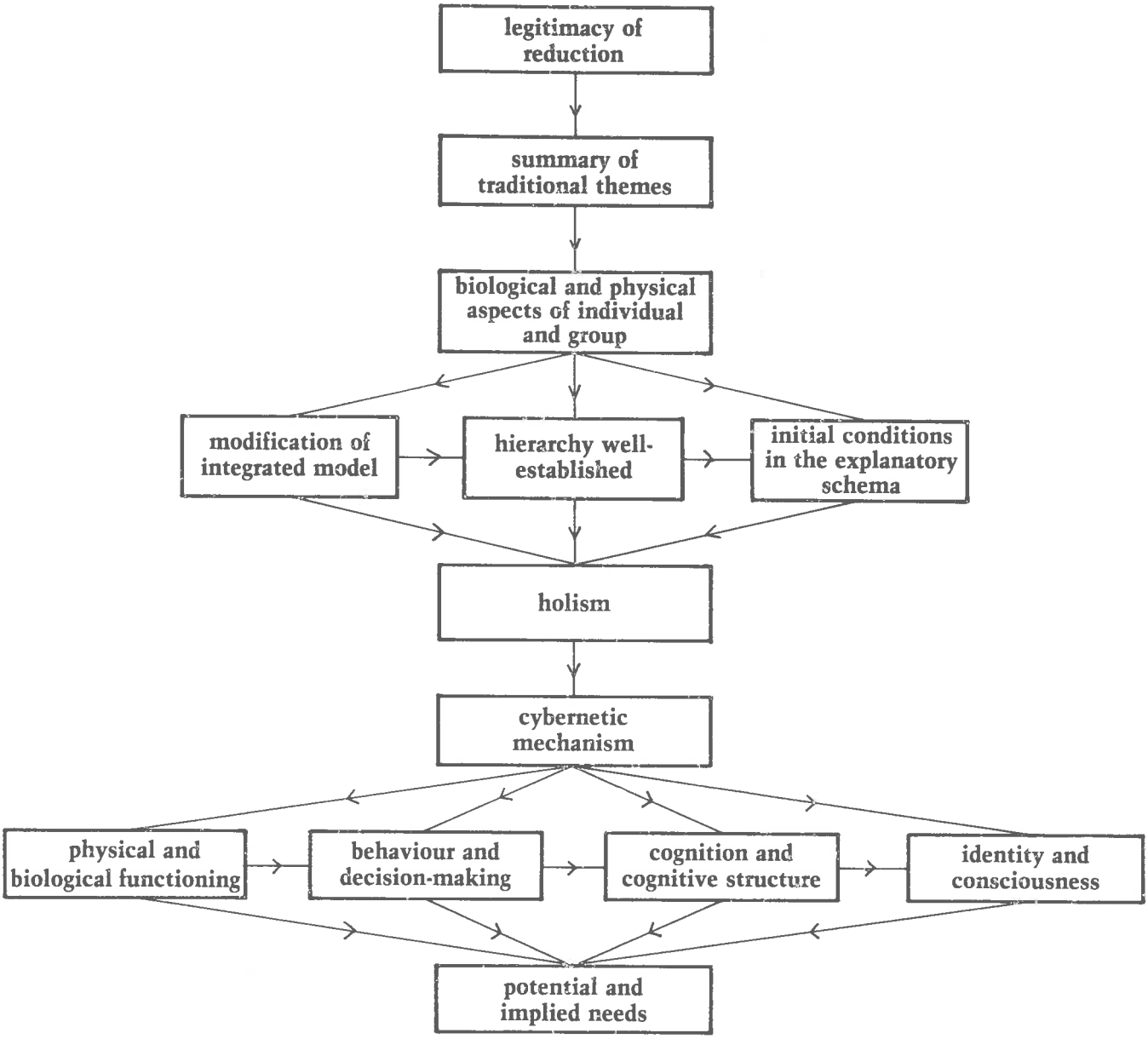

FIGURE 5:

Assessment II of the Proposed Integrated Model: The Traditional Perspective

defines the concerns that distinguish it from other disciplines. Reductive explanation would not be, on this view, typically geographic explanation.

There are further theoretical considerations that appear, at first glance, to strengthen this view. One of the most important is the attempt to identify a hierarchy in the functioning of collective human geographic phenomena. A hierarchy, as Nagel (1961,pp.435-441) points out, is not necessarily reducible: it may give rise to descriptions at different levels which are independent of each other. Moreover, when the hierarchy takes an aggregative form, as with spatial hierarchies, traits may emerge at the higher levels which are not found at the lower: it then becomes difficult to see how a reduction could succeed, since apparently it must include terms from the higher level of the hierarchy at a lower level, where they are not identifiable.

A third challenge to reductive explanation centres on the difficulty of defining the operation by which elements are to give rise to wholes. The relationship between element and set is very different from that between the neighbourhood of a point and the topological space in which it is embedded: yet both are legitimate models for interpreting the relationship of part to whole. Indeed, it might be argued that even the attempt to consider a part independently of the whole of which it is a part is an artificial exercise, and cannot in principle lead to the theoretical derivation of the whole in question. The difficulties presented by this argument were recognized and discussed in the context of individual and group in section 3.2.1. On these three grounds, then--the evident possibility of developing meaningful statements and theory at the collective level of analysis; the hierarchical organization of geographic phenomena; and ambiguities associated with the idea of relationship of part and whole--it would appear that the legitimacy of reduction in human geographic theory is further called into question.

On closer analysis, however, it becomes clear that the challenge is more apparent than real. With respect to the first of the above points, we would want to reply that the question is not so much one of the feasibility of establishing legitimate theory at the collective level, as on the value of that theory once established. The assessment must be a subjective one; and certainly it is difficult to deny validity and value to theory framed exclusively in collective terms, as human geographic theory throughout the first half of the twentieth century was framed. Historically, however, as we have seen, the attempt to formalize theory at the collective level, in the locational and spatial analysis of the quantitative school, was followed by a demand for reductive explanation which found expression in the still largely formal theories of the behavioural school. We have located in this demand a feeling that the understanding of human geographic phenomena afforded by exclusively collective theory was in some fundamental sense inadequate, in that it seemed too abstract, too far removed from the realities of individual human experience. On logical grounds, therefore, there is no reason to prefer explanation at any particular level, provided that meaningful statements can be constructed and theory adequate to a scientific research programme be developed: and certainly, therefore, there are no logical grounds for rejecting reductive explanation. Such rejection can only come on grounds of taste, or from subjective perspectives which may include the view that reductive explanations are not by definition geographic. If, however, the idea of geographic is defined in terms of subject matter, as we have attempted in this dissertation, rather than level of resolution, that difficulty, too, disappears.

With respect to the second objection to reduction, based on the implications of hierarchical organization, some clear misconceptions are

involved. Firstly, the existence of terms at a higher level in the hierarchy and not at a lower is no bar to reductive explanation, provided appropriate coordinating definitions are developed to link the different kinds of terms: this is the "condition of connectibility" (Nagel 1961,p.354). Secondly, the real difficulty in effecting a reductive explanation of hierarchical systems is not a logical but a practical one: it requires both theory and the specification of relevant initial conditions adequate to describe the full range of organization. This is the "condition of derivability" (Nagel 1961,p.354). Clearly in complex macroscopic systems, such as those with which the human geographer commonly has to deal, such a task becomes herculean; but it is not in principle inconceivable. From this perspective, the separation of descriptive terms at what are identified to be different levels of a hierarchy appears to be more a practical way of handling complex systems, than a reflection of irreducible observed fact.

The third objection, which questions the validity of dealing at all with parts in order to understand the whole, is difficult to take seriously. It is obvious that every part is a whole with respect to its own constituents, and that every whole is a part with respect to higher-order systems in which it is embedded. Since the structure of space-time, on current understanding, requires infinities in the approach to both the macroscopic and the microscopic, acceptance of the objection would require us to deal only at the level of the universe, a conclusion which, although of potential interest to speculative philosophers, would eliminate scientific investigation--indeed, understanding of any kind at any finite level--altogether. We need not, therefore, in the context of this dissertation, consider it further.

We thus conclude that there seems to be no reason in principle why a reductive explanation of human geographic phenomena at the collective level

should not be achievable; and that the proposed assessment, of the degree to which the integrated approach to the geographic individual developed in the preceding chapter is an adequate theoretical basis for understanding traditional insights, is a reasonable project. We proceed to this analysis, however, in full consciousness that, in the history of ideas, the making of broad assertions and generalizations, particularly about the intellectual trends of some eighty years, is at best a risky and highly personal business; and yet with the conviction that, if carefully and modestly done, such an exercise is capable of yielding valuable insights, of a kind that is essential to the successful completion of the task attempted in this dissertation.

2. It is evident that, although intellectual fashions and emphases in twentieth-century human geography have changed from time to time, two foci of concern have remained constant: the nature of individual man, and of his interaction with the environment; and the nature of human groups, or communities, and their interaction with the environment. These concerns are not mutually exclusive, since clearly (as has been argued throughout this dissertation) the notion of the group is predicated upon a model of the individual, and equally that model must reflect the reality of social and cultural environing. They are, however, treated as sufficiently separate themes in geographic theory to warrant independent statements of them, which might run as follows:

(a) In conformity with the paradigm established by Darwin, the fundamental facts about the individual are seen to be physical or biological. Although it is conceded that the individual has characteristic attributes that go beyond the purely biological, it is nevertheless held that, for the purpose of

considering his interaction with the environment, these attributes may be seen as structured and organized by the laws of the individual's physical and biological functioning. Thus although geographers have not in the main followed Darwin in his attempt to bring the totality of human functioning within the ambit of one overarching theory, they have nevertheless tended to retain a predominantly hierarchical model of the individual in which biological and physical attributes are primary.

(b) It therefore follows that the exchanges of the individual with the environment should assume an hierarchical form, in which biological and physical exchanges are primary. Overwhelmingly, twentieth-century geographic theory has accepted the Darwinian view that the individual human organism is subject, above all else, to the same natural laws as are all other life forms. There has been, nevertheless, an awareness of the importance of cultural aspects of human functioning, though there is no general agreement as to the precise nature of the relationship of cultural with biological factors: for instance, Wagner (1960) preserves the traditional hierarchy; Hewitt and Hare (1973) perceive an "interpenetration" of both in a "biocultural system"; Chorley (1973) insists on the increasing liberation of the individual from his biological environment, through the power of technology to transform the world. There is revealed here a crucial dilemma, which lies at the heart of much human geographic theory, and has reappeared in various guises throughout twentieth-century geographic thought: if it is accepted that the human individual, in his physical and biological aspects, is inseparable from the environment and subject to its laws; and if it is at the same time accepted that the human individual, in his mental, social, and cultural aspects, is clearly distinguishable from the natural world, and partakes in a dynamic whole

that is internal to the group of which he is a part, and therefore separated from the environment and its laws of functioning; what then is the best view of man-environment interaction? How can both simultaneously be true? Solutions offered have ranged from the sociobiological, in which, as postulated by Darwin, all social and cultural attributes of the individual arise from the action of biological mechanisms; to the humanistic, in which components of the environment are translated into human symbol and meaning and interact only within that framework. Most geographers, it would appear, have uncomfortably occupied the ground between these two extremes, without reconciling them, and yet unwilling to discard either. In more recent times, of course, as indicated by Hewitt and Hare (1973), the issue has been neatly side-stepped by regarding biological and cultural attributes of the individual as equivalent components of a single system, an artificial view which does little to resolve the central dilemma.

(c) The analysis of the human group in geographic thought has followed lines that might be predicted from the view of the individual summarized above. There has been, right from its beginnings, a recognition of the importance of the social, cultural, and historical characteristics of human communities in determining how they interact with the environment; and yet, overwhelmingly, geographers have endorsed Sauer's remark that

> The traits of making a living are for us the dominant things to observe. Until we know much more about them we do not need to concern ourselves much with other qualities of culture. (Sauer 1941b, p.11)

Such "traits of making a living" are said to have their foundations in the direct physical and biological interaction of the population with the environment and in the population ecology of the group. On this interaction is

seen to arise first technological and economic structures, then sociological and cultural. These structures develop their own internal dynamic and influence the ways of living that give rise to them. Once the basic relationships are established, they exert great influence over the particular form the community's development takes: the role of habit, and habitual modes of thought are seen to be characteristic of human communities. There have been some uneasy moments in the history of this well-established paradigm, such as Spoehr's (1956) demonstration of the apparent dominance of cultural priorities in assessing and utilizing the resource base; but it has remained, on the whole, remarkably stable, and has made the current enthusiasm in the journals for Marxist approaches a logical development for the discipline.

(d) However, it has been in the discussion of the relationship between human groups and their environments, between society and milieu, that the ambiguities foreshadowed in the foregoing discussion of the individual, and in the earlier discussion of Hartshorne's views, have emerged with greatest force. On the one hand, collective man is seen as separated from his environment. Thus, whether the relationship is seen in terms of environmental demand and human response, or of the exercise of human free will among a limitless field of possible choices, or of choice undertaken within clear environmental limits, or of simple reciprocity; the fundamental assumption is that of relation, of interaction, between essentially separated entities. This emphasis is particularly marked in the analysis of the effect of man on the environment: the creation of the visible landscape; intervention in a pre-existing environmental harmony; the concrete effects of firing, domestication, clearing, and dispersal of organisms; and even the contemporary global ecological crisis, are all predicated on the separation of man and the environment. Even

ecosystem approaches have, in essence, preserved the separation: it is man _in_ the ecosystem (Stoddart 1965), man _within_ the natural habitat (Sauer 1952) that is analyzed. The geographer, it seems, no less than human civilization as a whole, "has erected a wall between the animal world and himself" (Sorre 1961, p.72).

On the other hand, there is the geographic tradition that is based on the fundamental view of the unity of man and the environment. Thus from Ratzel's organismic notion of the state and its _lebensraum_, to the Vidalian insistence on terrestrial unity, on the essential unity of areas, and on their total character, it is holism that predominates, and relationship that is subsidiary. The further notions of _genres de vie_, and personality, derive from this fundamental view, and give recognition to something that is felt rather than perceived, intuited rather than measured: the idea that although the diversity and difference of human and natural elements exist, and will continue to be maintained, in a given geographical area, they do not detract from--indeed, they fully reflect--a harmony and unity that fundamentally underlies and structures them.

As we have noted, neither of these positions is uncompromising. The relational view, through the analysis of the ecosystem, admits the existence of at least some degree of environmental coordination; likewise, the holistic view incorporates in its view of terrestrial unity the identity of elements that comprise a geographic area. One might therefore be unwilling, particularly in the light of the obviously valuable work that arisen from both positions, to adopt, as Hartshorne has, one at the expense of the other: a resolution that incorporates both would seem to be preferable.

3. Let us take first the identification of the biological and physical aspects of individual and group functioning in the study of environed man. There are two separate matters raised here: the affirmation of the biological and physical nature of the interaction of man and environment; and the assertion of its priority with respect to other kinds of geographic phenomena, be they social, economic, historical, or cultural. We may examine these matters in turn.

From the perspective of the proposed integrated model of the individual, the identification of biological and physical factors seems to be a genuine addition, and to require incorporation somewhere in the model (see Figure 6). On closer inspection, however, it becomes clear that human geographers who have dealt overtly with models of the individual have, in the main, simply assumed that such biological exchange exists, that the existence of individual life is dependent upon their continuance, and have gone on to consider matters they consider to be of greater relevance. Thus, at the level of behaviour and decision-making, we find Golledge (1969, p. 106), for instance, distinguishing between physical space and life space, the first being actual, and the second cognitive, with the analytic emphasis on the latter; Jakle, Brunn, and Roseman (1976, p. 298) locating spatial activity in geographical preferences which arise from the interaction between perceptual and cognitive structures and the object environment, with analytic emphasis on the former; and Golledge, Brown, and Williamson (1972, p. 62) identifying three components of geographical decisions--psychological, economic, and geographic--with analytic emphasis on the first two. At the level of cognition and cognitive structures, it is common ground among geographic theorists that sensory perception is the irreducible basis of cognitive knowledge, and ultimately traceable to

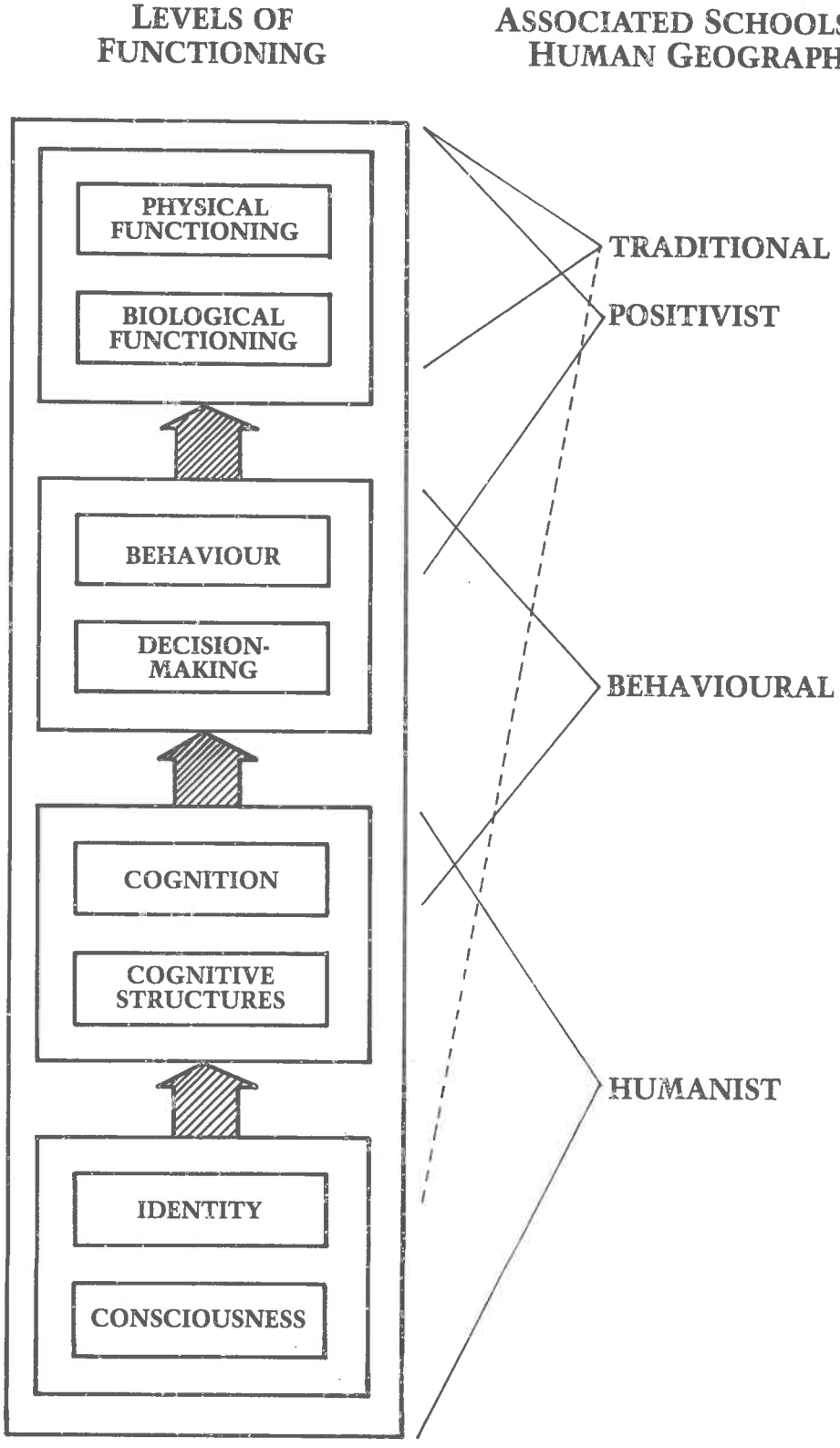

FIGURE 6:

Modification of the Integrated Model of the Geographic Individual

physiological structures, yet moulded and given final form by the existing lineaments of subjective experience. At the level of identity, appeal is made for instance, to physical contact with the environment, which nevertheless only gains significance by virtue of its relationship to the subjective aspects of individual life (Tuan 1974, pp. 96-99), and to physical setting as the focus of home, the interface between personal and geographic identity (Buttimer 1980, p. 171). Even at the level of consciousness, though more confused, there are references to the experience of spaces, landscapes, and places as that "geographicality" which is the "preconscious and preconcepted foundation of Geography" (Relph 1976b, p.1), and to spatiality, the entering into relationship with the objective world (presumably through physical, sensory interaction) as the "necessary condition of human consciousness. . . the beginning of human consciousness" (Samuels 1978, p. 27). But, in almost all instances, once recognition has been accorded to physical and physiological factors, the theoretical analysis has centred on the subjective aspects of human functioning which give such factors real significance in the lived world of human experience. In comparison with the traditional treatment of such factors, therefore, it is clear that the difference lies not so much in the inclusion or non-inclusion of biological and physical factors as in the priority accorded them.

Nevertheless, the change is a significant one, and must be resolved if the proposed reduction is to succeed. The traditional approach to environed man, we have concluded from the foregoing survey, insists upon the fundamental position of biological and physical factors in the hierarchy of human functioning, and the subordination of other levels to it; indeed, the stature of the discipline, identified throughout the twentieth century with

environmental insight, is seen to rest largely on this assumption. The proposed integrated theory, based on the approaches to environed man of the last two decades or so of human geographic theory, as we have seen, inverts this notion: it regards physical and biological aspects of human functioning, as the framework for theory, but in no real sense as the focus of attention, which is placed more on the subjective side of the interaction between mind and environment. Clearly the difference is more than just a shift in emphasis, and involves fundamental conceptual disparities that cannot be ignored.

Much would appear to rest on the force with which the reductive relationships of the hierarchy of the proposed integrated theory can be affirmed. Now, it will be recalled that the analysis of behaviour and decision-making by human geographic theorists was clear in its insistence on the need for embedding that theory in an adequate understanding of cognitive processes. Thus Golledge, Brown and Williamson (1972, p. 63), for instance, point to culturally conditioned attitudes at the root of preferences which frame spatial behaviour; Jakle, Brunn and Roseman (1976, p. 299) to the "cognized environment"; and Wolpert (1972, p. 273) to "needs, drives, and abilities" the cognitive structures through which the individual orders his search procedures and obtains information about the environment. In a similar way, the Piagetian model of cognitive processes was unequivocal in its insistence on the need to understand the more fundamental levels of subjective experience, particularly consciousness itself, through an analysis of overt behaviour and its physiological correlates (Piaget 1971, pp. 47-49). The ordering of the hierarchy of the proposed integrated theory, from most abstract to most concrete, from most subjective to most objective, from the internal and mental to the external and physical, seems, therefore, to be well established.

It follows that if we are unwilling to make fundamental alterations to the proposed integrated theory, particularly in the basic structure of the proposed hierarchy, it is imperative that we find a way of elaborating our theory so that this best and most continuously documented of human geographic observations--the importance of environment in the functioning of both group and individual--is derivable from it, and thus accounted for.

Such a solution may not be so difficult to find. We note, to begin with, that the integrated model is deliberately framed in terms that are independent of particular environments, cultures, or historical periods. It attempts to deal with generic human functioning, with the nature and structure of individual and group life. This is, as we saw in some detail, a fundamental requirement of the scientific status of theory, which must deal in law-like statements of at least methodological universality. But such theory does not tell us much about the particular individual and collective ways of life that have been the domain of the traditional human geographer, and in whose documentation the discipline of geography has been seen chiefly to reside. For that one needs specific information about the particular environments (or milieux, if we want to emphasize the degree to which collective life is historically and culturally influenced) in which those ways of life arise. This is entirely in accord with the schema of scientific explanation developed in chapter II, in which initial conditions must be specified before a law can generate a prediction of a particular event. It seems reasonable to conclude, therefore, that the particular environment in which a group lives is, in a sense, directly responsible for the particular forms of economic and cultural life that emerge there, as compared to the forms which emerge somewhere else. It is evidently a mistake, however, to attribute to the environment, and

therefore to the kind of biological and physical interaction so intimate to traditional human geography, a fundamental position in the ordering of individual and collective life: it is more a proximate cause, a necessary but not sufficient condition. It bears on the particular forms of geographic ways of life; but the existence of those forms, and of the structures and functions that obtain in all of them, must be located in a general model of human functioning of the kind proposed in this dissertation. Such an approach to biological and physical interaction with the environment does, however, at least allow for the preservation of traditional understanding and observation in human geography, and places it within the ambit of the scientific structure of theory and explanation developed here.

Interestingly, this approach is closely akin to Popper's (1957) treatment of historical explanation. He, too, identifies both laws and initial conditions in the study of historical phenomena; but, as was discussed in chapter III, sees as the task of the historian the identification of initial conditions of events, rather than the development of laws and theories, which he accepted must be transcultural and cross-historical, but which were therefore, in his view, doomed to triviality. On closer analysis, we found it necessary to dissent from that view; and we reject, too, the parallel argument that arises in the geographic context. We have argued consistently in this dissertation that for the discipline to achieve internal coherence, intellectual maturity and the development of effective strategies of intervention, the construction of powerful theory, framed along scientific lines, seems to be a prerequisite. The human geographer need not feel compelled to limit geographic understanding to particular ways of life in particular environments, but may seek--indeed, ought to seek--in addition the

more general structures that underly them, in order to develop richer insights and deeper explanations.

4. We now turn to the question of holism in human geographic theory. Holism is one of the main principles that divides the human geography of the first half of the twentieth century--which we have described as traditional human geography--from that of the last three decades. It has deep roots in the history of geographic ideas, tracing its roots back through geographers such as Hettner and Humboldt to the very beginnings of geographic thought in Strabo and the classical geographers. It is, as we have seen, a fundamental tenet of the Vidalian school, expressed in such terms as terrestrial unity, landscape, genres de vie, circulation, and personality. Hartshorne was the most recent, and perhaps the last, to attempt to frame the discipline systematically in its terms: following him, as we have noted, the emphasis has been on analytic understanding more in accord with the perceived ideal of scientific explanation.

Now, as we have argued in section 3.2, and in this chapter, it is difficult to see as valid any other approach to the understanding of wholes than through the parts: ultimately understanding must be analytic understanding. Yet it is certainly arguable that, if this is the only kind of understanding that is available, the geographic whole is not accessible to understanding at all; since clearly as soon as appeal is made to the parts, the whole, in some sense, disappears. This idea has some support in the nature of common experience. It seems simply to be a fact of the human conceptual apparatus to think in holistic, regional terms. No Australian, for example, would be in any doubt as to what was meant by "the West," "the North," "the

Centre," or even "the bush." No South Australian would be confused by "the West coast," "the Flinders," "the South-East, "the Mallee," and so on. Similar examples will be found in every continent, every country, every province and state. There is no question of pursuing an analytic course, of describing, for instance, all the elements of a region, and then appealing to their aggregate for definition and meaning: such abstract and intellectual notions are far from the common experience. It would seem, therefore, that geographic holism is part not only of the heritage of human geographic ideas, but of the fabric of human perception and cognition. It is thus doubly a necessity to see whether it can be satisfactorily accounted for by the integrated theory of the geographic individual proposed in this dissertation. It is important to note, however, that the aim of this exercise is to show not how geographic wholes, such as regions, are constituted from their parts, such as natural, economic, spatial and cultural factors, but how the conviction of geographic holism, in human geographic theory and in the common experience, might arise.

The central element of both the theory and the experience of geographic holism is a belief in the fundamental connectedness--even unity--of man and nature. The survey of traditional human geography outlined above seems to support the view that this theme, more than any other, has been the framework of geographic thinking in the first half of the twentieth century. As we noted, it goes well beyond the purely biological and physical, although this has been a dominant concern, to the more abstract affective and intuitive areas of human experience, as expressed, for instance, in the Vidalian concept of the personality of geographic regions. The question, then, is whether such geographic holism can be systematically derived from the integrated theory of the geographic individual proposed above. A derivation of this kind would

strengthen the integrated theory to the extent that it proves reasonable, or likely to be reasonable.

One way to approach the idea of the connectedness, or unity of man and nature, is through the notion of reciprocity. If we can show that the structure and function of both human and natural systems have been formed and transformed by mutual interaction, we can postulate a growing convergence and unity of the kind geographical holism represents. This approach was foreshadowed in our discussion of Hartshorne's analysis of relationship and holism in geography in section 5.1. In cybernetic theory, it is located in the property of feedback, which can be defined as circularity of action existing between the parts of a dynamic system (Ashby 1956, p. 53). Equilibration and regulation, terms familiar to us from the discussion of Piaget, result from the ongoing application of the feedback process. Though it is not reasonable to expect that the identification of feedback relations between human and environmental elements will provide a complete explanation of geographical holism, since in such macroscopic, complex systems feedback relations become innumerable, we may reasonably expect through it to gain real insight into the internal dynamics of holism. As Piaget remarks:

> . . . the living organization is the organization of an exchange system, and the term "organization" simply designates the internal aspect of a system which is in a state of perpetual adaptation . . . there is no organizing function, at whatever level, that does not harmonize with the environment. (Piaget 1971, p. 345)

The nature of reciprocity varies according to the level of its operation (see Figure 7). At the level of biological and physical interaction, it is, as we have seen, well established in human geographic theory: we have noted that, in the main, human geographers of the first half of the twentieth century have

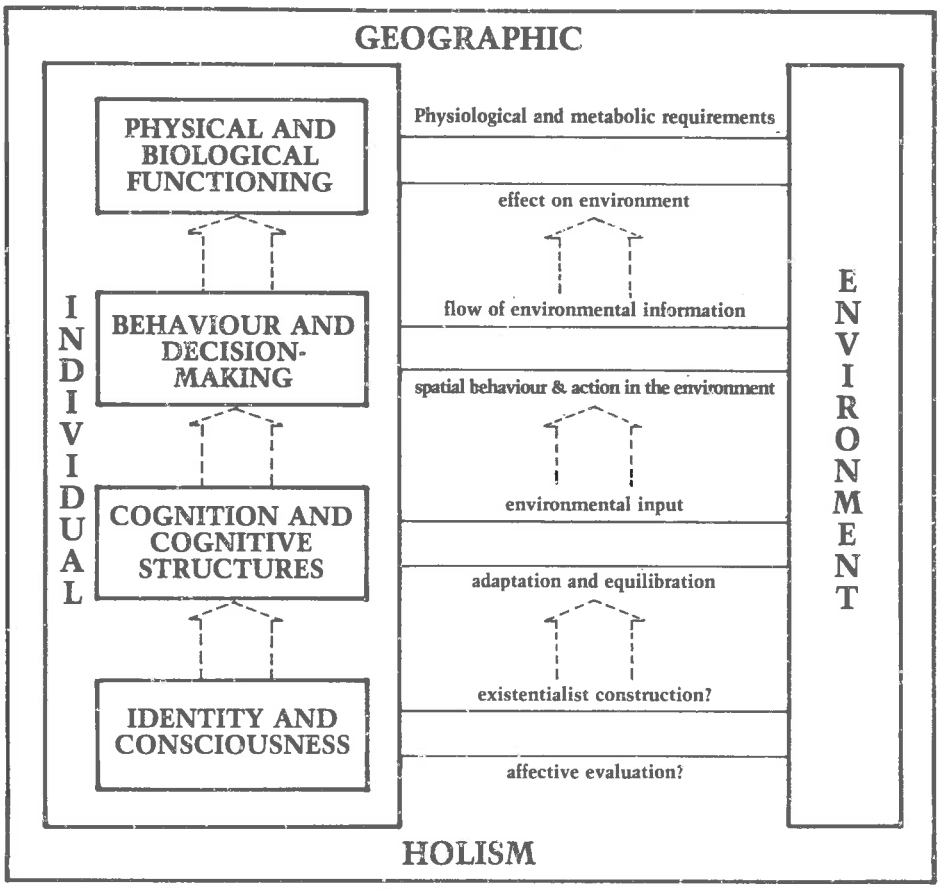

FIGURE 7:

The Analysis of Geographic Holism

followed Darwin in asserting that the individual human organism is subject to the same laws as all other life forms, and that through the agency of natural selection of the genetic base, the biological functioning of the human organism is over time brought into close and closer functional unity with the environment. At this level, moreover, it is not difficult to derive the macroscopic from the microscopic; the understanding of the biological environmental relations of human populations becomes simply an aspect of ecological theory in general, in which the reductive approach is well established:

> . . . to understand mechanisms of changes in a population, an ecologist will study mechanisms that operate on individual organisms and will try to view the significance of these population events in a community and ecosystem framework. (Krebs 1978, p. 11)

The expression is an illuminating one: "try[ing] to view the significance" of individual events at the community level is just what is involved in the process of developing reductive explanations, even though the formal conditions for reduction specified by Nagel (1961)--the conditions of connectibility and derivability--may not be explicitly stated. It may indeed be the relative simplicity of making this logical connection that has produced in traditional human geography such emphasis on biological and physical factors, on the influence of the environment on human functioning. We note, however, the point made by some human geographers, notably Chorley (1973) and Hewitt and Hare (1973) in modern times, and by the possibilist approach in earlier times, that this reciprocity does not provide a complete understanding of the relationship between human and natural systems, since it is mediated by the intermediary structures of technology, and hence by the structures of thought associated

with them. This recognition simply affirms the reductive hierarchy proposed in this dissertation. and requires treatment of reciprocity at these levels in order to develop a complete model of interaction.

At the level of behaviour and decision-making. the cybernetic model seems to work well. We recall that decision-making is identified as the variable which underlies behaviour, and hence the phenomena--location. spatial structure and process. ecological relations, and so on--with which human geography deals. We saw also that it was a fundamental tenet of this approach that individual decision-making could be aggregated through appropriate statistical and probabilistic models to yield models of collective behaviour. Jakle, Brunn and Roseman (1976, pp. 298-299) emphasize the ongoing. reciprocal nature of this process: spatial activity is derived from geographical preferences and decisions, which in turn have their foundation in the interaction between perceptual and cognitive structures and the environment. Studies of migration behaviour (Wolpert 1964, 1970, 1972), we saw, support this view, and point to a regulative interaction in which aspirations adjust to the attainable, as determined by the results of ongoing experience. We noted, however, that again this approach to decision-making does not account exhaustively for the human-environmental complex. even when aggregated to macroscopic domains, since the environment is always the cognized environment, and a full understanding of interaction at the behavioural level demands understanding of cognition and cognitive structures, as proposed in the hierarchy of the integrated theory.

At the level of cognition, the role of feedback, regulative interaction, and equilibration in the formation and development of cognitive structures is well-established in human geographic theory, where, as we have seen, the Piagetian conceptual framework has been largely adopted. In that framework, it

will be recalled, cognitive schemas evolve through an assimilative process by continuous interaction with the environment. In this view, then, the distinction between natural and human systems becomes increasingly difficult to maintain: it becomes impossible to think of cognition in isolation, independent of the environment, since without formative interaction with the environment no cognitive structures would be developed, and only the substratum of biological connections would remain; though that, too, is an artificial concept, since the mere existence of biological structures implies the existence of the environment in which they function. A similar conclusion, from a different perspective, is reached by Bateson (1979), whose approach to epistemology through information theory attempts to demonstrate that between structures of mind and structures of nature, or environment, there is a necessary and indivisible unity. Again, however, as we have seen, cognitive theorists accept that the structure and function of cognition is embedded in the more fundamental aspects of identity and consciousness, and requires an understanding of these levels in order to be complete.

With respect to identity, we saw an unequivocal insistence by human geographers upon the construction of identity by entering into relationship with the external world. Thus Jakle, Brunn and Roseman (1976, pp. 10-21) see identity as the product of interaction with the milieu at large; including, following sociologists such as Berger and Luckmann (1967), the sociological milieu, through the adaptation of roles and role-modelled behaviour to group expectations. Place is seen to have a similar effect on the construction on identity, as Tuan (1974) analyzed through the notion of topophilia. Lowenthal (1975, 1979, 1981) emphasizes the historical dimension of place in structuring identity; and idea of home is seen by a number of theorists (Wagner 1972;

Porteous 1976; Sopher 1979; Buttimer 1980; Knight 1982) to embody the role of the geographical environment in creating and preserving identity. It is worth nothing, too, that by environment, in this context, more than in any other of the integrated theory, is meant a local or regional environment, a place of continuous habitation and intimate familiarity, perceived and identified with as a whole. Here, we may therefore feel, is a central locus in the individual of those affective and intuitive convictions of geographic holism which we have suggested underpin the traditional geographic understanding of environed man.

When we come to consider the treatment of consciousness in human geography, however, we appear to find less unanimity. On the one hand, existential human geography asserts that the construction of consciousness is through spatiality, "the necessary condition of human consciousness . . . the beginning of human consciousness" (Samuels 1978, p. 27). This account, however, emphasizing the objectification of the world and the entering into relationship with it, is difficult to distinguish from the constructivist account of identity, and does not, we may feel, deal adequately at the more abstract level we might reasonably associate with the idea of human consciousness. Phenomenological human geography does, as we have seen, attempt to deal in such terms. It seeks the general "forms" and "essences" of consciousness, specifically identified as being "transcendental" in nature (Entrikin 1976, p. 618). But again it is clear, on closer inspection, that when applied to geographical theory, these abstract forms are really quite concrete; even though, in not being continuously in the forefront of awareness, they may be termed "preconscious," "preconcepted" (Relph 1976b, p. 1), "preflective," "taken-for-granted" and "precognitive" (Buttimer 1977, p. 281). They are, it is asserted, quite identifiable structures:

> . . . the varied and even contradictory experiences we have of spaces, landscapes, and places combine the qualities and appearances of these with our moods and attitudes, and these experiences are fused in "geographicality," the preconscious and preconcepted foundation of Geography. (Relph 1976b, p. 1)

Again, therefore, although there is a clear difference of perspective in the two approaches, in essence the conclusion is similar: that human consciousness is in large part the product of the experience of place and environment. The mechanism by which such experience is generated and develops is more explicitly detailed in the existentialist view, though even there clarity sufficient for the operationalizing of key terms is lacking. Such as it is, however, it seems to approximate the interactive, feedback model proposed here. In the phenomenological view, the internal dynamics of the central experience of space and place is entirely obscure.

There is reason, moreover, to feel uneasy about the difficulty of distinguishing between the geographic account of the construction of identity and the construction of consciousness through interaction with the environment. If the processes of formation are identical, as, it would seem, humanist geographers are asserting. can the terms be differentiated? Is not identity then the same as consciousness? And yet to assert that consciousness is nothing other than identity does not conform either to our intuitive notions of human functioning or to the perspectives of cognate disciplines--such as psychology and evolutionary biology--for whom consciousness is not only different to, but more fundamental than, identity. This matter is taken up on more detail in the concluding chapter.

It seems reasonable to conclude, then, that an interactive approach of this kind to the different levels of individual functioning shows real promise of accounting for geographic holism, as described in traditional human

geography. As an elaboration of the proposed integrated model of the environed individual, it inherits the same difficulties; particularly, as we have noted, in dealing at the most abstract levels of human experience, identity and consciousness. Again, because of their primary position in the hierarchy of human functioning, the ambiguities and unresolved conceptual difficulties associated with the geographic analysis of these levels presents a serious challenge to the whole approach. But, viewed in broad terms, this approach does seem to promise significant advances in the theory of geographic holism; and, in so doing, it provides an indication of one kind of geographical insight the reductive, or vertical, theoretical thrust advocated in this dissertation might provide. In the theory of human geography, holism is, as we have seen, a flexible--not to say formless-notion which oscillates uneasily between objective and subjective modes of analysis. On the one hand, as the Vidalian school exemplifies, there are the "tangible indices" of geographical phenomena, such as local natural resources, lines of material production, dietary patterns, agricultural and non-agricultural activities, and the dynamics of spatial distribution (Buttimer 1971, p. 55): all these can be dealt with quite objectively. On the other hand, there is the subjective, intuitive conviction associated with the idea of the personality of a region, a conviction which seems to defy analytic description, but which is, nevertheless, potent in the conceptual categories of the common geographic mind; and which, moreover, is fundamentally responsible for framing the domain in which the objective approach is to operate. To deal with the parameters of the objective approach alone is to risk depriving geographical holism of its significance in human experience: and the subjective approach carries the complementary risks of relativism and untestability. The potential value of the model advocated in

this dissertation may, in this context, be a capacity, in its more comprehensive, integrated account of human functioning, to provide a theoretical basis for incorporating the insights of both approaches. In coupling them to each other, it mutually frees them from difficulties which, as separate approaches, neither can adequately solve; and it provides for them just that enrichment which, as separate approaches, each demands. Thus genres de vie, for example, are seen to derive not just from the patterns of overt, physical behaviour in the environment, but from the more fundamental structures of subjective experience that are intrinsic to human functioning. In a similar way, the experience of home, or of personality of a region, is seen to be intimately associated with the biological, physical and sensory interaction that every individual environment experiences in interaction with the environment as well as with affective and intuitive factors.

Finally, in its greater comprehensiveness and power, it promises to provide a deep theoretical basis for the understanding of the way in which human communities utilize and identify with their environment; and, on this basis, to construct, for instance, modes of planning and administration that are more effective, more enduring, and better able to enhance the quality of human life, as it is lived in a given geographical area. It is worth emphasizing this kind of potential to operate at a macroscopic level, since, in analyzing environmental interaction in the individual, it is easy to question whether such an analysis has anything to do with the geographic domain, as we have been accustomed to understand it. But, as has been consistently proposed in this dissertation, the purpose of reductive analysis is to gain a greater power and depth of the theoretical base; and always that theoretical enlargement can be brought to the macroscopic level again, by the reverse

process of deduction. We note again that in the possibility of that process the main justification for the approach of this dissertation lies.

CHAPTER VI

CONCLUSION:
TOWARDS A HUMANIZED SCIENCE OF HUMAN GEOGRAPHY

Outline of the Chapter

6.1 Review

- Summary of main themes
- Conclusions
  - Integrated nature of the model
  - Scientific adequacy of the model
  - Account of consciousness required

6.2 The perspective of cognate disciplines

- Evolutionary biology
  - Role of consciousness
  - Self-reflexive nature of consciousness
- Psychology
  - Subjective methodology
  - Stream-of-consciousness approaches
  - ASC strategies
    - Contemporary
    - Traditional
  - Assessment
    - Empirical support
    - Theoretical interpretations
    - Research programme

6.3 Towards a humanized science of human geography

- Implications of approach not yet complete
- Continuation and enhancement of existing schools
- Potential integration of past and present
- Scientific structure
  - Cumulative development of theory
  - Cooperative exchange between schools
  - Empirical fact and field work
  - Theoretical development
  - Cooperation with cognate disciplines

CHAPTER VI

CONCLUSION:
TOWARDS A HUMANIZED SCIENCE OF HUMAN GEOGRAPHY

6.1 Review: summary and conclusions

It will be valuable, in attempting to draw the diverse and often complex arguments of the preceding five chapters together into a conclusion, first to review the major themes that have so far been developed in this dissertation.

Our point of departure was the evident fact of fragmentation (or diversification, depending on one's point of view) of contemporary human geography. Accompanying this process of fragmentation has been an unprecedented debate about the conceptual foundations of the discipline. So rapid, and so extreme, has been the divergence of the fundamentally different conceptual, methodological, philosophical, and ethical positions that the future of human geography as an integrated, viable discipline seems to be at risk.

These contemporary disagreements about the nature and goals of human geography stand in marked contrast to the relative stability of, and satisfaction with, the discipline during the first half of the century. In that time, debates about conceptual matters, such as that between determinism and possibilism, occurred within an essentially agreed framework of the nature, if not the specific kind, of the relationship obtaining between human and

natural systems. Here there was satisfaction with the real achievements of the discipline, and confidence in its continued relevance for the contemporary world.

In seeking the roots of the disintegration of the traditional discipline, it is valuable to seek the underlying conceptual dynamic, the conceptual continuity that, historically, has underlain the overt changes. From this perspective, it is clear that the relative contentment of traditional geography was illusory, since it failed to recognize the increasing shallowness of its theoretical base, and its consequent ineffectiveness in framing practical policy. Towards the end of the 1950's, and the beginning of the 1960's, a critique was launched that was so thoroughgoing in its scope, and so radical in its proposals, that it came to be recognized as a "revolution": the Quantitative Revolution. Although the adoption of quantitative methods was a central element of the transformation, it was in reality a conceptual revolution, and had its roots in the demand by Schaefer (1953), Bunge (1962) and others for the framing of the discipline as a science, with its basis in a core of recognizable, systematic theory. By adopting this course, for the discipline, it was felt that advantage could be taken of the systematic, cumulative growth, and the intrinsic grounding in reality, so attractively demonstrated by the natural sciences. So persuasive was this argument that it came to be widely and quickly adopted as the paradigm for the discipline.

By the late 1960s, however, concern had come to be expressed by human geographers that the increasing scientific rigour of the discipline had been achieved at the expense of the human realities it was attempting to reflect. An initial response to this concern was to seek models of the individual that, within the prevailing paradigm, might systematically enhance the relevance of

theory to human life and living: this approach led to the behavioural school. A more uncompromising approach was taken by a number of geographers, of diverse backgrounds and philosophical positions, who demanded a greater emphasis on the subjective experience by human beings of geographic phenomena: collectively they have come to be known as the humanist school. Marxist human geography, although not, for reasons of space, dealt with in this dissertation, also evolved out of this reaction.

Two discontinuities, or foci of change, can therefore be identified as fundamental to the present fragmentation of the discipline. The first separates the traditional human geography of the first half of this century from the human geography of the last two and a half decades. The second separates, within those last twenty-five years, the decade of unchallenged dominance of quantitative techniques from its succeeding years, where that approach, though still dominant, is under widespread challenge from behavioural, humanist, Marxist, and even traditional perspectives. It is clear that both discontinuities turn on two themes: the idea of a scientific discipline; and the idea of the relevance of a discipline to contemporary life. In coming to an understanding of the underlying conceptual dynamic that has led to the contemporary fragmentation. it would seem to be valuable, therefore, to focus on these notions. Through such an approach it is possible to conceive of an integrated human geography, framed as a science yet inclusive of fundamental human realities. in which both traditional and contemporary insights may find a place.

It is logical, then, to begin seeking this understanding in the nature of scientific inquiry itself. Although human geographers have so widely and enthusiastically adopted the model of the natural sciences for their

discipline, it appears, on closer analysis, that few have undertaken any rigorous analysis of the idea of science, or come to any profound understanding of it. This seems to be as true for early accounts of scientific human geography, such as Bunge (1962) and Cole and King (1968), as it is for later accounts, such as Smith (1975) and Amedeo and Golledge (1975). Even such a central text as Harvey's (1969a), which seeks methodological underpinnings for the whole discipline as a science, seems to flounder in misconceptions and contradictions. A more systematic and comprehensive approach to the nature of science thus seems unavoidable, if the inquiry is to proceed.

A chief difficulty of coming to an appreciation of science is that the philosophy of science is itself substantially fragmented. Prior to 1962, the prevailing orthodoxy was that of Popper, Hempel, Nagel, and others. It emphasized the deductive logic that was said to underlie scientific activity. It saw scientific inquiry as proceeding steadily through the creation of more and more comprehensive and powerful hypotheses, subjected to the most stringent tests, and accorded increasing degrees of confirmation and corroboration, until, on the basis of well established laws, whole bodies of theory can be constructed. Under this view, science was a logical, rational enterprise, in which the claims of competing theories could be evaluated by anyone competent to assess the evidence.

On closer analysis, however, it becomes clear that at almost every stage of this model subjective judgements have to be made. Kuhn directed attention to what he saw as this subjective ground of science. Seeking support from the history, rather from the proposed logic, of science, Kuhn identified a pervasive structure of normal science, dominated by a particular paradigm at any given time. Long periods of stability, during which science seeks simply

to solve the remaining puzzles of the paradigm, are brought to a conclusion only by the repeated identification of anomalies. The discipline is plunged into crisis which is resolved by the adoption of a new paradigm in a scientific revolution. The debate between competing paradigms is neither conducted nor concluded rationally, since scientific theories are inherently incommensurable: the decision in favour of a particular theory must be subjective, and is based as much on the future promise as on the present performance of the rival theories. In Kuhn's view, therefore, there is no overarching progress in science towards some goal of complete truth: there is simply the progress which derives, in each historical epoch, from solving the remaining puzzles of the prevailing paradigm.

Kuhn's challenge to the traditional orthodoxy has produced a widespread debate which is still far from resolved. Popper dismisses Kuhn's argument out of hand. For him, normal science is bad science; extended debate between different theories is not only historical fact but historically fruitful; and commensurability of scientific theories is both logically conceivable and practical. He sees Kuhn's approach as psychological, and therefore, given the disarray of that discipline, of little worth. Feyerabend, on the other hand, supports Kuhn's identification of a principle of tenacity in science, and his insistence on the incommensurability of scientific theories. He derives from this, however, a view of science as essentially irrational and anarchistic, in which the only principle is "anything goes" (Feyerabend 1975).

A more balanced response to Kuhn is that of Lakatos, who appeals both to logical and to historical considerations in proposing his view of science. He takes as a philosophical foundation Popper's sophisticated falsificationism, which accepts that methodological decisions about unproblematic background

knowledge, the nature of corroboration, and the application of statistical decision rules must be made in an *a priori* fashion before any scientific inquiry can proceed. Attention is then directed not to single theories, as in the orthodox model, but to series of theories, in which one theory replaces another on the basis of excess empirical. Where the initial theory is sufficiently powerful, such a sequence of theories assumes the character of a scientific research programme, in which a core of theory is taken as given, and therefore beyond the reach of experimental science, as distinct from a belt of associated theory, to which experimental findings are directed. The progress of science is therefore mainly on the level of theory; indeed, theoretical science is seen as largely autonomous. Intermittent verification is necessary, however, to maintain the momentum of the research programme. Ultimately, the orthodox logic of refutation is preserved, although it is seen to be applied more strictly to mature than to immature research programmes. In addition, historical processes which lead to the tenacity of scientific theories are accounted for. Finally, by adopting Popper's position on the commensurability of theories, and by resting commensurability more fundamentally on excess empirical content, Lakatos is able to insist on the progressive nature of the scientific enterprise, as defined by the move of scientific theory towards greater comprehensiveness, and finds it unnecessary to assert the kind of teleological convictions that Kuhn regards as inseparable from the notion of progress. As a model of scientific inquiry, then, the Lakatosian approach seems to be the most legitimate, both logically and historically, of any so far developed. It is therefore to this model that reference can best be made in the attempt to frame a discipline as a science.

Human geography, however, deals with human beings in interaction with the environment. If it is to be a science, it must be a social science; and the

philosophy of social science has located a number of fundamental problems in the attempt to treat human beings, both individual and collective, in a scientific way. Again, by and large, geographers have tended to ignore these problems, or to treat them only superficially, as in Harvey (1969a). Guelke (1982) is an exception, although he seems to fall into the opposite trap of accepting a particular philosophical position--that of Collingwood (1948)--too uncritically. Again, therefore, for human geography, as for all the social sciences, there seems to be no escaping a thorough and systematic approach to these problems if the conceptual basis of the discipline is to be adequately secured.

The attempt to create a social science, a science of collective or of individual human life, is predicated on the assumption that there is a unity of method between the natural and the social domains of inquiry. This doctrine, first enunciated by Mill, has been taken by a number of orthodox theorists as justification for applying positivist modes of analysis and methodologies of investigation to human phenomena. Thus Rudner (1966) advocates a more or less unaltered transference of positivist concepts to social phenomena; Popper (1957) espouses the use of standard explanatory schema in analyzing social phenomena, with a change of emphasis in the specific case of historical understanding; and Nagel (1961), while conceding the real philosophic and logical difficulties of approaching social phenomena scientifically, concludes that a cautious development in that direction can be justified.

Critiques of positivistic approaches to social phenomena have been, however, trenchant and convincing. Many have based their challenges on the seminal work by Winch (1958). Winch's position rests on the observation that the meaning of any statement about the nature of social phenomena is

'systematically ambiguous': it varies with the social context itself. Understanding of social phenomena cannot therefore be had through observation, in the manner of the natural sciences: it must come from the inside, by appreciating the rules and motives that guide behaviour in a particular society. Between natural and social inquiry there is thus a difference not just of degree, as positivists claim (social phenomena are more complex), but of kind. Positivist rebuttals of Winch's position have been, by and large, unconvincing.

The other seminal critique of positivistic social inquiry is that of Collingwood (1948). Mounted in the context of historical inquiry, Collingwood's critique, like Winch's, emphasizes the intelligibility of human action. It seeks understanding in the thought behind an action, rather than the external circumstances surrounding it. Like Winch, again, it emphasizes the uniqueness of historical contexts, and the central role played by imagination and critical thinking in coming to an understanding of the inner springs of action. Responses to Collingwood have, however, been more convincing than those to Winch. Carr (1961), for instance, speaking for those actually engaged in historical research, accepts the inevitably subjective nature of historical inquiry, but argues convincingly for a rational approach to historical understanding based on a provisional selection of facts, and on ongoing interaction between fact and interpretation. On this foundation, a broadly scientific approach to social and historical phenomena, capable of dealing in causal terms, becomes feasible. There are, furthermore, epistemological and methodological difficulties in the positions adopted by Winch and Collingwood, and both exhibit much confusion about the nature of science. Given the close correspondence of principle and method of Carr and

Lakatos, and apparent success of their shared model of inquiry in meeting the objections raised by Winch and Collingwood, and their followers, it is concluded that the Lakatosian scientific research programme offers a legitimate approach to the science of social, as well as natural, phenomena.

The most important element of a scientific research programme is, it has been noted, a powerful theoretical base. Power is conferred upon scientific theory to the degree that it is systematically structured along deductive lines, and to the degree that its primitive terms comprehensively reflect the way things really are. In the context of human geography, such primitive terms may not be immediately identifiable since, as a discipline, it deals predominantly with collectivities: groups, communities, neighbourhoods, societies. cultures. It is clear, however, that the analysis of human collectivities rests centrally on an understanding of the human individual; although, equally clearly, there is a reciprocal relationship between individual and group. Assumptions about the individual--about the internal structure of individual functioning and about the way the individual interacts with the environment--are fundamental to the modern theory of human geography: Haggett (1965), Smith (1975) and Buttimer (1980) are cited as examples. In framing human geography as a science along Lakatosian lines, it seems necessary first to seek a valid and relevant theory of the geographic individual.

A theory of the environed individual that is capable of generating and sustaining a scientific research programme must deal in generic properties, and at a fundamental level of human experience. In the geographic literature, different levels of the geographic individual have been analyzed and employed as a basis for the development of theory at the collective level. It is proposed, however, that an integrated model of the geographic individual is

likely to be a more effective theoretical approach, since it presents the individual as a whole, and thus more closely approximates the reality of human experience. It thus promises to meet the demands of humanist geographers, while retaining the structure of a scientific theory. It further holds the possibility of integrating a number of the widely divergent approaches of contemporary human geography.

From the perspective of this theoretical framework, the geographic treatment of the individual assumes a more ordered form. At the most concrete level is the approach to geographic action and decision-making, which emphasizes models of rational evaluation of information about relevant economic and geographic factors. It accepts, however, the priority of psychological and cognitive factors in structuring the acquisition, the assessment, and the behavioural implications of the information derived.

At the more abstract level of cognition and cognitive structures, human geographers have tended to adopt a model that is in broad agreement with the theory and empirical findings of Piaget and his co-workers, which centre on the construction and ongoing modification of cognitive schemas in interaction with the environment. This model emphasizes, however, the reciprocal influence of individual and collective schemas, and extends the scope of such schemas to "transcendent provinces of meaning," in which individual experience of the environment and the construction of individual identity are embedded. It therefore follows the direction indicated by Piaget of seeking in the roots of human experience itself, in identity and consciousness, the foundations of cognitive functioning.

In approaching this most abstract level of the geographic individual, human geographers have, in the main, looked to constructivist and experiential

theories, particularly existentialism and phenomenology. Identity is seen primarily in terms of identification with the milieu, both physical and human, as expressed in the idea of place: a well-established identity of this kind is seen to be essential to the stability and health of human functioning as a whole. The mechanism by which the geographic elements of identity are constructed have been sought in phenomenological method, which seeks to locate the "pre-concepted" foundation of subjective geographical experience, the experiential foundation of geographical knowledge. By contrast, existentialist geographers have adopted an objective approach to the way in which the human individual is compelled to "enter into relationship" with the environment and to create, therefore, his or her own form of consciousness.

The adequacy of this model of the geographic individual may be assessed in relation to the principles of scientific inquiry established above. There are three main elements of this assessment. The first examines the law-like status of the statements that comprise the theory. This is seen to be most substantial at the level of cognition, less for action and decision-making, and wholly ambiguous for identity and consciousness. The second examines the internal structure of the theory, where real difficulties are presented by differences of clarity, testability and predictive power of the various levels; and further examines its external supports, where behaviour and cognition are seen to rest on a vast body of associated theory and evidence, and theories of identity and consciousness emerge as lacking not only evidential support, but the conceptual precision that is necessary for the gathering of such support. The third examines the status of the integrated account as a scientific research programme, and concludes that it cannot yet lay claim to that title: the assumed background knowledge is highly problematical; and the core theory,

though well-established for cognition, is tentative only for behaviour, and hardly at all available in systematic form for identity and consciousness. Corroborative status follows a similar pattern. It is therefore concluded that central to the success of this model is an adequate understanding of the abstract levels of human functioning, as expressed in identity and, more fundamentally still, in consciousness itself. If such an understanding could be formulated, the comprehensive nature of the integrated account, the leniency with which an emerging research programme must be evaluated, and the theoretical coordination promised by the hierarchical relations of dependence together seem to indicate the potential legitimacy of the integrated account as a genuine scientific research programme.

The analysis so far suggests that this approach to the theory of human geography may go some way to resolving the second of the discontinuities proposed to be central to an understanding of the current state of the discipline: that between scientific and humanistic perspectives. There remains the first discontinuity: that between traditional and contemporary concerns in the discipline. An important criterion of the legitimacy of the proposed integrated account of the individual is its ability to generate a systematic understanding of traditional insights.

Certain themes have persisted throughout twentieth century human geography. A central question has been the nature of description that is to be used of human groups and their environments. One perspective, with its roots in Humboldt, emphasizes holism; the other, following Marsh, emphasizes relationship. Hartshorne, in his definitive review of the historical development of this theme, strongly supports the holistic view as characteristic of geography; but while this is in accord with much of the best

of twentieth century geography, as exemplified in the French school, Hartshorne's logic is difficult to follow. It is concluded that both approaches are legitimate, and apparently reciprocal.

In a similar way, the debate between determinism and possibilism, a central matter of theoretical contention in the discipline for over four decades, seems, on closer analysis, to have been largely misconceived. Proponents of both schools appear to have accepted the Darwinian position of the essentially biological nature of the interaction of human groups and individuals with the environment, while accepting the significance of human thought and action in framing that relationship. The difference is more one of perspective than of substance: reciprocity and holism again are shared positions of potential resolution.

What a human geography based on such reconciling principles would be like is shown by the French school. Although its founder, Vidal, is often associated with the possibilist position, a close reading of the works of Vidal and his followers suggests that, in the four decades or more of its existence, this school developed an approach to human groups and their environment that was conceptually integrative of the divisions that marked the discipline. In its main themes of terrestrial unity, active and passive relationship, landscape, genre de vie and personality, the theoretical foundation supports an insistence on the essential creativity and power of human technology and culture; and the holistic patterns of geographic association arise, in large part, out of the internal dynamics of groups and circulation. Such was the richness and subtlety of the regional monographs expressive of these themes that they became, for many decades, the paradigmatic exemplars of geography at its best: to some they have that status still.

Another central theme of human geography in the twentieth century has been that of the ecology of human groups in the environment. With its conceptual foundations firmly grounded in the biological approach to the interaction of man and environment, this development was coincident with the rise of biogeographical theory, of which it essentially occupied a subordinate sector. In models of the individual and of the ecosystem, it elaborated the biological foundations of geographical understanding, to the virtual exclusion of social and cultural factors. Even in coming to an understanding of the effect of man on the environment--a theme well established by Marsh, but revived only in recent times--this perspective prevailed. In its move away from the more subjective aspects of collectively environed life, in culture and society, towards the objective patterns of biological interaction. this conceptual tradition effectively represented a contraction in the scope of Vidalian insights. though it deepened the analysis in its restricted domain.

In evaluating the degree to which the proposed integrated account of the individual is capable of systematically generating theory which might account for these kinds of insights, one notes firstly the logical feasibility of a reductive approach to collective phenomena. Neither the ability to frame collective theory, nor the problems presented by hierarchical functioning, nor the difficulty of defining elements independently of wholes represents an inseparable obstacle to reduction.

Secondly, it is acknowledged that the identification of biological and physical factors, and the assertion of their priority in the hierarchy of human interaction with the environment, seem to demand an addition to the integrated account of the individual; although it is also remarked that such factors are present in the geographic treatment of the individual as accepted <u>a priori</u>

assumptions. Since the hierarchy of functioning of the integrated account, under which the more concrete levels of behaviour and action in the environment are found to have their roots in the more abstract structures of cognition. identity and consciousness, seems to be well established, the traditional location of biological and physical factors at the basis of the hierarchy is not accepted. Instead, it is proposed that they are best seen as representing initial conditions in the orthodox schema of deductive-nomological explanation.

Thirdly, it is proposed that the connectedness between man and nature which lies at the heart of geographical holism finds a substantial theoretical foundation in the integrated account of the individual following a cybernetic model of feedback and reciprocity. At biological, behavioural and cognitive levels, such processes may be clearly identified; and the analysis again supports the ordering of the hierarchical relations between the different levels. The treatment of identity and consciousness, however, again exhibits fundamental ambiguities and inadequacies, in both its theoretical structure, and in its inability to distinguish between identity and consciousness, as common experience and intellectual analysis seem to demand; and thus, because of its primary position in the hierarchy, places at risk the otherwise promising thrust of the integrated theory in accounting for the holistic insights of traditional human geography.

Looking in this way at the development of the dissertation as a whole, it may be concluded that its broad thrust has been to attempt to establish first a sound notion of science within which to frame the discipline; and then to develop a logical strategy--that of reduction, centreing on the functioning of the geographic individual--by which to proceed in actually developing the conceptual structure of the discipline. It has been undertaken in the

conviction that conceptual clarification at this fundamental level may lead positivist human geography towards a rapprochement with humanist positions, and towards a more effective incorporation of traditional insights. The strategy has therefore been to attempt to construct a theoretical stance, potentially integrative of the main contemporary scientific and humanistic schools of human geography; and to evaluate, firstly, to what extent such a stance meets the criteria of science previously established; and secondly, to what extent it is able to account for the insights of traditional human geography.

The conclusions of this inquiry may be stated as follows:

(1) A theoretical stance potentially integrative of the main contemporary schools of human geography can be constructed by adopting a hierarchical approach to the functioning of the geographic individual.

(2) This model of the geographic individual shows promise of meeting the criteria of scientific theory (as established) at the more overt levels of behaviour and decision-making, and cognition and cognitive structure; but is not yet able to meet those criteria at the more abstract levels of identity and consciousness. This inadequacy is so substantial, and operates at such a primary level of the model that it places at risk the validity and applicability of the model as a whole.

(3) With respect to traditional human geography, assumptions implicit to the model which relate to biological and physical interaction with the environment need to be more explicitly developed to enable it to generate an effective explanation of traditional insights. More fundamentally, the ability of the model to deal with the enduring traditional identification of, and approach to, geographic holism seems to be promising at the levels of biological and physical interaction, behaviour and decision-making, and

cognition and cognitive structure; but again is severely restricted by its inadequacies at the level of identity and consciousness.

A major conclusion of this inquiry is, therefore, that central to the success of the enterprise here undertaken is the development of a systematic account of identity and consciousness in the geographic individual. Our analysis suggests, moreover, particular properties that ought to be exhibited by such a systematic account: formalization and rigour in the framing of its terms and statements; a core theory sufficiently well articulated to be capable of deriving a scientific research programme; theoretical statements which reflect generic descriptions of human functioning in the environment, invariant across cultural and historical groupings; clear bridge principles, linking the account to bodies of theory external to it; and corroborative support from such external theories. These are formidable demands to meet; and yet they have been met by a great many theories in the course of the history of scientific inquiry. There seems to be no a priori reason to assume that they cannot also be met at the more abstract levels of human experience, though the difficulties of doing so are not underestimated.

Moreover, our analysis seems to indicate that the focus of the required account may be able to be narrowed still further. It was noted, in the course of our analysis of identity and consciousness, that human geographers have not, in the main, been able to distinguish between the two concepts in the environed individual. On closer inspection, it appears that this theoretical opaqueness has its roots more in the idea of consciousness than of identity. Identity is, at least to some extent, accounted for in constructivist terms that point in the direction of cognitive theory, and lend themselves to the kind of geographical treatment that we saw in the humanist account of place. Such

treatment, however, rests on the implicit distinction between identity and a generalized level of human functioning that is connected, either explicitly or implicitly, to the idea of human consciousness. Identity is described and analyzed in terms of an underlying continuum of human functioning, or human potential, that is given particular form by particular geographic conditions. The impress of social stereotypes and of place (Jakle, Brunn and Roseman 1976); of sensual and physical contact with the material environment (Tuan 1974); of the continuity of the past, of tradition and history (Lowenthal 1975; 1979; 1982) is said to structure identity: but in what continuum of human potentiality is that impress made? The implicit answer seems to point in the direction of that abstract, general functioning--necessarily invariant with respect to geographical conditions--that may be identified with the idea of human consciousness, or human awareness. Thus Relph (1976a) locates individuals along a continuum of "insideness and outsideness." primary existential categories that relate directly to the fundamentals of what it means to be human at all. Wagner (1972, page 50), in remarking that "The self derives its identity out of particular rhythms, places, actions and appearances" makes the distinction between "self" and "identity" explicit, and the relationship between them is seen to be the same one that is implicit in the other geographic views of identity. It is this understanding of the embedding of identity in consciousness. that has lead to geographical attempts to deal with consciousness. as witness the appeal to phenomenological method, and its search for the "pure essences" of human thought, the "pre-cognitive givens"—the phrases themselves speak directly of a ground of human awareness, on which the cognitive patterns that immediately direct thought and action are inscribed. The deficiencies of phenomenological and existential theory have lead, however,

to uncertainty about whether this kind of "preconcepted" level of human thought is a function of constructed identity or an aspect of human consciousness itself; hence the inability to specify functional characteristics that might distinguish between the two concepts. It may therefore be said--as an implicit part of the main conclusion--that, although the theories of both identity and consciousness in the geographic individual need to be radically reconstructed in some systematic way, of the two identity, through its partially existing links with cognitive theory, is better served; and the most obvious weakness in the integrated account of the geographic individual--and all the more urgently required because of its fundamental position in the proposed hierarchy--is at the level of human consciousness itself. It is to this requirement that we now turn.

6.2 The perspective of cognate disciplines

The proposal that a systematic account of human consciousness seems to be prerequisite to the theory of human geography may become more palatable to the contemporary geographer, who may be more accustomed to a view of the discipline which emphasizes concrete, as against abstract, phenomena, by referring to the treatment of consciousness in cognate disciplines. Geography has traditionally had strong links with biology; and evolutionary biology, in particular, attempts to deal directly with the uniqueness of the human species in the natural order. It would seem to be valuable to investigate the role human consciousness is held to play in that context. Then, if support is available for the idea that a systematic understanding of consciousness is somehow fundamental to a theory of the human individual, one would want to know whether such an understanding is to be had; and for that it seems logical to go to another discipline with which, increasingly, human geography has found areas of common interest: psychology. To these disciplines, then, we now turn.

6.2.1 Evolutionary biology

The contemporary view of the theory of evolutionary biology is that it can be divided into three historical periods: the Darwinian period of the late nineteenth and early twentieth centuries; the new synthesis of the 1930's and 1940's, effected by Simpson, Dobzhansky, Stebbins. Mayr, and others; and the modern period, in which molecular biology, and socio-biology (particularly in the work of Wilson) have greatly enlarged the scope of the theory (Mayr 1978). In the first and the last of these periods, the human species is seen as an

unusual, but not essentially different, member of the natural world: in the second, the emphasis is on its uniqueness. A brief examination of these views now follows.

Darwin's position is unequivocal: the human species is a part of nature, and even its apparently most distinctive mental characteristics can be accounted for within the framework of the theory of natural selection. Indeed, were this not so, the whole theoretical structure would, in his view, be threatened:

> If no organic being excepting man had possessed any mental power, or if his powers had been of a wholly different nature from those of the lower animals, then we should never have been able to convince ourselves that our high faculties had been gradually developed. But it can be shown that there is no fundamental difference of this kind. (Darwin 1871, p. 445)

His most extensive justification of this position centred on human emotions (Darwin 1872), but his more general account in The Descent of Man (Darwin 1871) sketches in addition explanatory accounts of curiosity, memory, imagination, reason, tool-making, language, the sense of beauty, religious beliefs, and, most importantly, abstraction and self-consciousness; together with morality and socialization. The conclusion is:

> There can be no doubt that the difference between the mind of the lowest man and that of the highest animal is immense . . Nevertheless, the difference in mind between man and the higher animals, great as it is, certainly is one of degree and not of kind. (Darwin 1871, p. 494)

Much of the argument underlying this conclusion requires the bridgehead of a lower species, as best exemplified by the Fuegians (Darwin 1839, pp. 148-149; 1871, p. 445). With the rise of anthropological knowledge, and increasing sophistication of the genetic underpinnings of the theory of natural selection, the validity of this view came to be questioned. In the new synthesis, we find

equally unequivocal assertions of the essential uniqueness of the human species. Thus Simpson (1967) argues that, although the first great principle of evolutionary biology, established by Darwin, is that

> Man is a part of nature and a kin to all life. (Simpson 1967, p. 282)

it is even more important that

> Man is an entirely new kind of animal in ways altogether fundamental for an understanding of his nature. Simpson 1967, p. 284)

A unique degree of four general qualities--intelligence, flexibility, individualization, and socialization--are seen in the human species to give rise to a number of distinctive characteristics, including ecological adaptability, the inheritance of learning, the ability to predict the outcome of actions, morality and ethics, and social organization. Most important is the ability to view and intervene in his own development:

> Man, alone among all organisms, knows that he evolves and he alone is capable of directing his own evolution. (Simpson 1967, p. 291)

There is, in other words, something in the way the human species is aware of itself that best establishes its uniqueness and defines its essential humanity.

Dobzhansky (1962) adopts a similar approach. In addition to the attributes identified by Simpson, Dobzhansky emphasizes the biological significance of upright stance for tool-making, brain size and brain differentiation; a model that is widely agreed upon (Washburn 1960; Clark 1962; Leakey and Lewin 1978) and has lately come to be termed auto-catalysis (Wilson 1975, pp. 567-569; 1978, pp. 84-88). He further singles out the characteristic of play as particularly well developed in the human species. Huizinga (1950, pp. 3-4), who is quoted in support of this view, remarks:

> Play only becomes possible, thinkable and understandable when an influx of mind breaks down the absolute determinism of the cosmos. . . Animals play, so they must be more than merely mechanical things. We play and know that we play, so we must be more than merely rational beings, for play is irrational.

Again, it would seem, there is something in the facility of the human species to know itself that separates it from other animal species. The same attribute is said to be central to the particularly human attributes of aesthetic experience and artistic expression:

> The aesthetic experience is, then, a necessary mode of our acquaintance with the world and with our own selves. (Dobzhansky 1962, p. 217)

In a later work (Dobzhansky et al. 1977), this notion emerges as a crucial element in the theory of human uniqueness. Having noted the difficulty of dealing scientifically with what is a purely introspective, or subjective, experience, the authors go on to assert that

> . . . self-awareness is the most immediate and incontrovertible of all realities. (Dobzhansky et al. 1977, p. 453)

Thus, for both Dobzhansky and Simpson, although on a number of parameters different in degree from other animals, the human species is rendered different in kind by virtue of its capability of self-awareness. This capability is seen to be the result of a particularly high degree, perhaps constituting a threshold, of size and structural complexity in the human nervous system, particularly the brain. It is said fundamentally to underlie those psychological and behavioural characteristics most closely associated with the humanity of the human individual.

A more famous statement of this position is that of the paleontologist and philosopher Teilhard de Chardin (1955, pp. 165-166):

> . . . reflection, is, as the word indicates, the power acquired by a consciousness to turn in upon, to take

> possession of itself as of an object endowed with its own particular consistence and value: no longer merely to know, but to know oneself; no longer merely to know, but to know that one knows . . . Admittedly the animal knows. But it cannot know that it knows: that is quite certain. If it could, it would long ago have multiplied its inventions and developed a system of internal constructions that could not have escaped our observation . . . In consequence it is denied access to a whole domain of realityin which we move freely . . . Because we are reflective we are not only different but quite other. It is not a matter of change of degree, but of change of nature, resulting from a change of state.

In a later work, that capacity of reflection is referred to as "consciousness raised to the power of two" (Teilhard de Chardin 1965, p. 224). The threshold implied here is described by Schumaker (1977, p.17), in a small but luminous work, as an "ontological discontinuity"' like Chardin, he locates the crucial element in

> . . . the fact that man is not only able to think but is also able to be aware of his thinking. Consciousness and intelligence, as it were, recoil upon themselves. There is not merely a conscious being, but a being capable of being consciousness of its consciousness . .

This view has, in recent times, been vigorously opposed. Jaynes (1976), for instance, has proposed that human consciousness is an operative phenomenon, developed subsequent to language. However, the evidence assembled by Piaget (as outlined in chapter IV), argues strongly against such a view. A more cogent and significant attack is that mounted by Wilson (1975, 1978). At the conclusion of a major synthesis of current sociobiological theory (Wilson 1975), he explores the possibility of a rigorous application of Darwinian theory to explain the characteristic attributes of the human species. It is accepted that

> We have leaped forward in mental evolution in a way that continues to defy self-analysis. The mental hypertrophy has distorted even the most basic primate social qualities into nearly unrecognizable forms. (Wilson 1975, p. 548)

But the major task is seen to be the tracing of the genetic path by which such qualities have arisen: this, it is asserted,

> will help to identify the behaviours and rules by which individual human beings increase their Darwinian fitness through the manipulation of society. (Wilson 1975, p. 548)

The author then goes on to sketch such explanations for the emergence, in the human species, of plasticity of social organization, reciprocal altruism, division of labour, role playing, communication, culture, ritual, religion, ethics, aesthetics, and territoriality. In a subsequent, and more polemic work (Wilson 1978), these explanation sketches are developed in greater detail and with greater vigour, if with less scientific perspective.

Wilson's views have given rise to an extensive and heated debate (see, for instance, Caplan 1978), most of which is not relevant here. It is important, however, to examine Wilson's response to the self-awareness criterion suggested above to be central to human uniqueness. His opposition to such a criterion centres on evidence from studies of the chimpanzee: he points to the genetic similarity of this species and man; to the ability of the chimpanzees to manipulate symbols and syntax; to their elaborate social organization (as demonstrated by Goodall 1971); and to evidence of rudimentary culture.

On the question of self-awareness, he quotes the finding of one study in which chimpanzees gave evidence of recognizing their mirrored reflections: this, he argues, is evidence of consciousness of self, and therefore of self-awareness (Wilson 1978, pp. 26-27). There is, however, considerable doubt about the validity of this conclusion. Firstly, the results reported in the experiment can be explained by orthodox learning theory, which does not require any assumption of self-awareness. Secondly, even accepting that the study demonstrates a degree of consciousness of self, this is by no means the same as

self-awareness in the technical sense of reflection, or of knowing that one knows, as was proposed above. The connection Wilson makes is a verbal sleight of hand, not a valid deduction. Thirdly, it seems odd that on such an important point, only one study is advanced in its support (Leakey and Lewin 1978, p. 189) to attempt to make the same point with the same study, particularly since Wilson has demonstrated his belief in the weight of corroborative evidence, as the 2500 or so references supporting his major 1975 text testify. In summary, we may legitimately take the view that the conclusion does not necessarily follow from the evidence; that alternative, well-validated theories account satisfactorily for the evidence; and that many more, and less ambiguous, studies would be needed to falsify a proposition that is, it would seem, widely supported, and is in accord with the common-sense of human experience.

Further support for this view is available in a recent evaluation of the contribution of sociobiological theory to the study of consciousness, which distinguishes more precisely between animal and human awareness:

> Whereas we may postulate "awareness" as a basic property of any behaviour, the ongoing "here-and-now" relating together of incoming sensations to provide a consciousness of "oneself as an object" requires a sustained act of attention utilizing a coded representation of reality in which the self as agent is included. (Crook 1980, p. 8)

Thus for Crook, a sociobiologist in the modern tradition, the conclusion is similar to that developed in the foregoing:

> . . . it is precisely from this "being aware of having experience" and being able both to communicate its features to another and to distinguish between oneself as experiencing agent and another's reported experience that the prime features of humanity arise. (Crook 1980, p. 5)

The tentative conclusions of this brief discussion of evolutionary biology are therefore firstly, that the human individual is, in some fundamental way,

unique in the natural world; and secondly, that central to this uniqueness is the self-reflexive nature of human consciousness. It should be noted that it is not here being asserted that an understanding of the self-reflexive nature of human consciousness itself constitutes a full and sufficient account of the human individual. Rather it is concluded that such an account must centre on that understanding and develop from there.

We may therefore feel encouraged in the identification by evolutionary biology of consciousness as fundamental to an understanding of human functioning, and to an appreciation of the uniqueness of the human species in the natural order. In our analysis, consciousness was singled out as a focus for theoretical development chiefly because of its priority, and the inadequacy of its treatment, in human geographic theory. The foregoing discussion suggests, however, that in pursuing an understanding of human consciousness, the human geographer would be placing the inquiry on a level which, as the proposed integrated account indicates, lies at the foundations of human experience, and therefore at the foundations of human thought and action in the environment. The question which now arises in whether or not a systematic account of human consciousness is in fact available, and whether it is in a form that can be satisfactorily employed in the construction of human geographic theory. For an answer to this question we turn to psychology.

6.2.2 Psychology

Human consciousness is, by definition, a subjective experience; subjective methodologies, broadly described as introspective (Josephson 1980b) or phenomenological (Battista 1978), have therefore been essential strategies of

investigating it. At the same time, as Mill (1974, pp. 849-850) insisted, consciousness is clearly in intimate association with the functioning of the central nervous system, particularly the brain: the neurophysiological and physiological correlates of particular states of consciousness have thus constituted a second focus of investigation. in this case objective, as foreshadowed by Piaget (1971, p. 49).

There arises first the question of what kind of consciousness is to be investigated. It is our common experience that consciousness fluctuates spontaneously in the course of a 24-hour cycle, from waking to sleeping and dreaming. A first approach has therefore been to investigate what may be termed the stream of consciousness (Pope and Singer 1978) in one or more of its normal modes. It is, however, widely recognized that identifiable states of consciousness different from these three can be systematically induced by a variety of techniques, which maybe regarded as experimental technologies for generating data about "altered states of consciousness" (ASC) (Tart 1969). The possible combinations of these approaches may therefore be represented by a two-by-two matrix, in which one factor varies between introspective and psychophysiological, and the other between stream of consciousness and ASC methodologies of investigation.

Stream of consciousness approaches have been consistently employed in the twentieth century, on the basis of theory initiated by James (1890). Battista (1978) identifies the central phenomena investigated to be perception; emotion and affect; cognition and intuition; self-awareness; and unition, the experience of "a whole and boundless relationship with everything" (Battista 1978, p. 61). While initial studies centred on simple introspection and self-report, techniques subsequently become more experimental and precise.

Singer (1978, pp. 189-191) identifies, as main categories of research, the investigation of individual and personality correlates through questionnaires and standard testing; of psychopathology; of behavioural correlates of daydreaming; of cognitive styles of functioning; of physiological correlates of daydreaming and of imaginitive processes, including eye movement, electroencephalography (EEG), and heart rate; of affective and stress content in daydreaming; and of make-believe play and fantasy in children. In these studies, the main technologies of investigation are broadly introspective--self-report, thinking out loud, thought sampling, event recording, questionnaire, and interview (Klinger 1978)--with some attempt to provide objective psychological or physiological correlates. Many studies have included combinations of both objective and subjective methodologies (Kripke and Sonnenschein 1978; Starker 1978). Much of the evidential and theoretical support for the establishment of physiological correlates of consciousness has come from neurophysiological sources, such as Eccles (1966) and Globus (1976). In some cases, well-established theories about particular states of consciousness, such as dreaming (Freud 1900), have been referred to in order to formulate hypotheses appropriate for testing with these more objective technologies (Starker 1978).

ASC strategies of research have varied even more widely (Tart 1969; Mishlove 1975). They have consisted of two main streams: those that have originated with the framework of twentieth century psychology; and those that derive from traditional sources. With respect to the first of these, hypnosis is one of the best known (Gill and Brennan 1959): there is, however, considerable doubt as to whether the states of consciousness induced by it are essentially different from the ordinary waking state (Kuhlenbeck 1957, pp.

284-286). A second, better documented technology is autogenic training (Rosa 1976; Kuhlenbeck 1957), which involves a number of techniques of concentration on achieving particular emotional, physiological, and visual states. As a well-established, standardized. and widely practised technique, it has evoked considerable interest, particularly in the light of its demonstrated therapeutic value. A recent, popular variant of this technique is alpha enhancement: there, however, the results appear to be less uniform (Travis, Kondo, and Knott 1975). Thirdly, research technologies based on pharmacologically-induced states have been employed (Grof 1975; Huxley 1959). Although it has been claimed that insight into unusual states of consciousness has been gained, both the degree of distortion of the ordinary states, and the clear evidence of psychophysiological damage associated with such technologies, have cast considerable doubt on their value in providing an account of the real nature of human consciousness. They have, in consequence, faded out of the mainstream of scientific investigation of consciousness.

A number of traditional techniques, broadly classed as meditative or contemplative, have been employed (Ornstein 1975; Tart 1969; Kuhlenbeck 1957; Gellhorn and Kiely 1972; Crook 1980). Well-known are EEG studies of Zen meditation (Hirai et al. 1959; Kasamatsu and Hirai 1963, 1966, 1969); and studies of yoga on a number of physiological parameters, including heart rate, respiration rate, blood pressure, and galvanic skin resistance (Wenger and Bagchi 1961; Chhina, and Singh 1961). Some studies have compared the two techniques (Kasamatsu et al. 1957). From accounts in traditional texts, or in contemporary teachings of traditional techniques, some researchers have developed their own variants of such techniques, and have demonstrated a degree of effect, either psychological (Deikman 1963, 1966; Linden 1973) ~~of~~ or physiological (West 1979).

It is of additional interest to note that Josephson (1980b) has raised the possibility of investigating consciousness by extending existing physical theory. He remarks that the notion of observation, and by implication, consciousness, is integral to the theory of quantum mechanics, which is the present basic theory of physics; and yet an adequate account of this notion is not yet available. He asks:

> Is it the situation that inner observation as well as observation of the external world should count as a quantum-mechanical measurement, and if so, what is it a measurement of? (Josephson 1980b, p. 195)

It is now necessary to ask whether any of these approaches to the human individual fulfil the scientific criteria that have been established in the preceding sections. Stream of consciousness research has been productive in identifying the kind of theory that ought to be developed, and in constructing models of such theory (for example, Battista 1978), but appears to have been hampered by lack of standardization and penetration in the methodologies of introspection employed. This seems to have resulted in an inability to gather evidence that relates to the common elements of human functioning: much data have thus been generated, but underlying patterns have been difficult to observe. In addition, it is doubtful that such theories as have been developed are capable of accounting for the kind of experiences often recorded in ASC research. They cannot, therefore, claim to be general theories of consciousness. Certainly none begins to meet the formidable criteria for a "General Scientific Theory of Consciousness" developed by Battista (1978, pp. 66-67), which include: a definition of consciousness; an account of the subject-object dichotomy; definitions and accounts of sensation and perception; definitions and accounts of emotion, affect, and cognition; the relationship of

intuition and dreaming to all these; a definition of self-awareness, and an account of its relationship to other states of consciousness; and accounts of unition, and of the ongoing nature of consciousness.

Turning to ASC research, it appears that neither the psychological nor the traditional approach has been based on theory that might be accepted as adequate. The psychological approach has been, in the main, inductive, involving a series of unsystematic probings in the dark, in little understood areas with little understood techniques, with the hope of eliciting some light. Few coherent hypotheses have emerged from this process, and even fewer have been subject to empirical test. No general theory of consciousness has therefore been sufficiently formulated, or sufficiently documented, to be regarded as well corroborated. The traditional approach, by contrast, has not lacked theory, which is abundantly available in the traditional sources of the particular techniques. The difficulty has been rather that, in the absence of adequate translations of the terms employed in such traditional theory into terms accessible to the modern scientist, it has remained relatively obscure, and hence, in the technical phrase, overidentified: more than one (perhaps many more) theoretical interpretation of its is possible (Harvey 1969a, p. 159). Hence it has been extremely difficult to assess the theoretical implications of the empirical evidence.

It therefore seems that neither the stream of consciousness approach nor the ASC approach, whether psychological or traditional, has been able to take the form of a coherent Lakatosian research programme. Thus the dialectic between theory and test which is to give rise, in a cumulative enterprise, to successively better theories has not developed. Specific insights, on the basis of theory and test, have certainly been gained; but, in the main, they

have been uncoordinated, and have not given rise to a theoretical structure of the substance required here. If this is true for the theory of consciousness, it is even more true for the theory of the individual as a whole, which seems, on the basis of the approaches so far outlined, to be unimaginably complex and difficult to develop. Finally, it maybe noted that there exists in these approaches a great range of notions about the nature of consciousness, and considerable uncertainty about what constitutes its self-reflexive capability. Both have contributed to the fragmentation of theory, and to the difficulty of developing an ongoing research programme.

We must conclude, therefore, that the prospects of finding a systematic approach to human consciousness of the kind that, on the basis of our analysis of human geographic theory, we have specified as necessary, do not yet appear to be encouraging. We are not, however, without hope. As this brief review has shown, it seems that the idea of consciousness is one towards which theoretical and research psychologists are more and more directing their attention. At least, as Battista (1978) best exemplifies, the requirements for a satisfactory theory are being identified; and that would seem to be a necessary first step in the framing of a theory sufficiently powerful to generate a vigorous scientific research program. There is reason, therefore, to feel some degree of confidence, even if the understanding of human consciousness proposed to be central to the theory of human geography is not yet available, that it soon will be. To that degree, the broad thrust of this dissertation seems to find support in the cognate discipline of psychology.

As a footnote, we may venture the observation that a satisfactory account of human consciousness seems more likely to emerge from what have been termed "traditional" approaches than from others, because of the abundance of theory

and the techniques of investigating consciousness already available there. The difficulty, as has been noted, lies in effecting valid translations of theoretical terms and concepts that often arise in cultural contexts and frameworks of experience very different from that of the orthodox Western psychologist. Nevertheless. it does not seem to be in principle an impossible task, as the discussion of Winch and Collingwood in chapter III concluded. There are, indeed. recent indications that it maybe achievable, and may give rise to the enduring traditions of research that characterize legitimate scientific research programmes (Orme-Johnson and Farrow 1977). However, it must be accepted that it is likely to be some time before such aproaches, if successful, are coordinated with the formal structure of contemporary psychology, and felt by human geographers to be a legitimate source of geographic theory.

6.3 Towards a humanized science of human geography

This dissertation is an attempt to develop a valid and useful approach to the functioning of the individual; and to take some first steps in placing basic concepts of human geographic theory in the perspective of that approach.

At the same time, it is acknowledged that the account given here is framed at the most general level, and that a great many more questions are raised by it than are answered. This need not be regarded in a negative light. It is widely accepted that one of the hallmarks of good theory is its fertility in expanding the range of previously established theories, and in predicting and explaining new phenomena; and the raising of new questions, of new problems, is held to be the necessary prelude to that expansion:

> . . . the growth of all knowledge consists in the modification of previous knowledge--either its alteration or its large-scale rejection. Knowledge never begins from nothing, but always from some background knowledge--knowledge which at the moment is taken for granted--together with some difficulties, some problems. These as a rule arise from the clash between, on the one side, expectations inherent in our background knowledge and, on the other side, some new findings, such as our observations or some hypotheses suggested by them. (Popper 1972b, p. 71)

Questions give rise to hypotheses, thence to theory and to test. Knowledge can only move and grow where existing understanding seems incomplete.

We thus turn, finally, to the question that looms over any attempt to modify or to reconstruct the theory of human geography: how might it work in practice? What kind of human geography is the proposed integrated approach to human functioning expected to generate? It does not always happen that a clear response to this kind of question is possible. Thus, for instance, Schaefer's (1953) demand for scientific theory assumed a concrete form in the now familiar

spatial analysis only after a decade of theoretical groping and experimentation. In a similar way, Buttimer's (1974) call for attention to the subjective world of value took some years to find geographic interpretations in the humanist school. On the other hand, where the scale of the change is smaller, and derivative theory more readily available, geographic interpretations may be easier to find: such a case is Guelke's (1974, 1981, 1982a, b) wholesale adoption of idealist historical method, in which he is clearly assisted by the long period of gestation of that method in its original context.

At first glance it would seem that, put in these terms, the approach advocated in this dissertation falls into the first, rather than the second, of these categories. It simply is not possible to say exactly what form a human geography based on the integrated theory of the human individual proposed here would take: the current areas of theoretical inadequacy preclude it. For a variety of apparently cogent reasons, it has been proposed that a hierarchical approach to the functioning of the geographic individual may be particularly fruitful; and if it is found, as we have found, that primary levels of the hierarchy are inadequate as to internal organization. rigour and precision, and external support and corroboration. it is clearly not possible to complete the account of the model itself, let alone explore its application in the theory and practice of human geography itself with any degree of certainty. That there will be such ramifications for the discipline we may, taking a broad perspective of the proposed model, reasonably feel confident. The exact nature of the geographic implications, however, depends--if the analysis of the foregoing chapters is right--on the nature of the systematic understanding of consciousness that is to be secured; and that is, as we have seen, a matter

that is presently far from clear. Only as the direction to be taken in achieving that understanding becomes clearer it is implied, will the whole structure of the proposed model begin to firm, and the theory and practice of human geography to which it gives rise begin to take definite shape.

But inasmuch as the model is an integrative one--that is, it attempts to include within its scope a number of the main schools of contemporary human geography--it can at least be said that its adoption as a foundation for human geography does not imply the rejection of any of these existing modes of geographic focus. On the contrary, the implication is that such spheres of activity will not only continue to exist, but will be enhanced by the embedding of their constituent theoretical concepts in an arguably more complete and more holistic account of the geographic individual. The social geographer, the urban geographer, the economic geographer, the rural geographer--all, it is implied, continue to investigate the chosen areas of their concern with the methods and concepts characteristic of them; but they do so within a theoretical framework which locates them within the structure of an overarching scientific research programme. Thus these different spheres of activity need no longer be seen as essentially separated from each other, as in the present fragmented state of the discipline: they have their identity and individuality, but on the ground of a shared understanding of the broad pattern of functioning of the individual in the environment.

It is in this theme of reintegration that the true contribution of such a theoretical approach as is proposed here may lie. A primary motivation for proceeding on the analysis of this dissertation was the prospect of a more integrated discipline, firmly grounded in the realities of human geographic experience, and less concerned with the exclusive claims of one or other

approach than with the mutual enrichment that may be possible between them, and the consequent greater enhancement they may together offer to the quality of human life. The model proposed here is directed, as we have seen, to the accommodation of positivist with humanist human geographies, and of both these with the main traditional insights. A discipline reintegrated in this way, and proceeding from a conviction of the deep theoretical connections underlying the surface differences would be a different discipline indeed from that which we observe today. On the one hand, the differences between positivist and humanist approaches assume an integrated perspective, on the ground of a common theoretical resolution of opposed themes at a higher (or deeper, or more powerful, or more comprehensive) level of theory (Hegel 1975, pp. 115-119). On the other, the severance of the past achievements and history of the discipline from the present, and the loss of historical continuity and erosion of identity which necessarily follows, are substantially vitiated; and with that restoration of disciplinary integrity comes the possibility of directing attention away from internal matters to external, to the potential of the discipline to ameliorate the human condition.

Of central significance to the nature of the projected human geography is, as has been consistently argued in this dissertation, its status as a science. Our analysis has indicated that there are grounds for optimism in this; that with advances in the theory of human consciousness and, to a lesser extent, in the theory of human identity, an integrated theoretical structure capable of generating a legitimate and vigorous scientific research programme may well be achievable. If it is, we may expect the shape of the discipline to reflect the chief characteristics and benefits of such a programme. The first of these is the cumulative development of theory, the ability to build theory across

several generations of geographers, to retain the insights of previous generations within the framework of new theory, without requiring the kind of radical theoretical shifts that have fragmented the discipline in recent times. There have been periods in twentieth century human geography--the Vidalian school is one example--when cumulative growth within the framework of a powerful research programme that includes both theory and empirical research has been possible; and the achievements of such periods have been particularly fruitful. One might project of a human geography arising from the proposed integrated theory that this kind of cumulative development may again emerge across a wide range of the discipline--including positivist, humanist and traditional approaches, if not sociological or Marxist--but in the context, and with the benefits, of modern priorities and insights.

A second potential result of framing the discipline along scientific lines is the possibility of greater cooperation between different arms of the discipline. In the present environment, this is largely precluded, because of the lack of an agreed frame of reference. With a common theoretical foundation, even if the work is carried on in widely different contexts--rural an urban, for instance, or behavioural and experiential--significant communication. and therefore, cooperation becomes at least possible.

It is worth noting that an important consequence of this approach for the discipline is to open communication to a wider range of cognate disciplines. Just as one may project the enhancement of currently estranged schools within the discipline, so one may foreshadow a similar effect for human geography as a discipline in relation to other disciplines; notably the social and human sciences. The kind of human geography proposed here seeks to use the findings of other disciplines where ever appropriate, and invites the use by other

disciplines of its own findings in a reciprocal fashion. In large part, this is a consequence of the theoretical approach that has resulted from our analysis of science, social science and human geography. The preceding sections have argued that concepts relating to the individual are primitive to theory in human geography, and fundamental to its validity. At that level of analysis, it would seem that the separation between disciplines or areas of knowledge is difficult to maintain. The geographic individual, for instance, is not far from the psychological, or the biological. Or, to put it another way, the individual is a whole, a living unity, whose existence is no more geographical alone than psychological or biological alone. Our view has been that it is not at that level that specifically geographic theory is to be found; but that in order for geographic theory to be valid and useful, in the senses discussed, it must be based on a valid and useful theory of the individual. In seeking the best theory of the individual, it would not therefore seem to ~~b~~ be an advantage--if, indeed, it is possible at all--to restrict inquiry to aspects of the individual regarded, by convention, as geographic. Indeed, it may be that our conclusions point the way to an integration of the sciences of human life (to propose a neologism that may embrace individual and collective approaches) on a profound and shared understanding of the human individual, each with its own domain of concern, theoretical structures, and methods of inquiry--a diversity on the ground of theoretical unity; but that is to carry speculation beyond the reach of available evidence. Let us limit ourselves to the observation that the way seems more open to fruitful theoretical exchange with other disciplines, and take encouragement from that.

There may, of course, be some who would resist the freer operation of the disciplinary frontiers, and who would hold self-sufficiency to be a primary

quality of a vigorous discipline. This view has a certain attractiveness; and yet, if we wish to have a discipline framed as a science, the canons of scientific inquiry are evidently against it. Accounts of scientific creativity have been given by Koestler (1964), and, more recently, by Gruber (1974) and Axelrod (1979). The underlying principles of scientific creativity which emerge there have been simply presented in a stimulating little work by De Bono (1967). He makes a valuable distinction between vertical and lateral thinking: the first is based on logic, and is the tool of normal science (to employ Kuhn's term); the second is based on creative, non-logical exploration of approaches alternative to the accepted paradigm (De Bono 1967, ch. 3). Feyerabend (1978, p. 47), indeed, requires this second approach as a logical necessity for the development of the best theory:

> A scientist who is interested in maximal empirical content, and who wants to understand as many aspects of his theory as possible, will accordingly adopt a pluralistic methodology . . . the alternatives . . . may be taken from where ever one is able to find them . . .

It would appear, therefore, that one element common to these views is a willingness to move outside established boundaries of theory and fact in an essentially exploratory and undirected manner. At some point, if scientific criteria are to be met, it is essential to re-engage in vertical thinking, to submit the codified results of such lateral exploration to the demands of logic and test. But the fertility of those theories that survive the test, both in generating further theory and test implications, and in yielding substantial achievement when applied in practice, is dependent on the quality of exploration from which it arises. It would therefore seem wise not to place too many <u>a priori</u> restrictions on the limits of such explorations.

We may take encouragement from the support in this area of Carl Sauer, arguably

the greatest of twentieth century human geographers, for this kind of a human geography. In his own work, Sauer as is well known, never scrupled to cross the discipline boundaries, particularly to biology and to anthropology. The nature of the subject, he asserted, demanded such flexibility:

> . . . geographic content, relations, and processes--in sum, geographic awareness--are of reason and necessity wider than what we. . . work at. Beyond and around what we study today lies an area of interest, of identification and concepts, which we do not intend to appropriate only to ourselves. The subject is and will be greater than the sum of our disciplinary efforts. (Sauer 1956a, p. 394)

Furthermore, he insisted on the value for the discipline of encouraging the individual geographer to follow his interests in the direction he felt to be of greatest importance:

> It seems appropriate therefore to underscore the unspecialized quality of geography. The individual worker must try to gain whatever he can of special insights and skills in whatever most absorbs his attention. Our over-all interests, however, do not prescribe the individual direction. We have a privileged status which we must not abandon. Alone or in groups we try to explore the differentiation and interrelations of the aspects of the earth. We welcome whatever work is competent from whatever source, and claim no proprietary rights. . . (Sauer 1956a, p. 396)

To circumscribe our vision in an arbitrary fashion; to refuse to seek understanding, and perhaps even wisdom, wherever it seems most likely to be found; to allow oneself to be bound, either by ossified tradition, or by transient fashion: all were anathema to Sauer. Only we, as geographers, would be the losers:

> If we shrink the limits of geography, the greater field will still exist; it will be only our awareness that is diminished. (Sauer 1956a, p. 394)

What has been said so far about the projected nature of the discipline relates mainly to its structural characteristics, particularly to its

integrative and scientific aspects. The question now arises of whether it is possible to say anything about the content of the discipline, as it might arise on the theoretical foundation of the geographic individual proposed in this dissertation.

It has been observed that the scope of the projected discipline includes existing spheres and modes of activity. Is it, then, simply an amalgamation of these existing sub-disciplines, loosely held together in a conceptual frame, but essentially independent of each other still, and going about their business much as they did before? A close inspection of the proposed theory of the geographic individual would suggest not. It has been argued that the observable actions in the environment undertaken by individuals or by groups, actions which constitute the empirical foundation of human geography, have their roots, and therefore their explanations, deep in the structure of human functioning. Moreover, it has been proposed that this structure may best be seen as hierarchical, in which the functioning of each more overt level is embedded in the substrate of a more abstract level, and finally in the continuum of human consciousness itself. On this view, therefore, at lest with respect to the geographical individual, human functioning is integrated and holistic within itself: one can highlight a particular aspect of it--behavioural, for example, or cognitive--but one cannot legitimately extract it from the whole, and attempt to treat it as something self-sufficient. Inasmuch as different schools of human geography, it has been argued, arise from, or employ, these different partial perspectives on the geographic individual, and derive their identity in large part from them, it follows that these perspectives are not just heaped together in an additive manner, but are intimately and profoundly related. Their connection is to a deep structure of

theory, from which each arises and to which each gives particular expression. Thus one deals no longer just with the physical requirements that give rise to specific forms of shelter; or with the behaviour that gives rise to particular spatial patterns of movement or activity spaces; or with the cognitive schemas by which individuals evaluate and order their environments; or with the elusive subjective structure of feeling and identity that lead people to relate to different environments differently. One does not attempt to construct theories and explanations, or to develop insights, exclusively within any one level, using only concepts and theoretical approaches held to be appropriate to that level. Instead, it is suggested, the human geographer, even while focusing attention on one, or on some other, of these areas of geographic concern, is able to deal comprehensively with the range of human life, from its expressed forms to its inner subjective springs. In this, the reality of human life and living, which is not segmented by its participants, but naturally pursued in an integrated and holistic way, is held to be better modelled.

Generalized descriptions of examples of this kind of approach are not difficult to construct, although detailed projections await the completion of the model in the manner specified above. Let us take, for instance, the attempt to account for a spatial and historical pattern of clearing of vegetation in the South Australian Mallee district. The approach we are here suggesting would imply that the first understanding to be gained in on the level of the individual farmer. There are a number of obviously relevant factors that one might identify as specifically geographic: the nature of the farming practices, and the economic climate in which they operate, as seen by the individual, are two. It is likely that, if asked to account for a particular programme of clearing that the individual Mallee farmer will refer

to these factors for an explanation of the action he has taken. The proposed integrated model, however, suggests that the analysis needs to be taken further than these more obvious considerations. A decision such as this, it implies, rests on the substrate of certain cognitive structures, which may here be identified with the nature of the individual and collective experience that have been codified in certain habits and beliefs that guide the farmer's actions on the land. One would predict, on the basis of this theoretical structure, that it is according to these schematized aspects of experience that particular aspects of the external circumstances (economic, agricultural) have been singled out as being relevant to the decision.

If one were to stop at this point in the analysis, one would have an orthodox analysis of the behavioural school, along lines similar to shoe recently suggested by Desbarats (1983). According to our proposed model, however, the analysis needs to be taken still deeper. One might look, it proposes, to the kind of identification with, or alienation from, the land that enduring contact, or family or district tradition--even, as Sonnenfeld (1982) has argued, the prevailing orientation of the culture, in interaction with which such localized traditions are developed--have provided. Here, the model suggests, we are dealing not just with aspects of human functioning that can be categorized as "affective" or "value-laden," but with that core of individual identity which powerfully and comprehensively frame cognitive and behavioural components. The model then implies (though in a way which, as outlined above, is not yet clear) the need to approach the <u>kind</u> of consciousness--the continuity of its style of functioning--on the substrate of which, it is proposed, all these factors arise. Finally, and most importantly, the hierarchical relationships between the different levels of analysis must be

elucidated, and the holistic nature of the individual farmer reconstructed. The attempt is thus to understand all the relevant facets of the farmer's action at every level, but to retain in the forefront of the analysis the actual individual, seen as a whole.

In accordance with the proposed model, collective implications are inherent in the analysis of the individual. Thus notions of economic climate, agricultural practice, collective experience and tradition testify to the reciprocity of individual and collective life. The model suggests the possibility of approaching action on a collective scale--the pattern of clearing in the Mallee district as a whole, for instance--by exploring this interaction. A full understanding of that pattern, it proposes, will derive from an account at the macroscopic level of the clearing of vegetation, from economic, historical, cultural and sociological perspectives, in terms of the analysis of the individual farmer already developed. It will require, furthermore, an account of the collective style of thinking, of the collective consciousness, that frames these different perspectives, as derived from the account of individual consciousness. Such a method of inquiry thus promises to integrate many of the contemporary approaches to human geography on a broad front of theory at the individual and collective level; and clearly it requires geographic skills of a high and comprehensive order. One may feel, therefore, that, if it could be achieved, it may well represent a level of explanation and understanding acceptable to a wide range of contemporary geographers, including the main schools referred to in chapter I.

Simple as it may look in this generalized form, the difficulties of systematically developing such an explanation, of tracing the main relationships between the different levels, and of welding them into a unified

account that adequately reconstructs the wholeness of the real world situation, should not be minimized. At the present level of development of the proposed integrated theory of the geographic individual, it must be accepted, it cannot be satisfactorily done, and necessarily awaits the development of theory in the research programme along the lines suggested below. It does, however, perhaps give some idea of the kind of integration the proposed theory might in practice provide. It is important to note, too, that such an investigation finds its significance not in the accumulation of particular facts about a particular domain of geographic concern--in this case the activities of farmers in clearing the Mallee--but in the relationship of those facts to the underlying theory. To the degree that the facts support the theory, intervention strategies may be developed: an analysis such as the foregoing has widely different implications, for instance, for a conservation or an agricultural policy than would an analysis based on economic considerations alone.

This last observation is worth considering a little further. It emphasizes the obvious fact that the application of a theory, the ability to intervene in the domain of its concern, is only as potentially effective as the theory itself is comprehensive. Where factors that significantly influence the phenomena are ignored, or are held to be neither necessary nor sufficient to explanation of the phenomena, or are not able to be dealt with in the terms of the theory, the ability of the theory to effect a reliable intervention is substantially impaired. One of the real potential advantages promised by the proposed integrated theory is the possibility of dealing more comprehensively with human functioning in the environment, and therefore of devising better and more effective intervention strategies. Given the degree to which modern human geographic theory now supports the procedures of the planning profession, this

may be no small contribution. From the beginning of this dissertation, the necessity for theory to meet demands for ease and effectiveness of application has been insisted upon: knowledge for its own sake has its appeal, but we have adopted the view that ultimately, if it is to be worth pursuing, it must be able to be applied to the enhancement of the quality of human life. The obvious fact is that, operating from a deeper and more integrated level of understanding of human functioning, that enhancement will be richer and more profound.

Thus, to pursue the example constructed above, intervention based solely on an economic analysis of the action of the farmer (or farmers) in clearing land--through financial incentives, for instance--are likely to have only limited effectiveness because it ignores the deeper subjective roots of such action. The development of a systematic and integrated account of more of the underlying factors, together with the overt influences of the economic context, may give rise to a very different strategy--perhaps involving education, or action in the local community--which has a better chance of a more complete and lasting solution. Again one hesitates to be too specific, because of the incomplete nature of the theory, particularly at the level of identity and consciousness, and because of the risk of trivializing the analysis; but the general thrust of the argument is perhaps the clearer for it. It emphasizes, too, the essential ecological nature of human affairs (to borrow a phrase, though with slightly different intent, from Sprout and Sprout (1965)); the idea that the various aspects of human life in the environment, whether at the individual or at the collective level, are intimately interconnected, and that considerable risk attaches to the attempt to limit analysis and intervention to any artificially isolated part of it, without regard for the location of that part in the whole.

In conclusion, we may now attempt to outline in a general way the kind of scientific research programme to which the theoretical structure proposed in this dissertation might give rise. The ultimate requirement of a discipline framed as a scientific research programme is that it "meet the facts", even if only intermittently, and thus preserves the connection of the discipline with the real world. This is an important aspect of human geography, for which the centrality of field work has continued to be a defining characteristic (Jones and Sauer 1915; Barrows 1923; Whittlesey 1925, 1927; Platt 1935, 1959; Wooldridge and East 1951; Sauer 1956a; Dickinson 1976; West 1979; Hart 1982; and so on). In this respect, the proposed integrated theory and research programme ensures that the discipline retain its roots in the real world, and that therefore the theories it devises can be applied to the betterment of human life. In the light of the difficulty experienced for some contemporary humanistic approaches--notably, as has been discussed, those based on existentialism and phenomenology--in providing real world interpretations, this, one may justifiably feel, represents no small achievement.

On the other hand, it ought to be true of the human geographic research programme, as it has been shown to be true of any genuine scientific research programme, that the primary development is theoretical. This is important, for it is not in observations that new ideas arise but in the mind of the observers; and it must be possible to foster intellectual creativity, and allow it to run the course of its logical implications, without checking its impetus, yet requiring it, ultimately, to meet the facts. It is a great virtue of the scientific research programme, as described by Lakatos, to have identified the true springs of scientific creativity, and to have framed scientific method in terms of it. In the contemporary state of the discipline, the theory of no one

school has been sufficiently powerful to generate such a programme, and take advantage of this inherent capacity. Structured on the basis of the proposed integrated theory of the geographic individual, such a capability would be available, and the dynamic of the discipline would in consequence tend more to the theoretical.

The specific objective of this dissertation, as established in chapter I, has been to probe the historical and conceptual underpinnings of the discipline of human geography, with a view to determining upon a position which might effect a substantial degree of integration between the various contemporary schools, and between contemporary and traditional perspectives, and from which a direction for the ongoing conduct of the discipline might be derived. That position has been identified with the proposed integrated model of the geographic individual, in the context of a scientific research programme. The analysis of the model undertaken in chapters IV and V, and in the second section of the present chapter, suggests certain stages in the research programme which might comprise a second phase of our inquiry into the discipline, a phase which necessarily falls outside the boundaries framed by the objectives of this dissertation.

In the analysis of the integrated model of the geographic individual, consciousness emerged as a central notion. Because of its fundamental position in the proposed hierarchy of human functioning, and because relative inadequacy of its treatment in the theory of human geography, an internally consistent, well-confirmed account of consciousness would seem to be pre-requisite to substantive theoretical and empirical advance. As was outlined in the preceding section, however, such an account is not easy to find, although there are indications of promising recent developments. The natural first stage of

the research programme thus becomes the pursuit of these directions, and the attempt to identify, or to formulate, a theory of consciousness capable of meeting these criteria. The confirmation criterion is an important one: it rules out, for instance, phenomenological and existential theories of consciousness, at least in their present formulations, and seeks, following Piaget (1971, pp. 47-49), a verifiable account, through psychophysiological correlates, that is capable of encompassing the whole range of human consciousness in its relationship to the environment (see, for such a range, Ittelson, Frank and O'Hanlon 1976, p. 202). Moreover, as the foregoing discussion has emphasized, the theory of consciousness developed at the individual level must be capable of elaboration to the collective level, and of accounting for the interaction with the environment within the analysis of geographic holism. Formidable as these criteria may appear to be, they are, as we have seen, systematically derivable from the analysis of human geographic theory we have undertaken, and seem largely to have been fulfilled at other levels of the model, such as the cognitive. There appears to be no inherent reason, as has been argued, why such criteria should not be able to be met at more abstract levels of human functioning.

Having established such a theory of consciousness, the second stage of the research programme becomes its incorporation in the proposed model of the geographic individual. This will require, firstly, the working out of the account of consciousness in the geographic context; that is, in specific relationship to the connectedness of individual and environment. It then demands the application of this understanding to the notion of geographic identity, which, according to the model proposed here, ought to represent a more localized expression of the abstract and general functioning that obtains

at the level of consciousness. Following the program of integration of theory, a constraint on this interpretation might be the ability of the reconstructed understanding of geographic identity to account for the substantive, documented factors located in existential and phenomenological perspectives. On this foundation. cognitive theory may then be approached; and here the challenge is somewhat more formidable, since the reconstructed account of identity and consciousness must not only be integrated with, but must in some sense frame, the comprehensive and well-confirmed theory already available at this level. Finally the reconstruction must be carried to the behavioural level, again in such a way as to preserve the essential insights established there. The main task of this stage is, in other words, to fill the theoretical gaps indicated in the foregoing analysis of the proposed model. One would then want to subject the reconstructed model as a whole to the same kinds of internal criteria that have been applied there, in order to test the prediction implied by our analysis, that if these theoretical deficiencies could be overcome, the status of the account as a scientific theory would be greatly enhanced.

The third stage of the research programme is clearly then to explore the implications of the reconstructed model of the geographic individual for the understanding of geographic collectivities. This will involve, firstly, an inquiry of the extent to which the traditional geographic categories of collective life--from economic, sociological, cultural, and environmental perspectives, for instance--can be understood in terms of the model of the individual. The elucidation of these relationships is a crucial step in the attempt to integrate human geographic theories and principles: the process, as has been analyzed in the course of this dissertation many times, is not one of formal reduction of one level of theory to another, but an exploration of the

fundamental assumptions about the individual that are made in the different approaches to collective life. Second, and perhaps equally as important, in the light of the conclusions of chapter V, is the application of the reconstructed model of the individual to the analysis of geographic holism, and the attempt to render that analysis a completed whole.

Together, then, these three initial stages of the projected research programme would complete, at least in its essential details, the theoretical analysis initiated in this dissertation. Their product ought to be a theoretical structure potentially capable of approaching most of the human geographic phenomena to which contemporary and traditional geographers have directed attention. The next phase of the research programme thus logically becames the attempt to confirm this theoretical structure on a wider scale. That attempt might well take a form similar to the one adopted in this dissertation. but more comprehensively; that is, it might seek to understand a wide range of traditional and contemporary work in the light of the reconstructed theory. At the same time, however, it would seem to be important to look to cognate disciplines--particularly, one might feel, to psychology, and to sociology and anthropology--in both empirical and theoretical contexts. to establish a wider and more substantial ground of confirmation. Again one would want to submit the results of this exercise to the same kinds of confirmatory criteria that have been developed and applied in this dissertation.

The successful completion of these two phases of the research programme ought to make available an internally coherent, well-confirmed theoretical base for the discipline. The final phase of the programme that can be projected from this point of its development then bears on the real purpose of this

lengthy, but essential, theoretical work: to develop, on a sound empirical basis, new human geographic insights. A sketch of the possible form this inquiry might take has been given in the example of vegetation clearing above. As that example makes clear, however, the ability to generate such insights rests on the comprehensiveness and coherence of the theoretical foundation; and the significance of such empirical confirmations, in accordance with the principles upholding the Lakatosian scientific research programme, lies in the support it provides for the theoretical foundation, rather than in the domain of the empirical demonstration itself. It is for these reasons that attention has been placed here, in attempting to project a possible direction for a research programme, on the systematic and thorough establishment of the theoretical structure, before approaching work in the field. One might further predict that theoretical development, and further empirical implications, might arise in the following stages of the research programme; but here projection degenerates into speculation, and we reach the end of what it makes sense to say about the pursuit of the direction established in this dissertation.

We may conclude with an observation about the nature of the achievement of this dissertation. To some geographers, accustomed to a concrete, real-world centre of gravity for their discipline, the conduct of an extended conceptual--or, as we have termed it, philosophical--exercise such as this may not be immediately appealing. It has clearly operated at an abstract level far removed from the immediate domain of human beings in interaction with their environment. It has involved lengthy chains of reasoning that require both patience and sustained critical attention from the reader. Moreover, some may feel that, in terms of clearly defined practical implications, the reward for embarking on this course of inquiry have not been great. At first glance, for

instance, it might seem that a geographer who upholds definition of geography like Stoddart's (1981) tripartite criterion--concern with the diversity of the Earth, with maps, and with fieldwork--would not be over sympathetic to such an approach as ours. We notice, however, that Stoddart also refers to the "visual imagination," to "aesthetic and even emotional appreciation" of landscapes, to a "sense of wonder," to "caring profoundly about man's terrestrial inheritance" and "the alleviation of human misery and distress" (Stoddart 1981, p. 296): and these are, indeed, concerns that have been central to the argument of this dissertation.

The answer to such reservations must lie finally in the nature of the task embarked upon in this dissertation: to establish a possible direction for the discipline in which a substantial integration of its contemporary schools, and of these schools with traditional positions, within the framework of the idea of science, might be achieved. Such is the contemporary and historical diversity of the discipline that any integrative attempt on this scale is compelled to operate from first principles at a general, abstract level, and to seek its theoretical foundations there. That the geographer should not turn philosopher is a concern we share; and yet we hold with Buttimer (1978) that it is only in a self-reflective exercise at this kind of level that real advance in the discipline as a whole may come. What we have attempted to do, therefore, in this dissertation is derive the fundamental principles by which such an exercise might most fruitfully proceed; to establish the internal logic of those principles; to confirm their potential value in approaching contemporary and traditional theory at both the individual and collective level; and to derive systematically, and to sketch, possible directions that a research programme based on those principles might take. These objectives, it

is submitted, have been achieved. An integrative theoretical structure has been proposed, its legitimacy and potential have been evaluated, and projections about its possible course of development have been made. Beyond this the present work cannot go, although its conclusions are encouraging to the pursuit of the directions indicated.

Intellectual inquiry has always been a demanding business. The idea of a discipline, indeed, in which such inquiry is carried out, connotes an attitude of rigour, of refusal to compromise the integrity of reasoned insight. The present inquiry has been carried out in the conviction that it is perhaps because the discipline has not, as Buttimer (1978, p. 73) points out, been eager to engage in reflective inquiry of this kind that it now finds itself in its present position of vulnerability; that geographers, no less than physical scientists, social scientists, philosophers and historians need to think deeply and systematically about the conceptual roots of their discipline; and that only in that level of understanding may be found principles and concepts that can support the steady, cumulative growth of the discipline, and the increasing power of its theory to effect good in the world. If something substantial has been achieved towards this end, the inquiry, we may feel, has been justified.

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