geography is currently in the throes of a paradigm crisis. Instead of asking the traditional question 'Is it geography?', or 'What *is* geography?', geographers are now asking 'What should geography be?' If a satisfactory answer is not found to the latter question the next question is likely to be 'Is geography relevant?'

It is clear from Chapter 2 that this was not the first crisis in geography's development: Ritter's teleological framework did not satisfy the determinists; and views that were scientifically acceptable to Ratzel and Semple were too deterministic for Vidal and Hartshorne.

KUHN'S PARADIGMS

Perhaps Thomas S. Kuhn (1962/1970a) was right when he claimed that science is not a well regulated activity where each generation automatically builds upon the results achieved by earlier workers. Instead it is a process of varying tension in which tranquil periods, characterized by a steady accretion of knowledge, are separated by crises. These crises can lead to upheavals within disciplines and breaks in continuity. Even Hettner (1930, p. 356) had suggested

> that a science does not always follow a straight line of development, but often zig-zags on its road to higher professionalization. This development often corresponds to changes in generations among its professional practitioners. It would be a bad thing if a new generation had no new thoughts . . . [but] a science should not suddenly be changed into something quite different from what it has been before.

In his last remark, Hettner was upholding the importance of historical continuity.

Kuhn rejected such veneration of the past, arguing instead that fundamental changes are often necessary to enable science to progress. While it is possible to determine objectively whether an explanatory framework is satisfactory and reasonable *within* a specific scientific tradition, we have to choose between different scientific traditions – and this choice is subjective. We must select what Kuhn calls **paradigms** (models or exemplars) for our science.

Kuhn defined paradigms (1962/1970a, p. viii) as 'universally recognised scientific achievements that for some time provide model problems and solutions to a community of practitioners'. Haggett (1983, p. 21) defines a paradigm as 'a kind of supermodel. It provides intuitive or inductive rules about the kinds of phenomena scientists should investigate and the best methods of investigation.' A paradigm is a theory of scientific tasks and methods that regulates the research (for example) of most geographers or, where there is a conflict between paradigms, of a group of geographers. The paradigm informs researchers what the object of their science should be, to which questions they should try to find 'acceptable' answers and which methods can be considered as 'geographical'.

Initially, Kuhn did not provide an altogether clear definition of the concept of paradigm. As Mair (1986) has pointed out, Kuhn later clarified some ambiguities and accepted that he had conflated two conceptually distinct, though empirically inseparable, types of paradigm. In the second edition of his book, Kuhn (1970a) argued that the most basic function of a paradigm is as an **exemplar**: a concrete problem solution within a discipline that serves as a model for successive scientists. Generally, such exemplars tie a scientific theory together, serving as an

example of a successful and striking application. The most important paradigms are those that generate whole new fields of scientific endeavour. Paradigms (in the sense of 'exemplars') may not always have this all-embracing effect, but will guide research as they are presented to students as models the students should try to copy. An example from geography would be the **regional monographs** written by Vidal de la Blache and some of his contemporaries, which established examples that have served a long-standing geographical tradition.

The other meaning of paradigm put forward by Kuhn is as a **disciplinary matrix** – 'the entire constellation of beliefs, values, techniques and so on shared by the members of a given community' (1970a, p. 175). A disciplinary matrix may be shared by a large group of members of a discipline while, at the same time, each member is working with different 'exemplars' in his or her everyday research (Mair, 1986, p. 352). It is in the sense of disciplinary matrix that the term paradigm has most commonly been applied to geography. The term 'paradigm' is used here in its disciplinary matrix sense as we attempt to interpret the history of geography in (simplified) Kuhnian terms. According to this Kuhnian model, scientific development consists of a series of phases (Figure 3.1; Box 3.1).

Box 3.1 Kuhn's theory of the development of a science

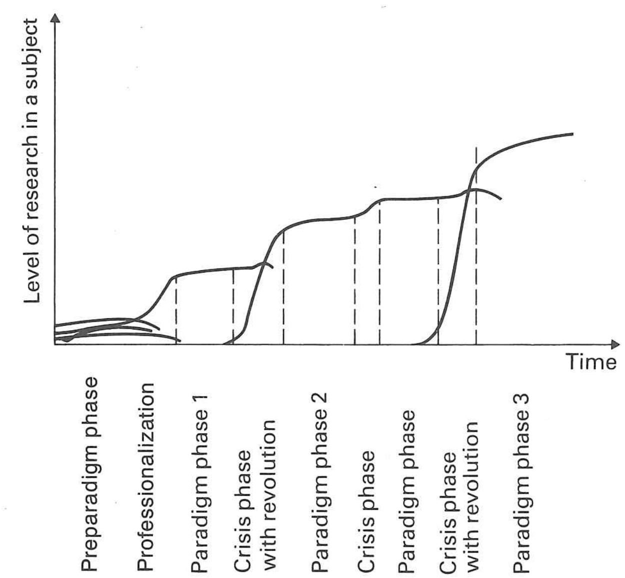

Figure 3.1 A graphical interpretation of Kuhn's model of a discipline's development. This figure indicates that paradigms and paradigm shifts dominate the whole discipline, whereas in his later work Kuhn pointed out that many scientific revolutions only impinge on the work of smaller groups of scientists within a discipline
Source: After Henriksen, 1973

In a branch of science that becomes the subject of thorough and systematic study, the **preparadigm period** is marked by conflicts between several distinct schools which centre around individual scientists. If we take paradigms to mean **exemplars** (that is, concrete examples of research to be followed), this phase could be labelled a multi-paradigmatic period, as each school of thought develops its own model solutions. **Professionalization** (or the subject's development from the preparadigm period to the stage of scientific maturity) has taken place at different dates amongst the different sciences. Kuhn argues that mathematics and astronomy left the preparadigm phase in antiquity. The transition begins when the question as to what a specific science is about becomes acute. This happened in geography when a university degree in the discipline was needed to qualify as a high-school geography teacher. A disciplinary matrix had to be defined for such a degree course, which would also secure and demarcate geography's domain from other university disciplines. One of the conflicting schools of thought will often dominate the others in that it seems best suited to win the discipline academic esteem. A paradigm is established that leads to concentrated research within a clearly distinguishable problem area – an activity described as **normal science**.

To bring a normal research problem to a conclusion is to achieve anticipated results from new empirical sources; Kuhn called this '**puzzle-solving**' because of its similarity to solving jigsaw or crossword puzzles. The research workers' perception is constrained by their paradigm; their observation of data is hence directed or attracted to the expected result; there is in-built opposition to unexpected discoveries. The paradigm can advance research and provide economy in the amount of research needed: research workers can go straight to the research frontier without having to define their philosophical bases and underlying concepts. Group identity among research workers is also a strong psychological advantage, which itself can stimulate scientific productivity.

Sooner or later a period of '**normal science**' is replaced by a **crisis phase**. This happens when more problems are accumulated than can be solved within the framework of the ruling paradigm. The crisis phase is characterized by a reassessment of former observational data, new theoretical thinking and free speculation. This phase involves basic philosophical debates and a thoroughgoing discussion of methodological questions. The crisis phase may end when it seems that no significantly better theory can be developed to solve the problems and thus, consequently, research must continue using the old paradigm. Alternatively, the crisis phase ends when a new paradigm attracts a growing number of researchers away from the old paradigm.

The acceptance of a **new disciplinary matrix** inaugurates a **revolutionary phase**. This means a break in the continuity of research and a thoroughgoing reconstruction of the research field's theoretical structure, rather than steady development and the accumulation of knowledge. Accepting a new paradigm is also revolutionary because it attracts the allegiance of the younger research workers who are opposed to established scientists. The new scientific 'reason' seldom triumphs by convincing its opponents; rather, it succeeds as they die and a new generation takes over. Younger workers who do not conform to the newly accepted paradigm are ignored by its followers, and researchers are continually forced to ask themselves whether the type of 'puzzle-solving' they are doing is the 'right' one.

Exchanging one paradigm for another is not a wholly rational process. The new paradigm will generally provide solutions for the problems the old one found difficult to resolve, but may not answer all the questions that were fairly easy to solve before. It is also seldom possible to argue logically that the new paradigm is better than the old. Even if a new paradigm can buttress itself with empirical and logical proofs, its original choice was basically subjective – an act of faith. Aesthetic considerations may influence the choice of a new paradigm – it may be regarded as simpler or more beautiful than the old one.

To many, Kuhn's picture of scientific activity is alarming: our faith in the objectivity of research is weakened when we consider how subjective the choice of paradigms can be and when we experience the often protracted opposition of some scientific workers to the establishment of new explanatory models. Few research workers welcome a general debate on the subjectivity of research – it may lead to the evaporation of respect for the research and the loss of financial support. On the other hand, as suggested by Peter Taylor (1976, p. 132), the youngest research workers who are at the bottom of the formal academic hierarchy have a clear vested interest in changing the existing scientific ideology, and thereby taking over from their elders.

The Kuhnian model has given the 'new prophet' a very effective weapon against the disciplinary matrix of a scientific 'establishment'. They do not need to justify their research as objective in itself; it is enough if they declare it to be objective within the subjective framework they have chosen. This can cause conflict amongst social scientists, as it is all too easy to equate the choice of a paradigm with the adoption of a particular value judgement. The ultimate conclusion may be that only those who affirm the same general outlook on the world and who have similar political beliefs are competent to evaluate a piece of scientific research.

CRITICS OF KUHN

Few geographers, said Bird (1977), have noticed that the concept of a paradigm ruling a community is akin to that of a dogma – which must be adhered to if a person is to be accounted orthodox. This is in line with the views of Karl Popper, the scientific methodologist who has most effectively criticized Kuhn. Popper (1970) maintained that an active and progressive science should be in a constant state of revolution. While acknowledging that Kuhn had demonstrated the existence of 'normal science' and 'paradigms', Popper deplored these periods as dangers to scientific progress. 'Normal science' becomes established when uncritical scientists accept the leading dogmas of their day and espouse some newly fashionable and formerly revolutionary theory. Popper feared that if scientists of this type were to dominate scientific thinking, this would herald the end of science. Dubbing 'normal science' the 'myth of the framework', Popper asserted that one of the scientist's most important roles is to break down myths. Given that we are always trapped within theoretical frameworks to some extent, we can break out of them at any moment if we act as true scientists. An active science will be in a state of **permanent revolution** (Bird, 1975). Some scientists have interpreted Popper's concept of a 'permanent revolution' as a prescription for what science should be rather than as a description of how it is actually practised. Kuhn (1970b), however, declared that Popper's demand for a 'permanent revolution' was built on assumptions that are just as unreal as are attempts to square the circle.

Paul Feyerabend rejected as historically false the Kuhnian model of alternating periods of 'normal science' and 'revolution'. He maintained that even in theoretical physics, which Kuhn (1962/1970a) used as his example, there have always been alternative basic theories that could act as 'exemplars'. Sciences do not show a chronological shift between periods of normal science and periods of pluralism. Hence, the historical development of a science might best be described by synthesizing the models of Popper and Kuhn.

This outline of Kuhn's ideas has largely been formulated within the context of a 'natural science', and a natural science does not question the basic implicit world-view that science is the study of the empirical world. A fundamentally different approach to the interpretation of the history of science was presented by the social theorist Michel Foucault (1972; 1980). Foucault's concern was to examine the political status of science and the ideological functions it could serve. Central to his argument is that there is a fundamental connection between power, knowledge and truth. Each society has its regime of truth, its 'general politics' of truth, i.e. the types of discourse it accepts and makes to function as true (Foucault, 1980, p. 131). Truth is therefore a relative concept, depending on the power relations within the society that produces it. To interpret changes in science throughout the course of modern history, Foucault focuses on the various worldviews (or structures of thought) people have held. These he calls **epistemes**. When epistemes change then science will change as well. This implies that societies create relative truth in order to reinforce the power relations within that society. We will return to this value discussion in Chapter 4. The discussion between Kuhn, Popper, Feyerabend and Foucault depends on their different ideas as to how a scientist works and how scientific theories and laws are established. At this point we therefore need to look briefly at models of scientific explanation.

INDUCTION, DEDUCTION AND ABDUCTION

Francis Bacon (1561–1626) defined the **inductive** route to scientific explanation (Figure 3.2). A scientist starts with a range of sense perceptions he or she works up conceptually and verbally into a number of loosely arranged concepts and descriptions that we like to call facts. Next, certain definitions are necessary to organize the data. Afterwards the facts are evaluated and arranged in relation to the definitions.

The ordering and classification of data are often the chief activity of a science in the early stages of its development. These first classifications may have only a weak explanatory function. Continuing study of the interaction between classes and groups of phenomena reveals a number of regularities; such regularities and laws may be called **inductive laws** since they are derived from the observations of a large number of single instances. The inductive route's weakness is, of course, the jump from a number of single instances to a general truth ('All swans I have seen are white: all swans are white').

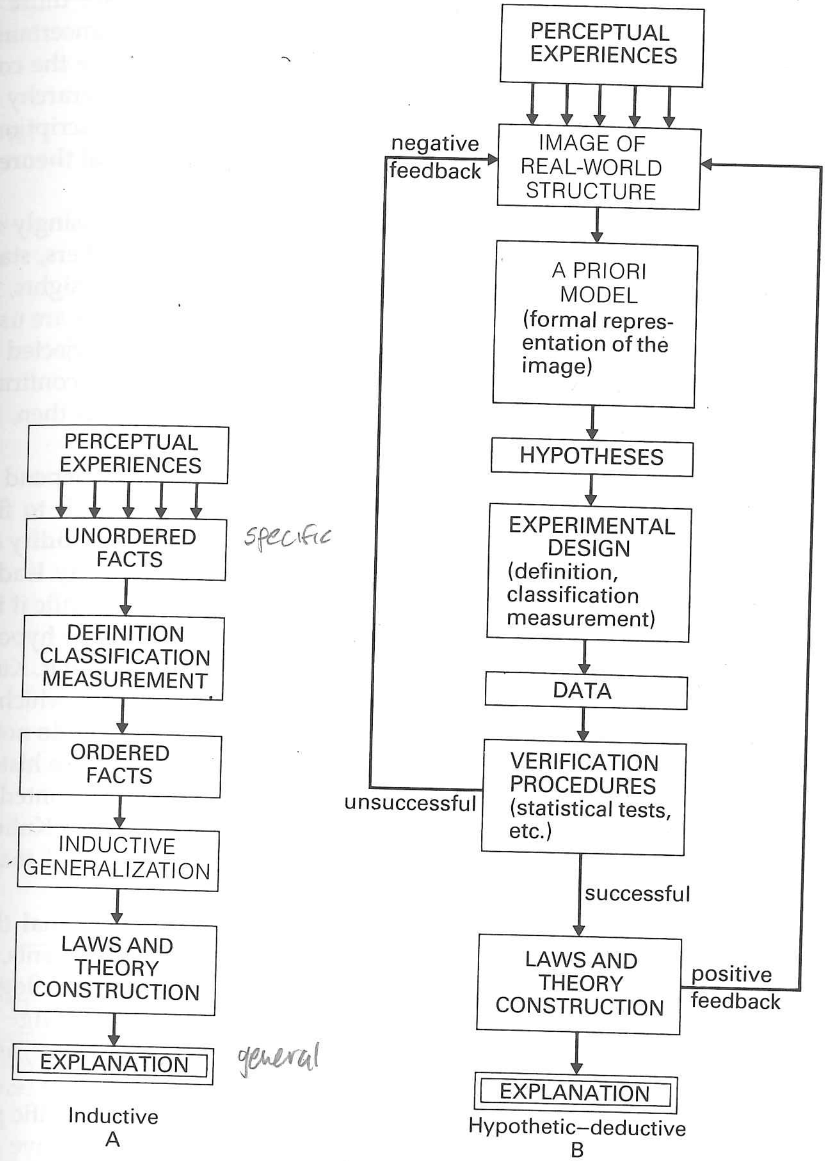

Figure 3.2 Inductive and hypothetic-deductive routes to scientific explanation
Source: From Harvey, 1969

Here we must clarify what a **scientific law** is. Braithwaite (1953, p. 12) defined a law as 'a generalization of unrestricted range in time and space', in other words, a generalization with universal validity. With this definition we can distinguish between **empirical generalizations** and **laws**. An empirical generalization is valid for a specific time and place but a law is universal. James (1972, p. 473) maintained that a law within Braithwaite's rigorous definition can hardly be formulated on the basis of geographical evidence. The only truly universal laws are those of physics and chemistry, although even in physics there are elements of uncertainty that make probability calculations necessary. Harvey (1969, p. 31) gave the concept of law a much wider significance and postulated a threefold hierarchy of scientific statements from **factual statements** or systematized descriptions, through a middle tier of **empirical generalizations** or laws, to **universal theoretical laws**.

Since the nineteenth century inductive arguments have been increasingly replaced by **hypothetic-deductive methods** (Figure 3.2b). Research workers, starting from an inductive ordering of their observations or from intuitive insights, try to devise for themselves **a priori models** of the structure of reality. These are used to postulate a set of **hypotheses** that may be confirmed, modified or rejected by testing them with experiments using empirical data. A large number of confirmations are supposed to lead to the **verification** of a hypothesis, which is then, for the time being, established as a law and basis for theory construction.

Karl Popper pointed out, however, that the truth of a law does not depend on the number of times it is confirmed experimentally; it is easy enough to find empirical support for almost any theory. The criteria for its **scientific validity** are not the confirmatory evidence, but that those circumstances which may lead to the rejection of the theory are identified. It follows that a theory is scientific if it is possible to **falsify**. Kuhn criticized Popper for believing that a theory, or hypothesis, would be abandoned as soon as evidence is found which does not fit it. Kuhn maintains that all theories will eventually be confronted with some data which do not fit. A fundamental theory is not rejected if individual research data do not fit it, for if it were, then all theories would have to be rejected. Up to now the history of science does not record any theory that has not eventually been confronted by contradictory circumstances (or instances of falsification). According to Kuhn, a fundamental theory is only rejected when a new theory is put forward that is *believed* to be superior (Johansson, 1973).

Feyerabend added that scientific development is much more **irrational** than Popper's scheme of falsification allows. As well as straightforward arguments, the proponents of new theories have also often used propaganda and psychological tricks. Feyerabend maintains that the development of scientific knowledge follows an irrational, almost anarchic path, along which almost anything goes, as far as methodology is concerned (Åquist, 1981, p. 11).

Kuhn did not accept Feyerabend's views on the irrationalities of scientific progress. He denied that he intended to present scientific theories as intuitive and mystical, more appropriate for psychological analysis than for logical and methodological codification. Kuhn asserted that every scientist must gather as much rational proof in support of a new theory as possible and to be precise and honest in his or her work. On the other hand, we need to realize that *data are dependent on theory*. All observations presuppose a certain conceptual apparatus by which the sense perceptions, or empirical verifications, can be arranged. Expectations and former ideas, that is, the hypotheses which have been set up, guide the interpretation of the data to a large extent.

Bird (1993, p. 2) points out that there is no agreed description of *the* scientific method, and continues:

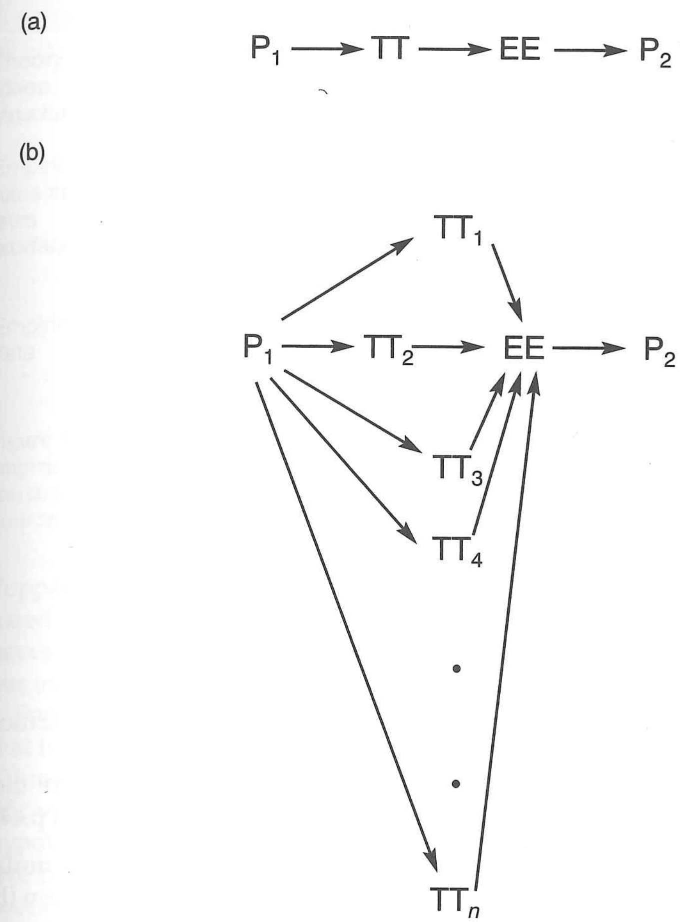

Figure 3.3 The hypothetic-deductive method as presented by Popper: P_1: initial problem; TT, TT_1–TT_n: tentative theories; EE: error elimination; P_2: new set of problems. (a) Sequence of scientific work with testing of one tentative theory; (b) sequence of scientific work with scientific examination of several competing tentative theories
Source: From Popper, 1972, pp. 119, 243; Haines-Young and Petch, 1986

> Just imagine the situation if it were. A totalitarian world of procedures would have to be learnt and obeyed. It is difficult to imagine such a universal framework lasting for very long. But that is not to say that there are no rules or tricks of the trade, although such as now seem useful are all on probation.

Bird (*ibid.*; in relation to human geography) and Haines-Young and Petch (1986; in relation to physical geography) find Popper's version of the hypothetic–deductive method and his **critical rationalism** most useful for scientific inquiry. The critical rationalist view of science can be said to build on three principles:

1) *The principle of falsification:* universal statements and theories can only be refuted, not verified.

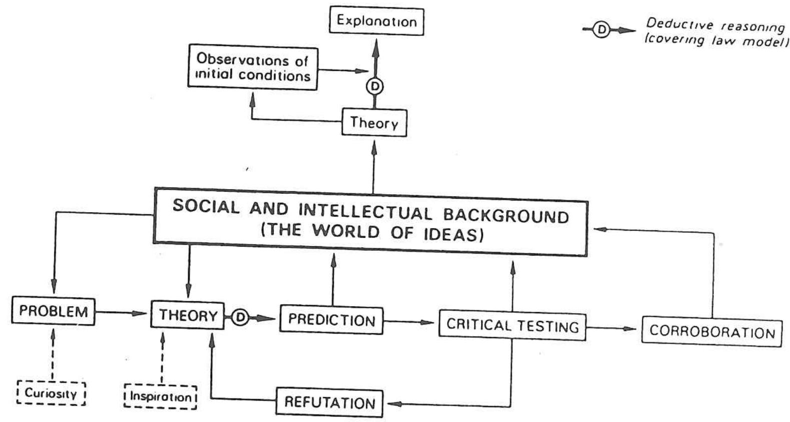

Figure 3.4 Critical rationalism
Source: Haines-Young and Petch, 1986, p. 45

2) *The principle of criticism:* scientific knowledge grows only when open to critics, trial and error.
3) *The principle of demarcation:* the characteristic of scientific statements are that they are empirically testable, capable of refutation if they are false. (*ibid.*, p.44).

The starting point for any scientific project (see Figure 3.3) is **problem formulation**. This is often a difficult intellectual exercise. Having discovered a problem (P_1 in Figure 3.3) and formulated the restricted questions within the problem area you might be able to analyse in your research, the next step is to decide on how to proceed and then to follow the chosen procedures. In practice this means that, after the problem formulation, you will look at earlier research within your problem area and then discuss and evaluate tentative **theories** (TT) relevant to your research questions. Then you need to find relevant methods for empirical testing, or **error elimination** (EE) of the theories. This may lead on to a residual irreducible problem or problems (P_2), which may be different from the initial problem. P_2 may lead on to suggestions for further research.

In a scientific thesis it may only be possible to test one tentative theory (Figure 3.3(a)), but this often means a restricted choice between several hypotheses or tentative theories. Figure 3.3(b) presents an alternative procedure as prescribed by Popper (1972, p. 243):

> An excellent example of the method in operation is provided by the work of Battarbee *et al.* (1985) on the causes of lake acidification in Galloway, Scotland. They examine the hypothesis that acidification is due to acid precipitation against competing hypotheses that it is due to heathland regeneration, afforestation or long-term post-glacial natural acidification.
>
> (Haines-Young and Petch, 1986, p. 63

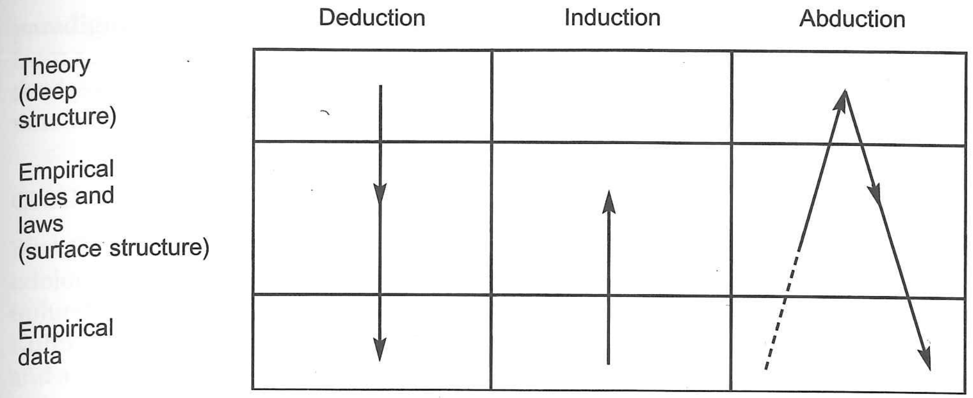

Figure 3.5 Deduction, induction and abduction. On abduction the first arrow from empirical data to empirical rules and laws is 'dashed' to indicate that the real abduction starts with the 'lift' from empirical patterns to tentative theory
Source: From Alvesson and Sköldberg, 1994, p. 45.

Popper maintains that it is only through criticism that false ideas can be eliminated and theories gradually improved. Since the truth of an observation can never be established finally, neither can our theories. The best we can say is that our evidence **corroborates** (makes more certain) a theory (Figure 3.4).

Popper's **hypothetic-deductive method** is based on deduction from theories that have been created in the imaginative mind. Alvesson and Sköldberg (1994, p. 43) maintain that such imaginatively inspired theory seems to be a form of scientific virgin birth – probably as rare in science as in nature. Tentative theories, or hypotheses, are almost always based on previous reflections on facts – an inductive phase. **Induction** starts with facts and deduction with theory, but it is not necessary to choose one of them exclusively. **Abduction** is the third alternative, recommended by Alvesson and Sköldberg (*ibid.*).

Abduction, like induction, is based in empirical facts (Figure 3.5), but the real process starts with the 'lift' from empirical patterns to tentative theories. Then follows the deductive process as proscribed by Popper – with critical testing leading on to corroboration or refutation, new problems and new testing, in a never-ending scientific process. Abduction may be likened to the process of medical diagnosis: the doctor observes the patient's symptoms and assumes they are caused by a particular disease. But his or her assumptions must be confirmed by tests and checked against other symptoms or comparison with data from other patients. In a scientific process there will be series of abductive 'jumps' between inductive and deductive reasoning, or between empirically loaded theory and theoretically loaded empiricism.

In this section we have discussed general theories of science that have mainly been developed with reference to theoretical natural sciences, such as physics. The transference of natural science methods into the social sciences has, however, brought about a vigorous debate among social scientists that will be discussed in more detail in Chapter 4. There may be similar problems in transferring the

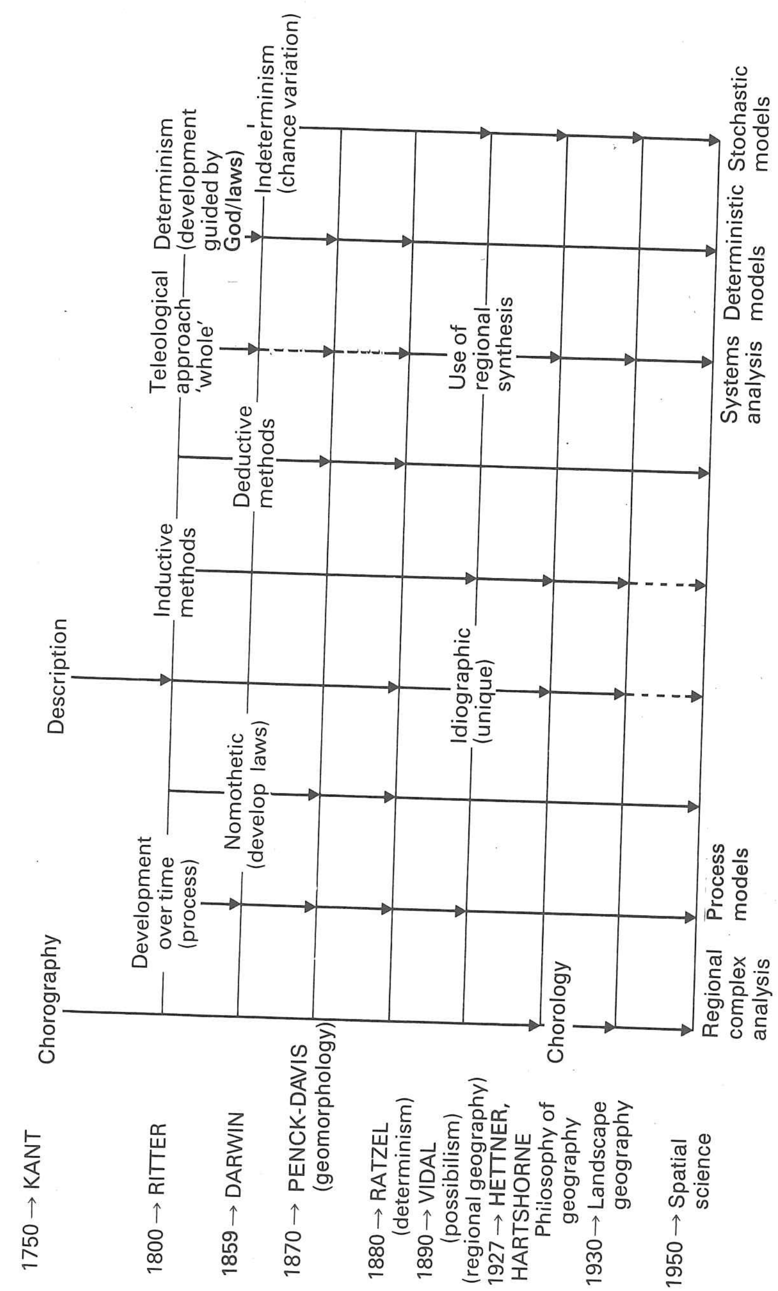

Figure 3.6 Ideas in geography 1750-1950 – two centuries of development

paradigm concept. Kuhn was trained as a physicist, and his theory derives largely from a study of the history of physics. How far is the history of physics relevant to the less theoretical and quantitative sciences? To answer this question we will now look at the history of geography in the light of Kuhn's model.

CHANGING PARADIGMS IN GEOGRAPHY?

Bird (1977) has argued that Kuhn has been the most influential scientific methodologist as far as geography is concerned. Mair (1986) suggests that geographers influenced by Kuhn fall into two groups. First there are those who have used Kuhn to legitimize their propaganda for a 'paradigm change' within the discipline and as a weapon against the scientific 'establishment' (see p. 85). Secondly, several geographical historiographers have tried to apply a Kuhnian model to the development of geographical thought (see, among others, Widberg, 1978; Schültz, 1980; Harvey and Holly, 1981; Stoddart, 1981; Martin, 1985; Johnston, 1997). The paradigm concept has taken on a life of its own beyond that originally envisaged by Kuhn, and as such has been regarded as a useful 'exemplar' (model or teaching framework) for histories of geography.

Figure 3.6 attempts to systematize the theoretical development of geography up to the 1950s, but it gives an incomplete and oversimplified picture as only the main concepts in the subject's development are shown. Over the course of time, these concepts have changed in significance and connotation. In Kuhn's terminology (Figure 3.1), until Darwin's time geography was in its preparadigm phase: Kant did not found a school of geography but indicated a role for the subject and suggested its position in relation to other sciences. Geography was, for him, a chorographic and mainly descriptive science, distinct from the systematic sciences and from history.

Ritter, on the other hand and contrary to Kant and his school, did not emphasize the distinctive roles of geography and history but instead emphasized developments over time, linking history with geography. Ritter was, however, the first geographer to describe his method clearly, and his account conforms to Francis Bacon's classical model of how a scientist works (Figure 3.2(a)). However, because of Ritter's teleological outlook, this first apparently active school of geography did not lead the subject into its first phase of a paradigm: contemporary developments in Darwinism meant the rejection of those ideas that might have led to a paradigm.

It should be emphasized that Darwinism did not represent a complete break with the major ideas upon which Ritter's geography had been founded. The study of development over time was still regarded as very important and a deterministic explanatory framework was strengthened further. The break with Ritter was about the forces that shaped development. After Darwin, scientists looked for the laws which controlled nature (and for materially conditioned social laws) and so, to a considerable extent, they adopted a **nomothetic** (law-making) approach to science.

Neef (1982, p. 241) may be right when he suggests that it was at this time that the most important revolution took place in geography, when the universities subdivided their faculties into separate disciplines and a **cosmographic** way of thinking was replaced by the **causal explanations** characteristic of the developing natural sciences.

During the latter half of the nineteenth century, to make geography acceptable as a science the newly appointed geography professors tried to recast the discipline as a nomothetic science. The hypothetic-deductive method was not, however, applied in a strict sense; we may agree here with Minshull (1970, p. 81) that determinists, as well as geomorphologists, stated generalizations first and then supplied a few highly selected examples as proof. Unlike physicists, geographers could not test hypotheses by verification procedures that involved a number of repeated experiments; statistical tests that might have played the same role as experiments were not as yet sufficiently developed to cope with complex geographical material.

Geomorphology and **determinism** may be said to represent geography's first paradigm phase. This paradigm was effective for geomorphology – it lasted for a good half-century and advanced the whole subject's scientific reputation through its accumulation of scientific information until alternative explanations could be put forward. While geomorphology expanded other branches of geography experienced a series of crisis phases. For this reason we can leave geomorphology to one side for the time being and concentrate on developments in human geography.

As the dominating paradigm in human geography, determinism had a short life, being challenged by the **possibilists** and the French school of regional geographers. These geographers stressed the idea that humanity has free will and participates in the development of each landscape through unique historical processes. Methodologically, geographers were trained to concentrate their study on the unique single region. This inevitably limited the development of theory (as normally understood in science) and made the hypothetic-deductive method redundant. The appropriate methodology under this approach would be to try to *understand* a society and its habitat through **field study** of the ways of life and attitudes of mind of the inhabitants. Such methods (in the form of **participant observation**) characterize the work of many social anthropologists today.

Fieldwork was regarded as of the utmost importance in the French school of regional geography. Vidal de la Blache based his *Tableau de la Géographie de la France* (1903) on studies in each *département*. Albert Demangeon walked every lane in Picardy before publishing his regional monograph on that *pays* in 1905. This fieldwork was, however, mainly concerned with presenting a picture of the material ways of life in the regions and had to be supplemented with the collection of factual material from statistical, historical and archaeological sources. In its handling of data – in the data's organization, classification and analysis – the regional approach to geography resembled the inductive method very closely. Regional geographers also sought general causal relationships but were rather unwilling to identify these as 'laws'. More explicitly **qualitative methods**, such as **participant observation**, were only adopted directly in local studies much later.

Although possibilists reacted against the determinists' simple explanatory models, many of their ideas were derived from Darwinism. They took over Darwin's concepts about struggle and selection although they also considered that chance and human will played an important role in development. While possibilism could be said to constitute a new paradigm, it did not immediately replace determinism. Partly because of the strength of geomorphology and physical geography, the deterministic explanatory model continued to survive side by side with possibilism.

For a long time, however, geographers continued to stress the central position of **regional geography**. Georges Chabot declared in 1950 (p. 137) that 'Regional geography is the centre around which everything converges'. It is, however, fairly obvious that the greatest advances in geographical research during the twentieth century have taken place within **systematic geography**. In geomorphology, biogeography, economic geography, population geography and many other branches of the subject, a range of new theories and methods have evolved. During the interwar period, landscape ecology and **landscape morphology** were subdivided, and many specialist studies were made on **urban morphology**. Research into the morphology of rural settlements was also separated from general studies of agrarian cultural landscapes.

Regional geography flourished in such countries as France, where in school and university teaching geography was closely associated with history and where the educational system fostered a national self-image of sturdy peasantry and cultured townsfolk. Regional studies were also important to the academic leaders of the emergent nations in central and eastern Europe, who were seeking to establish and preserve the uniqueness of their national heritage. This was to be achieved not just through their native languages but also by studying a whole range of traditional relationships with their lands, which had survived centuries of foreign domination.

While the peace settlement of 1919–21 created many new European nation-states, arguments over boundaries between the 'winners' and 'losers' of the war continued to draw extensively on local historical and geographical relationships. The economic revival of western and northern Europe from prosperity to affluence after 1945 has, however, been associated with the growth of essentially similar urban industries and services organized nationally (but today greatly influenced by the **globalization** of the economy). 'Regions' have come to be defined in strictly economic terms: 'regional policies' are devised to help areas that lag behind current norms of economic growth.

In the USA, Edward A. Ackerman (1911–73) argued that 'taken as a whole, those geographers who had mastered some systematic field before the war were notably more successful in wartime research than those with a regional background only' (1945, p. 129). He went on to point out ways in which emphasis upon systematic methods would best serve geography in the future. Although an immediate impact of his work is difficult to trace, his analysis encouraged the subsequent move towards training on the systematic side (White, 1974, p. 301).

Another reason for the limited progress of regional geography was the basic philosophy for the subject held by Hettner and Hartshorne. While both regarded the **regional geographical synthesis** as central to geography, they discouraged historical methods of analysis, arguing (with reference to Kant) for geography to be regarded as a chorological science. Hartshorne was strongly criticized by, among others, Carl Sauer who, within a year of the publication of *The Nature of Geography* in 1939, said: 'Hartshorne . . . directs his dialectics against historical geography, giving it tolerance only at the outer fringes of the subject . . . Perhaps in future years the period from Barrow's 'Geography as human ecology' (1923) to Hartshorne's latest resume will be remembered as that of the Great Retreat' (Sauer, 1963, p. 352).

The concept of the subject Hettner and Hartshorne had developed was, however, adopted by a large majority of human geographers from the 1930s until the 1960s. This, if anything, could be regarded as a **paradigm**. The disadvantage was that it did not lead to a universally accepted method of chorological regional description. Neither Hettner's ***Länderkundliches schema*** nor Hartshorne's identification of regions through 'comparison of maps depicting the areal expressions of individual phenomena, or of interrelated phenomena' (1939, p. 462) solved convincingly the methodological problems of regional synthesis. Vidal and the French school of regional geography, on the other hand, produced scientific works that served as **exemplars** for a large group of students, and thus could be said to have functioned better as a paradigm.

Schlüter and his followers also provided a basis for the study of the cultural landscape by developing methods of **landscape morphology** (see pp. 50–53). However, the majority of geographers, including the influential Hettner, regarded these methods as being too restricted in scope for geography as a whole, since landscape morphology restricted its analysis to the visible landscape whereas a proper regional synthesis also includes the 'invisible' transactions of social and economic life.

Kuhn's model, so far, fits the development of geographical science only superficially. As we have followed the early progress of the subject we have seen how new paradigms (in the sense of '**disciplinary matrices**') have, to some extent, included ideas from older paradigms. New paradigms therefore lose clarity and value as a guide for research until, in the end, more and more people define geography as what geographers do. Despite the impressions we may get from simplified accounts (for instance Wrigley, 1965), a closer look at the history of geography reveals that complete revolutions have not taken place; paradigms, or what may be more appropriately termed schools of thought, continue to exist side by side.

AN IDIOGRAPHIC OR NOMOTHETIC SCIENCE?

Another reason why paradigm shifts can be regarded as more apparent than real is that each new generation of workers, or each individual trying to change the scientific tradition of the discipline, will tend to ascribe a more fundamental significance to their own findings and ideas than they really have. A number of times in the history of geography we have witnessed a characteristic oversimplification of the views held by the immediately previous generation or, rather, of those held by the leading personalities of the current tradition.

A rather good example is the vigorous criticism of Hartshorne presented by Fred Schaefer (1953) in 'Exceptionalism in geography'. Schaefer attacked the 'exceptionalist' view of the Kant–Hettner–Hartshorne tradition – the view that geography is quite different from all other sciences, methodologically unique because it studies unique phenomena (regions), and therefore is an **idiographic** rather than a **nomothetic** discipline:

> Hartshorne, like all vigorous thinkers, is quite consistent. With respect to uniqueness he says that 'While this margin is present in every field of science, to greater or lesser extent, the degree to which phenomena are unique is not only greater in geography than in many other sciences, but the unique is of very first practical importance. Hence generalizations in the form of laws are useless, if not impossible, and any prediction in geography is of insignificant value. For Kant geography is description, for Hartshorne it is 'naive science' or, if we accept this meaning of science, naive description,
>
> (Schaefer, 1953, p. 239)

Schaefer maintained that objects in geography are not more unique than objects in other disciplines and that a science searches for laws. Having eliminated some of the arguments against the concept of a rigorous scientific geography, Schaefer sought to set down the kinds of laws geographers ought to seek. He also urged them to study systematic rather than regional geography.

Hartshorne (1955, p. 242) delivered a very strong counterattack on Schaefer in which he maintained: 'The title and organization of the critique lead the reader to follow the theme of an apparent major issue, "exceptionalism", which proves to be non-existent. Several of the subordinate issues likewise are found to be unreal.' Hartshorne admitted to having used the words **idiographic** and **nomothetic**, but rejected the idea that different sciences can be distinguished as being either idiographic or nomothetic. These two aspects of the scientific approach are present in all branches of knowledge (*ibid.*, p. 231). As early as 1925 Sauer had suggested that, although geographers had earlier been devoted to descriptions of unique places as such, they had also been trying to formulate generalizations and empirical laws.

Both Hettner and Hartshorne made a distinction between **systematic geography** (which seeks to formulate **empirical generalizations** or laws) and the study of the unique in **regional geography** (whereby generalizations are tested so that subsequent theories may be improved). Hartshorne (1959, p. 121) suggests that geographical studies show 'a gradational range along a continuum from those which analyse the most elementary complexes in a real variation over the world, to those which analyse the most complex integrations in areal variation within small areas'. James (1972, p. 468) emphasizes that there is no such thing as a 'real region'. The **region** exists only as an intellectual concept which is useful for a particular purpose. Later critics have read a much more metaphysical significance into the concepts 'unique' and 'region' than was intended by the geographers who were practising between the wars. Incorrect quotations from Hettner and Hartshorne have, however, gained an amazingly wide acceptance: 'It is discouraging to find some writers who continue to accuse Hettner and his followers of defining geography as essentially idiographic, thereby obscuring the underlying continuity of geographic thought' (*ibid.*, p. 228). James thus maintains that what has been called the '**quantitative revolution**' did not represent such a major change in direction as many think.

It can hardly be denied, however, that the interwar generation of geographers were sceptical of the formulation of general and theoretical laws, partly as a reaction against the crudities of environmental determinism. Arguments for idiographic rather than nomothetic approaches seemed to justify the scientific character of studies of the individual case.

ABSOLUTE AND RELATIVE SPACE

Harvey (1969) argued that the concept of geography as a chorological science of the individual case was not tenable because it built on the assumption of **absolute space**. Space in this sense is only an intellectual framework of phenomena, an abstract concept which does not exist in itself independent of objects. Werlen (1993, p. 3) suggests that it is a frame of reference for the material aspects of social actions in the sense of a formal-classificatory concept. In this sense space cannot have any explanatory power (Fosso, 1997, p. 16).

In a practical, classificatory sense, absolute (Euclidian) space is, however, rather useful, but it may be argued that 'faced by the seductive utility of Euclidean space we have allowed an interest in maps to become an obsession' (Forer, 1978, p. 233). Space is in this way treated as a container; first we delimit a spatial section of the earth, say the Newcastle region, and then start to examine its content. The notion of **vertical connections**, humanity's dependence upon local natural resources, was a conceptual basis for such studies. Good examples are found in the French school of regional geography (Box 3.2; Figure 3.7).

Box 3.2 The changing space relations of an English village

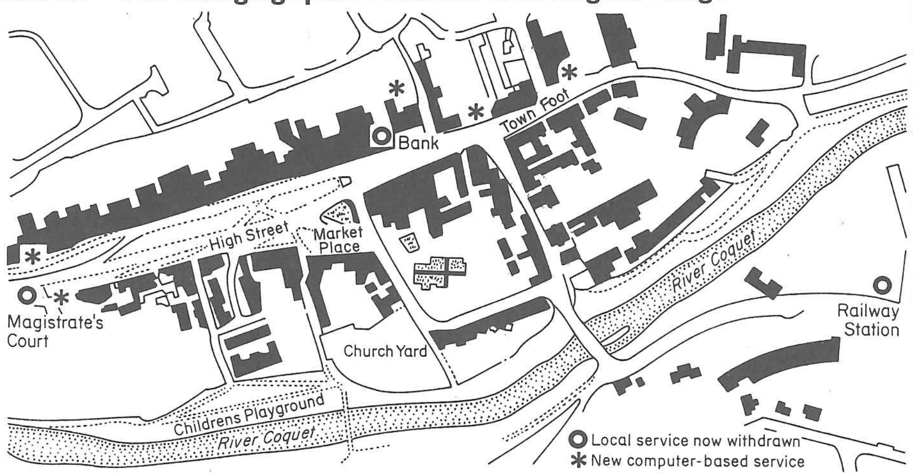

Figure 3.7 Rothbury

The rapid changes from vertical to horizontal connections during the second half of the twentieth century are illustrated from Rothbury (Figure 3.7), a small service centre of 2,000 inhabitants, 50 km north-west of Newcastle upon Tyne in an upland farming region. In 1950, Rothbury and its valley were substantially self-sufficient. Most people living in the village found work locally, all the nationally provided services were available in the village and most goods could be bought there. Now Rothbury is governed from Alnwick, 25 km away, and the high school is in Morpeth, also 25 km away. The courts and the railway station have closed and the future of the hospital is not certain. A recent survey showed that 38% of the villagers did little or no shopping in Rothbury and 80% of clothes and hardware shopping was done outside the village. One third of the work-force now drive over 25 km to work and a further 10% work from home, most using computer links. Five local businesses either design software, provide local computer facilities or sell their products nationally via the Internet. Village economy is now largely dependent on high levels of car ownership and on telecommunications.

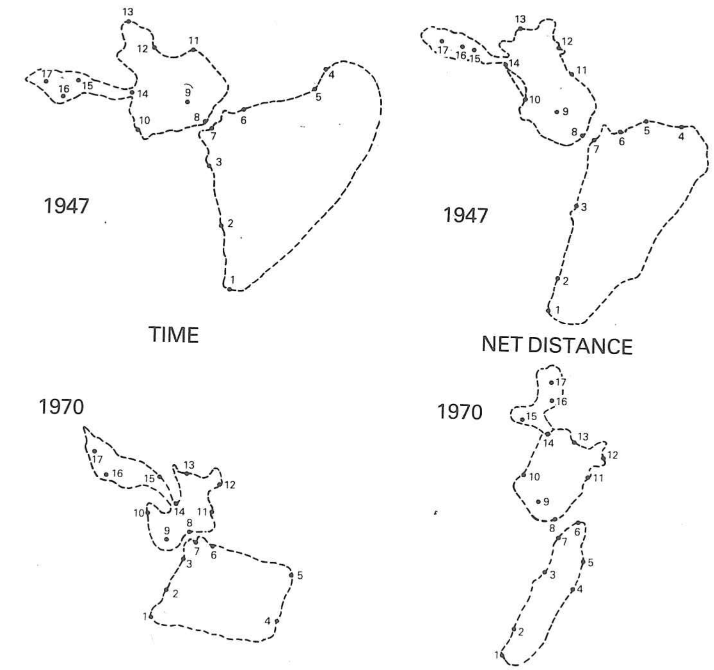

Figure 3.8 A demonstration of the plasticity of space. The four maps have been constructed from data on the New Zealand airline system and its changes from 1947 to 1970. The two maps on the left show how distance measured in time has changed as the airline network has grown and the speed of travel has increased. The maps on the right show how the net distance travelled has changed with the network
Source: From Forer, 1978

By the end of the nineteenth century, however, the traditional self-sufficient economies of Europe were giving way to an international market economy, and the value of this type of regional study was reduced. **Horizontal connections**, state and international policies, market forces, the interplay between regions, cities and countries, became more important for local development than the local connections between humanity and the land. This was (as noted earlier in Chapter 2) realized by Vidal de la Blache and it led to greater interest in horizontal, spatial structures.

By introducing the concept of **relative space**, horizontal, spatial relations and distance measured in different ways could be given explanatory power. Distance could be measured in terms of transport costs, travel time, mileage through a transport network and even as perceived distance: 'The shift to a relative spatial context is still in progress and is probably the most fundamental change in the history of geography as it opens an almost infinite number of new worlds to explore and map' (Abler *et al.*, 1972, p. 72). One important aspect of this is that geographical features such as settlement patterns, land use, diffusion processes, etc., demonstrate a location and dynamics that are due, to a large extent, to their relative positions in space.

Pip Forer (1978, p. 235) has observed that since distances in time, cost or even network mileage are partly artifacts of socioeconomic demands and technological progress, these types of spaces are naturally dynamic and truly relative. This leads him to the definition of **plastic space** – a space that is continuously changing its size and form. An illustration is given with his own time-space map of New Zealand (Figure 3.8) (*ibid.*, p. 247).

A DISCIPLINE RIPE FOR CHANGE

It was only after the Second World War that theoretical considerations on the relativity of space (and also research issues such as the study of diffusion models and location theory) came to occupy a dominant position in geography. One factor in the adoption of the **'new' geography** was the critical institutional situation many departments of geography found themselves in, particularly in the USA. In 1948, James Conant, president of Harvard University, had reportedly come to the conclusion that 'geography is not a university subject' (Livingstone, 1992, p. 311). The Department of Geography at Harvard was closed soon after, and the discipline was also gradually eased out of some of the other more prestigious private Ivy League universities.

Among the practitioners of the ever more theoretical sciences, the claim that the regional synthesis constituted geography's essential identity lent the subject a dilettante image. After the Second World War the North American universities were expected to produce problem-solvers or social technologists to run ever-increasingly complex economies (Guelke, 1978, p. 45), and geographers were not slow in adopting theory building and modelling that might promote the status of their science and their own academic standing.

Gould (1979, p. 140) recalls how the new generation of geographers were sick and ashamed of 'the bumbling amateurism and antiquarianism that had spent nearly half a century of opportunity in the universities piling up a tip-heap of unstructured factual accounts'. Morrill (1984, p. 64) claimed that the young generation's vision, although it might seem radical to those satisfied with an inferior status for the discipline, was in fact conservative in the sense that 'we wanted to save geography as a field of study and join the mainstream of science'. Even though, or perhaps due to this, the 'new' geography of the 1950s and 1960s was spearheaded by the Americans. They were also inspired by earlier theoretical works in Europe that, so far, had been almost overlooked.

The situation was much less critical in Britain because of the very strong and independent position geography held in both schools and universities. In many of the US states geography was more or less absent from the curriculum as a discrete discipline at high-school level, and less than 1% of students entered universities to study geography. Geography graduates had to find career outlets in applied research and planning. The continual threat of departmental closure or staff reduction based on independent evaluations of research productivity also explains the frenetic search in American universities for new ideas and research programmes.

We now proceed to look at the development of the **spatial science school** and assess whether the changes in geography (also called the **'quantitative revolution'**) really were a scientific revolution in the Kuhnian sense.

THE GROWTH OF SPATIAL SCIENCE

Location theory originates from economic theory. The classic location theories, including Johan Heinrich von Thünen's work on patterns of agricultural land use (1826) and Alfred Weber's study of industrial location (1909), are economic theories. Later economists and regional scientists, including Ohlin, Hoover, Lösch and Isard, developed theories of the areal and regional aspects of economic activity further. **Regional science** developed in some universities as a separate discipline; in yet others, this research came to be linked with economic geography or regional economics.

Walter Christaller (1893–1969) was the first geographer to make a major contribution to location theory with his famous thesis *Die Zentralen Orte in Süddeutschland* (1933), translated by Baskin as *Central Places in Southern Germany* (1966). Christaller, who had studied economics under Weber, declared in 1968 that his work was inspired by economic theory. His supervisor when he was working on *Die Zentralen Orte* was Robert Gradmann, a geographer who had himself made an outstanding regional study of southern Germany (1931) which, however, closely followed the current idiographic tradition in German *Länderkunde*. Although Christaller's thesis was accepted, his work was not appreciated during the 1930s, and when Carl Troll (1947) wrote a review of what had been going on in German geography between the wars, he did not even mention him. In Kuhn's terminology, Christaller's attempt to explain the pattern and hierarchy of central places by a general theoretical model was not acceptable within the reigning paradigm. Christaller never held an official teaching position in geography (Box 3.3; Figures 3.9–3.11).

Eventually Christaller gained a following, notably in North America and Sweden when it was realized that his central place theory could be applied to the planning of new central places and service establishments (Figure 3.11) and also to the delimitation of administrative units. Edward Ullman (1941) was one of the

Box 3.3 Christaller's Central place theory

A theme in **landscape morphology** (see Chapter 2) – the morphological network of central places in southern Germany, as seen on the topographic map – was the starting point for Walter Christaller when he, as a 40-year-old PhD student, developed his **central place theory**. He started to 'play with the maps', connecting towns of the same size with straight lines until his map was filled with triangles (Figure 3.9). These triangles appeared to show some regularities. If the region had really been a flat plain with uniform rural population densities, it would seem that the morphological features could be idealized in a hexagonal, hierarchical structure of urban places (Figure 3.10). Christaller used economic theory to explain the rationality of this morphological pattern.

During the Second World War Christaller was asked to use his theoretical abilities in the planning of new German settlements in eastern Europe. But it was only after the Second World War that central place theory had its first real application in the planning of the newly reclaimed Nord Oost Polder in The Netherlands (Figure 3.11).

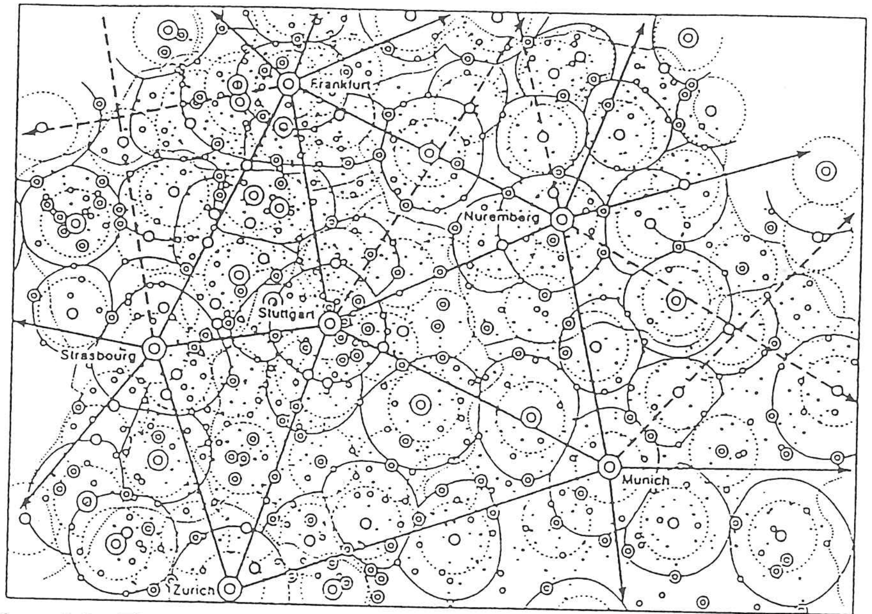

Figure 3.9 The geometrical hexagonal landscape of towns in southern Germany from Walter Christaller's classic study of central places made in the 1930s

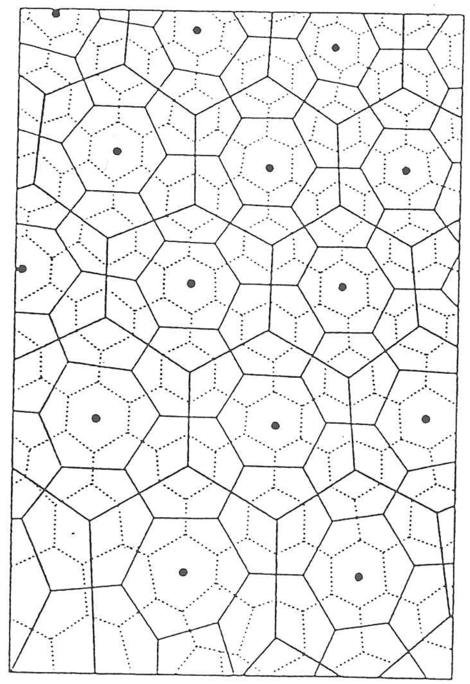

Figure 3.10 When population densities are uneven, the lattice of central places adjusts to the changes, closing up in densely settled areas and opening out in sparsely settled areas.

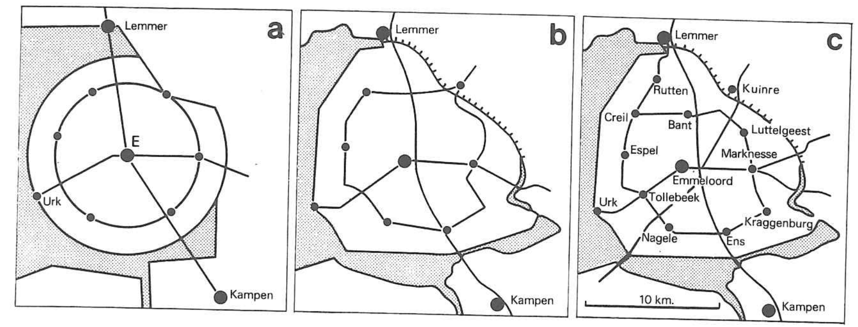

Figure 3.11 Walter Christaller's central place theory applied in the planning of the settlements in Nord Oost Polder in the Netherlands. (A) Geometrical diagram of the proposed settlement pattern; (B) plan of five new villages around Emmeloord; (C) the revised plan as executed
Source: After Meijer, 1981

first American geographers to draw attention to Christaller's work – American geographers were beginning to develop the theoretical models of urban structures and cities as central places that had been devised earlier by economists and urban sociologists (Harris and Ullman, 1945).

In an account of geography as a fundamental research discipline, the influential American geographer Ackerman (1958) encouraged students to concentrate their attention on systematic geography, cultural processes and quantification. A range of different statistical methods was gradually brought into use in several systematic branches of geography, enabling the development of more refined theories and models.

The acceleration of theoretical work was especially marked in institutions led by geographers who had studied the natural sciences, especially physics and statistics, and/or where there were good contacts with developments in theoretical economic literature. During the 1950s at several American universities, the frontier between economics and geography became very productive of new ideas and techniques.

A seminar for PhD students in the use of mathematical statistics conducted by William L. Garrison at the University of Washington, Seattle, from 1955 onwards was of particular significance. Garrison and his co-workers were mainly interested in urban and economic geography, into which they introduced **location theory** based on concepts from economics with associated mathematical methods and statistical procedures (Garrison, 1959–60). Many of the students from Seattle became leaders of the 'new' geography in the USA during the 1960s, including Brian J. L. Berry, William Bunge and Richard Morrill. Both Berry and Garrison later moved to work in the Chicago area. Through the inspiration of Berry the geography department at the University of Chicago became a leading centre of theoretical geography. It attracted a large number of PhD students and published a well-known series of monographs. Berry and other leading professors later left the department (which was closed in 1986), demonstrating the vulnerability of geography on the American academic scene. In the 1950s there was a simultaneous development of theoretical geography at the universities of Iowa (where Schaefer had taught until his death in 1953) and Wisconsin.

It is possible, as Johnston (1997, pp. 62–73) maintains, to recognize in this period four schools of quantitative geography in the USA. Three were developed in the departments of geography at the Universities of Washington, Wisconsin and Iowa, with Washington the most prominent centre of innovation. The fourth – '**social physics**' school – developed independently, drawing its inspiration from physics rather than economics. Its leaders were John Q. Stewart, an astronomer at Princeton University, and William Warntz, a graduate in geography from the University of Pennsylvania (who was later employed as a research associate by the American Geographical Society).

Empirical studies indicated that the movement of persons between two urban centres was proportional to the product of their populations and inversely proportional to the square of the distance between them. Stewart pointed out the **isomorphic** (equal form or structure) relationship between this empirical generalization and Newton's law of gravitation. Thereafter this concept became known as the **gravity model** (Box 3.4). Stewart's ideas about isomorphic relations between social behaviour and the laws of physics were introduced to geographers by a paper in the *Geographical Review* as early as 1947. Here Stewart (*ibid.*, p. 485) stated that human beings 'obey mathematical rules resembling in a general way some of the primitive "laws" of physics'. Warntz, working with Stewart, also borrowed analogy models from physics in his studies of population potentials (Warntz, 1959; 1964). He suggested that the mathematics of population potential is the same as that which describes a gravitational field, a magnetic potential field and an electrostatic potential field (James, 1972, p. 517).

Box 3.4 The gravity model

Early in the nineteenth century, some scientists suggested that the laws of physics could be applied to the study of human relationships and that the laws of gravitation might explain patterns of travel and trade between places. By the mid-twentieth century gravity models were widely applied within the spatial science school of geography. In its simplest form, the **gravity model** can be expressed as follows:

$$I_{ij} = \frac{k\,(P_i \cdot P_j)}{(D_{ij})^2}$$

where I_{ij} represents the interaction between town i and town j; P_i *and* P_j are the populations of the two towns; D_{ij} *is the distance between them; and* k is a constant.

The equation indicates that the interaction between the two towns (numbers of telephone calls, flows of traffic) is proportionate to the *product* (·) of their populations, divided by the *square* – ( )^2 – of the distance between them.

The work of Christaller, August Lösch and others was introduced into Sweden by Edgar Kant, an Estonian geographer who had tested their theories in his

homeland before taking refuge in Lund after the Second World War (Kant, 1946; 1951). His research assistant in 1945–6 was Torsten Hägerstrand, a brilliant young geographer who was also working on migration processes. Through his contacts with the Swedish ethnologist Sigfrid Svensson (who had made a number of studies of the relations between innovation and tradition in rural areas using the currently accepted methodology), Hägerstrand became interested in the possibilities of investigating the **process of innovation** with the aid of mathematical and statistical methods. In focusing on the **process**, Hägerstrand made a clear break with the current regional tradition. His dissertation 'Innovations – förloppet ur korologisk synpunkt' (1953, later translated by Pred, 1967, as 'Innovation diffusion as a spatial process') examined the diffusion (or spread) of several innovations among the population of a part of central Sweden. Some of these innovations concerned agricultural practices, such as bovine tuberculosis control and pasture improvement, and others were more general, such as car ownership. With the aid of the so-called 'Monte Carlo' **simulation**, which involves the use of random samples from a known probability distribution, he was able to construct a general **stochastic model** of the process of diffusion. Stochastic literally means at random; stochastic or **probability models** are based on mathematical probability theory and build random variables into their structure. Models may be classified as either stochastic or deterministic. In **deterministic models** the development of some system in time and space can be completely predicted, provided that a set of initial conditions and relationships is known.

The stochastic Hägerstrand model enabled the spread of innovation to be simulated and later tested against empirical study. It was demonstrated that the form of distribution at one stage in the process would influence distribution forms at subsequent stages. Such a model *could* therefore be of use to planners in support of future innovations they wished to bring about. The department of geography at Lund University soon became renowned as a centre of theoretical geography, attracting scholars from many countries. Almost from the beginning there were contacts between Lund and Seattle. Hägerstrand taught in Seattle in 1959 and Morrill studied with him in Lund, where his work on migration and the growth of urban settlement (1965) was presented.

In the years that followed, Hägerstrand's technical and statistical procedures attracted more attention than his theoretical analyses. He himself regarded his work as less important for its empirical findings than for its general analysis of the **diffusion process**. He stated in the first sentence of the dissertation that, although the material used to throw light on the process relates to a single area, this should be regarded as a regrettable necessity rather than a methodological subtlety (Hägerstrand, 1953 – 1967, p. 1). This was of course meant as a deliberate provocation to the traditionally bound regional geographers. Hägerstrand regarded his (1953) analysis of individual fields of information and their change through time as his most important contribution to geographical thought, as the study of such information fields is basic to a deeper understanding of the processes of diffusion.

During the 1960s, Hägerstrand went on to make detailed studies of individual behaviour, using three-dimensional models to portray the movement of individuals in time and space. An important feature of **time-space geography** is that time and space are both regarded as resources that constrain activity. Individuals have different possibilities of movement in space, conditioned by their economic status and technical possessions, but time imposes limitations on everyone. Subsequent studies in time-space geography, which have been carried out actively at Lund and elsewhere throughout recent decades (see, for instance, Carlstein *et al.*, 1978), have shed much new light on geographical aspects of human behaviour.

The 'new' geography spread over the world from the innovative centres, but it did not have the same impact in all countries. Christaller's work had aroused little interest in his home country, Germany. His theories had to take a detour into the English-speaking world – from whence they returned steeped in the 'new' geography – to be fully appreciated. Initial forms of quantification, such as the use of frequency distribution scattergrams, parameters and index numbers, were first applied around 1960. The introduction of **factor analysis**, notably in a classificatory study of Swiss cantons by Steiner (1965), was the real introduction to new quantitative approaches for most German-speaking geographers. The philosophical implications of **the spatial science school** were first presented by Dietrich Bartels in his book *Zur wissenschaftstheoretischen Grundlegung einer Geographie des Menschen* (1968).

Another reason for the delayed impact of the 'new' geography in Germany was that geographers tended to follow Troll's appeal in *Erdkunde* (1947) in that they accepted the principle that worldwide research should be a normal component of an academic career. Virtually all established geographers were attracted to employing their talents abroad, leaving their graduate students to cultivate research at home. Research abroad undoubtedly contributed to Germany's international reputation, particularly through the application of German methods of detailed landscape studies and cartographical work. German geographers also found that their existing techniques were well adapted to research abroad, particularly in the Third World where the statistical basis for quantitative analysis was sparse or unreliable.

WHAT KIND OF REVOLUTION?

Throughout the world there was marked opposition among established geographers to the learning and teaching of the new spatial science methods, and a reluctance to open professional journals to contributions, the editors did not understand. 'There was something electrifying about tilting with the dragons of the establishment' says Morrill (1984, p. 59), and for this reason the young generation of geographers had the feeling of being revolutionaries. In the USA the lack of publication outlets led to the establishment of a theoretically orientated journal, *Geographical Analysis* (*ibid.*, p. 65).

In 1963, a Canadian geographer, Ian Burton, arguing that what he labelled the '**quantitative revolution**' was over and had been for some time, cited the rate at which schools of geography in North America were adding courses in quantitative methods to their requirements for graduate degrees. It must be stated, however, that most geographers did not consider the theoretical developments within the subject as a revolution, and that many 'revolutionaries' were at pains to emphasize continuity in the ultimate objectives of human geography. The use of statistics for the making of relatively precise statements was generally accepted,

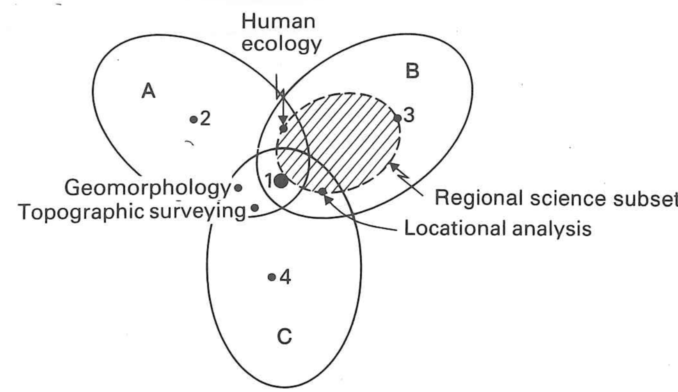

Figure 3.12 Geography and its associated subjects, A: Earth sciences, B: Social sciences, C: Geometrical sciences 1: The core of geography, 2: Geology, 3: Demography and other social sciences, 4: Topology with other geometric sciences
Source: After Haggett, 1965

although the related use of mathematics in modelling received much less attention (Johnston, 1978).

Most research workers regarded advanced statistical methods as being useful in some branches of the discipline; other branches, notably historical and cultural geography, felt less need for new techniques. Leonard Guelke (1977b, p. 3) claimed that 'To an extent that is not widely recognised, the move to quantification took place within the basic framework of geography put forward by Hartshorne in "The nature of geography" (1939)'. In many geography departments the works of both Hartshorne and Garrison were on the students' reading lists, but philosophical and methodological differences between them were not an issue in teaching up to the mid-1960s; Schaefer's criticisms had been forgotten.

Johnston (1997, pp. 74–5) points out that the leaders of the quantitative school did not study the philosophy they were adopting very deeply – apart, that is, from references to the works of Gustav Bergmann, a philosopher and close friend of Schaefer who had actually read the galley proofs for Schaefer's paper on 'exceptionalism' (Schaefer, 1953), contained in some of the papers of the Iowa group and, most notably, in William Bunge's thesis *Theoretical Geography* (1962, 2nd edn, 1966). Bunge, who had worked at Iowa for a short period, extended the arguments of Schaefer to the effect that geography is the science of spatial relations and inter-relations, geometry is the mathematics of space, and so geometry is the language of geography. The **chorological viewpoint**, emphasizing the character of and inter-relationships within specific places or regions, was rejected in favour of a geography based on **spatial analysis**, which stressed the geometric arrangement and the patterns of phenomena. **Relative position in space** – distance measured in various ways – became the main explanatory factor.

With some irony, Bird (1993, p.11) comments that 'the one and only revolution in geography' took place in June 1966: 'An event in the literature enables us to date the last straw, when the last idiographic bastion in geography was overthrown – the destruction of the idea that locations could never be anything but unique.'

In an article Grigg (1965) had tried to argue that all parts of the earth's surface are unique. If it is held that geography includes the study of the location of its data and that 'locations are unique', then geography cannot fully employ the scientific method. In June 1966, however, Bunge published a short commentary to Grigg's paper, asserting that 'locations are not unique', but general. Locations are comparable as witnessed by such terms as 'near', 'far', 'close', 'distant' and 'adjacent', which describe the relativity of locations. It is thus the **relativity of spatial locations** that can be analysed in a scientific way.

Quantification as such does not lead to any scientific revolution in the Kuhnian sense. The change from **absolute** to **relative space** as the focus of geographical study had, however, basic philosophical implications and was in this sense revolutionary. It is therefore better to talk of the **spatial science school** rather than **quantitative revolution** and **quantitative geography** to describe the new trends of the 1950s and 1960s (Figure 3.6 p. 68).

The major advances towards a unifying methodological and philosophical basis for the spatial science school were made in the 1960s by British geographers, notably Peter Haggett, Richard Chorley and David Harvey. *Locational Analysis in Human Geography* by Peter Haggett was published in 1965. The importance of this book lay in its overview of much new theoretical work in the subject. Haggett (*ibid.*, pp. 14–15) used the diagram reproduced in Figure 3.12 to illustrate the argument that there are three traditional subject associations of geography: with the earth sciences (geology and biology), with the social sciences and with the geometrical sciences. Haggett (*ibid.*, pp. 15–16) maintained that:

> The geometrical tradition, the ancient basis of the subject, is now probably the weakest of the three. Much of the most exciting geographical work in the 1960s is emerging from applications of higher order geometries . . . Geometry not only offers a chance of welding aspects of human and physical geography into a new working partnership, but revives the central role of cartography in relation to the two.

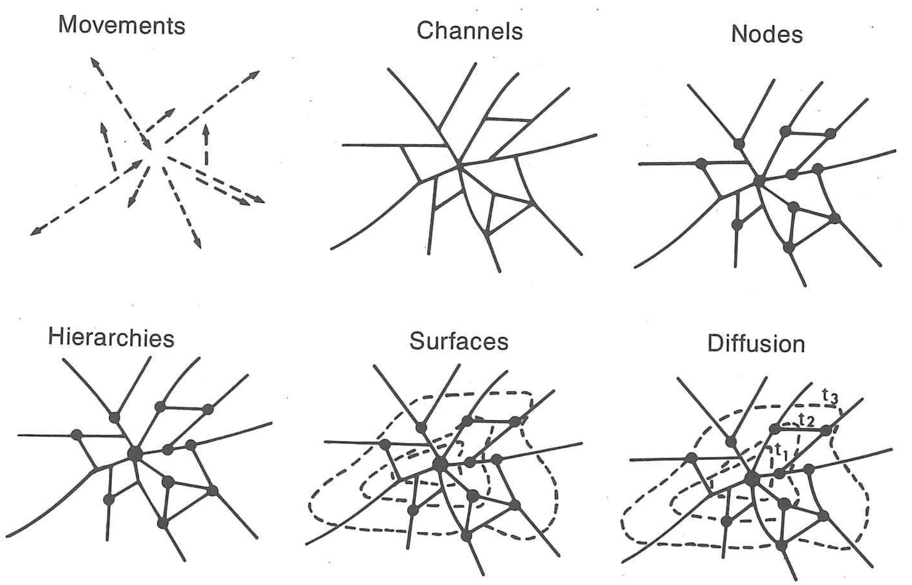

Figure 3.13 The basic elements in Haggett's model for the study of spatial systems, t_1, t_2 and t_3 representing stages in diffusion.
After Haggett, Cliff and Frey 1977 p. 7.

At the heart of geography as a science is the distributional view. Geography is a **discipline in distance**. When we discuss space it is not the container space 'that frames the totality of a landscape; we prefer to think of space as a system of distance relationships between objects' (Hard, 1973, p. 184). The study of spatial arrangements may be summarized in Haggett's (1965) diagram (Figure 3.13) of spatial structures. The sketch may be seen as a disaggregation of **functional regions** such as those established around central places in a Christaller model, into five geometrical elements (movements, channels, nodes, hierarchies and surfaces). A sixth element, diffusion, was added later (Haggett *et al.*, 1977). In contrast to the traditional system of self-sustained regions, the primary element in a modern society is the need and desire for interaction between places which results in a pattern of **movements**. These might be studied as the geometric pattern of straight lines between points, but in fact most movements are channelled along particular route corridors, such as roads. So we can study the patterns of **channels** which, together with **nodes**, represent an organization **network**. The **hierarchy** represents the relative importance of the nodes and the **surfaces** represent the system of land use as exemplified by the work of von Thünen. Patterns of human occupance are, however, not static. The process of change in time therefore involves **spatial diffusion** as developed by Hägerstrand.

Haggett's book led to a fundamental debate within the subject. The arguments presented by Kuhn (1962/1970a) on paradigm shifts within the world of science were applied to the debate. Thus Chorley and Haggett (1967, p. 39) stated that they had looked at the **traditional paradigmatic model** of geography and had found that it was largely classificatory and under severe stress. They suggested that geography should adopt an alternative **model-based paradigm**, and so made it clear that the new development within the subject not only represented a wider range of methods but also demanded a fundamental paradigm shift. Each geographer was given the choice between the traditional and the new model-based paradigms. Model building was set up as the aim of geographical investigation, a task to be performed with the aid of **quantitative methods** and the use of computers to handle data. A **model** was defined as an idealized or simplified representation of reality that seeks to illuminate particular characteristics. The concept is a wide one – for Chorley and Haggett (*ibid.*), a model could be a theory or a law or a hypothesis or a structured idea.

The rapid development of spatial model building and the use of quantitative techniques could not have taken place without computers, but computers did not determine the development of spatial science: 'Model building preceded the invention of the computer in many sciences, but in a discipline like geography which handles such large quantities of data it would hardly have been possible to develop operational models worthy of the name without computers' (Aase, 1970, p. 23). This technological development had given the subject new possibilities young researchers had no hesitation in exploring.

We may conclude that the technical development of the discipline (use of computers and mathematical-statistical methods) could hardly be called a 'revolution' in Kuhn's sense. To talk about a 'quantitative revolution' may thus give false impressions. But it is true that the 'new' geography provided the discipline with notable research projects that could serve as '**exemplars**' for new students as, for instance, Hägerstrand's diffusion model. And the renewed discussion on the basic problems of the subject that followed in the wake of the quantification process may be regarded as a sign of a crisis phase. Individual research workers felt themselves more or less obliged to take a stand and to clarify their own research situation, so there was little opportunity for straightforward **puzzle-solving**. The transformation to a **spatial science** on the basis that **locations are essentially relative** may also indicate a paradigm shift. But the meaning of this transformation was not generally understood or appreciated by the geographical community. Old ideas continued to flourish and new ideas cropped up as results of criticisms of the spatial science school. Bird (1993, p. 13) has characterized the changes in geography as **constant revisions**; they may also be regarded as a multiparadigmatic development, since different schools of thought continued to live side by side.

It may, however, be a characteristic of social science that new paradigms do not become so well established to enable a relatively long period of normal science. Or rather, we may have reached a stage of mature science where we experience '**revolution in permanence**', in the Popperian sense.

CRITICS OF THE SPATIAL SCIENCE SCHOOL

Opposing the so-called 'revolution', Stamp (1966, p. 18) preferred to call it a 'civil war', and noted that quantification had many points in common with a political ideology; it was more or less a religion to its followers, 'its golden calf is the computer'. Broek (1965, p. 21) stated that 'there are more things between heaven and earth than can safely be entrusted with a computer'. Even Ackerman, one of the advocates of quantification, warned (1963, p. 432) that 'the danger of dead end and nonsense is not removed by "hardware" and symbolic logic'. Minshull (1970, p. 56) observed that the landscape was becoming a nuisance to some geographers, that many of the models could only be applied to a flat, featureless surface, and warned there was a real danger that these ideal generalizations about spatial relationships could be mistaken for statements about reality itself.

Fred Lukermann (1958) reacted especially to attempts by the social physics school to establish analogies with physics, maintaining that hypotheses derived by analogy cannot be tested: falsification is impossible. In a series of papers in the 1970s, Robert Sack, a student of Lukermann, criticized the view put forward by Bunge (1962) and Haggett (1965) that geography is a spatial science and that geometry is the language of geography. Sack (1972) maintained that space, time and matter cannot be separated analytically in a science concerned with providing explanations. The geographical landscape is continuously changing. The processes which have left historical relics and which are creating new inroads all the time must be taken into account as important explanatory factors. The laws of geometry are, however, static – they have no reference to time. The laws of geometry are sufficient to explain and predict geometries, so that if geography aimed only at analysis of points and lines on maps geometry would be sufficient as geographical language. But, 'We do not accept description of changes of its shape as an explanation of the growth of a city . . . Geometry alone, then, cannot answer geographic questions' (*ibid.*, p. 72).

de construction of spatial science school

Another problem, Broek pointed out (1965, p. 79), crops up if we project a model derived from our own surroundings over the whole world as a universal truth and measure different situations in other countries as 'deviations' from the 'ideal' construct. Models based on research within the western cultural world cannot be elevated into general truths. Brian Berry (1973b) came to the conclusion that a universal urban geography does not exist, and that urbanization cannot be dealt with as a universal process: 'we are dealing with several fundamentally different processes that have arisen out of differences in culture and time' (*ibid.*, 1973b, p. xii). He divided the world into four universes: (1) North America and Australia, with their free market economies; (2) western Europe, with its planned welfare economy; (3) the Third World, with its economy split between a traditional and modern sector; and (4) the socialist countries, with their rigidly planned economies. Each of these has its own urban geography, which again will change through time.

Haggett *et al.* (1977, p. 24) also noted that 'the Russian translation of the first edition of this book (Haggett, 1965) made clear how heavily the locational explanations were rooted in the classical economics of the capitalist world. Inevitably, the lopsidedness of the book will appeal to certain readers and condemn it to others.'

THE ACHIEVEMENTS OF SPATIAL SCIENCE

It is commonly agreed that the spatial science school threw open the windows of a hitherto introverted discipline, which had had its major links with idiographic disciplines, such as history and geology. Disciplinary boundaries became much more open; methods and theories were openly borrowed from geometry, physics and social sciences as geographers became involved in multidisciplinary research projects. The 1960s and 1970s were optimistic decades for geographical innovators. Student numbers grew rapidly and career opportunities expanded considerably.

The redevelopment of geography as a social science raised the self-esteem of geographers and opened up a job market for candidates within planning and administration. Generalists (as geographers still were, but now with added technical and statistical knowledge) proved to be better adapted to the job market than candidates with narrower specializations.

Haggett (1990, p. 6) argues for practical and pragmatic approaches in geography: if 'science is the art of the soluble, then much geography is the art of the mappable'. 'Thinking geographically', liking maps and thinking by means of them is intrinsically linked with geography. More than any other natural or social science, geography is a **visual science** with similarities in this respect to architecture and the history of art. We like to climb a mountain in order to get an overview, a grand survey of the geographical patterns in front of us. We try to describe and explain the world as we perceive it.

Description and mapping were also central to the traditional schools of geography, but the spatial science school developed more refined methods that made spatial correlations and statistical tests possible. The most recognizable shift was, however, the downgrading of ordered description of what we know (**cognitive description**) and the development of sophisticated models and methods in **morphometric analysis** such as are described by Figure 3.13 (p. 84). Most of the models created were simplifications of spatial morphological patterns based on empirical data. Christaller's central place theory is an example of this. When trying to achieve a general, theoretical explanation of patterns, theory was imported from other sciences. In many cases it was economic theory.

A major achievement of the spatial science school has been the development of sophisticated methods for the detection of spatial patterns. Many of the models, including such a simple one as the 'gravity model', are good devices to compare data and thus to describe geographical differences. These approaches have given valuable insights into the geographical patterns which form the bases of our analysis or are the results of our decisions. But it might be argued that spatial science research developed greater refinement of description than explanation. Many commentators within human geography have pointed out that spatial science research has been confined to the empirical level, and that we need **structuration theory** to understand how 'real' or deep structures influence the empirical outcomes or events. (We will return to this in Chapters 4 and 5.)

But still, in the frenetic search for grand explanations, we often forget the value of descriptions that enlighten us. New descriptive models are certainly legitimate scientific endeavours as long as they create new knowledge. Spatial analysis provided better tools for descriptions, and new, intriguing developments have continued to be developed, particularly within physical geography and ecogeography.

Haines-Young (1989, p. 31) points out that 'the new information-based technologies' provide techniques, notably expert systems, which enable us to carry the problem of modelling geographical knowledge to a deeper level than has been possible so far. For example, a system designed to predict fire risk in the Kakadu National Park in Australia uses a database together with information supplied by the users of the park (see GIS, pp. 180–182).

Advanced **systems analysis** has proved its usefulness in **physical geography** and **ecogeography** – the study of humanity's role in changing the face of the earth. Gregory (1985), Goudie (1990) and Huggett (1993) provide many examples of these developments, and earlier editions of this book (Holt-Jensen, 1981; 1988) also included a basic introduction to systems analysis in geography.

Here we restrict the presentation to one example (see Box 3.5 and Figure 3.14 on the Sahel catastrophe), and agree with Unwin (1992, p. 129) that it is 'surprising that ecosystems were not more extensively used as a framework for empirical research by geographers'. One reason for this deplorable fact may be that biogeography has generally held a weak position in Anglo-American and Scandinavian geography; however, the situation is much better in Germany.

The potentialities of **systems analysis** in general, and **ecosystems analysis** in particular, have not been fully developed. Further, systems analysis could also be applied to human geography, as Mabogunje (1976) has demonstrated: it is well suited to giving a conceptual understanding of the factors that influence for instance, rural–urban migration in developing countries.

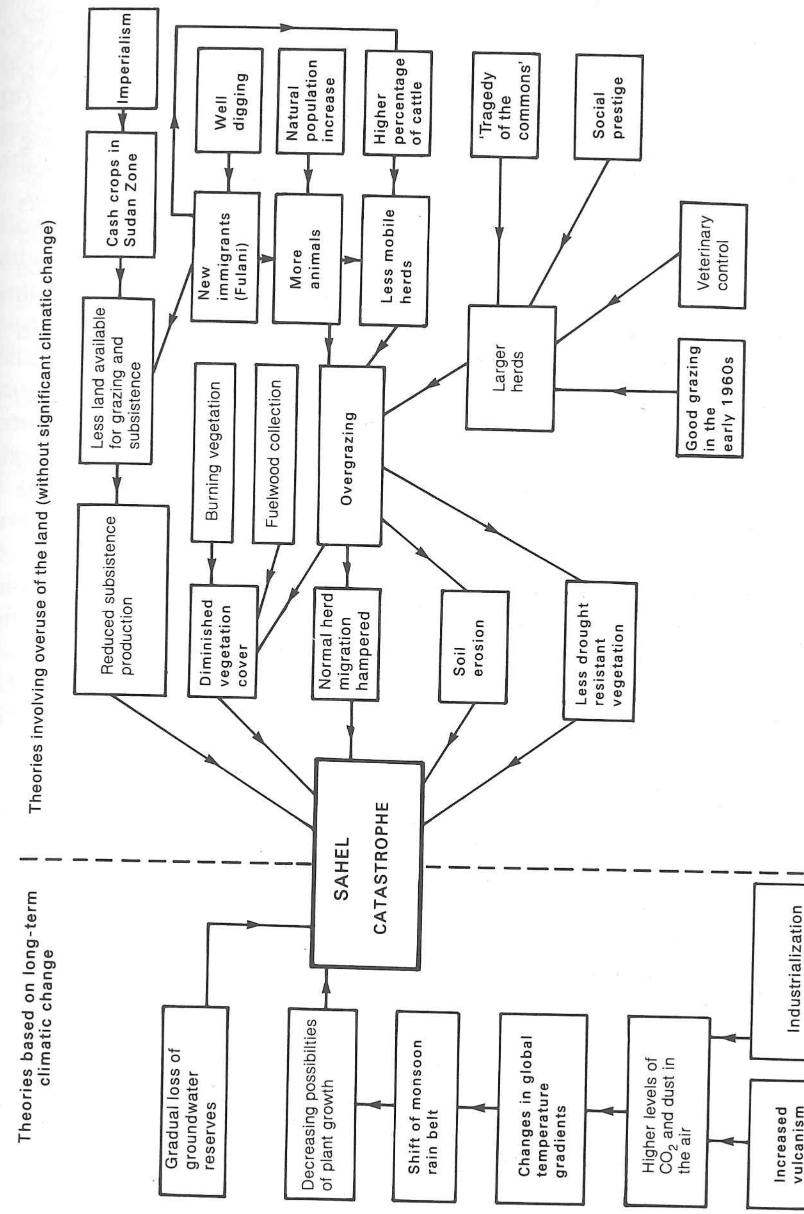

Figure 3.14 A practical example of a systems model illustrating the possible reasons for the Sahel catastrophe
Source: After Reenberg, 1982

Box 3.5 The Sahel catastrophe explained with the aid of a systems model

Chorley (1973a, p. 167) suggests that it is important that the challenge of environmental deterioration is met with new positive feedback in the form of better planning and control – a task for geographers. Let us consider the Sahel catastrophe, which is analysed in a *systems model* (Figure 3.14) by the Danish geographer Reenberg (1982). She uses the model to illustrate the great number of possible causes for the increasing desertification of the Sahel belt of Africa. One possible explanation is climatic change (on the left side of the figure). Climatic changes may be due to strictly natural factors that have prevailed many times in the earth's history. These changes may even have had specific causes, such as increased vulcanism. Alternatively, climatic change may also be induced by human activity, through increased industrialization and pollution. The assumption that increased desertification is the result of climatic change can be tested empirically by studying a series of meteorological observations taken over time.

Having studied the Sahel problem, however, geographers prefer the theories presented on the right side of the model. It is not possible to postulate a simple chain of causes and effects on this side of the model since many of the elements intermingle and work on each other in complex ways. Some of these elements are associated with the region's traditional ways of life. For example, many tribes calculate their wealth in terms of head of cattle, and so tend to overgraze. Other traditions include slash-and-burn methods of improving pasture and agricultural land, the collection of fuelwood and the production of charcoal.

While external influences (labelled 'imperialism' in the figure) were relatively small, some sort of natural balance was struck, which was regulated by mortality in periods of drought. However, external influences have encouraged the population to use the more fertile parts of the region for cash-crop production for sale in more industrialized countries. Consequently, areas of subsistence agriculture have been reduced and the pressures on these areas increased. Well meaning western initiatives, such as medical provision and veterinary control, have meant that the population of both people and cattle has increased, further exacerbating the problem of pressure on the land. Development aid in the form of well-drilling to provide supplies of clean water has also influenced the nomadic population's wandering routes, resulting in patchy overgrazing around watering places. Vegetation is destroyed and the desert expands day by day. A particular problem is the 'tragedy of the commons'. Although not continuously occupied in former times most land was held in some sort of tribal ownership. Western administrators, however, often classified land that was not currently being used as 'common land', effectively removing local tribal responsibility for this land which resulted in a loss of ecological balance.

From this somewhat simplified example we can appreciate the complexity of the Sahel problem, which does not lend itself to mere cause-and-effect explanations or to general structural theories. In undertaking a research project we may not be able to analyse more than a few factors; hence the systems model remains conceptual but, as such, can help us to understand complex relationships.

A 'CRITICAL' REVOLUTION?

In 1972 Haggett appeared confident that the spatial science school had taken the lead: 'Today the general acceptance of [quantitative] techniques, the more complete mathematical training of a new generation and the widespread availability of standard computers on campus make the conflicts of a decade ago seem unreal' (*ibid.*, p. 460). He also pointed out (*ibid.*) that 'the first years of over-enthusiastic pressing of quantitative methods on a reluctant profession have given way to the present phase in which mathematical methods are just one of many tools for approaching geographic problems'.

In the meantime, a new type of criticism had been developing. In *Directions in Geography* (Chorley, 1973b) a number of geographers who had, one way or another, had some hand in the spatial science innovations discussed possible directions the discipline might follow in the future. Many of the contributors suggested quite new directions for research, while others openly criticized different aspects of the spatial science approach. Harvey (1973, pp. 128–9) also became a notable apostate, declaring that 'the quantitative revolution has run its course, and diminishing marginal returns are apparently setting in . . . Our paradigm is not coping well . . . It is ripe for overthrow'.

In retrospect, the 1960s could be characterized as an era of '**hard science**' whereas, in the 1970s, there was much questioning of the law-seeking approach. Guelke (1977a, p. 385) concluded that 'the idea that a scientific discipline must necessarily have laws of its own is false. A discipline can be scientific if it uses or consumes laws from other areas.' Michael Chisholm (1975, pp. 123–5) noted that geographical 'laws' in general would not meet the exacting specifications needed to qualify as laws, since they are not generally verifiable. It would be more correct to talk of models and theories rather than laws in geography, as a **theory** should be defined as an articulated system of ideas or statements held as an explanation (*ibid.*, pp. 123–6).

In Chisholm's view the essential characteristic of central place theory, and other theories established by the spatial science school, is their **normative** character; the theoretical construct is not intended to show how the world is actually organized but to demonstrate the patterns that would occur if reality were rational.

Descriptive, or what Chisholm called **positive, theories** seek to account for observed phenomena, as did, for example, Copernicus' theory of the motion of the planets around the sun. The urban rank–size relationship is one of the more famous regularities observed in geography, and may as such qualify as a positive theory, according to Chisholm (*ibid.*, p. 148). In positive theories the observations of discrepancies between the predicted state and actual states of the system may stimulate an advance or changes in the theory, whereas **normative theory** is used to create a world that is 'rational'. Normative theories are useful in social studies and in town and regional planning, and it was in these fields that many trained geographers found career outlets in the 1960s and 1970s. It was the many assignations in this field, observes Olof Wärneryd (1977, p. 29), that encouraged the situation where geography lost contact with its earlier scientific traditions. As in other fields of social activity, there was a general belief in economic growth and economic theories; however, the values embedded in these were not seriously debated.

Another problem was that many of the geographical models used in planning were static – for example, **central place theory** played a major role in many development programmes but little attention was given to the fact that functional space is dynamic and in more or less continual change.

An example from Sweden, discussed by Gunnar Olsson (1974), may clarify this point. In the 1960s Swedish geographers were engaged in a far-reaching reform of administrative districts, which was expressly intended to abolish spatial social and economic inequalities in the country. The new municipalities were to be large enough to sustain the considerable burden of the municipal welfare services. The methodology used was to observe how the majority of people interacted in space and then to translate the observations into a Christaller-type model:

> Unnoticed by spectators and performers, the play was changed in the middle of the act. The ought of justice disappeared into the wings, invisibly stabbed by the is of the methodology. Exit man with his precise visions, hopes and fears. Enter Thiessen polygons with crude distance minimisations and cost-benefit ratios.
>
> (*Ibid.*, p. 355)

No one thought to ask whether people wanted to change their observed interaction patterns or whether the centralization led to disadvantages for some groups.

The spatial science of the 1960s made use of theories from other sciences, notably economics, which were thought to give an objective description of society and how it functions. Models were constructed which gave an apparent explanation, but they were misleading or directly fallacious in so far as they failed to recognize that social situations might be amenable to change: the models that were developed tended to support society's existing conditions. This is particularly true of process models, which encourage us to believe that a trend, once ascertained, will continue to operate in the future. Thus a theory which appears to be positive or based on realistic description is translated into the development's purpose through planning and political decision. It is during this sequence of events that the theory becomes normative. In the Swedish example, a planning decision that had the objective of changing a social structure in fact led to the conservation of the structure because of the theories and methods used in the planning process. One conclusion that may be drawn from this is that the distinction between normative and positive theory may be useful in a theoretical classification but we must not ignore the fact that the same theory can be used in both a normative and positive way.

Another conclusion drawn by a number of geographers in the late 1960s was that physical planning had not been as effective in fostering social change and equality as many people had hoped. For example, many of the land-use and transport plans that spread from North America to practically every large city in western and northern Europe, and in which many trained geographers had participated, seemed to have increased the segregation of social classes and to have sharpened differences in mobility between the car-owning and the car-less groups. In transport planning, the interaction pattern of the average family had been used as a guideline. Such 'deviant' travel patterns as those of old people with no access to a car had not been given much attention. The reason for this had been methodological; quantitative models were built to cope with aggregate and **'hard' data** – that is, data easily expressed in numbers. **'Soft' data**, which concern

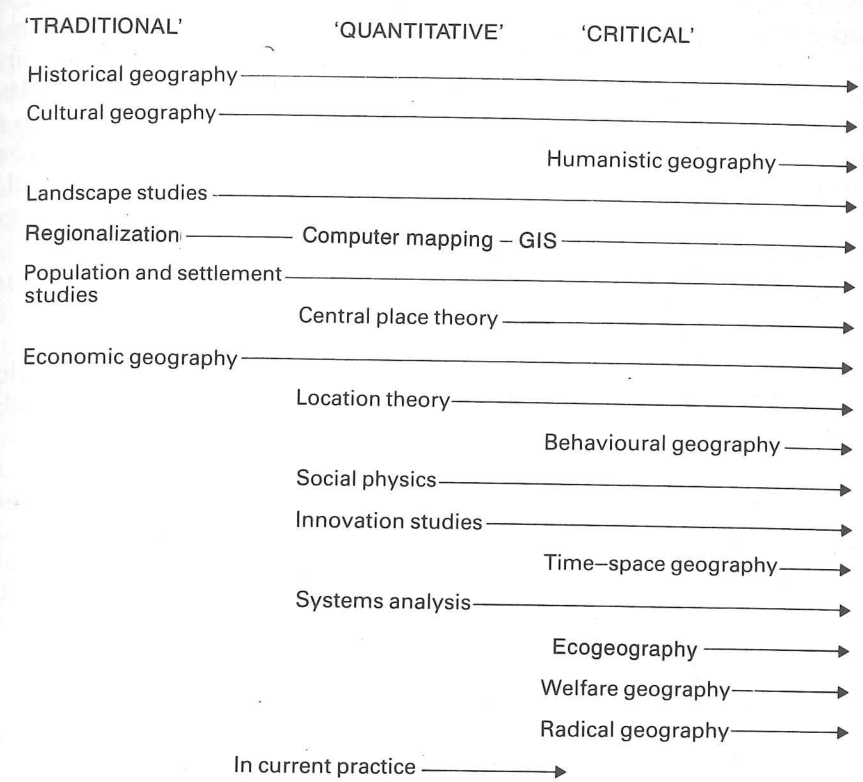

Figure 3.15 Schools of human geography 1950–1980. It must be stressed that this classification should be treated with caution, for the criteria for the classification are not consistent throughout the scheme. While 'traditional' schools are mainly classified on the basis of their themes, most, but not all, of the 'quantitative' schools are classified in terms of their type of theory. The 'critical' schools are a real mixture: some are classified on the basis of their special methodology, while others owe their position to a particular political approach. Systems analysis is better regarded as a general method which might be useful to a number of schools rather than as a school in itself. The expressions 'traditional', 'quantitative' and 'critical' are in inverted commas because the use of these terms as labels is highly debatable – as is the grouping of schools under each label

human attitudes and deviations in behaviour, could not be handled easily in such models. But even research workers involved in aggregate studies were bound to wonder about deviations from the 'normal', and this led to studies of the welfare of special groups of people, such as old people, and a growing concern for the position of the individual within a mass society.

Students of **location theory** also had second thoughts as they came to realize that '**economic man**', that decision-maker blessed with perfect predictive ability and knowledge of all cost factors, does not in fact exist. Locational decisions may be made on a rational basis, but this rationality relates 'to the environment as it is perceived by the decision-maker, which may be quite different from either "objective reality'' or the world as seen by the researcher' (Johnston, 1997, p. 150). It was thus necessary to develop alternative theories to those based on 'economic man' and to investigate the decision-makers' behaviour and perceptions. There are a number of different research trends within geography that stem from these observations. Figure 3.15 gives a simplified classification for the period between the Second World War and the 1980s. The 'traditional' and 'quantitative' schools have been dealt with earlier. Some of the trends within 'critical' geography will be outlined briefly below; a discussion of the philosophical basis of 'critical' geography is postponed to the next chapter.

A NEW HUMANISTIC GEOGRAPHY

The new concern for the individual led some geographers to favour a revitalization of some of the methods of the traditional schools in geography, including those of the French school of regional geography. This trend, which may be regarded as a new initiative by geographers who had not been involved in the models-orientated approach of the 1950s and 1960s, emphasizes the need to study unique events rather than the spuriously general. Anne Buttimer (1978, p. 73) argued along similar lines to Vidal de la Blache that historical and geographical studies belong together. She stressed the need to understand each region and its inhabitants from the 'inside' – that is, on the basis of the local perspective and not from the perspective of the researching 'outsider'. Leonard Guelke (1974, p. 193) advocated an idealist approach which 'is a method by which one can rethink the thoughts of those whose actions he seeks to explain'.

Guelke mainly based his arguments on the writings of R. G. Collingwood, the Oxford historian and philosopher, and especially on *The Idea of History* (1946). Guelke argued that these ideas are crucial to **historical geography**; it is important that historical geographers 'focus their attention on the meaning of human actions of geographical interest, not merely their geographical [physical] expressions' (Guelke, 1982, p. 12). 'Different people in making use of the Earth have, for example, created distinctive field and settlement patterns. These patterns are not arbitrary, but reflect the thinking of the people who created them' (Guelke, 1981, p. 132).

We must not be constrained by the view that geographers are always obliged to use the methods of natural science. A natural scientist is normally an outside observer: it is difficult for him or her to take an inside view. Although a historian, in Collingwood's sense, is unable to see directly into the minds of his or her subjects, he or she is able to understand their actions as being products (as are the historian's) of **rational thought**. The historian will therefore use the **hermeneutic** interpretative approach (see Chapter 4), involving *verstehen* or sympathetic understanding – trying to rethink the thoughts of historical characters, as far as his or her knowledge of their cultural background allows, but not trespassing into psychology. When we understand the beliefs which encouraged Columbus to sail west to reach India, we have explained the motives for the voyage. It is not necessary to discuss why Columbus held those beliefs for that would take us into the realm of historical psychology.

The **idealist approach**, as advocated by Guelke, thus restricts itself to beliefs that are in some way rational, and it leaves aside the emotional and psychological aspects of human behaviour. We cannot re-experience the emotional life of other people. The approach, however, does not let us know for sure whether we have really succeeded in finding the true explanation for historical events. The idealist philosophy has elements in common with **phenomenology**, which has also been suggested as a useful approach for geographers.

Phenomenologists do not, however, make sharp distinctions between intellectual and emotional life. The phenomenologist is more concerned with describing the life experience of the researcher. This description must inevitably be subjective because 'the approach fails to distinguish those elements of human existence that are open to subjective understanding and those that are not' (Guelke, 1981, p. 144). Phenomenologists are primarily concerned with our (subjective) **knowledge** of the world around us: they attempt to create an objectified knowledge of our experience of the phenomenal world. Our knowledge proceeds from the world of experience and cannot be independent of that world.

Yi-Fu Tuan, although not defining himself a phenomenologist in the strict sense, has written a number of inspiring essays and books introducing geographers to phenomenological ideas (1974; 1976; 1977; 1980). Tuan has stated (1971) that geography is *the mirror of humanity*: to know the world is to know oneself. Such a study is clearly based in the humanities rather than in social and physical sciences: 'The model for the regional geographers of humanist leaning is . . . the Victorian novelist who strives to achieve a synthesis of the subjective and the objective' (Tuan, 1978, p. 204). Tuan prefers to use the term **humanistic geography** for such studies, which are regarded here as including both idealism (in Guelke's terminology) and phenomenology.

In his book *Conceptions of Space in Social Thought* (1980) Robert David Sack rejected those conceptions of space that stem from the natural sciences and that were the only ones used in the spatial sciences. He suggests that, in other modes of thought such as art, myth, magic and the child's view of life, space may have very different meanings: 'If people see and/or evaluate things and space differently and ''non-scientifically'', then social science must somehow represent and capture those meanings' (*ibid.*, p. 8).

Such humanistic trends of thought may have been new to the English-speaking world, but they can be related to significant traditions in both French and German geography. Ewald Banse, whom Hettner described as the *enfant terrible* of geography, attacked the ruling disciplinary matrix of the subject as early as the 1920s, arguing that it only attempted to describe external reality. To understand the essence of things in depth, says Banse (1924, p. 58), the science of geography should be redefined as an art: 'Only the unified perspective of both the visible outer appearance and the inner core of things constitute real geography, which is a spiritual presentation of experienced impressions.'

Nicholas Entrikin (1976, p. 616) makes the point that the humanist approach is best understood as a form of criticism. However, as Johnston (1997, p. 196) points out, we may also hold the view that 'the human condition can only be indicated by humanistic endeavour, for attitudes, impressions and subjective relations to places (the ''**sense of place**'') cannot be revealed by positivist research'. This can provide insight into the essential structures of human relations to the environment.

It is also clear that we will always use and need **hermeneutic** or interpretative methods in geography. Dorling and Fairbairn (1997) point out that even a map is an interpretation of reality and has to be reinterpreted according to the purposes of each particular use.

SPATIAL SCIENCE CRITICIZED FROM WITHIN

In contrast to humanistic geography, **behavioural geography** may be seen as being based on criticism from within spatial science. This starts from a disillusion with **location theories** involving, for instance, the concept of **'economic man'**. However, the roots of behavioural geography are much older. In Europe, the Finnish geographer Johannes Gabriel Granö and his Estonian student Edgar Kant were attempting a behaviourist approach in the 1920s (Granö, 1929). Even in the USA, behavioural and quantitative approaches were contemporary developments: the behavioural approach was taken up in the late 1950s and the 1960s by Gilbert White, then at the University of Chicago, and his associates, who made a series of investigations into the human response to natural hazards, guided by theories of decision-making and influenced by methods used in psychology and sociology. It was regarded as more important to map the personal **perception** of the decision-maker than to describe the factual physical and economic conditions of the environment, since the decision-maker would act upon his or her own perceptions and not on the environmental factors themselves (White, 1973). Julian Wolpert introduced behavioural geography to many human geographers through a paper in 1964, which compared actual with potential labour productivity on farms in central Sweden. He found that the sample farm population did not achieve profit maximization and nor were its goals solely directed to that objective. The farmers were 'spatial satisfiers' rather than 'economic men'.

A further aspect of behavioural analysis is the concept of the **mental map** of the environment. Mental mapping has been taken up by a number of workers, among them Rodney White and Peter Gould (1974). A somewhat different approach to behavioural work is found in Allan Pred's (1967; 1969) two-volume work *Behaviour and Location*, in which Pred tried to present an ambitious alternative to theory-building based on 'economic man'. The **time-space geography** Hägerstrand and his associates (see p. 81) established may also be seen as a critique not so much of the spatial science school as of important aspects of social science research in general. In simple terms, it provides a method of mapping spatial behaviour while at the same time representing a reorientation of scale away from aggregate data towards studies of individual behaviour. More important, however, is its introduction of a new economic theory in which time and space are regarded as scarce resources, the allocation of which forms the basis of the social realities we study.

A corresponding concern for the individual within mass society is also basic to **welfare geography**, which developed as a special branch in the 1970s. Paul Knox (1975) stated that it was a fundamental objective for geography to map social and spatial variations in the quality of life. The study of such spatial inequalities was taken up by Coates and Rawstron (1971), Morrill and Wohlenberg (1971), Coates *et al.* (1977) and Smith (1979), and a number of others. While some of this work represented the geographer as a 'delver and dove-tailer', a provider of information, other examples, notably *The Geography of Poverty in the United States* (Morrill and Wohlenberg, 1971), also proposed both social and spatial policies for changing existing conditions.

Whereas, in principle, welfare geography works within the framework of the existing economic and social system, **radical geography** called for both revolutionary theory and revolutionary practice. Its aim was clearly voiced by Harvey in *Social Justice and the City* (1973, p. 137): 'Our objective is to eliminate ghettos. Therefore, the only valid policy with respect to this objective is to eliminate the conditions which give rise to the truth of the theory. In other words, we wish the von Thünen theory of the urban land market to become not true.' Harvey maintained this could only be done if the market economy was eliminated. The task, therefore, was the self-conscious construction of a new paradigm for social geographic thought, which might stimulate a political awakening and start a social movement with the ultimate goal of bringing about a social revolution.

Berry (1974) commented that Harvey relied too much on economic explanations; in our postindustrial society, control is no longer economic but political, making a Marxist analysis more or less passé. Morrill (1974), however, in reviewing Harvey's *Social Justice and the City* (1973), confessed that he was pulled most of the way by the revolutionary analysis, but that he could not make the final leap in accepting that our task is no longer to find truth but to create and accept a particular truth.

REVOLUTION OR EVOLUTION?

Just as Harvey favoured revolution in society, so he supported the **paradigm** concept and the simplified Kuhnian model of the development of science through revolutions: 'A quick survey of the history of thought in social science shows that revolutions do indeed occur' (Harvey, 1973, p. 122). Harvey assumed that the **motivation** for the construction of a paradigm in social science is the desire to manipulate and control human activity and social phenomena. The **spatial science paradigm**, according to this view, came into existence as a response to pressures from the material, or economic, base of society, which suggested the desirability of discovering ways of manipulation and control, particularly within the planning sector (Quaini, 1982, p. 155).

Harvey (1973, p. 121) did not regard Kuhn as a revolutionary because of 'his abstraction of scientific knowledge from its materialistic base'. According to Jan Widberg (1978, p. 9), Kuhn provides a dialectic-idealistic interpretation of scientific advancement. Widberg maintained that it is possible to describe the history of scientific thought within a discipline on the basis of four fundamentally different views. These are categorized through two sets of dichotomies:

	Mechanical	Dialectical
Idealistic	I	II
Materialistic	III	IV

A **mechanical view** implies that the development of science is linear, with each new generation continuing from the point where the old generation left off. There are no revolutions, only growing specialization, professionalization and the advancement of better methods and understanding. Kuhn expresses the **dialectical view** when he maintains that a science develops through contradictions and revolutions with changes of paradigms.

An **idealistic viewpoint** implies that ideas are the driving force behind scientific development and change. Each individual makes his or her own choice, and it is the genius of scientists that counts. A **materialistic viewpoint** implies that the material base governs the advancement of scientific knowledge. Scientific activity reflects the special interests of those who are in control of the means of production (*ibid.*, pp. 2–3). In Harvey and Widberg's telling, the story of geography has been the tale of the geographical legitimation of the social conditions that produced it.

But it is also possible to argue that in geography paradigms (or, rather, schools of thought) have not succeeded each other but, to a greater extent, continue to exist in parallel. New schools slowly absorb the older ones, leaving some former contradictions to linger on within the new structure. Figures 3.6 and 3.15 suggest that concepts and lines of thought survive after basic shifts have taken place in the discipline, and may crop up again in new clothing.

Anne Buttimer (1981, p. 82) maintains that the idea of a 'paradigm' has proved appropriate for describing developments within the physical sciences; it fits less comfortably in the story of biological sciences and finds itself on rough ground when applied to any field that aims at a comprehensive understanding of humanity and environment. Buttimer (*ibid.*) maintains that everyone who has studied in some depth the life, work and experiences of those individuals who have taken a leading role in the shaping of our science will appreciate that the paradigm model gives a distorted picture of actual developments.

Figure 3.15 illustrates the existence of parallel schools of thought within geography since the Second World War. This is in line with Johnston (1997, p. 389), who concludes that 'human geography is currently characterized by a multi-paradigm situation at the world-view level – and by a wealth of exemplars on which research is based'.

From this we may draw the conclusion that the dichotomy between a dialectical and mechanical understanding of the history of our subject is an interesting academic study, but the truth as we see it lies somewhere between the contradictions. Shifts of major importance do occur, but they seldom encompass the whole scientific community – old ideas and concepts remain with us to a large extent; new discoveries may sometimes have the character of mutations – and usually they look more like rephrasings of old truths.

To analyse the dichotomy between an idealistic and a materialistic view, we have to discuss social research and values more closely. Initially this means we need to get a better understanding of the debate on **positivism** and **critical philosophy**.

4

Positivism and its Critics

The prestige and support that aid research stem from the general belief that research is an almost objective, value-free activity. Most people think of research as an uncompromising search for truth, an activity unaffected by the emotions, beliefs, attitudes and desires of either the researcher or of the society in which the research takes place. When research workers discuss scientific results in the same way as politicians discuss political issues, many people will conclude that there must be something wrong with the research and hence its prestige sinks. Social scientists who work with less exact or less clearly definable data are particular targets for such attitudes.

Natural scientists, as a rule, work with discrete materials which lend themselves to experiment and objective analysis. Their conclusions are widely received as truths. Social scientists have a weaker database, are often personally involved in the research issue and can seldom experiment with their material. The question of objectivity is therefore most acute for social scientists. The results of social science research are often regarded by the public as if they were merely points of view. A discussion on the role of values in the social sciences is therefore important – not only for the viability of research activity but also for relationships between scientists and the general population, and for public attitudes towards science.

POSITIVISM AND CRITICAL THEORY

Scientific and philosophical discussion has produced two chief categories of meta-theory (superior theories or theories about theories) for scientific research: **positivism** and **critical theory**. 'Both trends are in a sense more "climates of opinion" than definite schools of thought. There is far more discussion within the trends than between them', says Skjervheim (1974, p. 213). There are a number of different versions and definitions for each of these meta-theories.

Critical theory is associated with a group of scholars frequently known as the Frankfurt School, represented (particularly since 1950) by Jürgen Habermas. As we do not restrict the discussion below simply to the work of the **Frankfurt School**, we will henceforth use the expression 'critics of positivism' rather than