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## **Unifying social science A critical realist approach**

### **1. Introduction**

The ideal of unifying science has appealed to philosophers and scientists since the beginning of western civilization. In the early 20th century it was perhaps most closely associated with the work of the logical empiricists of the Vienna Circle. More recently the desire to synthesize knowledge has been reflected in enthusiasm for interdisciplinary research, not least among social scientists concerned at the increasingly specialized nature of social inquiry. Critics argue that the current division of intellectual labour in the social sciences leaves knowledge of reality in a fragmented state at a time when increasingly complex social and environmental problems demand for their solution the integration of disciplinary knowledge (Landauer 1971; ESRC 1987; Gulbenkian Commission 1996; Sayer 1999; Van Langenhove 2000; Wallerstein 1991; Blackburn 2004).

This paper contributes to the debate about unifying science by addressing two questions: first, whether or not it is desirable, and, second, whether or not it is feasible to unify the social (and natural) sciences. My argument draws explicitly on the insights of a recently developed philosophy of science known as critical realism (Collier 1994; Archer et al. 1998; Danermark et al. 2002). Hitherto most philosophies of science have offered conceptions of unification through reduction: positivism, by reducing reality to atomistic events and states of affairs, and hermeneutics, by reducing reality to ideas and/or discourse. Consequently both philosophies are unable to give a convincing account of the

historical differentiation of science. By contrast, the conception of unification proposed here, which derives from critical realist reflections and elaborations upon the nature of science and of reality, is both non-reductive and able to make sense of the historical differentiation of science. It was only by returning to ontological theorising directly that critical realists were able to underlabour for a more coherent conception of science that avoided the problems associated with reductionism. Critical realists established by transcendental reasoning that there existed a hierarchy of unobservable structures and mechanisms, emergent at different layers of reality, responsible for generating observable events and states of affairs. It was the irreducibility of structures and causal mechanisms to empirical events that made it possible for them to be identified – whether it be through experimentation in the natural sciences or conceptual abstraction in the social sciences.

Critical realists, then, argue that the stratification of reality is reflected in the stratification of science, so that different sciences will take different strata as their objects of inquiry. However, in this paper I argue that the differentiation of the natural sciences reflects differences between strata whereas the differentiation of the social sciences reflects differentiation of the objects lying at one particular level. In other words, whereas natural structures and mechanisms emerge at different levels of reality, social structures and mechanisms emerge at the same level. If social structures and mechanisms are ontologically interdependent, I argue, the unification of the social sciences is required to understand this interdependence. Moreover the interaction between social and natural structures and mechanisms ultimately entails the unification of the social and natural sciences and a broader understanding of the term 'society'.

But if scientific unification, in addition to specialization, is desirable, I argue, its feasibility is more problematic. Whether or not the integration of disciplinary knowledge will be possible through 'interdisciplinary' or 'post-disciplinary' research practices will depend on the social and intellectual context of knowledge production; for whilst philosophical agreement between members of an interdisciplinary research team will

facilitate the synthesis of knowledge, the institutionalisation of scientific disciplines in universities may still obstruct it.

The structure of the paper is as follows. I begin with an overview of the two building blocks of critical realism: transcendental realism and critical naturalism. In the following section I draw on these philosophical theses to justify a conception of unified science before addressing the question of whether or not unification may be realized through interdisciplinary research.

## **2. Critical realism and science**

Let me begin, then, by giving an outline of the philosophy of science that underpins my argument. The term 'critical realism' refers to the combination of transcendental realism and critical naturalism as elaborated successively in the work of Bhaskar (1975; 1979). If the characteristic starting point of positivism is to ask how knowledge of reality is possible, the characteristic starting point of transcendental realism is to ask what makes knowledge of reality possible; more specifically, what reality must be like for successful scientific experimentation to be possible. Positivists hold that the aim of science is to record, through observation and experience, *naturally* occurring laws, which, according to the Humean theory of causation, take the form of constant conjunctions of events and states of affairs. In this way positivism restricts itself to what transcendental realists call the 'empirical' domain of reality. (Hence positivism may be categorised as an 'empirical realism'.) However, the problem with this conception of science is that it cannot explain why scientists themselves often produce constant conjunctions of events in laboratory experiments – that is, *artificially* – and why scientists have been able to apply knowledge gained from experiments to phenomena outside the laboratory.

Transcendental realism solves this problem by inferring from the success of laboratory experiments the existence of a domain of emergent structures and causal mechanisms.

Although these objects are unobservable they can be known to exist indirectly through the effects they have on observable phenomena. Hence the aim of science is not to search for empirical regularities of the form 'whenever x happens, y happens' but to identify those particular structures and mechanisms which are thought to be responsible for generating such patterns of events. In the natural sciences it is often the case that scientists can identify a particular structure, causal mechanism or power (such as gravity) by isolating it from external influences. When scientists 'close off' a part of reality in this way, the phenomenon observed will indeed be a constant conjunction of events – for example the observation that, when dropped through a vacuum, all objects accelerate at a constant rate and that pure water always boils at 100 degrees Celsius. Hence constant conjunctions of events and states of affairs are only produced and are rarely (if at all) naturally occurring. However, outside the laboratory in the open system that is reality one particular causal mechanism will be operating alongside many other structures and causal mechanisms, whose powers may counteract its own. Hence its effect will hardly ever be manifest as a constant conjunction of events. Scientific laws, therefore, should be regarded not as empirical regularities but as normic statements; that is, statements of the way underlying structures, mechanisms and powers *tend* to operate. For example gravity ensures that leaves fall to the ground but only in the absence of countervailing forces – perhaps thermal currents; and the atomic structure of water ensures that it always boils at 100 degrees Celsius, but not if salt is added to it.

In other words in the open world the domains of the 'empirical', 'actual' and 'real' are usually 'out of phase' with each other. Only in certain conditions – that is, in laboratory experiments – are they brought 'in phase' with each other so that the existence of one particular mechanism lying in the domain of the real can be identified directly with the effects it has on objects in the domains of the actual (where events take place however they are experienced) and the empirical (where actual events are experienced differently). It is of course through applying the knowledge gained from experiments that scientists have contributed to the invention of aeroplanes, nuclear bombs and various other devices.

According to transcendental realism, then, knowledge of reality, contra the claims of positivism, is presupposed by, not given in, experience. The ability of scientists to carry out successfully both theoretical and applied experiments presupposes that reality must be structured, stratified and differentiated; in other words that there exist various strata of unobservable structures, causal mechanisms and powers, which generate patterns of observable events and states of affairs, and which can be isolated, and so differentiated, from each other. Scientific development, therefore, is an ongoing, open-ended process of discovery. Once scientists have identified a particular mechanism operating at one stratum of reality, the existence of that mechanism in itself becomes something for them to explain through investigation of deeper strata of reality. For example in chemistry the theory of atomic number and valency was explained by the theory of electrons and atomic structure, which was in turn explained by theories of sub-atomic structure. However, although this process is cumulative, it is not monistic because knowledge of one stratum may have to be revised in light of new knowledge of the stratum beneath it.

Therefore, a transcendental inquiry into the possibility of scientific experimentation in natural science establishes that the natural world is structured, stratified and differentiated. But does the social world exhibit the same properties? In other words, is a naturalistic social science possible? Now the hermeneutic tradition of social inquiry (including its post-modernist and post-structuralist off-shoots) has always maintained that a naturalistic social science is impossible. Implicitly accepting the positivist account of natural science, it holds that social phenomena are different from natural phenomena in that the former, unlike the latter, depend on people's ideas and discourse. Hence the Humean theory of causation, the linchpin of positivism, is neither a necessary nor sufficient condition for acquiring knowledge of social phenomena. Rather, knowledge of social phenomena can only be acquired through interpreting, and thereby understanding, the meaning of individuals' actions, and through deconstructing individuals' discourse. Causal explanation is only possible in natural science.

However, a transcendental inquiry into the possibility of acquiring knowledge of social phenomena establishes that a *qualified* naturalism is possible after all. Both societies and people possess distinctive causal powers that make it possible for us to know about them indirectly through the effects they have on one another. On the one hand people have the power to reason and to act intentionally, a power which emerges from neurophysiological structures and mechanisms; on the other hand society has the power to influence the way people act, a power which emerges from social structures and causal mechanisms.

What, then, is the relationship between society and people? Bhaskar has specified the relationship between society and people in the form of the 'transformational model of social activity' (1979, 39-47). Bhaskar argues that society is not the product of intentional human action – the error of voluntarism – since society is the necessary condition for it. Thus talking presupposes the existence of grammatical rules, driving the Highway Code, cashing a cheque a banking system, and so on. Hence in drawing on pre-existing social structures people cannot be creating society; rather, they must be either (unconsciously) reproducing or transforming it. But just as society cannot be reduced to the actions of people, so people's actions cannot be reduced to society – the error of determinism. Thus, although the rules of grammar are the pre-condition for talking, they do not determine what people talk about because talking, as a conscious, purposeful human activity, also depends on human agency. In short society and intentional human action presuppose one another as conditions of existence.

Causal explanation in social science is still possible, therefore. Society is the material cause of social activity because it is society that supplies the raw materials for human action to work upon; while human agency is the efficient cause of social activity because it is human agency that makes human actions intentional. Hence, contra the claims of hermeneuticists, people's reasons for acting the way they did can be analysed as causes, and, contra the claims of positivists, people's conceptions of the activities they are involved in provide the starting point for the identification, through conceptual abstraction, of the material causes of – that is, the social structures and mechanisms

enabling and constraining – their activity. Conceptual investigation is necessary in social science because the reality of human consciousness, intentionality and reflexivity means that social systems cannot be closed off by experimentation in the way that natural systems can. Hence, if social reality is inherently open the chief criterion for choice of substantive theory will not be predictive accuracy but relative explanatory power.

### **3. Unifying the social (and natural) sciences**

Both positivism and critical realism, then, hold that the method of inquiry in the natural and social sciences is *essentially* the same (the thesis of naturalism). According to positivism the essence of scientific inquiry lies in the recording of naturally occurring constant conjunctions of events and states of affairs through observation and experience. In addition to a monistic account of scientific development positivism offers a deductivist theory of scientific structure, according to which an event is either explained or predicted by its deduction from a set of empirical regularities, initial conditions, and triggering actions. By contrast for critical realists the essence of scientific inquiry lies in a 'retroductive' movement from the level of events to underlying generative mechanisms and structures; and theoretical explanation involves the postulation of a structure or mechanism, which would account for the phenomenon to be explained, by means of analogical and metaphorical description. However, whereas critical realists realise that differences between natural and social objects mean that the method of inquiry in both sciences will not be *exactly* the same, positivists either ignore such differences – the thesis of scientism – or simply deny their existence – the thesis of reductionism.

Now reductionism and scientism are two highly influential theses in the social sciences. Indeed they underpin the recent tendencies towards 'disciplinary parochialism' and 'disciplinary imperialism' (Sayer 1999, 1). For example, ever since Becker claimed that 'the economic approach is a comprehensive one that is applicable to all human behaviour' (1976, 8) orthodox economists have no longer restricted themselves to the analysis of

rational behaviour inside the market; rather, they have challenged the traditional division of analytical labour by applying rational choice theory to non-market phenomena – that is, to the subject matter of political science, sociology and geography – with the result that new disciplines have emerged – public choice theory, rational choice sociology and geographical economics. Indeed Fine (2003) has identified a new breed of 'economics imperialism' that explains what economists used to consider 'irrational' behaviour, such as social norms and institutions, as the 'rational' response to market imperfections.

But the question remains whether economics imperialism is a desirable way to unify the social sciences. Lawson (1997; 2003) has argued that the essence of modern economics lies in its a priori insistence on the use of deductivist methods of analysis and that these methods are ill suited to the analysis of social objects because their use presupposes that social reality consists of nothing but atomistic events and states of affairs. In other words, a deductivist methodology and an empiricist epistemology presuppose an atomistic, empirical realist ontology. In orthodox economics social atomism finds expression in methodological individualism, the doctrine that social phenomena must be explained by recourse to the preferences of individuals. According to this thesis social structures are simply the voluntary creations of groups of individuals and do not possess distinctive causal powers. The opposing thesis, methodological collectivism, holds that social phenomena must be explained by recourse to social wholes. According to this thesis it is the individual who is the 'puppet' of external, deterministic social structures and who does not possess distinctive causal powers.

Now critical realists argue that both individualism and collectivism are misconceived sociological theses. The transformational model of social activity implies that society should be conceived as a totality of pre-existing *relations* between people (and between people and nature, and people and social products) who occupy various *positions* in society (such as university lecturer) and who, in virtue of their occupancy of these positions, carry out various associated *practices* (such as teaching, researching and examining). These 'positioned-practices', as they are known, may be either internally or

externally related to each other. However, it is only from *internal* relations that social structures and causal mechanisms emerge. Thus, landlords, in virtue of their position in the structure of property relations have the power, *de jure*, to charge tenants rent. Yet the fact that a landlord may be a pensioner and the tenant a student is not essential to the landlord-tenant relation: it is *external* or *contingent*.

A conception of society as highly (but not completely) internally related avoids on the one hand the dangers of reification and determinism associated with social holism, which implies a conception of society as comprising only internal relations, and on the other the dangers of voluntarism and creationism associated with social atomism, which implies a conception of society as comprising only external relations. For it should be clear from what has been said above that people cannot simply 'create' society because society always pre-exists them and provides the conditions for their intentional actions; and society cannot be a 'thing in itself', determining how people act, because society can be only either reproduced or transformed in virtue of the (un)intentional activities of people. Moreover just as society cannot be reduced to the actions of individual people, so it cannot be reduced to their ideas and language – as hermeneuticists have assumed – for, as I mentioned earlier, all social action presupposes as material context.

The upshot of the argument so far, then, is that society is a relational emergent property rooted in, yet irreducible to, human agency. But if society comprises a web of social relations, what are the objects of inquiry of the specialized social sciences? Moreover, are these objects related to each other in such a way that we might view them as in some sense unified? Now I argued above that social structures and mechanisms are the emergent properties of internal social relations, and that it is in virtue of the fact that these properties are causal that they constitute a distinct stratum of reality. But if social structures and mechanisms possess distinctive causal powers that make them possible objects of knowledge, so do natural structures and mechanisms. Hence, if each layer of objects is dealt with by a different science, we can easily see how the stratification of science reflects the stratification of reality; and if the historical order of the development

of science reveals that there is a hierarchy of strata, there must also be a corresponding hierarchy<sup>1</sup> of sciences:

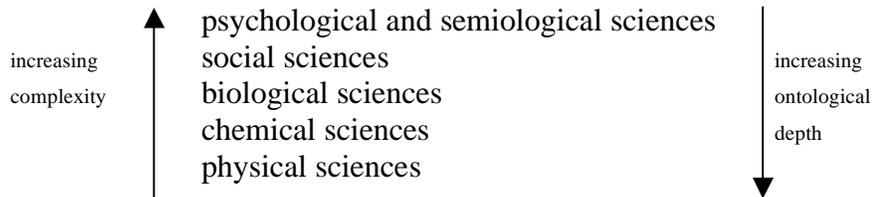


Fig. 1 (Based on Collier 1989, 45)

A movement down the hierarchy corresponds to an increase in ontological depth – that is, to the successive unfolding of deeper layers of reality. Each layer of reality, therefore, is said to be rooted in, emergent from, yet irreducible to the one beneath it. For example social structures and mechanisms are rooted in physiological structures and mechanisms because it is only through the actions of people that society is either reproduced or transformed; and they are emergent properties of human interaction since the causal powers social structures and mechanisms possess are qualitatively different from those that people possess. Hence social activity cannot be predicted from, and so reduced to, knowledge of human behaviour (social atomism); while human behaviour cannot be predicted from, and so reduced to, knowledge of social formations (social holism).

By contrast a movement up the hierarchy corresponds to an increase in complexity, in the sense that deeper strata of reality deal with the less complex and so more 'basic' aspects of reality, such as different types of particles, whereas higher strata deal with more complex and so less basic aspects of reality, such as human consciousness. The layers of reality become more complex as one ascends the hierarchy because more and more

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<sup>1</sup> The exact position of the psychological, semiological and social sciences in the hierarchy is still subject to dispute. If the strata are ordered according to the principles of composition and vertical explanation, the social sciences ought to be placed at the top of the hierarchy. However, if it is accepted that psychological and semiological mechanisms are explained by both biological and social mechanisms, and that psychological, semiological and social mechanisms ontologically presuppose each other, the psychological and semiological sciences ought to be placed either at the top or on the same level as the social sciences (Collier 1994, 130-4). However, this dispute does not affect the argument presented here.

mechanisms come into play. Thus social structures and mechanisms are governed not only by biological structures and mechanisms but also by chemical and physical structures and mechanisms. Moreover, while mechanisms lying at one stratum are *governed* by those lying at all levels below it, mechanisms lying at any one stratum may *affect* those lying at levels both above and below. Thus people may pollute the environment, while hurricanes may stop the production of goods and services (Collier 1989, 48-9; 1994, 45-50, 107-115).

Now figure 1 is a highly simplified representation of the hierarchy of the sciences. One might distinguish further levels of reality and corresponding sciences. For example within the biological sciences one might distinguish between physiology, cell biology, and molecular biology (or biochemistry), each of which deals with a distinct, irreducible stratum. But can one distinguish between distinct, irreducible levels within the social sciences or do social objects exist at only one stratum? The existence of separate social sciences, such as economics, sociology and political science, might suggest that there are indeed distinct domains of 'economic', 'sociological', and 'political' phenomena. Bhaskar himself offers little in the way of clarity on this issue. He argues that 'the predicates "natural", "social", "human", "physical", "chemical", "aerodynamical", "biological", "economic", etc. ought not to be regarded as differentiating distinct kinds of events, but as differentiating distinct kinds of *mechanisms*' (1978: 119). However, the question remains as to whether predicates such as 'social', 'economic' and 'political' refer to distinct layers of reality in the same way that predicates such as 'physical', 'chemical', and 'biological' do. I am not sure that we can regard them as such. For it is not at all clear to me that 'social', 'economic' and 'political' mechanisms constitute distinct, emergent realms of social reality. These mechanisms all share the same basic property – that is, the power to constrain and enable human agency. In other words they refer to different objects that have the same sort of causal power. For example, if we let 'economic' refer to the way in which material needs are provided for in society, 'political' to the differential ability of people to prosecute their interests in society – that is, to power and social conflict – and 'ideological' to the way in which ideas are used in arguments over entitlements to

resources, then we have economic mechanisms such as the production, distribution and exchange of goods and services, political mechanisms such as repression, coercion, domination, and subjugation, and ideological mechanisms such as obfuscation, illusion, and manipulation.

That these different mechanisms are interconnected, in the sense that they ontologically presuppose each other, gives further support to the view that they emerge at the same level of reality. For example the landlord-tenant relation is at once an *economic* relation, because it is concerned with the provision of a particular material need; a *political* relation because changes in tenancy law are the outcome of conflicts between landlords and tenants (and possibly other groups in society); and an *ideological* relation because different ideologies will inform arguments over housing provision. What we have, then, is a set of internal relations, which are themselves internally related to each other, and which we may think of as a 'totality' (Bhaskar 1979, 39, 48, 54-55). Therefore, rather than view the market for rented housing as just an *economic* mechanism, perhaps we should view it as a *social* mechanism, emergent from a combination of different types of internal relation. In short I am suggesting that we define social structures and mechanisms as *spatial-temporal complexes of different types of internal social relation* (since social formations change through time and space).

The answer to the question I set out earlier, therefore, is that the specialized social sciences are not dealing with objects that exist at distinct levels of reality but with objects that exist at the same level. The particular structures and mechanisms that particular social sciences investigate are *synchronically emergent*:<sup>2</sup> that is, they come into being

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<sup>2</sup> One of the reviewers of this paper questioned whether social structures are in fact synchronically emergent by pointing out that capitalism emerged diachronically out of pre-existing social structures. However, recognition of the diachronic aspect of social formations does not, I think, invalidate my argument, which is that social structures and mechanisms are ontologically interdependent – that is, they depend upon each other for their existence – whereas this is not the case for natural structures and mechanisms, since their relations of dependence are one-way not two-way. Thus biological structures and mechanisms depend for their existence on chemical and physical structures and mechanisms but not on social structures and mechanisms. The question of diachronic emergence – that is, how social formations change through time and space – is still important and it is only lack of space that prevents me from

simultaneously at the same stratum of reality. In other words I am arguing that the specialized social sciences do not stand in a vertical relation to each other, as the physical, chemical and biological sciences do, but in a *horizontal*<sup>3</sup> relation.

But if it is the case that different types of social relation are internally related to each other, *sets* of these relations – that is, social structures – may be either internally or externally related to each other. For example there may be an internal relationship between the market for rented housing and the banking system but there may be only an external relationship between the family and the market for rented housing (in the sense that the two structures may affect one another without being dependent on one another for their existence).<sup>4</sup> Indeed the possibility that two or more social structures and mechanisms may be internally related alerts us to the possibility that a new entity possessing irreducible causal powers may emerge. Engholm argues that we should think of this as 'a causal *nexus*, the articulation of a constellational entity, where the various participating mechanisms not only form an emergent force, *sui generis*, but perhaps also are moulded by the very processes of causation' (1999, 26).

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considering it here. Working out how the synchronic and diachronic aspects interrelate is, I think, one of the chief difficulties facing social scientists.

<sup>3</sup> Certain authors have argued that social structures and mechanisms are vertically related in the sense that some provide the foundation for, and so are more basic than, others. Collier, for example, interprets the Marxian base-superstructure relation as an 'instance' of Bhaskar's theory of stratification (1989, 59). He argues that the concept of 'determinance in the last instance' is an example of 'vertical explanation', that is, of the superstructure by the base:

'... at the level of vertical causality (the dependence of one stratum of generative mechanisms on another) it is true that the ideological and political mechanisms are what they are because the economic (and more generally, material) ones are what they are – *and not at all vice versa*' (ibid., 61, italics added).

And he argues that the concept of 'dominance' is an example of 'horizontal explanation', that is, the explanation of concrete events by conjunctures of generative mechanisms:

'... at the level of horizontal causality (the production of events as a result of a prior operation upon a pre-existing complex of generative mechanisms), generative mechanisms of any stratum may play their part, and no one can say in advance what the relative weight of those various parts might be' (ibid., 60-61).

While I agree with Collier's interpretation of 'dominance' I am not convinced by his interpretation of 'determinance in the last instance' because in my view base and superstructural relations require each other as conditions of existence. I provide a full justification for this view in chapter 5 of my PhD thesis (forthcoming).

<sup>4</sup> Whether or not there is either an internal or external relation between structures is of course a matter for empirical investigation.

It follows from this that predicates such as 'economic', 'political', 'legal' and 'ideological', as well as referring to different types of social structure and causal mechanism, should refer to different dimensions or *aspects* of social activity. Lawson, for example, has argued that the 'economic' is just one aspect of social activity:

I cannot think of a single sphere of human activity – from lending support to a football team, to listening to music, or even to making love – that does not (or could not) have an economic aspect... These and all other activities take place in space and time, both of which can have alternative uses. All human activities require material conditions... But at the same time very few activities, if any, have *merely* an economic aspect...' (2003, 162).

Hay, too, describes the 'political' as another aspect of social activity:

'Though all social relations may also be political relations, this does not imply that they are only political relations, nor that they can be adequately understood in such terms... The political is perhaps best seen as an aspect or moment of the social, articulated with other moments (such as the economic or the cultural). Though politics may be everywhere, nothing is exhaustively political' (2002, 256-7).

Moreover, if concrete events and states of affairs in open systems are generated by both social/psychological and natural structures and mechanisms, the term 'society' should take on a new meaning. As Benton and Craib put it,

'society cannot reasonably be represented as a single level in the hierarchy. Rather it is a heterogeneous complex of mechanisms drawn from several of the other levels: psychological, physiological/anatomical, ecological, chemical and so on' (2001, 128).

Clearly, then, we need a science that offers an understanding of social reality as a dynamic, organic whole, composed of configurations of different causal mechanisms and structures. Bhaskar's opinion is that this science can be either Marxism or sociology, both

of which take 'particular historically situated social forms' as explananda and '*relations of production* (of various kinds)' as explanans, and both of which therefore require as conditions for their possibility 'the special sciences and history' (1979, 56). But if we are to understand how configurations of natural and social mechanisms emerge and change through time, we need first to have identified those mechanisms individually. In short we need both specialization *and* unification in science to understand social reality. As Bhaskar himself puts it: 'if Marxism without detailed social scientific and historical work is empty, then such work without Marxism (or some such theory) is blind' (ibid.); and it is because Marxism tries to understand social reality as a dynamic, organic whole, that it cannot claim any one of the specialized social sciences as its disciplinary home.

#### **4. Interdisciplinarity and social science**

So far I have argued that, if social reality is unified in the sense of being interconnected, so are the social sciences, and that if social and natural phenomena are interconnected, so are the social and natural sciences. It is clearly desirable, therefore, that scientific practice should take account of these interconnections; that, in addition to specialized sciences concerned with understanding the operation of particular structures and mechanisms, there ought to be a 'totalising' science capable of expressing the idea that a concrete event in an open system is determined by a 'conjuncture' of structures and causal mechanisms (Bhaskar 1986, 107-111).

However, the question remains whether a 'totalising' science is feasible in practice. As mentioned above Bhaskar's view is that Marxism and sociology are both contenders for the role of understanding social life as a totality. I do not have the space here to examine the capability of Marxism and sociology to fulfil this role, except to say that, at the moment at least, neither Marxism nor sociology seems likely to take on the role of synthesizing the analytical results of the specialized sciences. Marxism is still lumbered with the charge of economic determinism while sociology continues to fragment into sub-

disciplines to such an extent that many commentators claim that sociology is now suffering from a 'crisis of identity' (Turner 1991; Crane & Small 1992). In my view this has always been so, for, right from its inception, sociology has tried and failed to claim for itself the study of society as a whole. In the late 19<sup>th</sup> century sociology had to swim against the tide of specialization sweeping across the social sciences. Once economics became the science of the market and political science the science of government, sociology became a 'leftover science', forced to study those aspects of society which economics and political science would not touch (Swedberg 1990, 11). It is true that the traditional division of analytical labour in social science is now changing (Ingham 1996). In response to the imperialism of economics, for example, sociology, political science and geography are moving into economics' traditional disciplinary territory. But these 'cross-disciplinary' approaches have not led to any genuine synthesis of knowledge; rather, they have led simply to the emergence of more sub-disciplines – the new economic sociology, the new political economy and the new economic geography (Swedberg 1987; Gamble 1995; Martin 2003).

Now it is arguable that one of the (rarely considered) causes of the fragmentation of social science is the fact that social scientists, even those within the same discipline, are committed to different approaches to social inquiry. For example, within political science one finds a range of different approaches to inquiry – behaviouralism, rational choice, institutionalism (old and new), interpretivism, Marxism, among others – underpinned by different philosophies of science (Marsh & Stoker 2002). In economics, too, a division has opened up between a 'mainstream' or orthodox core, which consists broadly of various schools of neo-classical thought, and a 'non-mainstream' or heterodox periphery, which consists of various schools of thought, such as institutionalism, Post-Keynesianism and Marxism, critical of the positivist assumptions underpinning the neo-classical approach (Harley & Lee 1997, 1431, fn. 4).

Now if philosophical divisions do indeed characterize the social sciences, what are the implications of this for interdisciplinary research? Let me first discuss what

interdisciplinary research is. This mode of knowledge production is most often understood, I think, as the attempt to combine or unify the methods and/or concepts of different academic disciplines that are all thought to have a bearing on a concrete phenomenon of interest. For example Berger defines 'interdisciplinary' as

[a]n adjective describing the *interaction* among two or more different disciplines. This interaction may range from simple communication of ideas to the mutual integration of organising *concepts, methodology, procedures, epistemology, terminology, data*, and organisation of research and education in a fairly large field' (1972, 25-6).

Moreover most commentators on interdisciplinary research usually have a particular form of disciplinary interaction in mind. As Cliff puts it:

Interdisciplinary research is defined as joint, coordinated and continuously integrated research done by experts with distinctly different disciplinary backgrounds producing joint "staff authored" reports. It differs from multidisciplinary research where experts from different disciplines work individually on different aspects of a specific problem and produce separate reports which may be published individually or as a collection' (1974, cited in Hickman 1980, 49).

In short interdisciplinary research is usually understood as a *collective* enterprise, in which researchers from different disciplines work together on a common subject, and from which will emerge an overarching theoretical framework that is more than just the sum of the contributing disciplinary perspectives – what Jantsch refers to as a 'common axiomatics' (1970: 411).

From the perspective of critical realism this understanding of interdisciplinarity as the unification of methods and/or concepts is problematic. Which disciplines will be required to explain a particular concrete phenomenon will depend on which causal mechanisms are thought to be generating it. Danermark gives the example of 'noise-induced hearing

impairment', a phenomenon generated by biological mechanisms (affecting the person's ability to hear), psychological mechanisms (affecting the person's experience of the hearing impairment) and social-cultural mechanisms (affecting the way deaf people are received by society) (2002, 57-8). Now if disability is a phenomenon caused by mechanisms operating at different levels of reality, integration through unification of method will not be possible because, as figure 1 showed, different levels in the hierarchy imply different degrees of complexity and ontological depth. Hence, while it may be possible for the biologist to identify and explain the mechanisms causing impaired hearing by means of experimentation, it may be impossible for the social scientist to identify and explain the relevant social mechanisms in the same way. Moreover if the nature of the mechanisms involved is different, the concepts devised to describe them will also have to be different, making integration through unification of concepts impossible.

The goal of conceptual unification, which most proponents of interdisciplinary research seem to have in mind, derives, I suspect, from the influence of physicalism – the thesis that the laws of the sciences can be reduced to the laws of physics (Oppenheim & Putnam 1958; Causey 1977). I am arguing in this paper that an understanding of unification as involving reduction should be replaced by an understanding of unification as interconnection. In other words unification in science should be understood as the attempt to explicate how mechanisms lying at different levels of reality interact to produce different concrete outcomes. Thus whether or not a hearing impairment caused by biological mechanisms will result in a loss of 'function', such as the ability to communicate with others, will depend on psychological mechanisms affecting a person's ability to lip read; and whether or not it will translate into a 'disability' will depend on social mechanisms affecting how non-hearing-impaired people treat deaf people. Thus if deaf people are stigmatised by society, they will be disabled whether or not they are provided with a hearing aid and can lip read (Danermark 2002, 61-2).

The idea of unification as interconnection is, I think, what Danermark means by the 'integration of knowledge' (ibid., 61). Thus Danermark states that

'a genuine integration of knowledge requires close collaboration with researchers from different disciplines. Basic knowledge about other disciplines or areas of knowledge involved in interdisciplinary research is of utmost importance. The reason for this is that, in order to understand what is happening at one level, one needs to have insight into how mechanisms working at other levels might influence the outcome...' (ibid., 61).

But will integration be possible if the researchers have different views about the nature of science and of reality? In an earlier passage Danermark does mention this situation:

'One common consequence, when researchers from different traditions and specialities gather in a scientific milieu, is that people with different, sometimes very different, perspectives on reality meet. In other words, very often they have different ontological perspectives' (2002, 56).

Danermark concludes that in such situations discussion of philosophical issues 'is both necessary and fruitful', and that the discussion should be conducted 'in a respectful manner and with tolerance for different ontological, epistemological and methodological perspectives' (ibid.). Now if, as a result of such a discussion, scientists came to an agreement about the constitution of reality, integration of knowledge would indeed be possible. For example, if a team of scientists investigating disability agreed that reality was structured, stratified and differentiated, it would be possible to integrate the analytical results of their investigations – in other words to show how the relevant structures and causal mechanisms interconnect – because the knowledge they produce would have the same status and validity. But it would surely be much more difficult to integrate the findings of a social scientist committed to an empirical realist perspective with the findings of a biologist committed to a depth realist perspective of the sort advocated here because, unlike the depth realist, the empirical realist could never accept the reality, and so causal efficacy, of unobservable entities. Similarly it would be difficult

to integrate the findings of a positivist social scientist with those of, say, a post-modernist. At best all that could be hoped for in such situations would be a juxtaposition of different analytical perspectives: in other words a multidisciplinary approach.

In short I am arguing that one of the conditions for the integration of knowledge is philosophical agreement about the nature of reality and of science. This does not mean that scientists have to agree to use exactly the same methods of investigation because, as I argued earlier, the methods used (and concepts devised) to explain a particular level of reality will be specific to that level. It is always the *nature* of the objects to be investigated that determines the choice of method.

However, one question that arises from a consideration of the philosophical conditions for the integration of knowledge is which philosophical perspective should be the common point of departure. For example an interdisciplinary research project might just as well be grounded in a positivist approach as in an interpretivist one. However, it is my view that only a critical realist perspective can provide a sensible and coherent grounding for interdisciplinary research because only critical realism can provide a convincing rationale for the need for specialization and integration in science. Positivism cannot explain convincingly why specialization should be necessary, for if the objects of scientific inquiry are simply empirical events, how are we to differentiate them whilst at the same time making sense of the *existing* differentiation of science? Differentiation can only be conceived as an arbitrary or conventional affair. The result of this, as Bhaskar puts it, is 'a crisis of definitions and boundaries' (1979, 62) – a crisis bound up with what was described earlier as 'economics imperialism'. For if the scope and boundaries of economics are the result of convention, what is to stop economics imperialists from challenging the conventional division of intellectual labour between economics and political science? If orthodox economists assume that 'economic man' is rational, why should they not also assume that 'political man' is rational? A similar argument applies to the interpretivist approach, for if social reality is constructed out of people's ideas and/or discourse, the objects of science will be social constructions. Hence their differentiation,

too, has to be explained as a result of convention and tradition, and is therefore subject to arbitrary change. If, by contrast, the objects of science refer to transfactually operative structures and mechanisms, which exist independently of their discovery, the differentiation of the sciences can be understood as a reflection of the differentiation of reality.

But if the possibility of integrating knowledge requires a facilitative intellectual/philosophical context, what are the social conditions that make possible the integration of knowledge? I do not have the space here to provide a comprehensive theory of the material context of knowledge production.<sup>5</sup> However, examples of the sorts of social relations involved in it will be those between lecturers and students, examiners and examinees, researchers and referees, researchers and directors of research, and between lecturers, researchers and students (since teaching and learning presuppose the existence of something to be taught and learnt, viz., knowledge, which researchers provide). In virtue of their occupancy of these positions – and it is of course possible to occupy more than one position at the same time – individuals will be engaged in a variety of material practices, such as lecturing, tutoring, learning, examining, refereeing, chairing committees and so on, so that we have what Bhaskar calls a 'position-practice system' (1979, 51). Now the tasks, duties, rights etc. associated with each position may be codified as formal rules in, say, a contract of employment, or exist informally as tacit norms. Thus the law obliges lecturers to carry out certain duties as defined in a contract of employment. But they are also aware, for example, that their professional status and career progression will normally depend on the establishment of a publication (and perhaps funding) record. Similarly, they also know that it is normal to be appointed to a lectureship only when one has obtained, or is about to obtain, a suitable research qualification, such as a doctorate.

Considered as a whole the sorts of relations and positioned-practices I have just discussed make up institutions – that is, universities – which are themselves related to other

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<sup>5</sup> Interested readers may wish to consult chapter 3 of my PhD thesis (forthcoming).

institutions, such as industry and the state (which provide the monetary resources for research and teaching). Now certain social mechanisms, emergent from conjunctures of these particular relations, will be directly implicated in knowledge production: for example peer review, which defines what a community of researchers will accept as valid knowledge; publication, which makes possible the transmission of new knowledge throughout the research community; and funding, which makes possible both the production of new knowledge (that is, research) and the transmission of existing knowledge (that is, teaching). These mechanisms are interlinked. Thus only research that has been peer reviewed and accepted by the community of researchers to which it is addressed will be published, while funding for research is allocated by the Higher Education Funding Councils on the basis of the results of the Research Assessment Exercise, which provides an external review of the quality of research.

For the integration of knowledge to be possible, therefore, the social context of knowledge production must facilitate interdisciplinary research by rewarding and so validating it. However, I think that the existing social context acts more to constrain than to facilitate integrative modes of knowledge production. The problem is that subject areas have become institutionalised: that is, the intellectual consensus defining the scope and boundaries of each subject area is reproduced, and so reinforced, socially. For example in most universities students internalise the norms and standards of one discipline (or two, if they are taking a dual honours degree). Once they reach research level these norms and standards will have become habitual ways of thinking so that they may find it difficult to think beyond the traditional intellectual territory of their discipline. Boundaries between subject areas are also reinforced by the existence of disciplinary journals, professional associations, and the Research Assessment Exercise, which reproduces intellectual divisions by defining units of assessment.

Now if the social (and intellectual) context of knowledge production is structured in this way such that subject specialization is rewarded at the expense of integration, it will be difficult for interdisciplinary modes of investigation to survive. This is not to say that

intellectual collaboration will never be attempted; rather, it is to say that it is unlikely. If it is expected that lecturers will be subject specialists, that they will research in, and teach, a specialty, lecturers who challenge these disciplinary norms may find it more difficult to publish interdisciplinary research, and they may find it more difficult to win support from colleagues and a head of department to teach an interdisciplinary course. In short lecturers committed to an interdisciplinary approach may find their career prospects diminished. As Milward and Kennedy put it:

'The university teacher judges his expertise and receives his esteem and rewards for the most part within the framework of one subject. His courses and examinations belong to the traditions of that subject, his publications are judged by other teachers in that subject, he attends its annual conference and, if he is successful, he is promoted through a small and fairly familiar peer group to a chair from which he continues to organise the teaching of the same subject. There are great penalties attached to breaking out of this cocoon into an insecure world of fewer peers, fewer conferences and fewer senior posts and the best and most confident of teachers is quite justified in looking very hard at what sort of prospects the system offers him if he at once casts aside his subject label' (cited in Squires et al. 1975, 23).

However, the possibility of publishing interdisciplinary work will vary between disciplines since some disciplines, such as political science and sociology, are more open to alternative forms of knowledge than others, such as economics. For example the journal *New Political Economy* was established in the 1990s with the aim of facilitating the (re)emerging interactions between economics and political science and, to a lesser extent, sociology (Gamble et al. 1996). Now while many political scientists specializing in, say, the political economy of development or international political economy will be happy to publish in a journal of this sort, I am not so sure that young, ambitious orthodox economists will want to publish in a journal that was not part of the 'Diamond List' of core, mainstream economics journals (Diamond 1989). This list, and various modified versions of it, has come to be regarded by the mainstream of the economics profession as

an informal indicator of the quality of research in economics. Thus research published in one of the listed journals, say, *The Economic Journal*, will be regarded by mainstream economists as superior to that published in non-listed, non-mainstream journals, such as *The Cambridge Journal of Economics*. Now if it is believed that RAE economics panel assessors use such lists unofficially to inform the judgements they make of an economics department's overall research quality, any head of department who wished to obtain a higher research rating is unlikely to want to appoint an economist who tends to publish interdisciplinary work in non-mainstream journals (Harley & Lee 1998, 24).

However, I am not claiming that mainstream economists do not engage in interdisciplinary work: as I mentioned earlier, orthodox economists have applied deductivist methods of analysis to subject areas traditionally covered by political science and sociology. Mainstream journals will regard work of this sort – that is, formalistic modelling – as valid knowledge. But they will not regard non-formalistic interdisciplinary work as valid and will most likely reject its publication. Now if mainstream economists regard interdisciplinary work as simply an extension of deductivism to other disciplines it is difficult to see how their analyses could be integrated with those of other social scientists, and indeed natural scientists, whose methods of analysis presupposed a conception of science entirely at odds with that presupposed by orthodox economics. This might seem a strange claim to make when most mainstream economists will claim to be following the methods of the natural sciences – particularly physics. Yet it makes sense once it is remembered that positivism, as an account of scientific development, is false.

But if the mainstream core of the economics discipline will be closed to interdisciplinary work underpinned by a critical realist philosophy, political science may be more open to it because, as I mentioned earlier on, there exists a range of different approaches to social inquiry co-existing within the discipline. Thus it would be quite possible to publish research underpinned by a critical realist approach and spanning the domains of economics, political science and law in political science journals because political science

is not dominated by a particular conception of science.<sup>6</sup> Indeed the fact that political science is grounded in so many different disciplines – law, history, sociology, psychology and economics – gives it an inherently interdisciplinary outlook. What this means, I think, is that although political scientists, *qua* political scientists, will focus their attention on the political aspects of social life, they are more likely than orthodox economists to be sensitive to the context in which the political operates; that is, to the connections between the different types of social structure and causal mechanism I discussed in the previous section of this paper.

Given the way the social and intellectual context of knowledge production is currently structured, it is unsurprising that attempts to integrate knowledge in the social sciences through interdisciplinary research have not been as successful as originally envisaged. In the post-war era institutions of higher education throughout the West have established research institutes and centres explicitly designed to encourage interdisciplinary research (Ikenberry & Friedman 1972). Little is known about how these institutions operate and, in particular, about the degree of integration they make possible. However, Rhoten recently investigated the operation of six interdisciplinary research centres in the United States. Significantly she found that the research networks in these centres (which dealt with both the natural and social sciences) appeared to be 'more multidisciplinary than interdisciplinary', so that there was 'more of an inclusion than an integration, of different disciplines'. She also found that in certain cases there were 'clear divisions between represented disciplines and distinct clusters of monodisciplinary relations' and that, overall, there tended to be more ' "information sharing" ' than ' "knowledge creating" ' collaborations (2003, 5-6). Rhoten's findings are supported by Birnbaum's factor analysis of eighty-four interdisciplinary research projects. Birnbaum looked at how project

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<sup>6</sup> For example the Political Economy Research Centre at the University of Sheffield organized a research project on the political economy of the company in the late 1990s. The researchers involved in this project had been trained in political science, law and economics. The research output was published as jointly authored book chapters and articles in political science and law journals. Only one article arising from the project was published in a non-mainstream economics journal, under the sole authorship of the single, non-mainstream economist involved in the project. I provide a full account of the nature of the project and the output arising from it in chapter 4 of my PhD thesis (forthcoming).

performance, frequency of integrating devices, time spent by project leaders on administration and planning, and extent of interdisciplinary collaboration related to three different academic research context: 'permanent institutes', 'adaptive institutes' and 'independent projects'. He concluded that overall interdisciplinary research institutes had little effect on the extent of interdisciplinary collaboration and the management of interdisciplinary research projects. As he put it:

'Permanent institutes do seem to facilitate interdisciplinary research but adaptive institute projects do not differ significantly from independent projects. Compared with permanent institute projects, independent projects were not found to differ with regard to performance, interdisciplinary collaboration, or the time spent by principal investigators in assembling resources and planning. This is a surprising finding given the argument that institutes should facilitate interdisciplinary research. The only significant contribution made by institutes was found to be the number of integrating devices provided' (1978, 94).

Simply moving academics out of a subject-based department and into a new building, then, will not change the prevailing social and intellectual context of knowledge production. If this context continues to constrain integration more than it facilitates specialization, bringing researchers from different disciplines together may well provide more opportunities for interdisciplinary work but it will not necessarily lead to serious attempts at integration. In any case even if researchers were committed to integrating knowledge, methodological, epistemological and ontological conflicts might still be an obstacle to intellectual synthesis. Sayer's vision of 'post-disciplinary studies', therefore, looks to be a distant prospect (1999, 5).

## **5. Conclusions**

This paper set out to address two questions: whether or not it is desirable, and whether or not it is feasible to unify the social (and natural) sciences. My answer to the first question is that the unification of the sciences is desirable. I argued that reality is structured,

stratified and differentiated: that is, that it consists of a hierarchy of different structures and causal mechanisms, emergent at different levels of reality, some of which may be isolated from the others in laboratory experiments. I also argued that whereas the natural structures and mechanisms emerge at different levels of reality, social structures and mechanisms emerge at the same level. Hence the differentiation of the natural sciences reflects the differentiation of objects *between* strata whereas the differentiation of the social sciences reflects the differentiation of objects *within* a single stratum. Now if any concrete phenomenon in an open system is generated by conjunctures of structures and mechanisms, whether natural or social, it makes sense to explain that phenomenon, not through knowledge produced by one particular science but by knowledge produced by all sciences that have a bearing on it. The unification of science, therefore, should be understood as the integration of disciplinary knowledge – that is, as the explication of the way different types of structure and causal mechanism interact.

My answer to the second question is that the unification of the sciences is feasible only if certain philosophical and social conditions are satisfied. I argued that the integration of knowledge, understood as the attempt to understand how reality is interconnected, would only be possible if scientists agree that reality is structured, stratified and differentiated – that is, if scientists share a critical realist perspective on science and reality. However, I argued that even if this philosophical condition is met, the social context of knowledge production might still constrain intellectual collaboration by encouraging specialization at the expense of integration. A transformation of the social context of knowledge production will be required therefore to facilitate both specialization and integration in science, for both modes of scientific inquiry are necessary to explain reality.

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