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Unity in social science?

What do we mean by unity in social science? Which types of unity exist and in which ways can they be achieved, if at all? Is the process of unification likely to increase the understanding provided by individual sciences? Is unification an unattainable ideal, which is nevertheless useful as a heuristic guide to social scientific research? Is it a necessary ingredient of scientific theorising, or, on the contrary, does it constitute a danger to the diversity of approaches and methodologies characterising the social sciences?

The present issue - the first of a series of thematic issues that *GJSS* plans to publish once a year – reflects on these questions through a wide spectrum of approaches and expertises. The authors and editors of these papers came together in a workshop titled 'Unification in the Social Sciences?' which was held at the London School of Economics and Political Science on the 24th and 25th of September 2004. We are grateful to Max Steuer, Hans Radder and Peter Abel, who encouraged this work both intellectually, in their role of invited speakers, and practically, by helping us in organising the event. Several participants and commentators, including several members of the *GJSS* board, also contributed to making the workshop into a stimulating venue for discussing this theme and for providing feedback and further inspiration to the contributors to this issue.

An almost obvious sense in which unification is needed within social science emerges as soon as one considers interdisciplinary research. This is because the integration of approaches coming from different disciplines (which we take to broadly characterise all types of interdisciplinary projects) requires what might be called *small-scale unification*, so as to create a coherent approach towards the analysis of a specific issue. In this relatively unambitious, yet important sense, unification is a necessity within the social sciences. There is also, however, *large-scale unification*, which takes inspiration from the abstract laws that are said to characterise the physical sciences. Within this view, all social science should or could, irrespectively of the specific context or issues at hand, be unified by reference to a general theory or methodology.

Typically, a variety of disciplinary approaches and themes characterises the papers within this issue. Dominic Holland interprets the *ontological stance* put forward by Roy Bhaskar, together with other representatives of the philosophical school known as Critical Realism, as constituting a promising, non-reductive foundation for unification in both the social and the natural sciences. A response by Jeroen van Bouwel highlights some problems posed by critical realist methods and theory, thus raising interesting questions concerning the feasibility of Holland's project. A different proposal comes from Rasmus Winther's sophisticated comparison between biological and sociological forms of reasoning and theorising. Winther recognises different *styles* of pursuing scientific research, which he posits as crucial elements to be considered within a unification project. Marcel Scheele and André van Dokkum are less interested in methodological issues than Winther. They propose, each in his distinctive way, to emphasise the role of *concepts* and conceptual distinctions (such as the ones made within analytic philosophy). Scheele demonstrates how unification can be achieved by a conceptual analysis of the notion of institution as it is used across the social sciences, from sociology to law. Van Dokkum turns instead to theories about *types* and hierarchical levels in science, as put forward by both cultural anthropology Gregory Bateson and a number of analytic philosophers. In the concluding piece, Peter Caws traces the roots and basic motivations for scientists' attraction towards unifying strategies. His account points to the *human sciences*, rather than the natural sciences, as the most credible foundation for pursuing unity within social scientific theories, while preserving their disciplinary pluralism.

Given such diversity, it is perhaps surprising to acknowledge that all the contributors to this issue are interested in the second type of unification, which we defined as large-scale unification. This preference is even more telling when considering that the original title of the London workshop, which we left deliberately vague, encouraged both negative and positive answers to questions about the feasibility and fruitfulness of unification. Through the arguments contained in this collection, two points are immediately made clear to the reader. The first is that large-scale unification holds a firm grip on the scientist's imagination, despite the success of decidedly anti-unificationist approaches such as postmodernism, post-structuralism and constructivism (which incidentally are not discussed by any of the papers, not even by Holland, despite the critical post-positivist potential of Critical Realism). The second point, made especially clear by Caws and Winther, is that there are reasons for this fascination. No matter how sceptical one can be about the actual prospects for unification, its attractiveness as an ideal remains strong. The possibility of unity among different disciplines is strongly associated by many social scientists to the possibility of increasing the scientific credibility of social science as a whole, especially in the face of the so-called 'hard', natural sciences. Further, unification strategies are tempting from the pragmatic point of view: what could be more efficient than finding laws, or concepts, or methodologies that hold throughout all of social science and allow for sweeping generalisations to be applied in all domains? And indeed, there are cases in which the focus on unity makes it possible to summarise and generalise the theoretical and normative implications of a series of specific approaches; in which it allows to apply ideas developed within a single discipline to one or several others, sometimes with very fruitful results; and provides tools for insightful critique.

All these pro-unity considerations stem from a meta-scientific perspective and are formulated as such. It is evident that most papers presented here are thoroughly inspired by analytic philosophy and/or philosophy of science. André van Dokkum discusses explicitly the advantages to be gained by exploiting philosophical views within social science, while Marcel Scheele provides a demonstration of this argument for the study of institutions from the legal, social, political and economic perspectives. The adherence to an analytic approach comes, however, with certain assumptions that we would like to briefly discuss here, especially with an eye to implications for the practice of the sciences in question. One such assumption concerns the nature of concepts as something that can be fixed and clarified – either by unifying the social sciences through a process of increasing generalization (in which more particular meanings are 'shedded' and we are left with a core, as in Scheele) or by the transfer of concepts from one discipline into another (in the example given by Winther, from biology to the social sciences). The possibility that conceptual transfer between disciplines has a metaphorical quality – that the meaning of the concept is necessarily adapted to the disciplinary context within which it is being used – remains unexplored. Instead, there is a great trust that the transfer of concepts in themselves makes a given body of work interdisciplinary – that the concepts somehow transform their host discipline rather than the other way round.

This, in itself, is an interesting stance, and one that is not at all self-evident for someone working on actual empirical research. It often happens that the meaning of concepts cannot be stabilized, but changes according to the context in which they are being used. If this is the case, a researcher that borrows concepts from other disciplines cannot but adapt them to the disciplinary framework that he is socialized into, thus changing their meaning and practical applicability. The occurrence of this process, as well as its significance with respect to unification methods, is especially evident within issue-based interdisciplinary research, much of which employs a variety of different approaches and perspectives in order to explore a topic in as many ways as possible. Here, unification is made difficult both by the practical difficulty of transferring concepts between disciplines and by attempts to unify them while making them 'rich' enough to be applied in empirical research (as opposed to the philosophical exercise of upward abstraction that is rather uncritically supported within many of these papers). Such research does not necessarily require large-scale unification. It might be argued that the fact that, within certain contexts, the full understanding of a social phenomenon may well require more than one approach, more than one way of formulating questions and viewing social ontology. In some cases, a persistent differentiation between different modes of social enquiry may be what makes interdisciplinary research potentially so rewarding. This certainly points towards a more collective model of research. Attempts at large-scale unification, be it the subsumption under one broad understanding of social ontology as proposed by critical realism according to Holland, or the further abstraction and generalization of concepts as proposed by Scheele, may not always necessarily bring insight.

This last point is taken up within Van Bouwel's discussion of Holland and we believe it to constitute an useful lense through which to read this whole issue. These papers present strong arguments for the usefulness of unification within certain contexts and point to interesting ways of pursuing this ideal, each of which deserves full consideration within social scientific practice. Are there contexts, however, in which unification becomes a sterile, even misleading ideal? We hope this issue to become a stimulating source of reflection on this issue and many more. If interested in responding to any of these papers, or in sending a relevant contribution, please contact us.

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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[1] of sciences:

increasing complexity ↑	psychological and semiological sciences social sciences biological sciences chemical sciences physical sciences	↓ increasing ontological depth

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

[1] 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 *ideologica*l 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*:[2] that is, they come into being

[2] 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*[3] 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).[4] 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.

[3] 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 convinved 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).

[4] 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 19th 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.[5] 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

[5] 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.[6] 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

[6] 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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The devision of labour in the social sciences versus the politics of metaphysics

Questioning critical realism's interdisciplinarity

Abstract

Some scholars claim that Critical Realism promises well for the unification of the social sciences, e.g., Unifying social science: A critical realist approach *in this volume. I will first show briefly how Critical Realism might unify social science. Secondly, I focus on the relation between the ontology and methodology of Critical Realism, and unveil the politics of metaphysics. Subsequently, it is argued that the division of labour between social scientific disciplines should not be metaphysics-driven, but rather question-driven. In conclusion, I will therefore defend a question-driven pluralism as a guide for interdisciplinarity.*

1. Unifying social science from a critical realist perspective

The Critical Realist perspective was born out of a vigorous critique on the positivist conception of science. It pleads for the reorientation of social science, unveiling the *epistemic fallacy* committed by positivists. This is the fallacy that transposes what is an ontological matter into an epistemological matter; a failure to adequately sustain the distinction between ontology and epistemology, resulting in the relative neglect of ontology. The *positivist* social scientist analyses statements about being solely in terms of statements about knowledge, and thus reduce ontology to epistemology. Therefore, as a reaction to this neglect, it is 'opportune to develop a perspective on the way that social reality is' (Lawson 1997, 154).

Hence, after the unveiling of the epistemic fallacy, the focus should be replaced on ontology. Central in the focus on social ontology, then, figures the transcendental argument for social structures, elaborated by Roy Bhaskar. He derives an account of a metaphysics of science by enquiring what the world must be like before it is investigated by science, and for scientific activities to be possible. Bhaskar's transcendental realism defends the existence of social structures and society as follows:

> '(…) conscious human activity, consists in work on *given* objects and cannot be conceived as occurring in their absence. A moment's reflection shows why this must be so. For all activity presupposes the prior existence of social forms. Thus consider *saying*, *making* and *doing* as characteristic modalities of human agency. People cannot communicate except by utilizing existing media, produce except by applying themselves to materials which are already formed, or act save in some other context. Speech requires language; making materials; actions conditions; agency resources; activity rules. Even spontaneity has as its necessary condition the pre-existence of a social form with (or by means of) which the spontaneous act is performed. Thus if [as previously argued] the social cannot be reduced to (and is not the product of) the individual, it is equally clear that society is a necessary condition for any intentional human act at all' (Bhaskar 1979, 34).

This argument is used to establish that 'the social cannot be reduced to (and is not the product of) the individual, it is equally clear that society is a necessary condition for any intentional human act at all' (Bhaskar 1979, 34). Bhaskar had formulated an argument about underlying mechanisms and structures in the *natural* sciences as well. This argument could, however, not be directly transferred from the natural sciences (in which scientists are able to acquire knowledge of underlying mechanisms at work via experimentation) to the social realm; some substantial modifications were necessary, as experimentation in social sciences is rare. So the 'proof' of the existence of structures in the social realm provided by the transcendental argument for social structure, is not analogous to the argument from experiments in the natural sciences. The argument from experiments starts from a widely accepted and successful practice (or method of investigation), while the argument for social structures starts from a (folk) social theory.

Nevertheless, Bhaskar's ontological framework (including, e.g., the *Transformation Model of Social Activity*) becomes a distinctive feature of the Critical Realist contributions to social science. Using a common (unified) ontological view of social reality across the different social sciences, the Critical Realist contributions promise the unification of the social sciences.[1] The growing amount of social scientific literature employing Critical Realism, e.g., the work of Margaret Archer in sociology, Tony Lawson in economics, Heikki Patomäki in political science, etc., might strengthen Critical Realists in their conviction that social science moves towards unification and its optimal state. I will, however, argue in this article that a unification using Critical Realism would lead the social sciences to a suboptimal state.

2. The politics of metaphysics

The Critical Realist unification starts with Bhaskar's *a priori* or necessary truth concerning social ontology based on a (questionable) transcendental argument; the existence of social structures is based on a transcendental derivation as quoted above (rather than on a careful mix of observation and induction, starting from empirical research and taking into account social scientific practice).[2] I do not want to argue that social structures (or other ontological aspects of the Critical Realist's stance) do not exist, but that the way it has been defended by Bhaskar and Critical Realism in general, is problematic, just as it has been problematic in earlier attempts to impose preconceived ontological ideas in the (philosophy of the) social sciences (e.g., Watkins 1973). The attempt to justify the claim that the world has indeed the form argued for in transcendental realism does not convince (or, better, does not convince me more than other stands in the unending battle of metaphysical intuitions we experience in the philosophy of the social sciences). Moreover, as the ontological choice made by Critical

[1] This point was, i.a., made by Christopher Lloyd (1993, 195) and another version of it can be found in this volume, in the paper *Unifying Social Science: A Critical Realist Approach.*

[2] Bhaskar's transcendental argument has been criticized by, i.a., Boylan & O'Gorman 1995; Cottrell 1998; Parsons 1999.

Realism does have an impact on methodological options, I want to warn for an *ontological fallacy*: taking an *a priori* ontological stance that transposes or reduces epistemological and methodological matters into an ontological matter. Analogous to the *epistemic fallacy* it points at a failure to sustain adequately the distinction between ontology and epistemology, that is, a failure to deal with both ontology and epistemology in a non-reductive way.[3]

Whichever starting point we prefer in studying the social world, we will always adopt some ontological assumptions (it is unavoidable and necessary). With Critical Realist applications however, the ontological assumptions are 'proven' to be true *a priori*, and this raises serious doubts on whether they will at all be revised.[4] Secondly, starting from the Critical Realist ontology has some methodological consequences that are insufficiently spelled out. The methodological consequences of Critical Realist's ontology seem to follow 'automatically', and hence do not have to be spelled out. There is a lack of attention paid to the form of explanations and to methodology in general. Margaret Archer (1995, 159), for instance, couples her ontological realism with a methodological realism, but does hardly develop this last one.

If one is convinced that the relation between the individual and the structure is correctly described by (a version of) the Transformational Model of Social Activity (TMSA), one will not consider explanatory theories that are not in line with TMSA, e.g., Rational Choice Theory, but that might provide good (and better) answers to *some* explanation-seeking questions. These answers would be considered (at least) incomplete by Critical Realists. Due to Critical Realism's lack of reflection on the usefulness of different forms of explanation in the social sciences and on pragmatic aspects of explanation, good and useful explanatory information will be lost (cf. Van Bouwel 2004a; Weber & Van

[3] Wade Hands (1999, 181) has been pointing at the risk of an *ontological fallacy* as well.
[4] If so, who decides, when and how to do that? Diachronic, 'uncoordinated' revisions (in the different fields) might undermine the unification, which is based on a common ontological framework.

Bouwel 2002).[5] The focus is on ontology (and their convictions of how the social world *really* is), at the expense of methodology.[6] One should be wary of the heavy metaphysical furniture imposed by Critical Realism, and of its *politics of metaphysics*.

3. The division of labour in the social sciences

Discussing the possible ways to divide labour in the social sciences, many terms have been introduced: interdisciplinarity, multidisciplinarity transdisciplinarity, unidisciplinarity, postdisciplinarity, cross-disciplinarity, non-disciplinarity, mono-disciplinarity, etc., and terms to characterize the power balances: imperialism, colonialism, isolation, integration, etc. The central problems to be solved are - given the plurality we find in the explanatory practice of social scientists: **(a)** to what degree should we integrate the plurality of theories, methodologies and forms of explanation; **(b)** what is the purpose of integration or what drives the integration?

Starting with the latter question **(b)**, the integration can be theory-, method- or problem-driven (cf. Shapiro 2002), or, I would like to add, metaphysics-driven. A good example of a theory- and method-driven integration is the so-called *economics imperialism*, which tries to unify the social sciences based on neo-classical economics and applying rational choice theory. Another theory-driven unificationist project is Wallerstein's *world-systems analysis* (grouped around the concept of *historical system*, cf. Wallerstein 1991). Critical Realism is an example of metaphysics-driven unification (driven by the politics of metaphysics). I have been defending that the integration of the social sciences should be

[5] I do have to mention, however, that Tony Lawson (1999) does recognize that the context and explanatory questions at hand do affect the explanatory practice, but he considers it as a second-order issue and does not acknowledge the consequences these pragmatic factors might have on the form of explanation, as I argue in Van Bouwel (2004b). In the same article, I show how the Critical Realists' *discours* (about how they want CR to be) does not always fit with the positions they actually defend.

[6] I do not claim that they do not pay attention to methodological issues at all, but that the arguments formulated against other theoretical perspectives, start from ontology. E.g., Tony Lawson (2003, 21) in his critique of *mainstream* economics states explicitly: 'My argument is ontological. I do emphasise this.'

problem- or question-driven (e.g., Van Bouwel 2004a), aiming at efficiently answering (explanation-seeking) questions.

Answering the first question **(a)**, then, i.e. to what degree should we integrate the social sciences, can be done by stressing the need for unification (and a form of unidisciplinarity), or by cherishing plurality. The question-driven approach opts for the second, but is, nevertheless, critical to the current disciplinary division of the social sciences. It does encourage us to cross -or institutionally dismantle- disciplinary borders in order to find the best answer to an explanation-seeking question. Rather than driven towards unification, it is driven by a quest for the best answer, maximally using (and comparing) the plurality of theories, methodologies and forms of explanation present in social scientific practice. Hence, it cherishes plurality and claims that pluralism is optimal for the social sciences as the different forms of explanations, theories and methodologies provide us with different kinds of useful explanatory information; depending on your motivation or knowledge-interest, one of these different kinds of explanatory information is the most apt.[7] Defending a theory-, method- or metaphysics-driven unification (like Critical Realism) would imply that some (useful) forms of explanation would be lost, and wanted kinds of explanatory information would become unavailable to us, hence some explanation-seeking questions would not, or inefficiently, be addressed.

4. Conclusion: Question-driven pluralism guiding interdisciplinarity

Unifying social science under the banner of the *a priori* Critical Realist ontology (and its methodological implications) does not seem the right way to overcome the intellectual division of labour in studying the social world. Its politics of metaphysics does not take into account the plurality of knowledge-interests (and the difference these imply in the explanatory information that is required), neither the plurality of existing forms of

[7] We have defended this point in a more detailed and technical manner, in: Van Bouwel & Weber (2002), and Weber & Van Bouwel (2002). In these articles we make the idea of 'best answer or best explanation' more explicit and elaborate a framework for explanatory pluralism.

research and explanation in the social sciences. Useful explanatory information would get lost if the social sciences were unified in a Critical Realist framework.[8]

As an alternative, I have sketched a *question-driven interdisciplinarity* that will make maximal use of the plurality of existing forms of explanations and theories in different disciplines (depending on the question at hand) and cherish explanatory pluralism, rather than following an imposed (a priori) ontological framework which narrows down the use of existing forms of explanations and replaces it for an all too demanding standard of explanation, neglecting the impact of knowledge-interests and pragmatics. The dialogue and interaction between disciplines is then driven by the questions and problems at hand, not by the need to 'prove' that one's convictions concerning metaphysics, theory or method are superior (in all situations) and should be the basis for unification.

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[8] Useful explanatory information that gets lost, is, e.g., the information obtained from explanations formulated in the Covering Law Model (CLM). This form of explanation provides us with specific explanatory information needed in order to answer *some* explanation-seeking questions in the best possible way (Weber & Van Bouwel, forthcoming). CLM is, however, not embraced by Critical Realists, e.g., Lawson (2003, 143): 'the reliance on "deductivist" explanation/prediction (…) is an error', and Lawson (1997, 36): 'we must embrace a very different conception of explanation to the deductivist covering-law model.' See, as well, Lawson (1997, 16-17).

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An obstacle to unification in biological social science

Formal and compositional styles of science

Abstract

I motivate the concept of styles of scientific investigation, *and differentiate two styles,* formal *and* compositional. *Styles are ways of doing scientific research. Radically different styles exist. I explore the possibility of the unification of biology and social science, as well as the possibility of unifying the two styles I identify. Recent attempts at unifying biology and social science have been premised almost exclusively on the formal style. Through the use of a historical example of defenders of compositional biological social science, the Ecology Group at the University of Chicago from, roughly, the 1930s to the 1950s, I attempt to show the coherence and possibility, if not utility, of employing the compositional style to effect the synthesis of biology and social science. I also relate the efforts of the Ecology Group to those of investigators in the Sociology Department of the University of Chicago. In my conclusion, I discuss the usefulness both of employing the category of styles of scientific investigation in historical and philosophical studies of science, as well as the concept of compositionality in scientific studies. I end the paper with some tentative suggestions regarding the importance of compositionality for an analysis of human society.*

1. Framework, methodology and goals

There are many theories, and even *manners of theorizing*, concerning biological and cultural aspects of ourselves. The relations between theories concerning these two aspects are rich and politically important. They are fraught with ambiguity, inconsistency, and bias – heated debates over concepts such as 'race' or 'nature/nurture' serve as reminders of this. What concerns me here is the intersection between theories of, and in, these two domains. In particular, I will investigate two different styles of investigation regarding the *evolution* of *social* properties and relations. I call these styles *formal* and *compositional*. I argue that the possibility of unification in social science (*one* of the two themes of this edition of *GJSS*) will require analyzing, and overcoming, radically different ways of doing research, as we can see in the *specific* examples of two different ways of attempting to unify biological theories with the study of society and culture. Perhaps there is no complete way to overcome these different styles of biological social science, nor even of unifying biology and social science [i.e., pluralism (of various sorts); the *other* theme of this edition of *GJSS*!] and these may not in themselves be unfortunate conclusions. Furthermore, currently the state of biological social science is one of radical plurality. Whether pluralism or unification of theories and styles of investigation of biological social science will or should be the ultimate goal, and whether either a unification of these styles, or of biology and social science, or both are even desirable, an analysis of these styles is important and even requisite in order to understand research in the area where biology and social science overlap.

Let me first articulate the two styles of scientific investigation in biology that I have analyzed elsewhere: *formal* and *compositional* biology (Winther 2003; 2006a, b). Each style has distinct and internally consistent ways of reasoning: explaining, modeling, and abstracting. Whereas formal biology revolves around mathematical laws and models, compositional biology examines material parts and wholes. The difference between these styles is not a matter of the natural domain studied or scientific specialty included. Rather, they differ in their *methodologies* of theorizing and experimenting.[i] Each style can, and does, examine the same biological system (e.g., social insects or organisms) in distinct ways, sometimes even reaching conflicting conclusions about the system's processes and entities. Conflicts arise especially since each style *yearns for completeness* – that is, each style employs its own method toolbox to develop a coherent and general theory (with a characteristic theoretical structure: '(causal) arrows' or 'equal signs'), which the style then takes to be necessary and sufficient to explain *all* the data in question.

With respect to these two styles of investigation in biology, philosophers generally believe that formal biology is more philosophically robust, interesting, and important. Evolutionary genetics, which employs the formal style and is concerned with the dynamics of evolutionary change in populations, is often considered the paradigm theoretical biology. Significant philosophical analysis has been devoted to it. Compositional biology, on the other hand, is sometimes accused of being merely stamp-collecting or being obsessed with mechanistic detail. This is unfair because the compositional style is truly scientific, as I show here and elsewhere (Winther 2003; 2006a, b). It is also unfortunate and even ironic that the compositional style, which governs *most* of biology, has received the *least* philosophical attention. Added impetus for my project in the philosophy of biology comes from social, economic, and ethical concerns endemic to molecular genetics, biochemistry, biomedicine, physiology, as well as developmental and cellular biology, all of which are biological and medical sciences employing the compositional style. For example, genetic engineering, stem cell research, and medical therapies stemming from bioengineering, are more appropriately analyzed as cases of compositional, rather than formal, styles.

In this paper on *biological social science*, I will analyze two different family of views - formal and compositional - concerning the evolution of culture and society, stemming from biologists interested in the evolution of culture and society.

I will now present, in a telegraphic manner, my examples of, respectively, formal and compositional biological social science.[1] With respect to formal biological social

[1] None of the biologists I explore had or have any deep training or expertise in social science. I will therefore remain vague about which theories of which exact social sciences (e.g., economics, anthropology, sociology, etc.) these biologists were attempting to unify with biology. This vagueness does not, however, affect my general argument that there are radically different manners of bringing together biology and social sciences.

science, there is the gene-culture co-evolutionary theory of Cavalli-Sforza and Feldman (1981), or Boyd and Richerson (1985), or even the memetics of Dawkins (1976) and the adaptationism and genetic determinism of the sociobiologists (e.g., Wilson 1975). The compositional biological social science that I will analyze in detail is the theory of animal and human societies that the mid twentieth-century University of Chicago Ecology Group composed of W.C. Allee, A.E. Emerson, Ralph Gerard, and, last but not least, the formal-compositional hybrid figures of Thomas Park and Sewall Wright were trying to develop. The synthetic biological social science that was being forged at Chicago by scientists, as well as by sociologists, like Robert Park and Ernest Burgess, is a superb case of a compositional biological social science in the making. Revisiting this work may very well reopen some abandoned intellectual tracks that may prove to be useful in our attempts to evaluate the unity-pluralism of biological social science. Despite the modeling, empirical, political and rhetorical efforts prevailing in the last two generations of biological social science, we certainly need not whole-heartedly accept *formal* biological social science as the final and only way of understanding the relationship between biology and social sciences.

Let me now turn to the relation between the empirical content of the theories and styles of theorizing of biological social science, *and* the crucial political, ethical and social implications of these theories and styles.[2] The compositional style is, in some respects and for some purposes, more empirically adequate than the formal style. This should not be underestimated. But this does not imply, by any means, that biological social science - compositional or formal or some other style - is necessarily desirable or that more empirical adequacy inherently leads to a more responsible politics or ethics. Whatever its empirical adequacy may be, I accept that biological social science is, by its very nature, not socially, ethically, and politically unproblematic. It can even be directly pernicious. However, the important normative questions surrounding the very existence and purpose of biological social science (to which I will briefly return in the conclusion) will not be my primary concern here.

[2] I thank one of the reviewers and the editor for challenging me on the very desirability of any kind of biological social science.

Furthermore, these normative questions need not even, strictly speaking, concern me here. While it may seem like a contentious point, I believe that the empirical content, empirical methodology and the theory of a science (e.g., biological social science) *underdetermines* its ethical and political interpretation. The scientific data, methodology (both, of course, partially determined by theory) and theory *do not come with an interpretation of their political, ethical or social implications already attached.* For example, some have read the Chicago Ecology Group's attempts at a compositional biological social science synthesis as an attempt to defend 'group conformism and blind discipline' (Keulartz 1998, 138; see also Simpson 1941; Novikoff 1945). These critics read the Group as defending *totalitarian* ideals – the social group dominates and controls the individual. I believe that it is not accidental that Novikoff and Simpson both wrote their criticisms during the Second World War. Other commentators have, instead, gleaned *social-democratic* ideals from the theoretical efforts of this Group – for example, the Group stressed the importance of cooperation over competition in animal as well as human societies (e.g., Mitman 1992).

Thus, while scientific data, methodology, and theory are certainly not independent of political and ethical views, extremely different political, social and ethical interpretations can be gleaned from the same data, methodology, and theory. This underdetermination stems, in this case, both from ambiguity in the views of the Group, and from underdetermination, as a logical phenomenon in the sense of the Duhem-Quine thesis, from the same information. Regarding ambiguity, the Ecology Group, on the one hand, discussed social integration and mechanisms of dominance and subordination parts could have on one another as well as the whole could have on the parts. But, on the other hand, it also stated that 'the part-whole relationship is reciprocal' (Gerard & Emerson 1945, 583). There is flexibility and openness in gleaning political and ethical interpretations from these biological social scientific claims. To consider another example, it barely requires mentioning that Darwinism has been interpreted for all sorts of liberal-democratic, communist, and fascist agendas and purposes. While there may not be *radical* underdetermination of the social, political and ethical implications of biological social science, there most certainly is *partial* underdetermination (both as a logical point and as a point of ambiguity). Finally, in this paper I seek more to understand compositional biological social science and less to judge it. I certainly agree that one could judge it for its purposes, interpretations, and dangers; but one can also attempt - with some success - to present, describe, and analyze its scientific data, method, and theory *analytically* prior to investigating its variety of socially-relevant implications.

2. Two styles of biological social science

A style of scientific investigation involves a general set of commitments to a particular way - in theory and practice - of doing science. There are a variety of philosophical examinations of styles (e.g., Hacking 1985; 1994; 2002; Maienschein 1991; 2000; Harwood 1993; Crombie 1994; Martínez 1995; Vicedo 1995; 2000; Suárez & Barahona 1996). Although it would be very useful to do a comparative analysis of these different proposals it is beyond the scope of this paper to do so. What I will do here, instead, is to motivate some of the ideas regarding styles articulated, respectively, by Hacking and Vicedo. Hacking has been seminal in championing this manner of understanding scientific investigation. Vicedo provides a nice comparative review of the position of various authors; at the end of this section, I will briefly focus on her conclusions.

Hacking has defended the utility of the notion of styles of reasoning for two decades. Most generally, styles are ways of doing things. Hacking (1985; 1994; 2005) defends Crombie's six styles of 'scientific thinking in the European tradition': axiomatic postulation, experiment, modeling, taxonomy, statistics, and genetic (historical) thinking (Crombie 1994). This is a valuable categorization of styles, even if it is also problematic. I will set the problems aside here and will, instead, focus on Hacking's philosophical analyses of styles.

I will emphasize three aspects of Hacking's rich characterization of styles: (1) their general constitutive role in science, (2) their role in defining what is 'true-or-false' and *not* what is 'true', and (3) their role in determining what is 'objective' (within a research tradition or theory/theoretical perspective). I shall now examine each in turn.

Styles are constitutive of scientific work. Although Hacking does not quite provide a transcendental argument for their existence,[3] it is evident from his discussion that he believes that without them, science (or most of human activity) could not proceed. Regarding their general constitutive role, he notes:

> 'Every style of reasoning introduces a great many novelties including new types of: objects; evidence; sentences, new ways of being a candidate for truth or falsehood; laws, or at any rate modalities; possibilities. One will also notice, on occasion, new types of classification and new types of explanations' (Hacking 2002, p. 189).

To this list I would add new ways of unifying, understanding, and modeling. In short, styles present new ways of reasoning, hypothesizing, evaluating, investigating, building, planning, organizing, etc. And there are *radical differences* in the styles available for scientific research, as can be seen from Crombie's list.

It is not clear what conclusions can be drawn from this, however. First of all, style identification, individuation, and definition is very difficult. Hacking admits that styles can hybridize and intertwine (e.g., 1985, 148; 2002, 184), so it is not clear whether we can clearly differentiate styles from one another or whether we have a 'continuum' of variation and hybridization of styles. Second, even if we provisionally grant that styles can be differentiated, does the existence of radically different styles actually ground a transcendental deduction for the necessary and constitutive role of styles in scientific work? Might it not be the case that scientific theorizing and practice would be possible even without styles? And do they completely determine scientific work – might they not partially underdetermine that work? These questions are directly analogous to the questions non-Kantians have been posing the Kantians regarding the constitutive nature of the categories of reason (e.g., causality, substance, and unity) – these may very well exist (somewhere and somehow), but does that mean that they must exist prior to, and be constitutive of, any possible experience? Must they stem and be imposed 'from above' ? I suspect that the debate about styles would exhibit similar patterns. Hacking's claims are

[3] He does, however, say: 'My study is a continuation of Kant's project of explaining how objectivity is possible' (Hacking 2002, 181).

clear: 1. there are different styles and 2. styles are constitutive of scientific work. However, the justifications for these claims are not immediately clear.

Let us turn to objectivity and truth. Hacking defends the idea that the criteria and nature of objectivity are grounded in particular styles of research:

> 'This is not because styles are objective (that is, that we have found the best impartial ways to get at the truth), but because *they have settled what it is to be objective* (truths of certain sorts are what we obtain by conducting certain sorts of investigations, answering to certain standards)' (Hacking 2002, 181, emphasis mine).

As part of their constitutive role, styles 'settle[] what it is to be objective'. We are not given a definition of the thorny concept of objectivity, but sense can be made of Hacking's claim even without it. Styles determine what sorts of claims can even be publicly evaluated and (potentially) accepted, through both empirical and theoretical means. This can be better understood if we look at his notion that 'the very candidates for truth or falsehood have no existence independent of the styles of reasoning that settle what it is to be true or false in their domain' (1985, 146). A style determines the possibilities of a proposition even being a sensical one with some (or other!) truth-value attached: 'A style… makes it possible to reason towards certain kinds of propositions, but does not of itself determine their truth value' (1985, 149). Hacking writes:

> 'Each new style… brings with it new sentences, things that were quite literally never said before. This is hardly unusual. That is what lively people have been doing since the beginning of the human race. What's different about styles is that *they introduce new ways of being a candidate for truth or for falsehood*' (Hacking 2002, 190, emphasis mine).

This does not mean that Hacking is a relativist vis-à-vis truth; he considers himself an 'arch-rationalist' (1985, 150-151).[4] To be more specific, he is a relativist about truth-or-

[4] The categories 'relativist' and 'rationalist' may not be particularly informative here, however.

falsehood, not about truth! He is also neither a relativist nor an idealist about reality. Reality 'exists' independently of styles, but we cannot know very much about it without styles. Ultimately, there is a complex relation between styles and reality, but the very possibilities of objectivity and truth-or-falsehood are necessarily constituted by styles. These philosophically-sophisticated views are worthwhile pursuing further.

In a helpful comparative review, Vicedo analyzes the views of Hacking, Mainschein and Harwood on styles. Her discussion is rich and suggestive, and here I only have space to highlight the six conclusions she reaches regarding styles. This summary provides another way to understand the centrality and utility of styles. These are the six conclusions:

1. 'The study of styles leads us to focus on the processes and practices of science.
2. Justification in science is not between science and the world.
3. Rationality is publicly constructed.
4. The existence of different scientific styles implies that there is no unified scientific method.
5. Science does not proceed by a linear replacement of theories and methods.
6. We need to construct a systematics of scientific methods'
   (Vicedo 1995, 249-252).

This list points to some of the more important theoretical and practical consequences, for the philosophy of science, of focusing on styles. After this brief discussion of some of the conceptual foundations of styles, I will now turn to my case study.

<u>2.1. Formal biological social science</u>

Formal biological social science, like formal biology, develops abstract formal models (see endnote i) to provide explanations of the evolution of human society and culture. The basic structure of this framework was developed by Hamilton, Wilson, and Dawkins during the 1960s and 1970s. For example, Hamilton's inclusive fitness theory has been used extensively by, for example, Wilson to explain putatively problematic phenomena such as social altruism and group cohesion (Wilson 1975). It is worth pointing out that Hamilton eventually abandoned his own inclusive fitness framework for a formal hierarchical selection approach (Hamilton 1975). This occurred especially as a consequence of Hamilton's interactions with George Price (see, e.g., Price 1970; 1995; the latter paper appeared posthumously and was edited by Steve Frank). However, many of Hamilton's followers, including sociobiologists such as Wilson, continued to use his earlier approach of explaining human evolution as a result of maximizing gene-level-centered inclusive fitness. I will not here concern myself further with sociobiology as it has been explored in detail elsewhere (e.g., Segerstrale 2001). The genetic determinism and adaptationism of sociobiology is a well-known attempt at synthesizing biology and social science using the formal and conceptual tools of neo-Darwinian evolutionary genetics.

Here, I want to focus on both Dawkins' framing attempts as presented in his famous best-seller *The Selfish Gene*, and two recent attempts to address - employing formal methods - the relationship between biology and culture. I will not examine in detail either Cavalli-Sforza and Feldman's or Boyd and Richerson's mathematical attempts to analyze the biology-culture relation, but will, instead present their remarkable ways of framing these issues.

Dawkins analyzed culture in terms of units that can be transmitted and which have differential fitness: 'Cultural transmission is analogous to genetic transmission in that, although basically conservative, it can give rise to a form of evolution' (1976, 203). He called these replicator units 'memes': 'Examples of memes are tunes, ideas, catch-phrases, clothes fashions, ways of making pots or of building arches' (1976, 206). He speaks of meme pools, survival value of memes, meme mutations and copy-fidelity, and meme inheritance from 'brain to brain via a process which, in the broad sense, can be called imitation' (1976, 206). This is a particularly clear case of framing an analysis in a new domain (i.e., culture) based on previously developed theoretical tools and concepts (i.e., the formal style of evolutionary biology). While it is true, as the philosopher of biology, Kim Sterelny, has pointed out to me, that in his *The Selfish Gene*, Dawkins neither presents a single mathematical model nor (practically ever) discusses mathematics directly, this book was, in effect, a condition for the possibility of, or at the very least, helped set the stage for, formal work on cultural transmission.

Cavalli-Sforza and Feldman (1981) developed a mathematically-rich theory of cultural *transmission* and *evolution*. They 'accept as culture those aspects of "thought, speech, action [meaning 'behavior' (CS&F)], and artifacts" [a definition of *culture* that they take from Webster's Dictionary] which can be learned and transmitted' (1981, 10). They then differentiate two levels of selection, natural and cultural, pertinent to two orders of organisms, first-order organisms (e.g., humans) and second-order organisms (e.g., cars and violins) (1981, 14-19). They note that classic Darwinian fitness and natural selection pertains to first-order organisms, whereas cultural selection, involving both learning and acceptance, is relevant to the cultural trait, that is, the second-order organism. They define cultural selection as 'the rate or probability that a given innovation, skill, type, trait, or specific cultural activity or object - all of which we shall call, for brevity, *traits* - will be accepted in a given time unit by an individual representative of the population' (1981, 15). The objects of cultural selection are conceptually consistent with Dawkins' memes. Both second-order organisms and memes are theoretical constructs of cultural 'traits' that meet the requirements of evolution by natural selection (i.e., heritable variance in fitness). A formal theory of transmission and selection can therefore be developed for them.

In this context, it is of interest to cite, at length, a key methodological passage from Cavalli-Sforza and Feldman:

> 'We have chosen to develop a mathematical theory, and we are well aware of the serious disadvantages that result from this decision. The necessary over-simplification is usually so great, especially in the applications to human behavior, that there is often a danger of distortion. Our position however, is that a mathematical theory is always more precise than a verbal one, in that it must spell out precisely the variables and parameters involved, and the relations between them. Theories couched in nonmathematical language may confound interactions and gloss over subtle differences in meaning. They avoid the charge of oversimplification at the expense of ambiguity. Another reason for favoring a

> mathematical treatment is our belief that the theory of biological evolution owes much of its present strength to its mathematical background, primarily in population genetics. Quantitative predictions can provide the potential to test the validity of the quantitative theory' (Cavalli-Sforza & Feldman 1981, v-vi).

Cavalli-Sforza and Feldman have faith in the importance of mathematical theory, particularly that stemming from population genetics. And although they differentiate Darwinian/biological selection from cultural selection, their models and modeling methodology vis-à-vis cultural selection very much follow in the vein of the formal theory of population genetics.

Boyd and Richerson (1985) published their book a few years later and make explicit and repeated reference to Cavalli-Sforza and Feldman's text. In their work they provide two introductory and conceptually rich chapters, entitled 'Overview' and 'Some Methodological Preliminaries', from which I will highlight some ideas. First, they take issue with critics of any form of biological social science who claim that 'because humans acquire so much of their behavior culturally rather than genetically, the human evolutionary process is fundamentally different from that of other animals'. In contrast, they note, 'since the neo-Darwinian theory of evolution does not explicitly account for the cultural transmission of behavior from one generation to the next, *there has been no way of knowing* whether this argument is cogent' (1985, 1-2, emphasis mine). And, although they are rather humble in their presentation of results, they do claim that:

> 'There are important differences between the genetic and cultural inheritance systems, and the theory will by no means neglect them. However, the parallels are profound enough that *there is no need to invent a completely new conceptual and mathematical apparatus to deal with culture*' (1985, 4, emphasis mine).

The 'not-completely-new' apparatus that they develop is what they call 'dual inheritance theory' in which 'the determinants of behavior are assumed to be transmitted via two structurally different inheritance systems' (1985, 2). In effect, they claim, there are two channels of transmission. In discussing the function, in the sense of the *origin*, of the two systems, they note that: '[W]e will argue that the structural differences between the two systems may well have arisen because the two systems are functionally analogous, that is, both systems serve to enhance ordinary Darwinian fitness' (1985, 31). Given that they do not make a distinction between two levels of selection or two orders of organisms, this theoretical structure does, admittedly, exhibit important differences with the theories proposed by Cavalli-Sforza and Feldman, as well as by Dawkins. Darwinian fitness is a sufficient and ubiquitous measure of selection for Boyd and Richerson, but not for Cavalli-Sforza and Feldman or Dawkins. Despite this, the similarities vis-à-vis presenting a formal theory in the spirit of Neo-Darwinian evolutionary genetics are far more important than the differences.

Thus far, I have briefly described some of the arguments that ground two formal frameworks that serve as key examples of formal biological social science. One last point, pertinent to this issue of *GJSS*, needs to be made with respect to these modeling attempts. Both books espouse a *unificationist* view of mathematical theory and, thus, implicitly, accept what one can call an epistemic or theoretical monism: that there is one correct way to develop and understand our theories. Given space constraints, two quotes will have to suffice to justify my claim. In the first paragraph to their preface, Cavalli-Sforza and Feldman note that:

> 'What emerges from the theoretical analysis [of cultural transmission] is the idea that the *same frame of thought* can be used for *generating explanations* of such diverse phenomena as linguistics, epidemics, social values and customs, and the diffusion of innovations' (1981, v, emphasis mine).

The desire and reality for unification, especially in the context of producing explanations, is here clearly manifested.

In a discussion of 'the utility of general theory', Boyd and Richerson state that:

> 'The most important function of general theory is to *link the many disciplines* contributing to the understanding of a complex problem like the evolution of human behavior. The general theory suggests what properties of sample theories [simple

> models that also have some generality – these desiderata are "competing", pp. 24-25 [5]] are essential in order to make the theory complete. It makes it possible to *deduce the consequences* of alternative sample theories in one discipline for the phenomena studied by another' (1985, 27, emphasis mine).

Clearly, there is also a general desire here for theoretical unification. While neither of these sets of authors explicitly claim that their models and modeling methodology is the *only* way to understand the relation between the biological and social, I think that given their defense of mathematical modeling (see also Boyd & Richerson 1985, 30-31) in the context of neo-Darwinian theory, *as well* as their explicit defense of unification, they do, in fact, adopt a theoretical monism. This seems to be an implicit assumption, in significant respects, in the formal style; it is a less prevalent commitment in the compositional style.

2.2. Compositional biological social science at Chicago

There was a whole program of study at the University of Chicago from, roughly the 1920s to the 1950s, in biology and sociology which employed the compositional style. In what follows, I will analyze the University of Chicago Ecology (and Sociology) Group(s) from the point of view of a philosopher who is interested in styles of scientific investigation.[6] My data are key papers by the main players, my method is philosophical analysis, my goal is the understanding of scientific methods and disagreements stemming from commitments to different styles of research.

2.2.1. Ralph Gerard's 'Orgs'

I will start with two important texts from Ralph Gerard, a University of Chicago physiologist with close ties to the Ecology Group, in particular to Alfred Emerson, the termite expert whose views on superorganisms I will also explore below. Within the span

[5] On the trade-offs present in modeling, see Levins 1966; 1968.

[6] Greg Mitman, a historian of biology, published a thought-provoking book on the history of the University of Chicago ecologists, which includes extensive analysis of the liberal and social-democratic political proclivities of this group. Mitman paints a detailed historical and sociological context, but provides little by way of conceptual or philosophical analysis (Mitman 1992). While I have learned much from Mitman's book, my project is different.

of a few years, Ralph Gerard wrote two stimulating articles on his views concerning integration, at both organism and society levels. These texts are suffused with discussion of the compositional relation and its relevance to biological and social levels.

The key concept in Gerard's conceptual work is that of the 'org'. He considered it a way to denote the 'broader connotation' and 'inclusive sense' of organism (1940, 341). This is how he presented his first definition of org:

> 'An org has persistence in time and boundaries in space, both of which may be short or ill-defined. During its recognizable integral existence, however, or during some differential segment of it, the org endures in approximate equilibrium. Within it there exist interactions between parts and between parts and whole which also endure as constants. True, the mechanisms of coordination may themselves be dynamic equilibria, as we shall see, yet in integrating the parts into the whole, the lesser orgs into the greater one, they are essentially static forces independent of time's arrow' (1940, 341).

Orgs can exist at a variety of levels and they have spatio-temporal individuality. Interactive mechanisms within the org are of particular types that continue to influence the org throughout its existence. Gerard made the important distinction between interactions among parts, and interactions among parts and wholes. His compositional manner of defining an org was even more evident in his 1942 definition:[7] 'An org, then, is a unit system, composed of lesser units as its parts, in which reciprocal influences exist between the parts and the whole. Orgs differ in two general ways; degree of integration and level of organization' (Gerard 1942a, 74). In both definitions, we see that he was concerned with *mechanisms of integration* and *levels*. I will now analyze each of these issues in turn.

Regarding mechanisms of integration, Gerard held that *gradients* were the central sort of mechanism. This was an idea he almost certainly learned from his teacher and later colleague, the influential physiologist at the University of Chicago, Charles M. Child (e.g., Child 1940; see Mitman 1992, 162). A gradient is some sort of signal,

[7] This definition is found in a paper presented at an important symposium gathering Allee, Emerson, Thomas Park, and the famous sociologist Robert Park, among others (Redfield 1942a). See footnote 13 below.

whether it be biochemical, metabolic or nervous, which is emitted from one part and then gradually diffuses throughout the org, sometimes along very particular channels or in specific directions. Here is what Gerard had to say about gradients vis-à-vis their role in integrating orgs:

> 'But perhaps the *most important coordinating mechanism in present day epiorganisms*[8] *is the gradient*, which acts in surprising detail like that in organisms. To be sure, the quantitative scale is not in such things as metabolic rate or mechanical power, as in the organism; nor are the units in a constant spatial sequence. Also, the mechanisms of gradient operation is surely different in the two cases - though we know less about that in the multicellular body than about that in the social group. But the relation of *dominance and subordination*, of *ascending control* as a powerful agent in enforcing org unity, and determination of the *differentiation* of units for special org functions by this agent, are closely homologous in the organism and epiorganism. … Consider an army, a university, a labor union, a banking house, a department store, the Masonic Order, the National Government, the British peerage. In each case there is a clear hierarchy with successive levels of dominance and subordination, from general or president or director or king to private or clerk or common citizen' (1940, 408, emphasis mine).

Control and dominance 'enforc[es]… unity' and is also involved in the ever-increasing 'differentiation of units for special org functions'. Despite differences in 'quantitative scale', it is clear that Gerard desired to formulate strong analogies between organismic and social mechanisms of integration.

Furthermore, Gerard was not only concerned with specific types of mechanisms, but also with the *relative* causal power or dominion of certain parts or of the whole vis-à-vis these mechanisms. In this context, he noted:

> '…the vital problem [is] the character and direction of the determination or control or correlation or causation or force, as you will, acting between part and

[8] An epiorganism is a society of organisms (Gerard 1940, 340)

> whole. As to direction and degree, the possibilities are limited; either the constituent unit or the org may determine the other partly, completely, or not at all. If *neither determines* the other at all, there is clearly no org but rather chaos. If each *determines the other completely*, there results a closed isolated system; only the entire universe can qualify as such. If determination is complete in one direction, say the org is fully controlled by its units, the system can be externally influenced only at the unit level, that from which control is directed; and, in effect, the *reciprocal* direction of control is non-existent. But that is *tantamount to denying organization, for the essence of an org is that the units in it act differently from solitary ones by virtue of their incorporation in the system.* … It follows, then, that determination between the org and its units is *always reciprocal* and *always partial* and that the system can be modified by the environment acting upon it at either level. But enormous quantitative variation is possible within this frame, as is clear from the study of organisms' (Gerard 1940, 341-342, emphasis mine).

This passage has significant philosophical content to it, particularly with respect to the problem of aggregativity or additivity between levels.[9] Complete absence or presence of control by one level over the other leads to 'chaos' or a 'closed isolated system', respectively. Both of these, Gerard believed, are highly unlikely, if not impossible, outcomes. But how does the reciprocity work? Can both levels be *simultaneously* and *interactively* influential, or is there a sort of zero-sum game of influence here (i.e., in so far as control is exercised by the whole, control is lost by the parts, and vice-versa)? And, if it is a zero-sum game, then which level has *more* control? In the 1940 paper, he endorsed the zero-sum game scenario and held that the whole - the higher-level org - has more control: 'It is perhaps obvious now, and will become more so, that as the integration of an org increases the determination of the *unit by the whole* also *increases relative* to that of the *whole by the unit*' (1940, 342, emphasis mine; see 1942a, 74; Gerard & Emerson 1945, 585). That is, an increasingly integrated org (the usual and 'natural'

[9] That is, aggregativity = 'the whole is equal to the sum of its parts', whereas non-aggregativity = 'the whole is greater (or less?) than the sum of its parts'. See Wimsatt 1986.

outcome of developmental and evolutionary processes) leads to increased control of the component units by the whole.

This, however, is a conclusion that he distanced himself from, to some extent, after being criticized by Novikoff (1945) for holding *totalitarian* ideas in which the group (i.e., the whole) dominates individuals and the parts. In a response to this paper, Gerard and Emerson emphatically agreed with Novikoff that 'the part-whole relationship is reciprocal' (Gerard & Emerson 1945, 583; see also 'one for all and all for one' on p. 694 of chapter 34, a chapter for which Emerson has responsibility in Allee et al. 1949). Furthermore, even in his 1940 paper Gerard had also noted that:

> '…it is possible for men to be part of a highly integrated society and yet feel, as individuals, more free, actually to have more avenues open for satisfying self-expression, than when they are epiorganisms of their own, like single-celled organisms. Which of us would exchange our present state for the privilege of roaming the woods naked and unarmed, without language or fire?' (Gerard 1940, 412).

Thus, Gerard's exact stance on the power and control relationships between the parts and the whole remain unclear.

Gerard clearly pointed to *levels* of organization as pertinent to orgs. For example, he argued that 'an org at one level may itself be a constituent unit of another org at a higher level' (1942a, 75). He made the further unsubstantiated claim that '[t]he degree of integration of an org at any particular level is determined by the relation between the *penultimate* units and the whole rather than by the relations within these or more subordinate units' (1942a, 75, emphasis mine). Thus, functional compositionality is primarily a relationship between contiguous levels. It remains unclear why lower-level units[10] cannot have any effects on the system. Furthermore, there 'is a greater differentiation of its constituent units [units found at level N_{i-1}]' with 'advancing org integration' (1942a, 75). For example: 'A more integrated organism, compared to a less

[10] That is, units at lower levels N_{i-2}, N_{i-3}, … $N_{i-\mathrm{n}}$ do not have *any* effect on the whole (org) at level N_i. Here N_i denotes the focal level, and levels are individuated, from lowest to highest, as N_0 (i.e., $N_{i-\mathrm{n}}$), N_1, N_2, etc. Gerard (1940, 342) suggests this presentation.

integrated one, has more kinds of cells which are largely more differentiated and therefore interdependent' (1940, 348). As we shall see, Gerard and others took increasing differentiation and division of labor, at a variety of levels, as a key component of increasing integration and complexity of the whole.

Gerard extensively discussed the 'org' with respect to human societies. Let me start by citing a passage that highlights some of the rather extraordinary, even humorously so, analogies that Gerard saw between biological and social orgs:

> 'Hierarchically homologous organs or organ-systems include, with some inevitable overlap with tissues: the skeleton, which may be compared with houses, roads, harbors and civil engineers, architects and workers responsible for them; the skin and other protective systems with the military and penal bodies; muscles with farmer and labor groups; the circulatory system with all sorts of carriers and their producers and operators; the liver with grain elevators, merchandizing concerns, perhaps banking institutions; the reproductive system with the family and some aspects of other formative social groups and agencies, including school and church; endocrines with mechanical, electrical and other engineers, tool and machine manufacturers, perhaps publishers and advertising agencies; the nervous system with governmental bodies, aspects of schools and publishers, radio, motion picture and theatrical organizations; limbs and other structural regions of the body with cities and villages, etc. Certain body functions even are represented by concretized social organs - as memory and libraries, metabolism and banking, trading and manufacturing organizations' (1940, 406).

Although a charitable reader might very well be tempted to ask what the utility of these analogies are, Gerard, as well as Emerson, thought that they were useful in highlighting central properties at different org levels. These properties could then be empirically investigated. For example, the role of science in increasing integration in society could be studied (e.g., Gerard 1940), as could the social role and biological basis of ethics (e.g., Emerson 1942, 174-176; Gerard & Emerson 1945). And, although he did not mention it further, Gerard did state that '[s]ocial inheritance may be as compelling as that transmitted *via* chromosomes' (1940, 405).

Thus, in his work there is a fundamental ambiguity, if not tension, that I am unable to explore further. While espousing democratic and liberal ideals, Gerard also felt that a highly integrated social system, with division of labor, was desirable.[11] As Gerard wrote, with some justification: 'That social control will increase, I am certain; but that an abject citizenry *must* result, I can not agree. I have already pointed out that freedom implies conformity rather than license…' (1940, 411).

From this analysis of Gerard's views, I hope that it is clear that he adopted a compositional style and took the part-whole relation as central. As mentioned, Gerard, a physiologist, was part of an active research group trying to understand social behavior in organisms of all sorts, including humans. An important and creative textbook in ecology emerged out of this nexus of collaboration. Although Gerard was not one of the authors of *Principles of Animal Ecology* (1949), his work is cited in the references and he is mentioned in the acknowledgments as having commented extensively on one of four sections of the volume (Allee et al. 1949, ix). It is to this key text, and the central ideas regarding a compositional biology and biological social science that it captures, to which I now turn.

2.2.2. Principles of animal ecology (1949)[ii]

In this textbook, W.C. Allee, A.E. Emerson, O. Park, T. Park, and K. Schmidt presented a fresh and integrated view on concepts such as: community, individual and group, natural selection and cooperation. Let us explore their accounts of each of these notions in turn. I will subsequently discuss Emerson's view of the superorganism and, briefly, analyze the position of Allee, Gerard, and Emerson on the role of science and ecology in social science and ethics. All of these views are also discussed in the textbook. In the subsequent section, I will examine the ecologist Thomas Park as a hybrid and transitional figure from a compositional to a formal style.

For these authors, ecology and community are highly related concepts. They noted that:

[11] And it was for this latter wish that Simpson (1941) and Novikoff (1945) accused him of being a totalitarian.

> 'The definition of ecology as the science of communities may be valid in its total implications. …in ecology there may be ecological relations of parts of organisms - the nephridial system, for example - of the whole animal, of populations, whether aggregated or dispersed, of associations and communities, and of biomes. At *whatever level* one begins, and *whatever the point of view*, one must study all possible unitary levels before coming to a full understanding of the ecology of either an isolated isopod moving slowly upstream in a small brook, or of the vast biome in which the brook itself is a minor and almost negligible incident' (p. 3, emphasis mine).

Ecological relations exist at myriad synchronic compositional levels and from various theoretical perspectives. Broadly speaking, then, there are communities at a whole variety of levels, but, pragmatically, the authors maintain the common usage of *communities* as assemblages of species throughout their text (e.g., pp. 695-729; see also 440).

Their general notion of a community is multi-level and both ontogeny and phylogeny pertain to it. [12] Here is one of their definitions of community:

> 'A fresh definition of the community concept is offered in the present work: In large, the major community may be defined as a natural assemblage of organisms which, together with its habitat, has reached a survival level such that it is relatively independent of adjacent assemblages of equal rank; to this extent, given radiant energy, it is *self-sustaining*' (p. 9, emphasis mine).

This definition lends itself to a multi-level interpretation if one replaces 'organisms' (or, later in the text, 'species') with 'org', and 'habitat' with 'environment'. Certainly the notion of 'self-sustaining' is connected to org individuality. In line with their organismic conception of even communities, they observed that communities, in aiming toward a climax assemblage of species, were subject to a sort of 'ecological homeostasis' (p. 6). They also argued that: '[s]uccession and development ["of contemporary species associations"] may be conceived as the ontogeny of the community and its parts. The

[12] In their classic definitions: individual development and species evolutionary change, respectively.

evolution of interspecies integration may be thought of as the phylogeny of the definitive grouping of species within the community' (Allee et al. 1949, 695).

The community - as a species association - is clearly conceived of as an organism with an ontogeny and even as a clade with a phylogeny! Furthermore, the extremely illustrative Table 30 provides a 'Comparison of the Cell Doctrine and Organismal Doctrine with the Community Doctrine'; in this table, multiple comparisons of properties across these three levels of organization are provided. These properties concern: anatomy, ontogeny, division of labor, cycles, homology, senescence and rejuvenescence, phylogeny, etc. They are developed in great and suggestive analogical detail (Table 30, p. 440; see the properties of *populations* on p. 264;[13] see my Table 1).

[13] These are: '(1) A definite structure and composition is constant for any moment of time, but fluctuates with age. (2) The population is ontogenetic. It exhibits (as does an organism) growth, differentiation and division of labor, maintenance, senescence, and death. (3) The population has a heredity. (4) The population is integrated by both genetic and ecologic factors that operate as interdependent mechanisms. (5) Like the organism, the population is a unit that meets the impact of its environment. This is a reciprocal phenomenon, since the population is altered as a consequence of this impact, and, in time, it alters its effective environment' (Allee et al. 1949, 264). They did, admittedly, point to some 'dissimilarities' between organisms and populations immediately after presenting this list.

Table 1. Comparison of the cell doctrine and organismal doctrine with the community doctrine

Cell	**Multicellular organism**	**Community**
Composed of definitive protoplasms	Composed of definitive cells and tissues	Composed of definitive organisms and species
Has anatomy (cytological)	Has anatomy (tissues and organs)	Has anatomy (pyramid of numbers)
Has symmetry and gradients	Has symmetry and gradients	Has aspects of symmetry and gradients (stratification)
Has ontogeny (cell development)	Has ontogeny (embryology)	Has ontogeny (succession)
Has limitations of protoplasmic amounts (size, surface-volume ratio)	Has limitations of cell numbers (size, surface-volume ratio)	Has limitation of population numbers
Regeneration of parts	Regeneration of parts	Regeneration of parts
Division of labor between protoplasms	Division of labor between cells	Division of labor between organisms and species
Cycles of protoplasmic behavior	Cycles of cellular behavior	Cycles of organismic and species behavior
Self-sustaining organization (dynamic equilibrium)	Self-sustaining organization (dynamic equilibrium)	Self-sustaining organization (dynamic equilibrium)
Succesful integration of whole determines survival of parts and repetition of parts	Succesful integration of whole determines survival of parts and repetition of parts	Succesful integration of whole determines survival of parts and repetition of parts
Homology of cytological parts	Homology of tissues and organs	Homology of phylogenetically related species in different communities
Senescence and rejuvenescence of cell	Senescence and rejuvenescence of organism	Senescence and rejuvenescence of community
Phylogeny of gene pattern	Phylogeny of cellular pattern	Phylogeny of species pattern
Selection of whole cell unit determines survival of gene patterns	Selection of whole organismic units determines survival of cell pattern	Selection of whole community determines species and organism patterns
Controls internal protoplasmic environment and establishes optima	Controls intercellular environment and establishes optima	Controls environment within community and establishes optima
Selects or rejects protoplasmic building materials	Selects or rejects tissue-building materials	Selects or rejects organisms (species) that harmonize or do not harmonize with community
Retrogressive evolution of cytological structure (chloroplasts)	Retrogressive evolution of tissue structure and of organs (eyes of cave fish)	Retrogressive evolution through species elimination

It is no accident that they adopted an organismic approach to communities. They exhibited a very general commitment to finding analogies among all levels of biological organization. In an important sense this represented a search for biological unification using some kind of conceptual abstraction. They explicitly stated the *compositional* nature of this unification impetus: 'A binding principle in ecology, as in many other phases of biology, deals with the integration of individual units into larger wholes' (p. 8).[14] They had a vision of a compositional synthesis of biology using central concepts such as communities, parts and wholes, individuals and groups. I will now turn to the latter pair.

While they wanted to *generalize* the meanings of 'individual' and 'group', as in the case of 'community', they often returned to the vernacular meanings of these terms. Particularly in a chapter on 'animal aggregations', for which Allee had primary responsibility,[15] these concepts were extensively discussed. In this chapter, three general principles accounting for the 'contemporary organization of vertebrate groups' were presented:

> 'the holding of territory; domination-subordination [hierarchies]; and leadership-followership. These different types may occur in fairly pure form, or they may grade into each other, even in schools of fishes, to give complicated organizational patterns' (p. 411).

Allee argued that group integration and cooperation are essential for group survival. For example, what is today known as the 'Allee effect' in ecology is the idea that there is a *minimal* group density, below which even individual animals suffer since they have a difficult time with, for example, finding mates (p. 399 ff.). His presentation of group benefits of cooperation also included benefits of aggregation at the cellular level (including multicellular organisms) (pp. 397-399). Thus, individual and group benefits[16] stemming from individual cooperation applies to a variety of levels of organization. Allee's analysis of group benefit was also related to 'organismic levels and selection' (p. 683 ff.), a theme to which I will now turn.

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[14] Compare the claims about *mathematical theory* as the link among many disciplines (Boyd & Richerson 1985), and as the frame of thought for generating explanations (Cavalli-Sforza & Feldman 1981). In these radically different ways of viewing unification, we can also see the strong contrast between formal and compositional styles.
[15] For a list of which author had the key responsibility for which chapter, see p. viii, where Allee is also thanked by the 'junior authors' for his 'leadership'. Some of Allee's key books and papers are Allee 1931; 1940; 1942; 1943; Allee & Park 1939.
[16] Which, in Gerard's language, would be unit and org benefits. Individual/unit and group/org are recursive categories.

The relationship of levels of organization to (levels of) selection was crucial to the framework of the Chicago Group. They stated their position by also referring to the work of Darwin, Spencer, Weismann in the nineteenth-century, and, in the twentieth-century, Allee, Emerson, Gerard and Emerson, T. Park, A. Sturtevant and Sewall Wright, among others (footnote, p. 684). With respect to the relation between levels of organization and levels of selection, they argued:

> 'At this point we shall consider the fact that these levels of individual and group coordination are subject to selection as units and are often under the influence of different selection pressures for different arrangements within the same organismic system. The existence of complex internal adaptation between parts of an organism or population, with division of labor and integration within the whole system, is explicable only through the action of selection upon whole units from the lowest to the highest. Conversely, these integrated levels would not exit as entities unless selection acted upon each whole system' (p. 684).

The integrated levels depend on selection of systems at these very levels. This is a strong functional argument.

The levels of selection debate has developed extensively since the 1949 book was published, in part at least, as a *reaction* to some of the arguments presented by the Ecology Group. Sober (philosopher) and D.S. Wilson (biologist) observe how G.C. Williams (1966), author of a genic selectionist treatise:

> 'went to the University of Chicago as a postdoctoral student in the 1950s. Chicago was a bastion of group-level functionalism, and Williams attended a lecture by Alfred Emerson, a termite biologist who interpreted all of nature on the model of a termite colony. As he later recounted the event to one of us (DSW), "If this was evolutionary biology, I wanted to do something else - like car insurance." Williams began work on a book that was meant to clarify the uses and misuses of adaptationism in evolutionary biology. When *Adaptation and Natural Selection* was published in 1966, it became a modern classic' (Sober & Wilson 1998, 35-36).

However, hierarchical levels of selection is still a respected and defended position, and some of its defenders are students, or students of students, of members of the Chicago Ecology Group.[17] What is of interest in the somewhat vague early formulations of hierarchical selection by the Ecology Group is that while they appealed to systemic coordination at a variety of levels, they also, in the spirit of Williams' own later position, adopted a kind of *genetic* explanation of these hierarchical selection processes: '…any genes promoting cooperation are spread into a large organization. The cells of a multicellular organism or the segments of a metameric organism have the same genes' (p. 687).[18] I will not further explore these issues here, but there is no question that for the Ecology Group, cooperation at multiple levels can itself be selected.

Thus far, I hope to have convinced the reader that these authors were defending a highly compositional, as opposed to formal, understanding of biological systems. But my paper is about biological *social* science. What did the authors think of the relevance of their work to social science? There are a variety of answers to this question. As a group of authors, they held that:

> 'Much of human sociology is an integral part of ecology. There are reciprocal influences between these two sciences… We have purposely avoided emphasis on human sociology, but we hope that in time a maturing ecology will be properly fused with that field' (Allee et al. 1949, 2).

Now, with respect to inheritance and variation they noted that:

> 'Human social evolution is beyond the scope of this book. Biological evolution involves germinal changes. Social evolution of man involves cultural changes. …[But] We also think that human society has many superorganismic characteristics' (Allee et al. 1949, 693-694).

[17] E.g., Michael Wade was Thomas Park's student and, upon finishing his dissertation, himself became a member of the University of Chicago faculty; Charles Goodnight was Wade's student; see Wade 1992; Wade & Goodnight 1998; Lloyd 2000.
[18] On the important difference between "units" and "levels" of selection, see Brandon 1982; Lloyd 1988, 2000; Laubichler 2003.

The time was not quite ripe for attempting, as an integrated Ecology Group, a unification of biology and social science.

However, some individual members of the Chicago Group were willing to be bolder and go further. For example, Allee was cited in the following passage: 'The term "social" may be used in a general sense to include "all groupings of individuals which are sufficiently integrated so that natural selection can act on them as units" (Allee, 1940)' (Allee et al. 1949, 687). This is a rather biological way of providing a unified definition of a concept important to both biology and social science. Furthermore, Allee (1943) and Gerard (1942b) were cited as endorsing the following views:

> 'Sacrifice by some individuals for the good of the group, and sacrifice by some infraspecies groups for the good of the species, are exhibited in both biological and social systems; thus many ethical principles have a biological foundation (Gerard, 1942a; Allee, 1943)' (Allee et al. 1949, p. 694).

In addition, Emerson also held that: 'I, for one, see no reason why scientific method may not be applied to the study of social coordinating factors in human society.' (1942, 175; see also Emerson 1939a).

In the context of the textbook (Chapter 24, 'The Organization of Insect Societies') and Emerson's views on the synthetic possibilities of a compositional biological social science, let us turn to the idea of the *superorganism*, probably first articulated by William M. Wheeler in 1911, and subsequently developed by Emerson. In suggesting the idea of the superorganism, Wheeler noted a long series of analogies between ant-colonies and organisms. Emerson was influenced by Wheeler, both by his published work and by having spent time with him in 1919 at the Tropical Research Station in Kartabo, British Guiana (Mitman 1992, 112). Emerson published an impressive review of the concept in 1939 (Emerson 1939a). For him, the analogies with organisms were even homologies (Emerson 1939a, 196; see also Gerard 1940). Listing the section headings provides a feeling for the colony-level organismic emphasis of Emerson's article: 'Division of Labor'; 'Ontogenetic Coordination and Integration', which is divided into five sections – 'Chromosomal Foundations of Integration', 'Activity Gradients and Symmetry', 'Chemical

Integration', 'Nervous Integration', and 'Rhythmic Periodicity; and 'Superorganismic Phylogeny' (Emerson 1939a; on the last topic see also Emerson 1938). He wanted to emphasize the colony whole '[w]ithout attempting to minimize the importance of the study of the parts at any holistic level' (Emerson 1939a, 183). In another paper from 1939, a diagram depicting the forces influencing the colony presents a nested structure of compositional forces (Emerson 1939b, 288; Allee et al. 1949, 722; see Diagram 1) Emerson also developed the idea of superorganismic homeostasis with negative feedback loops (Emerson 1956). In addition, the superorganism resulted as a consequence of higher-level selection (e.g., Emerson 1942, 171). In a discussion following the presentation of his 1939a paper, he noted that a superorganism 'is *both* a social organism and a group of organisms' (Emerson 1939a, 208, emphasis mine). This of course is relevant to the discussion above regarding Gerard's views on control by the parts versus the whole. He also ended the 1939a paper stating: 'Let us not, however, raise the superorganismic concept to an all or none principle. Let us rather use the perspective it gives us to stimulate further study and understanding.' (1939a, 201).

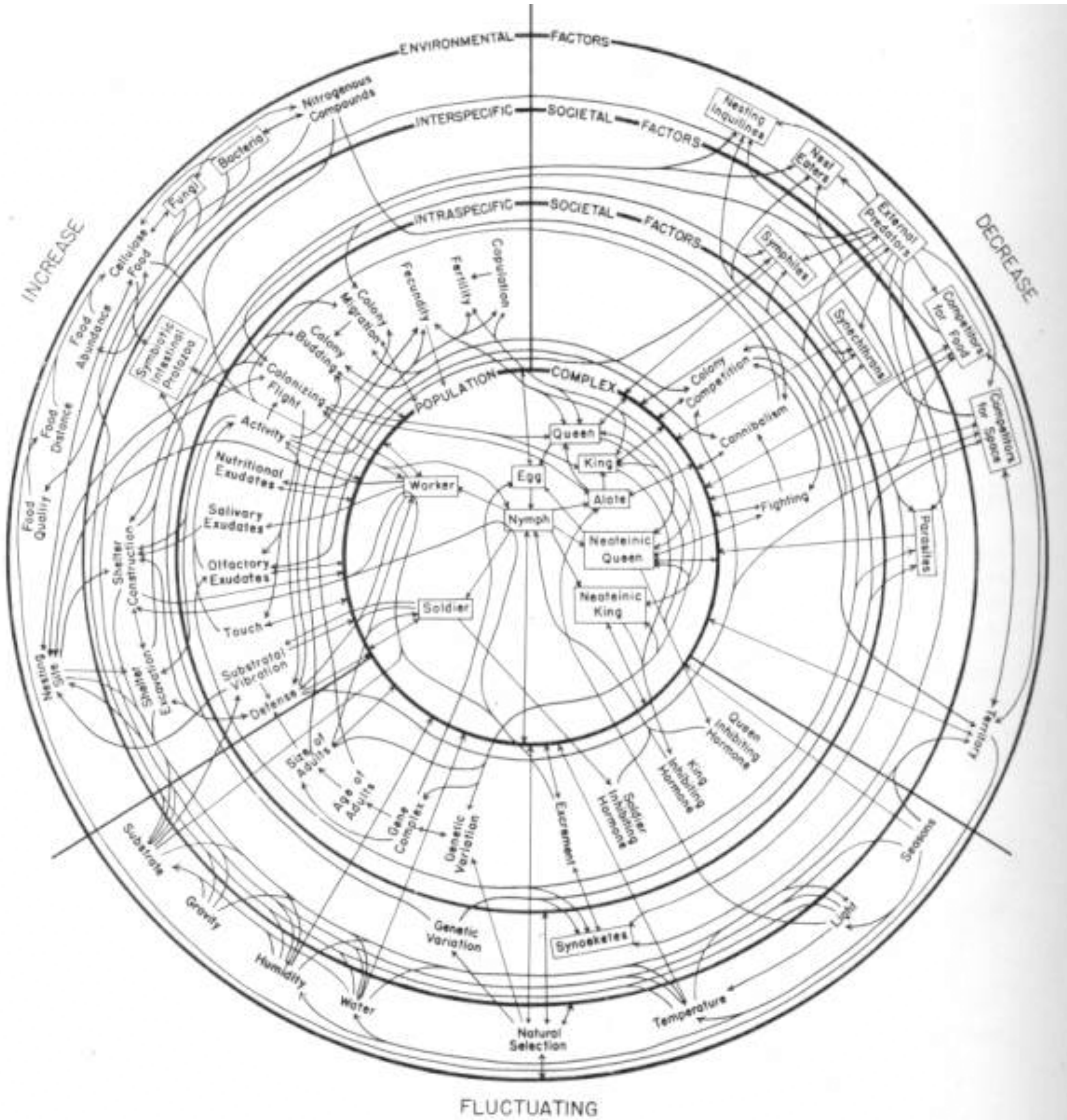

Diagram 1. Factoral complex influencing the population of a typical termite of the family Rhinotermitidae. Arrows indicate the direction of the effect.

Emerson felt that his studies of termite superorganisms gave him some (limited) tools to attack the problem of human social organization. He stated that,

> 'The application of the concept to human society is beyond the competence of the speaker. In certain respects, the comparison seems valid, but in others which rest upon "social heredity" as compared to germinal heredity, striking differences occur which may be too

> great to make their analogy significant. I feel, however, that biologists and sociologists need to study comparable facts critically in the light of new discoveries, and in numerous cases, what may seem superficial analogy may be shown to be due to fundamentally similar causal factors. "Social heredity" bears certain similarities to germinal heredity and may in part be under the influence of a sort of natural selection (Fisher 1930, 183).[19] Social integration in human society shows certain similarities to social integration in insect societies. These similarities, as well as differences, should be studied as scientifically as any of the more physiological integrating mechanisms of the organism' (Emerson 1939a, 198-199).

Thus, there are sufficient similarities between social groups in insects and humans to merit an exploration of these analogies for a larger understanding of 'sociality' in general. Emerson also felt that 'the division between the social and the non-social is not sharp' and that 'the demarcation between the social system and the ecological community is also not sharp' (Emerson 1942, 173). As mentioned above, Emerson believed that the 'scientific method' could be used to understand group processes of all sorts, including behavior motivated by ethical principles and mores.

For Allee, Emerson, and Gerard, the time was ripe, or perhaps 'the drive of immediate necessity' (Allee 1943, 517; almost needless to say, this was written during the Second World War) was sufficiently present, to embark on a synthetic compositional biological social science. There was, however, disagreement over this point among the other authors of the Allee et al. 1949 textbook, as well as among the other members of the Chicago Group in general. Investigators like Thomas Park, or even Sewall Wright, appear to have been much less willing to formulate claims regarding the unification of biology and social science. It is Thomas Park's idea of the 'analytical studies of populations' that I will now explore. Park is an interesting transition figure between a compositional and a formal study of ecology.

[19] It is interesting to note that Emerson cites Fisher, perhaps the most mathematically gifted of the three founders of neo-Darwinian evolutionary genetics (Fisher, Sewall Wright, and J.B.S. Haldane). There is a general point to be made here. Even though the authors of Allee et al. 1949 availed themselves of evolutionary theory, the text and most of their own work (except for Park, see below) was clearly done within a compositional style framework and relied relatively little, if at all, on formal methods. They approached their work through the compositional, rather than the formal, style.

2.2.3. Thomas park as a hybrid and transitional compositional-formal figure

Thomas Park combined a deep understanding of the biology of populations with a strong interest in formal statistical and analytical aspects of the same. He did 'not think it necessary' to 'dwell at length upon the biological reality of the population' (Park 1939, 235). To him, that was an obvious fact: 'I consider the population as much a biological unit as the organism.' (1942, 137). Specifically, he listed five 'biological properties', which in Allee et al. 1949 (Chapter 18, 264) he further claimed were 'exhibited by population and organism alike'. The five properties of a population are that a population:

> '(1) possesses a definite structure and composition, constant for any moment of time but fluctuating with age; (2) is ontogenetic, exhibiting growth, differentiation and division of labor, maintenance and death; (3) is genetic, inheriting from each preceding generation a system of gene frequencies; (4) is integrated and coördinated; and (5) meets, as a unit, the full impact of the environment which may modify it and which, in turn, it may modify' (1939, 235; for a very similar list, see Allee et al. 1949, 264, footnote 13 above).

These are, to a large extent, indeed organismic properties. In addition, he presents a highly 'organismic' diagram that depicts 'the integrative factors that through their interaction control the size of the population during the entire course of its life-history' (cite on Park 1942, 122; diagram on Park 1942, 123; Allee et al. 1949, 390; see my Diagram 2). There is no question that Park shared the compositional and hierarchical organismic framework of the fellow members of the Chicago Ecology Group.

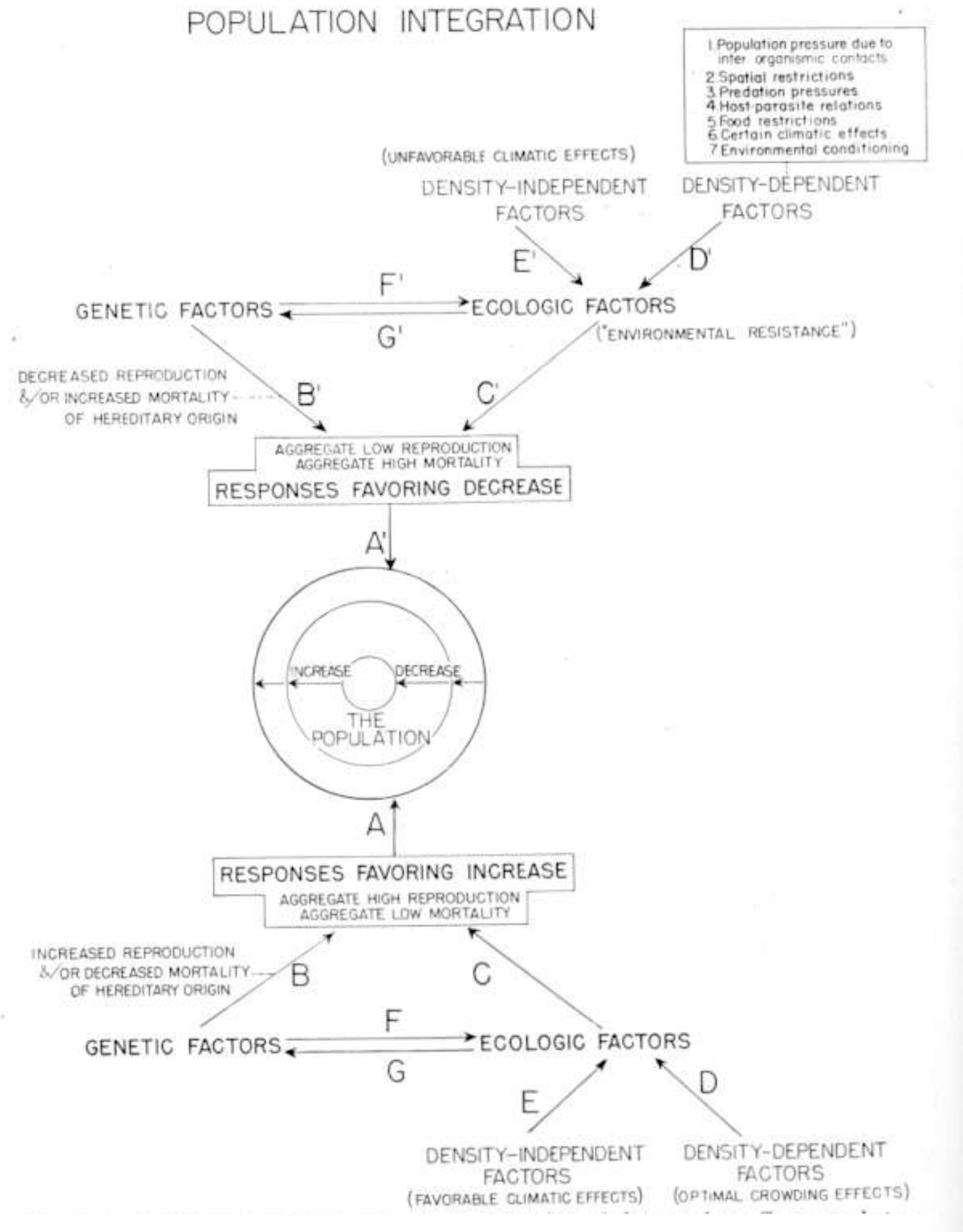

Diagram 2. A schematic representation of the interplay of factors that affect populations.

However, Park was also an essential figure in *formalizing* ecology, and that can already be gleaned from his 1939 and 1942 texts, in addition to the chapters he contributed to Allee et al. 1949. He provided an apology:

> 'I should feel that I had failed if I left the impression that, while integration in the organism is biological, integration in the population is statistical and, by that token, the two are not in any sense comparable. This would be fallacious. The dynamics of populations are equally biological but they are best expressed in statistical terms. Evolution is recognized as a biological principle yet the theory of evolution is populational in character and best depicted statistically' (Park 1942, 137).

Park was worried about being misunderstood as a strict adherent to a solely statistical frame of mind. However, one could very well imagine that Park might have resisted the following passage from the introduction of the co-authored text:

> 'Some few [ecological] relations can be given fairly exact mathematical treatment. There is much room for pure humility among ecologists who are trying to cope with these loosely formulated relationships, most of which *cannot be expressed in exact quantitative formulations*' (p. 11, emphasis mine).

After all, Park noted that '[t]he distinctive features of the population are correlated with the fact that it is a statistical entity' (1939, 235). In this paper, Park provided a nice categorization of mathematical work already developed in 1939 'in the ecological population field'. He claimed that this work 'fall[s] into three categories: the use of statistical methods, the development of empirical curves to describe the growth of populations and the rationalization of equations that picture inter-species competition' (1939, 237). He then proceeded to highlight the formal work of Gause (e.g., mathematics of inter-species competition – 'competitive exclusion principle'), Lotka and Volterra (mathematics of predator-prey relationships), and Pearl (the Logistic Curve of population growth), among others. Furthermore, he claimed that 'ecology is, to a large degree, a quantitative science: the ultimate problems are group and group-interaction problems. To get at such interactions we *must* employ quantitative methods.' (1939, 251, emphasis mine). Ultimately, Park is remembered particularly for his elegant experiments showing the indeterminacy and stochasticity of particular species extinctions in multi-species environments of *Tribolium* (e.g., Simberloff 1980). These experiments fundamentally required statistical and formal techniques. Furthermore, perhaps his most important student, Michael Wade, is also famous for his development of formal theory. Wade's profound knowledge of *biology*, however, is also testimony to him as a hybrid figure.

Park sought to include *both* biological and statistical aspects of ecology. This is a noble cause. In the context of the development of formal biology - i.e., population genetics (1920s and subsequently) and theoretical mathematical ecology (1950s and 1960s and subsequently) - it is, and remains, difficult to relate (let alone hybridize) these two styles of analyzing, developing, and testing theory and practice. During the 1950s, and more so during the 1960s, compositional concerns were, to a very important extent, lost from ecology and population biology (but see discussions in Levins & Lewontin 1985), and the goal became the establishment of formal analytical equations. A full exploration of the demise of the compositional style in ecology and in biological social science, a style so strongly endorsed by the Chicago School of Ecology, would require a complementary investigation of the rise of formal ecology, including the role played by Thomas Park and Sewall Wright (for roughly half a page of mathematical population genetic theory written by Wright, see Allee et al. 1949, 649). This is beyond the scope of the current paper.

As a future component of this project, insightful criticisms of the synthetic-unificationist biological social science project at Chicago, such as those articulated by Simpson (1941) and Novikoff (1945), and even by allies such as Needham (1945) should be explored. What is of interest with these concerns, in contrast to G.C. Williams' criticisms, for example, is that the former were still wholly within the compositional framework. Neither Simpson nor Novikoff criticized the hierarchical part-whole conception adopted by the Chicago Group. That had changed by the time of Williams (1966).

2.2.4. The sociologists Robert park and Ernest Burgess[iii]

I now want to discuss - with less detail because I still need to explore the University of Chicago Sociology Group more (e.g., Park, Burgess & McKenzie 1925; Abbott 1999) - two texts written by Robert E. Park (no relation to Thomas Park): his 1942 contribution to the Redfield Ecology-Sociology interdisciplinary volume and, more importantly, the 1921 textbook he prepared with Ernest Burgess. Let me immediately state that Park was probably *the* most important sociologist at the University of Chicago during the early 20th century, and his contextual, perspectival, interactionist, embedded, and survey-oriented sociology had a significant influence on American sociology (e.g., Abbott 1999, 208). He was also known for his *ecological* theory of sociology.

Park's sociology was thoroughly compositional. A metaphor he used is highly indicative of this. He noted that there is 'nothing so thoroughly rational and nothing so completely intelligible as a machine. Once one understands how to take a machine apart and put it together again, there is no longer any mystery about it. …its behavior is completely predictable' (1942, 231). He elaborated this metaphor and connected it to biology:

> 'This is …what is meant by making a thing intelligible, and since the task of science seems to be to make things intelligible, it performs this function by treating things as machines, that is, things that can be taken apart and put together. Where, as in the case of living organisms, science has been able to take things apart but has not been wholly successful in putting them together again, living creatures and life itself have remained, from the point of view of science, more or less a mystery' (1942, 232).

Intelligibility comes from understanding the compositional nature of a whole. This is not necessarily an espousal of aggregativity (see footnote 9 above), but it is an espousal of the fundamentally compositional nature of systems. It is unclear whether he thought that, in biology, we will succeed in attaining a *full* understanding of the system that will allow us to resolve the mystery of living creatures. It is clear, though, that he thought such understanding has thus far eluded us.

Let me now turn to his and Burgess' thoughts regarding society, as found in their 1924 (2nd edition) textbook. A brief explanation of the textbook and of my analytical methodology is in order. This sociology textbook was in wide-spread use for many years. The format of the book is an approximately 60 page introduction followed by 13 chapters on diverse sociological topics (e.g., Human Nature, Society and the Group, Social Integration, Conflict). For each chapter, they have a brief introduction followed by their rationale for choosing the texts they present from many diverse authors. After the rationale, they have brief excerpts from the work of various authors – for example, the authors in Chapter 3 include William M. Wheeler, John Dewey, Robert Park, Émile Durkheim, and Albion Small. This mix of biologists, philosophers, and sociologists is representative of the 1924 book. At the end of each chapter, they presented an 'Investigations and Problems' section, which includes further thoughts and reactions to the texts. After this section, they have a reference list, as well as brief, but useful, 'Topics for Written Themes' and 'Questions for Discussion'. This is a very creative textbook. For purposes of my paper, I focus on the main introduction to the book as well as Park and Burgess' subsequent introductions to each chapter. I am primarily interested in *their* views and not in the positions of the many other authors contained in the textbook.

Park and Burgess noted that there are some fundamental questions regarding the differences between humans and other organisms that need to be answered in order for us to determine what properly counts as sociology:

> 'In other words, the social organism, as Spencer sees it, exists not for itself but for the benefit of the separate organs of which it is composed, whereas, in the case of biological organism the situation is reversed. There the parts manifestly exist for the whole and not the whole for the parts.
>
> …
>
> The fundamental problem which Spencer's paradox raises is that of social control. How does a mere collection of individuals succeed in acting in a corporate and consistent way? How in the case of specific types of social group, for example an animal herd, a boys' gang, or a political party, does the group control its individual members; the whole dominate the parts? What are the

> specific *sociological* differences between plant and animal communities and human society? What kind of differences are *sociological differences*, and what do we mean in general by the expression "sociological" anyway?
> Since Spencer's essay on the social organism was published in 1860, this problem and these questions, in one form or another, have largely absorbed the theoretical interest of students of society. The attempts to answer them may be said to have created the existing schools into which sociologists are divided' (pp. 27-28).

They posed the question of what differentiates humans from other organisms in order to seek a proper delimitation and specification of sociology qua discipline. Sociology is concerned with how corporate and consistent action can stem from a set of parts. It stands in contrast to anthropology, which they took to be 'the science of man considered as one of the animal species, *Homo sapiens*' (p. 10). Sociology and history, unlike anthropology, are more concerned with 'man as a person, as a "political animal", participating with his fellows in a common fund of social traditions and cultural ideals'. (p. 10). Furthermore, sociology is distinct from history. History 'seeks to reproduce and interpret concrete events as they actually occurred in time and space' and also 'seeks to find out what actually happened and how it all came about' (p. 11). Instead, sociology 'seeks to arrive at natural laws and generalizations in regard to human nature and society' and also 'seeks to explain, on the basis of a study of other instances, the nature of the process involved' (p. 11). Unlike history, sociology is abstract. Furthermore, unlike anthropology it concerns our political, rather than biological, aspects.

Their own argument and thesis was specified later in the introduction. Their theoretical view was a compositional, interactionist, perspectival, embedded and pragmatist one:

> 'While it is true that society has this double aspect, the individual and the collective, it is the assumption of this volume that the touchstone of society, the thing that distinguishes a mere collection of individuals from a society is not like-mindedness but *corporate action*. We may apply the term social to any group of individuals which is capable of consistent action, that is to say, action, consciously or unconsciously, directed to a common end. This existence of a

> common end is perhaps all that can be legitimately included in the conception "organic" as applied to society.
>
> From this point of view social control is the central fact and the central problem of society. Just as psychology may be regarded as an account of the manner in which the individual organism, as a whole, exercises control over its parts or rather of the manner in which the *parts co-operate together to carry on the corporate existence of the whole*, so sociology, speaking strictly, is a point of view and a method for investigating the processes by which individuals are inducted into and induced to *co-operate in some sort of permanent corporate existence* which we call society' (p. 42, emphasis mine).

This is an explicitly compositional view in which corporate action of the parts is the defining aspect of a society. Later in the text they do note that a 'cardinal problem' is the one concerning 'the social one and the social many' (p. 161). They also claimed that: 'All the problems of social life are thus problems of the individual; and all problems of the individual are at the same time problems of the group.' (p. 57).[20] Furthermore, their view regarding the 'touchstone of society' does not imply that there has to be a consensus among the parts in order for 'corporate action' to occur, but merely that sufficiently stable cooperation has to exist – cooperation does not necessarily require consensus. This cooperation grounds corporate action, which itself allows sufficiently common interests and preferences to be satisfied. It is also clear that they believed that parts do not always cooperate sufficiently well to achieve the desired outcome(s) and that is exactly where social control enters. I will not here flesh out their views on social control. For my interests in exploring what a compositional (biological) social science would look like, it is sufficient to observe what the main aspects of their view concerning social action are: (1) there is a clear part-whole relation, (2) action is done through cooperation, and (3) control, when necessary, is enforced.

In the above quote and elsewhere, Park and Burgess engaged in an important activity of line-drawing between the social and the biological. Above they noted that the

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[20] See also: society 'as a unit', p. 848; 'individual atoms', p. 867; 'community as individuals' versus 'community as a whole', p. 956.

only legitimate sense in which 'organic' can be 'applied to society' is by the existence of a 'common end', or what I interpret as purpose, design, and teleology. This is a notorious problem in the case of evolutionary biology and I will side-step it here, but it is interesting that they state here that they consider this the *only* link between the two realms. Elsewhere they observe:

> '…Society now may be defined as the social heritage of *habit and sentiment, folkways and mores, technique and culture*, all of which are incident or necessary to collective human behavior.
> Human society, then, unlike animal society, is mainly a social heritage, created in and transmitted by communication. …Society viewed abstractly is an organization of individuals; considered concretely it is a complex of organized habits, sentiments, and social attitudes - in short, consensus' (p. 163).

Human and animal societies are differentiated in terms of 'social heritage', which is transmitted through communication. It is interesting that unlike, for example Emerson, they did not use the heredity metaphor to describe communication. The heredity metaphor was used primarily by those with a biological background.

Thus, it would seem that they did not hold that biological metaphors or theoretical perspectives have *any* merit in a social context. It is true that in a number of places they sought to explicitly draw a sharp line between human and animal groups (e.g., existence of culture as sentiments, mores, techniques, etc. that are transmitted). However, the compositional - and to a lesser extent, analogical - thinking that they adopted seems to permit them to import crucial concepts from the biological realm with which they further developed their sociological framework. There is a concern with organisms and biological phenomena throughout the book. For example, biological texts regarding competition and assimilation (Chapters 8 and 11, respectively) appear; groups of plants and animals are analyzed in addition to human social groups. Furthermore, they stated that the 'the economic organization of society, so far as it is an effect of free competition, is an *ecological* organization' (p. 508, emphasis mine). This metaphor is generative in that they used ecological knowledge to explore new ways of thinking about economic organization. Another generative use of a biological metaphor can be seen in their pithy description of two forms of social interaction: 'If *mutation* is the symbol for accommodation, *growth* is the metaphor for assimilation' (p. 736, emphasis mine). These metaphors provide the conceptual space to conclude that the former 'may take place with rapidity', whereas the latter is 'more gradual' (p. 736). This is a clear case of the generative use of biological metaphors. As I have shown is the case for the Chicago Ecology Group above, there existed a combination of fear and trepidation, together with an explicit endorsement, of the analogy and proximity of biological and social orders and processes.

In Allee et al.'s 1949 textbook there is an acceptance of laws as empirical regularities; in Park and Burgess's 1924 textbook there is an explicit distrust of laws. In their introduction, Allee and his co-authors wrote: 'We regard the so-called "laws of nature" as empirical, derived from the facts, and not the facts from the laws' (p. 5). In their introduction, Park and Burgess, in contrast, revealed a strong distrust in laws and abstract thinking of a certain sort:

> 'It has been the dream of philosophers that theoretical and abstract science could and some day perhaps would succeed in putting into formulae and into general terms all that was significant in the concrete facts of life. It has been the tragic mistake of the so-called intellectuals, who have gained their knowledge from textbooks rather than from observation and research, to assume that science had already realized its dream. But there is no indication that science has begun to exhaust the sources or significance of concrete experience. The infinite variety of external nature and the inexhaustible wealth of personal experience have thus far defied, and no doubt will continue to defy, the industry of scientific classification, while, on the other hand, the discoveries of science are constantly making accessible to us new and larger areas of experience' (p. 15).

There is a certain anti-theoretical stance in this quote, but given the rest of their book, and their intricate classification of the order and process of society, it is impossible to believe that Park and Burgess were *fundamentally* anti-theoretical. In fact, I believe that their selective opposition to theory stemmed from a deep suspicion toward mathematical abstractions and closed and rigid laws, whereas they continued to hold that conceptual classifications could be useful. With respect to their suspicion of mathematical abstractions, they wrote:

> 'Society is not a collection of persons in the sense that a brick pile is a collection of bricks. However we may conceive the relation of the parts of society to the whole, society is not a mere physical aggregation and *not a mere mathematical or statistical unit*' (p. 161, emphasis mine).

This expresses a clear distrust of conceiving society as *merely* a quantitative unit. Given that Park's research school involved surveys, etc., further work is required in order to explore exactly in which respects Park distrusted quantitative methods and the aim of finding quantitative relations.

The compositional social science research program Park and Burgess were developing contained crucial biological aspects. They also exhibited a deep resistance toward mathematical abstraction. In the next section I will provide, among other discussion, some concluding thoughts on the two Chicago Schools.

3. Conclusions and suggestions

There is no monovalent interpretation for how to - or even whether one should - unify biology with the social sciences. Furthermore, the differences between the research programs of, say, Cavalli-Sforza and Feldman, and Emerson and Gerard are astronomical. There is no question that there is a radical disunity in these two ways of articulating a biological social science. And if it is indeed the case that a compositional biological social science is rare today, that would certainly not count as unity: it would be an *absence*. Only if we continue (retry?) to develop a compositional biological social science can we even begin to understand how to unify these two fields (if that is what we desire).

I have also used this case study to inform an analysis of styles of investigation (see section 2 above). I have employed the compositional and formal styles because these are the styles I have examined in *biology* (Winther 2003; 2006a, b) and they are also the ones that I think guide research in the two cases of biological social science I have elucidated. I do not think that they are the only styles nor that they are necessarily independent from each other. But they can be individualized and they do motivate very different kinds of scientific research. Elsewhere, I have explored the different possibilities of integrating and unifying different styles and theoretical perspectives of research (Winther 2001; 2003; 2005). I found that for a number of future scenarios, the outcome would be pluralism rather than unification. Unification could, however, certainly also result. But even then, there are open questions: unification *of what?*, *for which purposes?*, *under what interpretations?*, and *to what extent?* These are questions for future research.

In this paper, I also brought up a compositional social science presented by two sociologists, Park and Burgess. They were mostly interested in sociology – biology was a concern, but it was not their central worry. Why did I bring them up? There are so many other schools of thought during that time period that I could have analyzed. Functionalism, inspired by Durkheim, was being developed. Marxism had existed for over half a century. A number of other anthropological and social schools of thought, and issues, existed (see Barnard & Spencer 1995).

However, Park and Burgess are special in the context of my analysis. First, they had links to the Ecology Group (but see endnote iii). More importantly, however, it is clear that there was a compositional style at the heart of their analysis and they relied on biological concepts as sources for some of their sociological analysis. *Using* concepts from another analogous field, and, thereby, find generative links between two fields, is a way to at least begin to negotiate a unification of some kind. Likewise, the Ecology Group, coming from the *other* side of the biologist-sociologist divide also used biological concepts - which they were exceedingly familiar with - to draw analogies and formulate concepts and explanations in a domain that they were less familiar with, human society. Note that Park and Burgess moved concepts primarily in one direction, from biology to sociology, whereas the Ecology Group employed concepts in *both* directions. For example, they used social concepts to understand populations of termites or flour beetles (*Tribolium*) as well as inter-species communities. Furthermore, they also drew on biological concepts to understand human society in all its complexity, including symbolic representation and ethical principles. Put differently, Park and Burgess, and the Ecology Group, started in different places, moved in different directions, and employed different tools, but shared the same goal: to forge strong analogies [homologies? metaphors? indications of the 'same' (at a particular level of abstraction) causes and interactions at play?] between the biological and the social.

It is unfortunate that their attempts at synthesis were exhausted or cut short.[21] At least in the case of the Ecology Group, I can mention some causes for its demise: (1) an increasing formalization of ecological theory, (2) an increasing concern with lower-, and mono-, level genetic and selective processes, (3) an increasing reliance on cybernetic, informational, and computational metaphors to present and generate ecological theory, and (4) an increasing rationalization and specialization of disciplinary structures so that broad-scale analogies and disciplinary synthetic efforts became increasingly discouraged (on this last point, see Gerson 1998). I do not know enough to speculate about the changes that occurred in the Chicago Sociology Group. One of its strands did lead to Symbolic Interactionism (e.g., Becker & McCall 1990), but this school was much less concerned with biological concepts.

What would a full and unified compositional biological social science look like? Is it an appealing image? Is it so much better than the genetically-based and/or formally-based biological social science that currently surrounds us? Could it, in the final analysis, be synthesized with the dominant biological social science now? And what shape and dynamics would other kinds of biological social sciences have? I think it is incumbent on me to at least try to answer these questions.

[21] As one reviewer pointed out to me, Talcott Parsons also employed the compositional style in his sociology. I know significantly less about Parsons and I understand that many of his functionalist views are problematic in a number of respects, including the social oppression that they *can be* interpreted as endorsing (as sociologist Elihu Gerson has informed me, Parsons' views can be summarized as 'a place for everyone and everyone in their place'). Here I simply point to a context that could be further developed in light of this paper.

As a prefatory comment, let me note that I *do* think that there are a number of important people today investigating compositionality in biological systems. Ghiselin's (1974) and Hull's (1978; 1980) proposal and analysis of species as individuals is one such example. Gould's (2002) processual hierarchical selection model is another example. Furthermore, Levins and Lewontin are fascinating and key philosophical biologists – they are compositional formalists of sorts. Their 1985 classic book is filled with discussions of the mathematical, as well as the qualitative,[22] analysis complex articulated systems. They are both formal and compositional biologists. So is the extraordinary philosopher of biology, William Wimsatt. There are also other excellent scholars working on compositionality. Furthermore, in organismic and *systems* biology, the concepts of *homology*, *individuality*, and *part* are central and there is significant discussion of these structural and processual concepts at a variety of levels of abstraction [e.g., Bolker & Raff 1996; Hall 1994; Hansen 2003; McShea & Venit 2001; Müller & Wagner 1996; Raff 1996; Wagner 1995; 2001; Welch & Waxman 2003; Winther 2001; 2005]. In biology, compositionality is alive and well. In biological social science, however, it is practically absent (but see Eldredge & Grene 1992).

In ending, let me attempt to provide some answers to the above questions. What follows does *not* count as 'careful scholarship'. But it counts as sincere reflections on difficult issues.

We live in an age where formal laws - simple, universal, and deep - are held in high esteem. Our technocratic proclivities and continued desires for Grand Unified Theories and universal algorithms seem to continue to close off spaces for narratives, metaphors, and complex understandings of articulated compositional systems. Or, rather, these technocratic proclivities fight with perhaps equally powerful proclivities, by other agents of a more 'holistic' (New Age?) persuasion, to express the richness of experience in a non-viciously abstract manner, to use William James' expression. Often, the desire to share stories, and the nature of the narratives, are strongly correlated with a compositional framework, in which systems are admitted to be complex and highly

[22] And the two are not distinct. Levins has developed mathematical methods to assess qualitative properties, as also described in Levins and Lewontin 1985.

articulated, with multiple functional and processual loops (e.g, Wimsatt 1997). However, compositional frameworks can also, more rarely, be aligned with simplicity.

The point is, however, that a compositional biological social science could very well allow us to bring in ecological complexity, rather than genetic simplicity, into our understanding of ourselves. We will no longer (solely) search so avidly for genetic necessary causes of our behavior, or try to do the genetic fitness bookkeeping that will allow us to explain why we perform behavior X with respect to person 1, but behavior not-X (or Y) with respect to person 2. Instead, we will look for complex ecological relations and interactions. We will do more justice to the fact that we are part of a system, and that we can study forms of interactions and forms of life as both embedded observers and agents in that system.

Furthermore, with a biological compositional social science, we will be able to do justice to so many of our metaphors. How many ecological metaphors do we not use in describing the behavior of others, including political and economic agents? (E.g., 'That competitor company is a true predator' or 'Money flow is energy flow'.) Of how many organismic similes do we not avail ourselves? (E.g., 'He is cunning like a fox' or 'She is brave like a lioness'.) Certainly the superorganismic analogy is not dead either. It captures the imagination of many and the representation of social insects in movies and fiction is legion. It is interesting to see how biological and computational metaphors and 'creatures' are being increasingly hybridized (e.g., Haraway 1991).

Perhaps the biology-social science link will inexorably exhaust itself as a source of generative metaphors and concepts, and a unification or even coordination will remain impossible and undesirable. But *compositional* aspects in the relation between biology and social science seem to be perennial. Consider the Canadian movie 'The Corporation', by M. Achbar, J. Abbott, and J. Bakan. The movie is suffused with ideas regarding compositional relations. A corporation, we are reminded, is constituted by a *group of people*, yet it is, for legal and economic purposes, *an individual*. Clearly, there are bound to be many subtleties and difficulties with this general statement, but this statement can be more fully understood through a compositional analysis. For example, as one of the framing techniques, the movie portrayed corporations as demented psychopaths since their behaviors fit many of the criteria the DSM (*Diagnostic and Statistical Manual of Mental Disorders*) presents for that mental disorder. Further investigation of the veracity of the stated criteria as DSM criteria for being a psychopath need to be made, but again, the inference here is highly suggestive: since corporations are individuals, with a correlated psychology, they can be, and should be, judged as such. Remedies, including therapy, could and should be found. Today they exist with too much impunity. Compositional issues abound in human society.

Renato Rosaldo in his *Culture and Truth*, provides windows into current anthropology and cultural studies. He discusses 'positioned subjectivity' and the fact that we always already have a perspective(s) when we face the world. This is not a *necessary* aspect of a compositional view, but it is highly consistent with a research style that emphasizes compositional relations. Rosaldo writes:

> 'The notion of relational knowledge presented here has been woven from concepts developed through previous chapters of this book. Consider how the introductory notion of the "positioned subject" anticipates the idea of "imperialist nostalgia", in which the "detached observer" appears as a complicit actor in human events rather than as an innocent onlooker. Furthermore, recall how narrative analysis requires a "double vision" that moves between narrator and protagonist and how my discussion of "subjectivity in social analysis" emphasizes the insights offered by "subordinate knowledge". Throughout, I have stressed, first, that the social analyst is a positioned subject, not a blank slate, and second, that the objects of social analysis are also analyzing subjects whose perceptions must be taken nearly as seriously as "we" take our own' (p. 207).

In social science we must take into account the phenomenological self and its associated perspectival experience of the world. Clearly this is something we can practically only study in humans, where we have our own experiences, and our symbolic interactions, with which to understand one another and ourselves. Note again, that an 'object of social analysis' and a 'complicit actor', etc. are themselves a *part* of both social analysis and of society, more generally speaking. The part-whole relation really is a very deep relationship and merits more investigation.

On another note, the ideas of compositionality and positioned subject can, *perhaps*, be combined to make a more responsible politics and ethics. Clearly there are always many kinds of interest groups in society – they are *part* of society. And each group is composed of (is?) positioned subjects. The environment and inanimate nature can be a positioned subject too, as actor-network theory in sociology of science tells us. Furthermore, *perspectives* on the anatomy and physiology of society, such as Feminism, Marxism, and Environmentalism also exist side by side. So is perennial negotiation the solution? (E.g., Latour 1999.) Here is where the eternal dilemma of objectivism and relativism enters. Clearly, ethicists, politicians, activists, and, perhaps, some scientists, can say that there is a better set of social structures, and a better set of perspectives on that structure. For example, more equitable wealth distribution is *superior* to less equitable wealth distribution, *ceteris paribus* (but what goes into this clause?). Think of Rawls' argument concerning the original position, for example. Perhaps a compositional biological social science will be a medium through which informed expert judges could make decisions on these complex matters, decisions which require a fairly broad understanding of the groups (parts) involved and their positions.

I have few answers here to my questions and topics. I do suggest, however, that we are far off from having any sort of unification between biology and social science. I believe that it is worthwhile to investigate the possibilities, desirability, and implications of such a synthesis. In this context, a compositional style - in addition to formal and potentially other styles - must also be pursued.

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Endnotes

[i] There are clearly many ways of mathematizing and formalizing (i.e., formal methodology). In my work on formal biology, I have focused on the formalization of evolutionary genetics (e.g., Winther 2003; 2006c). The mathematics present in evolutionary genetics involves classic techniques from algebra and calculus. Increasingly, simulations of various sorts have also become important. And statistical techniques are crucial for the evaluation of theory in light of the data. Given this diversity of mathematical methods even within formal biology, we now seem to arrive at a problem regarding the clarity of the formal/compositional biology distinction. Undoubtedly, a philosophical investigation of *other* areas, even of those that are 'compositional', such as evolutionary developmental biology, will show both 1st that many different mathematical (formal) techniques are used in biology and 2nd that the compositional style can, on occasion, employ methods from the formal style. See, for example, mathematical work on gene regulation and morphological development by a variety of authors interested in evolutionary developmental biology (Arnone & Davidson 1997; Davidson 2001; Goodwin 1989, 1994; Kauffman 1993; Salazar-Ciudad et al. 2001; Salazar-Ciudad & Jernvall 2004). But, in light of this, let me bolster my distinction between formal and compositional biology by noting, in response to (2), that many compositional biological sciences rely primarily on non-mathematical techniques and representations. On this point, the philosopher of biology Kenneth Schaffner insightfully states: 'In addition to the extensive variation, which defeats simple axiomatization of biomedical theories, the axiomatizations that are formulated are usually in qualitative biological (e.g., cell) and chemical (e.g., DNA) terms and thus do not facilitate deductive mathematical elaboration' (Schaffner 1993, 117; see also Schaffner 1980). Furthermore, in response to (1), there certainly is a vertiginously large variety of mathematical methods used in biology, but formal biology focuses on those which most resemble the kind of gold-standard we have inherited from theoretical physics: closed-form analytical equations. Many of the simple and classic equations of evolutionary genetics are of this form. *Theoretical structure in formal biology is organized around analytical equations*. Many of the other formal presentations of knowledge in other domains of biology (including the 'compositional' domain) lack this compactness and, perhaps more importantly, breadth of scope of application (one form of universality). Another area in which compositional studies employ formal methods is *formal mereology* (e.g., Simons 1987; Smith 1982; 1996; see also Simon 1996). Mereology is the study of part-whole relations. Formal logic has recently been applied, in creative ways, by these and other authors, to elucidate part-whole relations. But these investigations stem much more from the point of view of philosophy and formal computer science, rather than of either theoretical or experimental work in biology. Furthermore, this work has focused primarily on spatio-temporal properties of the part-whole relation and is not particularly close to biological practice. On the other hand, a set of philosophical analyses significantly closer to the actual practice of compositional biology revolve around the organization, dispositions and functions of *parts* (e.g.,

Kauffman 1971; Wimsatt 1974; 1986; 1994; 1997; Cummins 1975; 1983; Levins & Lewontin 1975; see also Haugeland 1978; 1998) and around the concept of *mechanism* (e.g., Wimsatt 1976, Bechtel & Richardson 1993; Glennan 1996; 2002; Machamer et al. 2000; Craver 2001; Winther 2006a; see also Schaffner 1980; 1993). It is in this literature that I believe we will be able to get to the theoretical core of compositional biology. Here is a sketch of that core. The fundamental concern in compositional biology is articulating the various properties, relations and processes of biological parts and wholes using *whichever* methodology may be available or useful. Mathematical methods and derivations, which are a kind of deductive or subsumptive method, can indeed be used. Another form of deductive (-like) explanation - *reduction* - can also be employed when the theories/theoretical perspectives applying to the parts and wholes are distinct (e.g., Schaffner 1993; Sarkar 1998). But even in the case of reduction (and certainly in the case where we stay within the same theory/theoretical perspective), we ultimately desire to characterize a compositional relation (which could, but need not, include material causal relations), and not, in particular, abstraction or formal relations (or hierarchies). We seek to understand, for example, what kind of function a particular organization of parts has within a particular whole. So although a variety of explanatory strategies are consistent with that characterization (including, on occasion, but relatively rarely in compositional biology, mathematical methods), presenting the compositional relation is, in the final analysis, the aim. And, at any rate, biologists tend to adopt properties, concepts, and strategies close to that relation, such as mechanisms and part-dispositions, which themselves can themselves be rather abstract (but almost never mathematical) claims. *Theoretical structure in compositional biology is organized around the part-whole relation and its various aspects.* As the philosopher of biology, John Beattty, put it to me colorfully: in compositional biology, the goal is to *draw (causal) arrows* rather than *write equal signs*. This is itself a heuristic rule and should not be taken too literally. While distinguishing these two styles from each other (and from other styles, such as *narrative* biology) is very much work in progress, I do believe that the formal/compositional biology distinction stands up to a fair amount of scrutiny even if there are areas of intertwinement and even if the distinction is difficult to articulate precisely (see also Winther 2003; 2006a, b). I thank one of the reviewers for asking me to be much clearer about both the formal/compositional distinction and 'formal methods.'

[ii] I discuss this book explicitly and in detail because it is the main (and only) product the Chicago Ecology Group wrote as a *unit*. It is important to mention in this context that the Redfield (ed.) volume was also, in part, a product of the Group. However, this volume consists of papers by individual authors. Robert Redfield was, at the time, professor of Anthropology and Dean of the Division of Social Sciences at the University of Chicago (Redfield 1942a, cover page; Mitman 1992, 151). It is worthwhile citing extensively some passages from his introduction to the volume in order to provide an idea of the *explicit* compositional biological social science synthesis that was attempted (relevant page indicated in brackets): 'This symposium had a double origin. Representatives of the Division of the Social Sciences planned a program of papers having to do with some of the more comprehensive and underlying aspects of society. The program was to emphasize three borderland fields of recent research interest - borderland from the point of view of the student of human society. In the first place there was the disposition in recent years for students of primitive society on the one hand and of modern society on the other to study their subjects in common terms: the significant event here was the rapprochement of anthropology and sociology. In the second place recent investigations of the social behavior of monkeys and apes had made a fresh contribution to the understanding of the origins of human society. In the third place the rapidly developing work of students of mammalian and bird societies had aroused the interests of sociologists and anthropologists. …The essential idea was to present human society as an example within a class, societies, and to have a look at some of the resemblances and differences among examples of the class. [1] [2] In the meantime biologists at the University were making ready a program of papers concerned with the ways in which parts are organized into wholes in life forms. Here again there was a wish to represent new frontiers of research, and to consider special problems in wider contexts. …There was… a disposition to recognize that the integration of parts into wholes within an organism, and the integration of parts into wholes within a population or social aggregation, were not entirely separate problems, but that they could be considered in relation to each other, and together. … The social scientists then accepted with enthusiasm a suggestion from the biologists that the two programs be consolidated into a single symposium with the present title. [2] [5]

…What these papers seem to be saying, in most general terms, is this: The organism and the society are not merely analogues; they are varieties of something more general: the disposition, in many places in the history of life, for entities to undergo such modification of function and such adjustment to other similar entities as result in the development and persistence of larger entities inclusive of the smaller [5]' (Redfield 1942b).

[iii] It is important to note that their famous sociology textbook (Park & Burgess 1921, 1st edition) appeared nearly three decades before Allee et al. 1949. Furthermore, while these scholars were all at Chicago, and while R.E. Park contributed to the 1942 Redfield volume, it is not clear how strong the link between Chicago Sociology and Ecology actually was (e.g., Mitman 1992 barely mentions R.E. Park; in one of two places where he is discussed, Mitman, p. 92, notes that 'despite Park's ecological interests and his close proximity to the zoology faculty [physical or causal?], he rarely cited Allee's work' – Park did, however, cite *Child's* work, also at Chicago, but at least a generation older than many in the Chicago Ecology Group and not a member of the Group). Thus, this link, for which I do not have particularly strong evidence, has to be investigated further. While a causal and historical link remains to be clearly established, a link in terms of the similarity of the *content* of the ideas is clearly present. As we shall see, there are some extremely insightful passages on compositionality to be found in the Park and Burgess book. I ask the reader to peruse the current section of my paper more for the ideas themselves than for a clearly integrated historical narrative linking Chicago Sociology and Ecology. I thank one reviewer and the editor for pointing out this problem to me.

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Social facts from an analytical perspective

The example of institutions as a unifying notion in the social sciences

1. Introduction

In many social scientific disciplines the notion of institution plays a role. Textbooks often start out with a remark to the effect that social institutions are the central subject of investigation. Social sciences in general are concerned with the interaction between people; institutions are the structures that are the result of the many types of interactions between people. From this perspective the conclusion is warranted that institutions form an important part of the basic furniture of the social world, *qua* social world. They are an important type of system that the social sciences, in their quality of being a *social* science, investigate.

This might be most clear in fields like 'institutional economics' and so-called 'new institutionalism' in political science, but also in a field like cultural anthropology this is the case, because cultures or aspects of cultures are often regarded in terms of institutions. Legal theory is also said to deal with 'legal institutions'. Even in a field like collective rational choice theory, which is an extremely individualist approach to collective action, the notion of institutions plays a role (e.g. in the way that the existence of institutions can be explained by a mechanism of rational choice) (cf. Scott 2001, 8).

In this article I investigate how the notion of institution can provide a perspective on the unification of the social sciences. Although unification is not necessarily a holy grail of scientific methodology, it is in so far interesting and important that it opens the possibility of theoretic overview and comparison across specialised disciplines. I regard unification at least as a potential useful tool for research and therefore the present investigation as worthwhile. The common usage of the term 'institution' indicates that the term may provide a tool for such a unified view. ‘Social science is about institutions’, whether you call yourself a sociologist, an economist, a researcher in collective action theory, a social psychologist or a legal theorist: you investigate institutions from your particular perspective. There will be differences in approach and theory, to be sure, because different fields concentrate on different *aspects* of institutions, but there will be no difficulty in translating findings in those fields, one can imagine.

However, even if the picture I sketch is correct, we don’t get a unified view for free. Across different disciplines there are different uses of the term ‘institution’ and one may wonder whether there is one underlying notion of institution underlying the term. This is a difficult question, because despite the frequent use of the term it is often not used as a precise technical term, but rather as a broad covering notion defining a field of research. The notion is often defined rather loosely to indicate a field of investigation and to provide a general framework of investigation.[1] This needs not be a problem in itself. The notion of institution might be best used for a loose general framework, differently conceptualised in different disciplines, providing inspiration for different research questions and methodology.

In this article, however, I shall argue that the widespread use of the term ‘institutions’ provides an opportunity for unification of the social sciences. I will investigate whether there is some core notion of ‘institution’ that is maintained across disciplines, although with local variations. And, if this is the case indeed, can this serve some social scientific and/or philosophical goal? A social scientific goal might be found in the possibility of mapping the similarities and differences between the notion in different fields. This provides opportunity to adopt useful results and methods across disciplines in a relatively easy manner. A philosophical goal may be served from an ontological point of view: if institutions form an important part of the nature of the social realm, then what is the nature of this social realm?

[1] The following observation is also of interest. Although the term is often used in textbooks and references in the social sciences, as often as not the notion of institution itself gets a separate entry in the index. Rapport & Overing (2000) contains the notion of ‘institution’ in its index, but has no separate lemma. Anthony Giddens’ textbook states that sociology investigates institutions (amongst others) and gives some examples, but the term does not show up in the glossary (Giddens 1992).

This article is written from the perspective of analytic philosophy. The basic philosophical interest is a sound conceptual analysis, which means that I will try to analyse the central concepts that constitute the notion under consideration. These concepts I take from several disciplines and discussions concerning the notion of 'institution'. However, although I use material that is present in the social scientific literature this does not mean that the analysis is purely *descriptive*. A choice for a particular concept will exclude alternatives. Philosophical or conceptual arguments, rather than social scientific or empirical arguments will lead the analysis at that point. The resulting view (or 'conceptualisation') will act like a definition and be, in that sense, normative. Some (social scientific or philosophical) uses of the term 'institution' might differ from the analysis presented in this article, but that is to be expected. The assessment of such issues will depend on the strength of the philosophical argument. The philosophical point of departure here, in any case, will be the field of intentional action theory: social institutions have something to do with intentional actions of groups of agents. In other words: social institutions consist of intentional agents, together with their interrelations.[2]

The structure of the article is as follows. First I discuss the notion of institution from the perspective of different disciplines in the social sciences. I also investigate which methodological differences this results in across these disciplines (section 2). Then I propose a core definition of the notion of institution, departing from philosophical action theory (section 3). Then I see what consequences this proposal for a core definition might have for the possibility of unifying the social sciences (section 4).

[2] I will not treat the question whether or not this is a reductionist view of the social sciences. It is *not* reductionist in the sense that social facts can be reduced to facts about individuals, because the relations between individuals are essential to social facts. It is also *not* reductionist in the sense that social facts do not 'really' exist. It is (more or less) reductionist in the sense that it denies an *independent* reality to social facts. My favoured approach would be in terms of the supervenience of institutions on individuals and their interrelations. This is a non-reductionist approach, but does not make institutions having an existence independently from individual agents.

2. The notion of institution in the social sciences

In this section I review what kind of work the notion of institution normally is supposed to do in the social sciences and what problems and opportunities this provides. I present a view from an ontological perspective, because I am mainly interested in the notion of institution as a general term indicating the basic furniture of the social world.

Up to a certain extent institutions are a kind of ‘social substances’, because they are social entities that keep their identity over time. Providing a recognisable identity, despite changes, is often exactly their function in social scientific research.[3] But if we take this ontological perspective, the notion of institution seems to be somewhat stuck between a rock and a hard place. On the one hand, it is used to denote enduring social *structures*, which point our intuitions toward *objects* – institutions are a kind of objects analogous to physical entities, they are identifiable, and persistent through time.[4] On the other hand, the notion is used to denote enduring *social* structures, which point our intuitions towards the, often volatile, relations between humans: volatile relations are not enduring entities.

Also, on the one hand they seem to indicate observable entities. Not entities like *physical objects*, but rather like observable *systems* and *processes*. In our society we can quite easily indicate institutions like the institution of marriage, education, industrial manufacture, religion, politics and so on. We can observe individual instantiations of these institutions and their general patterns. On the other hand, it may be wondered whether these observations are observations of institutions *as such*. Aren’t we rather observing individual agents, relations between agents and the artefacts they use? Aren’t institutions additional theoretical constructs rather than observed systems?

Finally, on the one hand the term ‘institution’ is used profitably in discussions in- and outside the social sciences. On the other hand, this also causes confusion, for instance because there is a distinction between uses of the term that indicate something that we

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[3] This is especially clear in the functionalist approach to social scientific research. The function of some institution is often the central explanandum within such a theory. E.g.: ‘An institution or a behavioural pattern X is explained by its function Y for group Z (…)’ (Elster 1994, 404). Also (Turner 1997, 4).

[4] E.g. ‘(…) institution in sociology, meaning established aspects of society (…)’ (Marshall 1994, 250).

also call *organisations* (such as banks, political parties and mental institutes)[5] and uses of the term that indicate something we would also comfortably call *social rules* rather than institutions or organisations (such as the institution of monetary economy and democracy, where 'democracy' is interpreted as a set of rules for making political decisions) (e.g. Congalton & Daniel 1976, 33).[6]

Next to the conceptual differences between the various uses of the term 'institution' that can be found in the literature, there are also methodological differences amongst disciplines in the social sciences that are associated with these different notions of institution. Differences concerning the notion of institution are not the sole cause of these methodological differences, but they do contribute to them. The following examples serve as an illustration.

In the first place there is a difference between quantitative and qualitative approaches in the social sciences.[7] In political theory, for instance, qualitative approaches prevail under the assumption, amongst others, that political processes can only be understood against the background of particular social structures with specific meanings. Institutions are 'infested with meaning' and 'meaning' is not a category that can be quantified.[8] Similar ideas are current in anthropology, which concentrates on the *interpretation* of (sub)cultures. This is not to say that quantification does not exist in, for example, political sciences (as testified by the large market for election polls), but this approach does seldom concentrates on institutions.

In institutional economics on the other hand, quantitative approaches are common under the assumption that intentional agents can be uniformly analysed: particular meanings in different cultures apparently are less relevant, but the assumption of

[5] E.g. Ultee, Arts et al. 1992, 146; Red Feather Institute 1977, 63; Geense 2002, 9.

[6] In this area we encounter other difficulties as well, because what some particular institution exactly *is* that makes up some phenomenon under discussion is often unclear and hotly debated. Concepts in this domain are often supposed to be so-called 'essentially contested concepts', a termed coined by W.B. Gallie (1956) and adopted in political theory in studies of institutions, such as democracy, power and freedom (Lukes 1974; Connolly 1974). Whether the adjective of 'essentially' is correct or not, the concepts are in fact subject to intense debate.

[7] To some extent this difference is related to the question whether the social scientific methodology can and/or should be similar to methodology in the natural sciences (cf. Winch 1988).

[8] Some theorists equal a process of institutionalisation with 'value infusion', for instance (cf. Selznick 1957).

instrumental rationality as a driving force for agents is common in many investigations (cf. Scott 2001, 28-37).

In the second place there is a difference that has to do with a distinction between formal and material approaches to the subject matter. According to legal theory, the main area of study concerns formal rules of legal conduct. These formal rules constrain the legal behaviour of individuals. A code of law may be interpreted to some extent in terms of institutions; the term 'legal institutions' testifies this.[9]

Other approaches, for instance in social psychology, conceive of institutions from a material or substantial point of view. Social norms or institutions are analysed in a substantial sense, for example by investigating how purported rules are internalised psychologically by individuals and how social institutions arise out of individual psychological facts, e.g. in evolutionary explanations of marriage patterns. However, such research does not necessarily take into account, for instance, that on the one hand the biological makeup of humans over the world is relatively uniform, while on the other hand, institutions of marriage are quite different across cultures. The notion of 'marriage' that is used in evolutionary theories can differ from what a marriage actually consists in amongst different societies. This needs not be a problem for that research (depending on its specific goals), but recognition of a point like this might serve interdisciplinary purposes.[10]

In the third place there are approaches that consider institutions to be explicit social structures that can be designed, built, and revised in some objective way, using the right tools. Institutions then are perceived as explicit structures of rules that guide behaviour for a particular, well-defined goal (cf. Goodin 1996; Swanson 2002).

Other approaches deny that institutions are rules that are explicitly formed and followed. They view rules as growing organically and/or being followed unconsciously

[9] Cf. MacCormick and Weinberger (1986, 24-25) for a view on legal institutions that is similar, but not identical to the general idea of social institutions: 'The sociology of institutions is a next-door neighbour of [our] ITL [Institutional Theory of Law] in theoretical terms, not the same theory.' (p. 25).
[10] Cf. Thornhill (1991), and Hewstone and Stroebe (2001, 197-238) for evolutionary approaches to marriage institutions.

by the participants of the institution. This is true, for instance, of some approaches in anthropology (Elster 1994, 404).

This diversity in conceptualisation and in method might indicate that the term ‘institution’ is used to refer to many different things. The term might hide an underlying diversity. It is my conviction, however, that next to this admitted diversity there is also a common core to many of these notions, which binds them together (for instance by forming a common intersection of the several uses). Identifying this common core is one useful way to identify possibilities for unification of different disciplines in the social sciences. Not by turning them into one discipline, but rather by showing possibilities for building interdisciplinary bridges.

3. A core notion of institution

In this section I propose a core definition of institutions that may serve a useful role in the unification and cross-fertilisation of the social sciences. This definition was inspired by my research on the social aspects of the use of technical artefacts (Scheele 2005; 2006). This research is in the field of analytic philosophical *action theory*. Use of artefacts is a type of action, subject to philosophical analysis. Social institutions are constituted by the actions of collectives or groups of individuals. For the purposes of my analysis, it is sufficient to consider the elements of action and collectivity as *necessary* conditions of the social realm. What needs to be added in order to formulate sufficient conditions will not be investigated here.

The core of the definition I propose is that institutions consist of actions of collections of intentional agents. Furthermore, these actions are performed in a social setting in a sense that they are subject to social criticism and sanctions, in other words, they are socially enforced. Finally, they exhibit some measure of stability, which makes it possible to be identified by researchers and participants alike. The definition that I use then is the following: An institution is a collective pattern of action that is socially enforced with a measure of stability. However, the interesting conceptual work does not lie in this definition, but rather in an analysis of what this definition precisely means. For that, we need an analysis of the constitutive components of the definition. I distinguish four constituents in this notion, which I discuss in turn. They are actions (or intentional actions), collective patterns (or systems), social enforcement (or normativity) and measure of stability.

3.1. Action

I start with the idea of action, because that is the central distinguishing feature that makes 'patterns' a psychological and not natural scientific phenomenon. 'Action' is a term that is used to help distinguish two types of events: purely natural events and events that involve some form of intentionality. 'Behaviour' is used to denote natural events or processes and 'action' is used to denote events that involve intentionality (e.g. in their causal history). In the present context we should conceive of the notion of 'action' as a psychological term, whereas 'behaviour' is a physical (or natural scientific) term. Rocks exhibit behaviour, trees exhibit behaviour, computers exhibit behaviour. Action, on the other hand, is behaviour that is intentional under some relevant description. That means that within the causal chain leading to the behaviour 'intentionality' played some (relevant) role – although some behaviour might be an accidental effect of some other intentional state.

The notion of 'intentionality' is a term that indicates mental states and processes that (potentially) lead to (observable) behaviour of agents and is generally neutral on the details of the psychology. The main elements of intentions are captured by analysing intentions as functional states of beliefs and desires leading to action (where both are not necessarily *conscious*). My action of drinking a beer might be explained in terms of a belief that the glass I took contained beer and of my desire to drink beer. Beliefs and desires I need not be explicitly conscious of.

The distinction between behaviour and action can be put in the following terms. Two *physically* identical movements or behaviours of agents are not necessarily described correctly in *fully* identical terms. If an agent takes a step and thereby accidentally falls off a high cliff, this falling down would be called behaviour and not action. If an agent walks intentionally off a cliff, this (behaviour or movement) would be called an action. Actions are intentional under some relevant description. The distinction is present in the causal history (either fully natural/physical or partly intentional). Although the basic distinction is not difficult to grasp, there are very many conceptual problems connected to intentional actions, which need not bother us in detail here.[11] These difficulties, about the relation between intentions and actions and the criteria of relevance are the main topics of action theory.

Within the social sciences, the relevant description needs some reference to the social realm (in such a way that the role of institutions becomes clear). The question is where and how exactly do those relevant intentions come into play. In order to answer this question, we need to know how the idea of 'intentions under some relevant description' works and this is best done by way of an example.[12]

1) Agent *a* exhibits behaviour *x*.

    Suppose that agent *a* exhibits the following motor-behaviour. His arm stretches and his hand opens. A handful of *sodium fluoroacetate* falls into the well, above which his hand was hanging.[13] From this description it cannot be said whether this was an action or behaviour and if it were an action *what* action it exactly was. We need to supply the description with some information about the intentionality of the agent.

2) Agent *a* acts in way *y*.

    Agent *a* throws rat poison in the well. From this description we discover that the agent knows what *sodium fluoracetate* is and that he intended to throw it into the well. We do not know whether this was the full extent of his intention (possibly only wanting to get rid of the stuff) or whether he intended more effects.

[11] Some central philosophical problems concern the status of unintended side effects or consequences and the possibility of 'weakness of will'.
[12] The example is a modified version of the famous example in Anscombe (1963).
[13] Sodium fluoroacetate or sodium monofluoroacetate. Also known as 'compound 1080'.

3) Agent *a* acts in way *y* in order to obtain goal *g*.

Agent *a* throws rat poison in the well in order to kill many inhabitants of the village. From this description we know that *a* is not merely someone who had a handful of stuff which fell into the well, neither did he want to get rid of it and thought this a handy way; no, he is a murderer who intended to kill people by this method.

4) Agent *a* acts in way *y* in order to obtain goal *h*, but (accidentally) obtains goal *g*.

Suppose that the situation is identical, but the agent *believes* that the stuff he holds in his hand is a life enhancing potion and *desires* to benefit the people in the village. This would make his action different in so far, that he did not *intend* to murder the people in the village: he might be termed a murderer *de facto*, but not *de jure*.

These four situations describe four events that are physically indistinguishable. Exactly the same behaviour occurs. The difference between these situations (or descriptions of the situations) is present in the intentions (i.e. beliefs and desires) of the agent. Furthermore, it is possible that only two actually different events were described: the event that can be described by formulations 1-3 and the event that can be described by 1-2 and 4. For a given event it is hard to say whether one of those descriptions is *the* correct description. This might very well depend on the particular question someone wants to get answered (e.g. 'How did rat poison get into the well?' - 'Because *a* threw it into the well.' or: 'Why did so many people die last year in that village?' - 'Because *a* threw rat poison the well in order to murder them.' etc.).

This kind of difference in description is what is meant, when one says that actions are intentional under some description; the intention makes that difference. *That* it *is* an action and not mere behaviour is established by the presence of *some* intention. *What* action it is, is (partly) determined by *what* intention is actually present.

I will give a second example in which the relevant intentions are directed at social facts. This does not provide an analysis of institutions yet, but it shows how social facts can be relevant to agent-actions and vice versa.

1) Agent *a* exhibits behaviour *x*.

   Suppose that agent *a* exhibits the following motor-behaviour. His arm holds a pen and through a complex movement a complex ink-mark is created on paper. This is a description of *behaviour*.

2) Agent *a* acts in way *y*.

   Agent *a* puts his signature on the bottom of a densely written paper. This is an action, whether this action stands on its own or it was an action within a particular context and for a certain goal is left open.

3) Agent *a* acts in way *y* in order to obtain goal *g*.

   Agent *a* puts his signature on the bottom of a contract and thus closes the deal. This action was done with the intention to sign a contract and thus formalise a relationship with one or more agents.

4) Agent *a* acts in way *y* in order to obtain goal *h*, but (accidentally) obtains goal *g*.

   Agent *a* puts his signature on the bottom of a paper of which he believes that it is a non-committal letter and his signature only confirms the receipt of something. Unbeknownst to him it was a contract and he has thus unintentionally signed a contract. *De facto* he has signed a contract but (hopefully to him) not *de jure*.[14]

Institutions are, at least in part, constituted by the actions of (collectives) of intentional agents. In order for an action to be part of some institution, the intentions of the individual should somehow be described with reference to that institution. How this should be done is a difficult question, because, as we have seen, an institution need not be *explicitly* recognized as such by the agent who does the acting. In order to understand the

[14] The precise outcome of the case would depend on many factors, but in contract law, normally the 'intention' of signing a contract is needed for a contract to be valid, although it might be hard to prove that the contract was signed unintentionally. There is thus a distinction between the legal facts of the matter and the legal criteria of evidence.

notion of institution we should add additional concepts to the concept of intentional action.

However, we have a first way of distinguishing institutions or institutional action from mere behaviour, namely through the notion of action. Behaviour of individuals, whether or not they are (intentional) agents and whether or not they can be seen to behave as a group, is never institutionalised, because, by definition, it is not intentional under any description.

3.2. Pattern of action/system

The second constituent of this notion of institution involves the idea that we are talking not of individual actions, but of *patterns of action*. This is true in two senses. On the one hand the type of actions falling under an institution are not one-off actions, but are repeated: they take place regularly (this point is relevant under the heading of stability as well). On the other hand an institution is not a one-man show, but consists of collective actions.

Patterns of action of a collection of individuals make up a kind of system; or better: if they make up a system, we have reason to conclude that we can speak of an institution. Systems can be observed in a sense. Not in the same way that a (solid) physical object would be observed, but rather as we would observe a process.

In order to be able to set up a systematic method of research, we should define what we mean by the notion of a system. A classic definition of 'system' is the following: 'A complex of elements in interaction being of an ordered (non-random) nature.'[15] The point of this definition is to simultaneously address the point that we are talking of an agglomeration of individuals – intentional agents in our case – and their interrelations – social relations in our case. A second definition is useful as well: 'System. An organized whole made up of components that interact in a way distinct from their interaction with other entities, and which endures over some period of time.' (Anderson & Carter 1974, 164). This definition is useful because it gives us a natural way to distinguish the system we are interested in a certain case, because the 'distinct interaction' in this definition can

[15] From Bertalanffy (1968) *General systems theory*. Quoted by Heifner (1999, ix).

be understood in terms of the agentive intentions that are relevant for a particular institution. This connects the notion of system to the notion of action.

This way of defining systems gives us opportunity to connect the notion of agentive action as discussed above to the wider social context, because the 'relevant description' of actions can and should involve this social context. So, now a provisional definition of institution would be something like 'A collective pattern of action', where this collective pattern is relevantly included in the intentions of the agents acting in that collective. This can be implicit or explicit, unconscious or conscious.

Possibly, this provides a characterisation of social groups from the perspective of agents that regard themselves as such and act accordingly. But does it actually fix a notion of social facts in general and institutions in particular? It seems that we need more than this, which will be made clear by way of an example. It may be a collective pattern of action that everyone – or a large portion of people – opens an umbrella when it rains. This activity might even be *coordinated* because it would be ill advised to do this unthinkingly and with no regard to people around you. As such, however, this is not a *social* activity and in that sense it does not fall under a social institution. The action, although intentional, can be explained in terms of individual intentions that may be somehow coordinated. This type of collective action might even be called a collective *habit*. Suppose now that, *if* someone opens his or her umbrella when it rains, this is supposed to be a black umbrella if he or she is in London. This latter example does introduce a social context and might be part of an institution. But first I will discuss what kind of additional elements are present in an institution; then we shall be able to see the distinction between what may be called social patterns of action and institutions.

The characterisation of social facts as conceived here goes back to the ideas of Max Weber. He characterises acts that are directed at other agents in a meaningful context as social acts (Weber 1968, 300-301). An example he gives concerns the difference between two bicycles colliding, which is (the result of) agentive action and also concerns several agents, but is, as such, not a social act, whereas the subsequent argument between the two agents in that accident is a social act.

This conception of social facts is quite minimal and might not be sufficient to account for all types of social acts, but it seems to be a necessary condition. This is, for our purposes, the most important, because the notion of social facts that concerns us here should be able to encompass many different kinds of social facts, acts and relations.[16]

3.3. Social enforcement

One way to put the difference between a notion like collective habit and institution is to say that the notion of institution is not merely a *descriptive* notion, but also a *prescriptive* notion. Within an institution you are *supposed* to act in a certain way and the norm that this supposition refers to is a social norm.[17] For institutions that are put down in terms of laws this is most easily seen. Legal institutions do not merely describe how people behave in certain societies in certain situations, but they prescribe (or forbid) actions as well.

Again we should take the intentional aspect of action into account. The actions that we want to call social and, in that sense, fall under an institution, are not merely (collective) habits, but are 'normative' in some sense. In the following definition of institutions this is very explicit: '(…) social practices that are regularly and continuously repeated, are sanctioned and maintained by social norms, (…)' (Abercrombie, Hill et al. 2000, 180). So, the 'relevant intention' is directed at some norm or rule. The point is that actions that are part of an institution are *enforced* in some way, or even the institution itself is enforced. The normative force of the institution is essential to it, because satisfying some norm or rule is part of the action that falls within it. The point is that from the collective or social point of view non-rule following (or non-normative) behaviour would be individual action and not social action.

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[16] This minimal view can be elaborated in different ways. Current important philosophical views on this topic are held by John Searle (1990; 1995), Michael Bratman (1993; 1999), Raimo Tuomela (1995) and Margaret Gilbert (1989). Some of these views stick as much as possible to the mentioned minimal view, others argue that more is needed for full blown 'social-acts'. For purposes of this article I limit myself to the minimal view.

[17] In other words, it is a hypothetical imperative where the antecedent refers to a social context. This is different from a hypothetical imperative where that antecedent refers to a goal some agent might have; the latter would be a norm of rationality or means ends coherence. This is reflected in the following characterisation of institutions: 'Institutions impose social constraints on individual behaviour. They are shared rules that are supported by various enforcement mechanisms' (Kiwit, Mummert et al. 2000,1).

Again the distinction with physical facts is instructive. The physics of a situation constrain my actions in various ways. For example, if an agent wants to cross the street he can only do this if he is physiologically able to walk, there are no large obstacles blocking the way and if there are no speeding cars close enough that might run him over. These are physical facts. The constraints they pose for behaviour are *objective,* in the sense that they cannot be avoided by only using the intentional capacities of an agent.

Social facts also pose constraints for action, but in a different way. In a sense their constraining force is subjective. The point is as follows. If an agent stands at a crossing with traffic lights and the pedestrian lights are red he is constrained in his actions. The constraint is, however, *not* similar to the physical constraints. A red light *means* that he *should* stop there, but there is nothing that (physically) stops him from ignoring the traffic light. In fact, red traffic lights are often ignored.

But there are two ways in which an agent cannot ignore these (social) constraints. In the first place an agent's individual set of reasons for (not) doing something is partly formed by social facts: Someone's upbringing might cause him to hesitate psychologically before ignoring rules like this; the (social) rule has been imprinted, as it were.[18] In the second place there are social sanctions associated with ignoring such social facts. These may range from 'raised eyebrows' and being reprimanded (which have indirect, psychological effects) up to up till being fined (or worse) for a breach of rules. But, one must realise, these social sanctions are not necessary consequences in the same sense as physical results are necessary consequences. They are mediated by the intentions of one's fellow social beings (people *need not* reprimand someone for walking a red light and police officers *need not* fine him –and in practice not all breaches of rules are sanctioned).[19] Physical facts are not that 'forgiving': If someone crosses a road the moment a car is driving 50 km/h at that very place, there is no intentional mediation whatsoever concerning the result.

Social sanctions introduce a *rule* into behaviour. This makes the fact that a line of behaviour is followed intentional with regard to that rule. So the actions of agents within

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[18] The precise mechanism of which is also studied in social psychology.
[19] It may be the case, of course, that some consequence of a physical event does not occur *necessarily*, but rather *probabilistically*. However, in this case the event would still not be intentionally mediated.

an institution are *intentional under the right description* (i.e. containing reference to a rule). This description may or may not be explicitly and in every detail known to the acting agent, as long as the action can be (correctly) described as being part of the institution. We may make this ‘unconsciously being part of an institution’ explicit in the following way. The person in question might behave in general according to the institution, but not (explicitly) know this. But there may be evidence that a rule is being followed, because the agent does allow himself to stand corrected at occasions when the rule was not (or incorrectly) followed. The fact that he accepts correction may be seen as evidence for the fact that there was implicit knowledge of the institution. A well-known example are the rules of grammar. Many people cannot reproduce those rules, but do follow them. This can be seen by the fact that many people do accept correction when they make a mistake.

It should be realised that the notion of social enforcement is no simple notion to handle and involves several parameters; it is therefore hard to operationalise in practice. What is meant by the strength of enforcement can differ in several ways. In the first place an institution can be more or less strongly enforced by being *always* enforced. Every breach of the rules is sanctioned somehow. The less percentage of cases in which this is done, the less strong an institution will be, up to a point where we would cease to speak of an institution at all. This, in turn, can be understood in two ways. In the first way this may be the case absolutely. In the second way this may count only with respect to cases that are observed by some agent. If we think of the example of the rules of traffic these points come down to the following. The higher percentage of violations of traffic rules is sanctioned, the stronger this institution is enforced. But this can be understood in two ways. The percentage may be taken to indicate the absolute chance that some offender is sanctioned, but also the chance that, on some observation of an offence (say, by an authority), the offender actually is sanctioned.

In the second place, an institution can be more or less strongly enforced through the severity of its sanction. The more severe the sanction is, the more strongly enforced an institution may be said to be. I won’t give an example of this. It will be clear that these features have a complex interaction together that might defy quantitative analysis of *the* strength of institutional enforcement. However, they should be reckoned with within an analysis of some particular institution.

The third aspect of our discussion shows how to connect the patterns of action to the idea that institutions are not merely descriptive, but also prescriptive. A *rule* needs to be followed, a social rule. Now we have a definition that reads: An institution is a (collective) pattern of action that is socially enforced.

3.4. Stability

Finally some pattern of action needs to be in place for some time in order to call it an institution: it needs to be recognisable as such. How long this will take is not definite and will differ from case to case. The main reason that we need some reference to stability is that we need some reference to the idea that an institution is an observable social structure that can be identified through time and is persistent through time. Up to a certain extent, this idea is expressed by the idea of a pattern or a system earlier, but explicit reference to a *measure* of stability is useful for the following purpose.

Institutions, although they need to be recognisable through time, can also change; they are not rigid. Change can come from without or within, but in either case it is implausible to say that in all cases of change the institution goes fully out of existence and a new institution comes into existence.

In addition to this, introducing a measure of stability helps delimit an array of types of institutions, from the very stable to the unstable. Of course, the boundary between extremely unstable institutions and not-an-institution (anymore) may be hard to draw, because of the vagueness of the notion of stability. It is useful, though, to be able to refer to such an array, because this can bring together very different types of social structures in the category of institutions, while simultaneously having an account of differences between institutions.

Our complete definition of ‘institution’ thus becomes: An institution is a collective pattern of action that is socially enforced and has some measure of stability.

3.5. A range of institution-types

Before I apply this notion to several different social sciences I need to make one thing clear. The definition introduced here allows for a (quite broad) range of institution types. Types of institutions can be distinguished in many ways; I have indicated two central variables above. One might also think of the number of participants/members or the goal of an institution as additional distinguishing factors.[20] For our purposes it is enough to realise that these distinctions define a concept that has several dimensions of determination, which make it a multidimensional concept.[21]

In this chapter I stick to the two mentioned variables. On the one hand there is the point that institutions have different measures of stability. The measure of stability can differ from institution to institution with extremes on either side (the boundary between a very low stability and no institution at all will be vague, as was remarked earlier). On the other hand, the measure of enforcement can differ as well with extremes on either side (and again, the boundary between very weak enforcement and no institution at all will be vague).

Examples of very stable and strongly enforced institutions are often the (formal) laws of a country. Many laws don't change often and are enforced quite explicitly.

There are also, however, relatively *unstable*, yet strongly enforced institutions. Think of a newly created organisation with very strict by-laws that are immediately and fully enforced. After a day or two, for some reason or other (not necessarily this strict enforcement), the organisation is disbanded. Examples can be found in newly formed

[20] Cf. Scott (2001, 51-52) for a delineation of functions along 'three pillars': regulative, normative and cultural-cognitive. Another distinction that is often made is between explicit (codified) and implicit norms or rules, which is somewhat similar to the distinction between formal and material approaches to the subject matter mentioned in section two. There is no absolute better or worse distinction, this rather depends on the particular goal of some analysis.

[21] Which is a very natural way to analyse concepts if you accept a version of the statistical theory of meaning. Meaning can then be analysed in terms of general conceptual spaces (modelled by, e.g. attractors in cellular automata). This idea has its roots in Quine (1961), Wittgenstein (1984) and was developed popularly in philosophy, for instance in Churchland (1979), Churchland (1995).

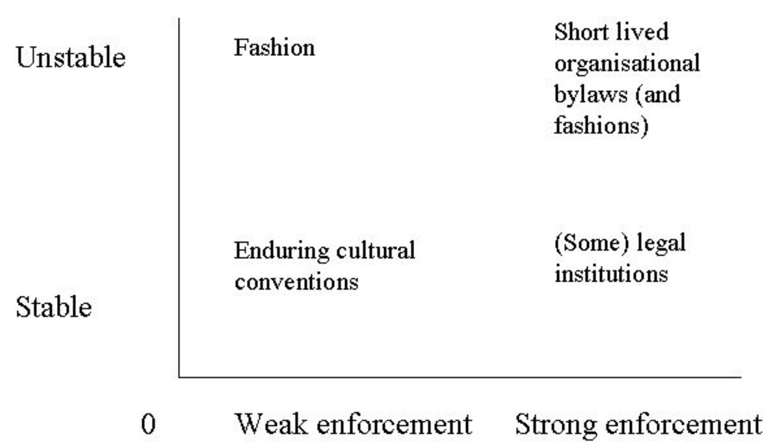
political parties. Many of them are formally created and created with strict rules, but many of them also cease to exist very quickly.[22]

Social or cultural institutions can be very stable. Think of the conventions about addressing people or about the way you eat. Not all of those conventions are enforced strongly though, some are, some are not (this also differs across different social groups and contexts).

Some cultural institutions can be unstable, such as momentary fashions. Again, such conventions or institutions can, but need not be strongly enforced.

It may be said that institutions can be of many different types and kinds and the examples given here by no means exhaust the possibilities. Also similar institutions may differ in kind from society to society and group to group, as the diversity in the institutions of marriage and fashion possibly best illustrate.

Very schematically we can put this in a graph:



[22] Political parties in the Netherlands often start up in this way, by writing formal statutes, which then are regarded as the act of forming the party. However, the far majority of (new) political parties hardly survive their first year (cf. Schikhof, forthcoming).

4. Institutions: Unification and bridges between disciplines in the social sciences

In this final section, I present some ways in which several different approaches to the idea of institutions, discussed in section two, can be brought together by departing from the core notion presented in the last section (e.g. because it is the intersection of the different notions of institution). This exercise shows that the definition presented above is a useful, albeit not the only possible, way to think of the notion of institution. Then I will show how this core notion can be useful in solving some of the methodological problems also mentioned in section two.

I talked about the opposition between institutions as enduring social *structures* versus relatively fluid social *relations*. Sometimes institutions are seen as the 'entities' that keep a society together, they are explained by and explain in turn enduring features within society. These stable and enduring aspects are mainly represented by the idea of stability in my definition. Stable patterns of action are structures of sorts. Explanations of institutions and using institutions can be connected to the idea that in my view much partaking in institutions is implicit or tacit. If an institution is in place, agents will often unconsciously follow the rule and such patterns of action are (psychologically) hard to break. On the other hand, institutions are not analogues of physical objects, but are *social* structures. Institutions still consist of *agents* acting under some description; the description involves inter-agent relations.

Also I pointed to an ambiguity in, or a difference between, notions of 'institution' that refer to something like 'organisations' and institutions that refer to 'rules'. Both types of use can be understood within the framework of the present definition. Organisational structures like 'a factory' or 'an institute' or 'political party' do not fall under this definition, because they are not (purely) 'patterns of actions etc.' of people. However, if we think of the work that the notion of institution is supposed to do, we see that the actions of agents and the constraints posed by (social) rules are invoked in explanations that refer to institutions, while organisational structures and the physical properties that are invoked relevantly to such descriptions can be fruitfully described as 'factual' boundary conditions for which the notion of 'institution' needs not be invoked.

With respect to the activities of the people in the organisation that are directed at some goal, we can speak of an institution and of institutionalisation. The point is that an organisation is not itself an institution, but rather contains aspects of institutionalisation.[23] The present definition allows for the structural aspects that are directly relevant to actions *in* the notion of institution – i.e. the systemic aspect of organisations – while leaving out aspects that can best be investigated by other means. On the other hand, the present definition shows how the general idea of (social) rules can be incorporated into a more systematic view allowing for comparisons with views in other disciplines.

If we accept that the definition that was developed in this article is a useful *core concept* of institution, we can use it to derive some methodological benefits. Such methodological points were also mentioned in section two. The present discussion does not intend to solve all the differences noted there, but rather to show in what direction we may be thinking when approaching such problems.

In the first place this investigation suggests a relation between formal and material approaches to institutions in the social realm. If we consider the definition of institutions, there is nothing that says that the relevant rules of behaviour should be formal or formalised. Informal rules can be rules nevertheless. In legal theory, one of the interesting features of investigation is precisely the relation between legislation, which is often formal, and practice (e.g. in the form of jurisprudence and policy of governments).[24] The balance between these factors is of importance and much theory is devoted to their interrelations. Especially the limit of 'interpretational freedom' by judges is an important issue. Examples are questions such as whether laws can or cannot be interpreted teleologically and/or historically, instead of literally.

In organisation theory we find something similar. On the one hand, there is the fact that organisations are built up by creating it on paper: by writing up by-laws. On the other hand, organisations are not identical to their formal rules, but rather consist of

[23] In some definitions of institution the idea of 'goals' is given a prominent place. E.g. 'An institution is an enduring set of ideas about how to accomplish goals generally recognized as important in a society.' (Johnson 2000, 157). My remarks above about implicit or tacit conforming to institutions indicate that I do not agree with this definition of institutions as valid in all cases.

[24] Systems of common law emphasize the role of jurisprudence even more.

individuals that act in cooperation. But what is the relation between the material behaviour and the formal rules? Are the formal rules *only* normative or do they contain descriptive elements as well? Here a comparison to interpretation methods in legal theory may be useful, because such questions are highly relevant in the interpretation of laws. In the legal domain such problems are discussed and the results of these discussions may be used in organisation theory. For one thing, investigators of organisation run the risk to fall into the mistake of only researching the formal aspects of organisations, such as the by-laws and the minutes of meetings. This is obviously too limited a research, as is immediately made clear by the comparison between formal laws and organisations.

A second example concerns views on the interpretation of social facts and processes. For instance, concerning the difference between collective action theory and cultural anthropology. In action theory a central heuristic principle is the principle of rationality. It states that an interpretation (and evaluation) of action should preserve the rationality of the actor as far as possible (cf. Quine 1960; Davidson 2001). The full extent of this principle can be subject to discussion, but that need not concern us here. The point here is that an interpretation and evaluation of action takes place within a framework of rational action, because that provides for a systematic method of action interpretation. A classic straightforward example in linguistics is the use of the double negation. Certain groups of English speaking persons habitually use the double negation in their speech. Assuming their language on a par with "high English" would render their speech totally meaningless and their behaviour irrational. Interpreting them rationally means to interpret them differently and not to interpret the double negation as a positive. This principle of rationality then is often used as a starting point for investigating institutionalised behaviour within and across various groups (and cultures).

In Cultural Anthropology it is often argued that cross-cultural generalisations cannot be made, because there is an important incommensurability between what activities (etc.) *mean* across cultures.[25] Under that assumption the 'rationality-criterion' cannot be applied at all, because that criterion presupposes some kind of *prior*

[25] Whether this only is true for 'radically differing' cultures or also for 'subcultures' within a single society we may leave open.

understanding of the actions (institutions/cultures), which would render the interpretation circular. Some conclude from this that anthropological research should be approached in a particularistic manner, because broad generalisations are impossible in this domain. Intensive fieldwork is required to learn something of that culture and the results cannot be generalised or cannot be theorised about, but should be put down in individual narratives or stories.[26] Institutions thus are not seen as (social) structures that can be explicitly described but are shown in the meaningful behaviour of the participants of some culture. They are implicit, unconscious sets of basic rules defining the outlook of the participants of some community that cannot be described from the outside, but should be 'lived in' in order to be described correctly (however meagre).

Although cultural anthropology will remain a different discipline from collective action theory, I think that their view of societies and or collective behaviour can be brought closer together by taking my core definition into account. In both cases the interpretation of *intentional agentive action* is relevant. In both cases there is conformation to rules. Collective action theory tends to generalise too soon by concentrating on one *super rule,* namely a rule of rationality. One should also take into account the individual (or cultural) *meaning* of behaviour (and its outcomes). This is implied by my action-theoretic approach to institutions. Anthropology, on the other hand, tends to overemphasize the *differences* between 'meaningful actions'. The approach from action theory shows that there are many aspects about actions that can be generalised about, without having fully detailed knowledge about specific cultural peculiarities, which makes cross-cultural comparisons between institutions possible.

A third example is somewhat similar to the second and concerns the point, noted above, that on the one hand, cultural institutions are implicit behavioural structures and grow on a society, as it were, and on the other hand they can be explicitly and consciously designed. As was said in the discussion about intentionality, intentions may or may not be consciously held by an acting agent. The same can be said about collectively following a rule. An agent or group of agents may explicitly follow some

[26] This point of view has also roots in metaphysical views on the nature of societies, but I disregard those aspects here.

norm or set of rules, but also implicitly. Evidence for this can be found in the possibility that an agent, or several agents accept to stand corrected for some breach of rules, without being able to give the rule explicitly and in detail.[27] Cultures and cultural rules may be said to 'grow on a society', which means that they are possibly (but not necessarily) implicit and unconscious.

Other institutions on the other hand (such as legal institutions or organisational by-laws), could very well be designed and form explicit and conscious institutions. However, the distinction between 'grown' institutions and 'designed' institutions may not be as big as it seems. For instance, not all people participating in some designed institution may be conscious of those rules; just as some people participating in some 'grown' institution may be conscious of the rules. Also, a designed institution may look good on paper, but be different in practice, a practice that is not always made fully explicit. So, although the two types of institutions differ, there are also important similarities that make a comparison between them (and thus between fields of investigation) potentially fruitful.

This concludes my argument that a possible core definition of 'institution' – proposed and explained was the definition of a stable collective pattern of action that is socially enforced – not only provides an opportunity for interdisciplinary understanding across social sciences on a conceptual level, but also some opportunity for methodological bridges across disciplines.

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[27] Cf. section 3.3.

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Interdisciplinarity in the social sciences

Bateson's problem, analytical philosophy and anthropology

Abstract

Two formal questions are relevant with respect to the question of unifying the social sciences: First, would this synopsis really be sufficient for the satisfactory and exhaustive investigation of the study objects, amongst which culture, society *and* mind*? Second: Would it be possible to safeguard the* consistency *of the proposed synopsis?*

A rather underexplored area in socio-cultural anthropology and the social sciences in general is the body of questions that deal with the logical hierarchy of concepts. One of the few authors who paid attention to these questions was Gregory Bateson. The (im)possibility to distinguish levels in concept like 'culture', 'society' and 'mind' is relevant for the question whether or not the social sciences can be unified. As Bateson pointed out, logical hierarchy problems have been rarely systematically investigated, while it would have been most appropriate to do so with the help of analytical philosophy starting from Russell's paradox.

If concepts can be used as a reference for a studied object *as well as the environment* in which one studies, *the question of how to deal with levels within social sciences becomes pertinent. I denote the quest for a theory dealing with the hierarchy of analytical levels in social sciences as* Bateson's problem. *Analytical philosophy, with such scholars like Russell, Gödel and Curry, can be a great help when investigating*

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Baseton's problem. In doing so, I intend to demonstrate that Bateson's problem is of such a nature that something like a common foundation for all social sciences is not very likely.

1. Introduction

When we talk about the possibility and desirability of unity within the social sciences, we must occupy ourselves not only with methodological problems concerning certain academic disciplines vis-à-vis each other, but also with the question of how much space for unity the subject matters of the social sciences actually grant these sciences. Starting from a situation of diversity in the social sciences, it would perhaps be possible to unite these sciences into a synopsis or a total social science by formulating a body of methodological principles and fundamental assumptions from which the presently existing content of the concerned sciences could be derived. Two formal questions are relevant with respect to such a process: First, would this synopsis really be sufficient for the satisfactory and exhaustive investigation of the study objects, amongst which e.g. *culture*, *society* and *mind*? Said otherwise, would the unified total social science be *complete* in the sense of covering all that there is to be investigated? If not, this would leave open the possibility that apart from the incomplete generalisation we proposed, another social scientific approach would have to be formulated, which would create the problem of unification all over again. The second question is: Would it be possible to safeguard the proposed total social science's *consistency*? An inconsistent body of assumptions and principles is usually not considered a convincing scientific way of describing the world.

The question whether the currently existing social sciences are compatible at all is often formulated with an eye to the results of the natural sciences, which somehow appear to have succeeded better in achieving mutual compatibility, even when there exists a considerable degree of labour division. The state of affairs in the social sciences leads to

a sense of awkwardness. Howarth (2004, 229) describes this sense thus: ‘The scandal of the human and social sciences is their interminable dispute.’ But as yet there does not exist a convincing motivation why one should agree with this assertion of scandal. It could well be that the study objects of the social sciences themselves allow for less mutually compatible theories than those we usually encounter in the natural sciences.
It would not be convenient to invoke solely the social sciences themselves for providing insight in this topic, since this would simply result in begging the question. Therefore I propose to draw on some ideas taken from analytical philosophy. With special attention to the topics of *completeness* and *consistency*, I intend to show that analytical philosophy can be used to shed light on the problem of unity and diversity in the social sciences. Analytical philosophy studies formal aspects of such topics as ‘deductive theories’, ‘sets’, ‘signs’, ‘meaning’, ‘concepts’, and the like. Since it is not itself a social science, analytical philosophy perfectly suggests itself for studying the formal aspects of theorising in the social sciences.

Since I am mostly familiar with cultural anthropology, the focus in this article will be on applications of analytical philosophy on that science. Nevertheless I believe the investigations to have wider relevance. In many respects I follow ideas of Gregory Bateson (1904-1980), an anthropologist with a keen interest in related topics, such as human communication, learning, and the interaction of humanity with its environment. I will discuss a theory Bateson referred to in several places, namely Bertrand Russell’s theory of logical types, but I will also extend the discussion by dealing with the paradox named after Haskell Curry as well as Kurt Gödel’s first incompleteness theorem. These topics all deal, in different ways, with problems of completeness and consistency. Crucial for Bateson’s approach as I will discuss it, is that they are connected to the investigation of *hierarchies of levels*. Amongst others, I will shortly discuss ideas of Renato Rosaldo and Christoph Brumann from this viewpoint. Even though their approaches seem quite different at first sight, they appear to have a lot in common with respect to type-theoretical questions.

My use of analytical philosophy shall be twofold: In the first place it is intended to deliver an example of interdisciplinary research in so far as the application of analytical philosophy to social sciences is concerned. In the second place I shall try, with some understandings of that application, to say something about the unity or diversity of the social sciences. Despite its high potential for interesting investigations, the use of analytical philosophy has been far from widespread among the social sciences. Dumont (1983, 228) has stated that Bateson was one of the few anthropologists who clearly saw the necessity of recognising a hierarchy of levels. Since Dumont's statement, however, not much has been done to explore further the lines of thought Bateson had been setting up. Though discussion of levels is not always absent (cf. Howarth 2004, 241), the related problems of completeness and consistency in the social sciences are far from exhaustively explored yet.

In the sequel, I will first give an exposition of the basic ideas in which Bateson was involved; second, I elaborate a bit on these ideas; and third, I discuss what such studies could tell us about the unity or diversity of the social sciences.

2. Bateson's problem: The distinction of hierarchies in the study of social phenomena

Frequently in his writings, notably *Steps to an Ecology of Mind* (1972) and *Mind and Nature* (1979) and the 'Epilogue 1958' of *Naven* (1958 [1936]), Bateson refers to Russell's theory of (logical) types (Russell & Whitehead 1910, 39-68). Russell developed this theory as a mathematical device to avoid the emergence of a paradox, which he himself had discovered in what is now known as *naive set theory*. Russell's paradox results from the possibility in naive set theory to define some class *A* as *the class of all and only those classes that do not contain themselves as an element*. If one tries to answer the question whether the class *A* does or does not contain itself as an element, one gets into trouble, because if it would, it would not, and if it would not, it would, according to the definition of *A*. The theory of types avoids the emergence of this paradox through the prohibition of mentioning in an object's definiens an object that is of the same 'type' as the definiendum. Thus the definition of *A* above is forbidden, because it treats 'class of classes' on equal footing with 'class'. As a definiendum, *A* appears as a class of classes, but the paradox arises because it is judged in the definiens as a mere class along with other classes. Basically, the theory of types introduces a *hierarchy* between types, blocking Russell's paradox because now classes are of a different, lower, type than classes of classes, and the definition of *A* above is declared invalid.

Now Bateson was convinced of the relevance of the theory of types to the work of scientists. For a good understanding of what he meant, it is worth to quote the following *in extenso*:

> 'It is the observer who creates messages (i.e., science) about the system which he is studying, and it is these messages that are of necessity in some language or other and must therefore have *order*: they must be of some or other Logical Type or of some combination of Types. The scientist's task is only to be a good scientist, that is, to create his description of the system out of messages of such logical typology (or so interrelated in their typology) as may be appropriate to the particular system. Whether Russell's Types "exist" in the systems which the scientist studies is a philosophical question beyond the scientist's scope – perhaps even an unreal question. For the scientist, it suffices to note that logical typing is an inevitable ingredient in the relationship between any describer and any system to be described' (Bateson 1958 [1936], 294; emphasis original).

The search for a suitable - if any - application of a theory of types in social scientific theorising will be labelled here as 'Bateson's problem'. In the quotation Bateson clearly sees logical types as relevant only for the *theories* to be formulated, not for the *subject matter*. However, the suggestion that it is beyond the scientist's scope to investigate the existence of types in the systems to be studied is debatable, if it is considered that the absence or presence of logical types in the studied systems would in itself be an important feature (i.e. of those systems) to be studied.

There are at least two reasons to think that if we occupy ourselves with Bateson's problem in proposed theories, we must also do so with respect to the subject material that these theories are about. First, there is the possibility that theories in some way adequately describe studied phenomena (this is, after all, what is intended). Theories are meant to represent the matter they are about: In the terminology of Bateson (1958 [1936], 294-95), they are aggregates of messages of description, which are *mapped* on diagrams of logical types. Consequently, one might expect that if studied phenomena are successfully mapped on some type-theoretical framework, one could look for hierarchies in the subject matter corresponding to the hierarchies in that framework. Indeed, Bateson himself gives the following judgement: 'The typological relations between the messages of a description could also be used, subject to rules of coding, to represent relations *within* the system to be described' (1958 [1936], 295; emphasis added). So even in the case logical types could not be found in the studied material by direct observation, they might be present in some theory about that material, and one could conclude from the theory (assuming it is adequate) that logical types must be present in the studied material. On the other hand, if the concerned theory is unable to accommodate logical types, one can ask whether this inability is actually due to the set-up of the studied system.

A second reason for dealing with the problem of logical types within the subject material is that the distinction between the studied systems and the theories about them may not be as absolute as Bateson suggests in the quotation above. Again an example of the contrary is given by Bateson himself, namely when he deals with the application of ecological theory: '[T]he problem of how to transmit our ecological reasoning to those whom we wish to influence in what seems to us to be an ecologically "good" direction is itself an ecological problem. We are not outside the ecology for which we plan – we are always and inevitably a part of it' (Bateson 1972, 512).

The observations made thus far have relevance with respect to the social sciences when it is considered that an inadequate understanding of hierarchies (or blurring thereof) in theories and/or studied systems might hamper valid descriptions of social reality: 'If social scientists would keep the levels straight, they would not use phrases like "society forces the individual" or "history teaches" ' (G. Bateson, in: M.C. Bateson 1994 [1984], 207). Even if theories in social sciences are formulated in more differentiated ways than Bateson indicated here, this remark reminds us that we should thoroughly investigate hierarchy questions concerning their consequences for describing and explaining the phenomena studied in the social sciences. I will take up this issue in the sequel, recapturing some of Bateson's thoughts and try to indicate how some further steps could be taken in the research he set out.

3. Bateson's problem with respect to social collective notions: Some examples

Bateson (1979, 229) states with respect to logical types: 'The name is not the thing named but is of different logical type, higher than that of the thing named.' If one accepts this statement, then for a consistent use of the theory of types one must remember that the thing named not only *is not* but also *does not contain* the name (since this would be contradictory to the acceptance of the name's being of higher logical type than the thing named). It is at this point, however, that difficulties emerge. Let us consider some study objects in the social sciences in which the distinction of hierarchies is particularly tough, namely such objects referred to as 'culture', 'society', 'cognition' or 'mind'.

The concept of 'culture' (or such notions like 'social setting' and 'context') has a long history within and outside anthropology. It was and is used both to *denote* collectively certain people's behaviours, thoughts and the products thereof, as well as to *explain* these phenomena. Following Bateson, one could expect problems with the explanatory power of theories using 'culture' if one could not 'keep the levels straight' (i.e. could not avoid the inappropriate blurring of different logical types) in the application of the term. But first it must be investigated whether it is possible at all to straighten out the levels. Indeed this is rarely systematically done in anthropological research. To show that such investigations do have relevance, I now wish to discuss some examples derived from quite different currents in anthropology.[2]

For instance, the cultural-relativistic doctrine that ‘a culture can best be understood in its own terms’ is often invoked without any indication that Bateson’s problem might crop up here. In the doctrine it is assumed that the best descriptions and explanations of a culture are provided by the terms, ideas and assertions of the concerned culture itself (Herskovits 1972, 38), and not by the terms, ideas and assertions derived from other cultures. The methodology of cultural relativism is considered a basis for an *objective* understanding of cultures (Herskovits 1972, 38, 40-41). An opposing view would have it, that it is exactly a view informed by a foreign idiom, which makes it possible to give an accurate account of a culture. For participants using only their indigenous cultural idiom, it would be difficult, if not impossible, to give an explicit analytical account of their own behaviour patterns and beliefs (Benedict 1946, 13-14; Van Baal 1974, 1-13). There is a dilemma here: In the latter view, the ‘levels are kept straight’, because the messages of the theory about the studied culture do not contain elements of that culture. On the other hand, a situation is created in which researchers using some cultural idiom would be able to study any culture on earth except the one consisting of that same idiom; something unwanted in the light of comparative studies. On the other hand the cultural-relativistic methodology violates the type-theoretical principle (as formulated above) that the *name* cannot be included in the *thing named*. By this violation it would introduce circularity in the methodology concerning both descriptive and explanatory aspects of cultural analysis; to explain a culture terms are used that originate from that same culture.

Bateson’s problem is not only relevant for cultural relativism, but also for more recent proposals for the study of culture. Renato Rosaldo, contributor to the famous *Writing*

[2] The examples below are really meant as illustrations of the point that Bateson’s problem emerges in widely diverging anthropological research approaches, not as a complete historical overview.

Culture volume (Clifford & Marcus 1986), writes for example that 'indigenous usage [of language] is always correct in its own setting' (Rosaldo 1986, 83). If the possibility that indigenous practitioners of indigenous language can make this judgement themselves is not ruled out (which Rosaldo indeed does not seem to do), people can justify any usage of language with the invocation of their 'own setting'. Just as with cultural relativism, a violation of the above-mentioned type-theoretical principle occurs. This is methodologically problematic, for it leaves aside the question of what counts as 'indigenous' or 'own' setting in the first place, something which cannot be answered by invoking the 'indigenous' and the 'own setting' again, since this would constitute a complete blurring of question and answer. (The judgement *about* what counts as 'indigenous' would be at the same level - be of the same type - of what is *invoked* as indigenous.) It also has quite some moral implications, for it prohibits those denoted as 'non-indigenous' to judge, evaluate, debate or criticise statements made by the studied people denoted as 'indigenous' who speak from the certainty of their all-validating 'own setting'.

Post-*Writing Culture* definitions of 'culture', sometimes intended as a rehabilitation of the concept after the distrust caused by the 'post-modern' movement in anthropology (cf. Brumann 1999), also mostly leave Bateson's problem implicit. Pascal Boyer (1999, 206), a representative of the school of evolutionary psychology, gives: ' "ideational culture" [is] the set of mental representations entertained by members of a particular group that makes that group different from others', a definition that does not prohibit the inclusion of mental representations about 'ideational culture' in the set of mental representations. Said otherwise, Boyer's definition allows for the inclusion of the definiendum in the definiens, again an instance of not 'keeping the levels straight'. Another proposal for definition, by Christoph Brumann (1999), takes on the form of a matrix. The proposal entails that rows 'stand for individuals' and columns 'for identifiable ways of thinking, feeling, and acting' – features (1999, S6), where it can be indicated whether or not a certain individual has, believes or practices a certain feature. '[T]he term ["a culture"] refers to an abstract aggregate, namely, the prolonged copresence of a set of certain individual items' (1999, S6).

Brumann suggests that individuals can be ascribed membership with respect to a culture according to their sharing of features in the matrix with others (1999, S6-S7). But he also allows features themselves to represent statements about individuals belonging to social collectivities, because ‘any observable feature can be included in [...] a matrix, including emic[[3]] categories […] and self-categorization’ (1999, S6n8). This means that while viewers of the matrix may ascribe membership of individuals concluding *from* information in the matrix, such judgements can also be included already *in* the matrix through a feature. Here the levels are not kept straight; in fact, the judgement of the viewers who draw conclusions from the structural aspects of the matrix may be inconsistent with the information represented by some single feature. It seems very strange, however, either to forbid that the matrix could be judged from the outside, or to block the possibility for features to refer within the matrix to self-categorisations also known in outside judgements. Clearly we have here an instance of Bateson’s problem (are cultural categorisations of a higher logical type than the features in the matrix or not?), but the issue is not seriously addressed in Brumann’s article.[4]

Francisco Gil-White (2001), like Boyer representative for evolutionary psychology, gives a clear recent variant of the view that subscribes to a distinction between observer and observed. He argues that people associate in groups in accordance with modularly conceived brain functions: ‘[Such] “modules” [can be] described in a cognitive sense […] as a set of processing biases and assumptions activated by the domain-relevant inputs [e.g.] social groupings’ (2001, 517). Crucial to Gil-White’s approach is the distinction between ‘ordinary folk’, who are ‘naive essentialists’ (p. 516), and anthropological researchers, who are not (pp. 515-16). The theory of cognitive modularity is then supposed to explain why ‘ordinary folk’ are ‘essentialists’ who ascribe the cultural transmission of norms and behaviours to biological descent (pp. 518-19). However, it remains unclear how this theory

[3] ‘Emic’ is an anthropological term that indicates that conceptualisations of the studied people are concerned, instead of conceptualisations from the outside, such as those of professional anthropologists (called ‘etic’).
[4] Brumann (1999, S24) mentions the possibility of ‘a characteristic combination of nonunique traits [being] itself a unique meta-trait’ but only as being proposed by a ‘die-hard sophist’ (1999, S24).

should be related to the minds of the researchers, because even if they would be essentialists, they could no longer be considered *naive* by the nature of the research as being *about* essentialism. In this sense the theory creates its own exceptions.

I conclude that Bateson's problem is a recurrent phenomenon in anthropology (even if implicit) for which solutions have been offered during the decades either by accepting the equality between observer and observed, with the danger of confusion, or by distinguishing between them, at the price of the observers' (and perhaps others') exclusion from the scope of the proposed descriptions or theories. The diversity of the approaches contrasts with the commonality of the problem they have to deal with.

4. Stepping across levels: Curry's paradox and Gödel's incompleteness theorems

I have tried to make clear in the previous paragraph that Bateson's problem is a relevant topic for proposals from different theoretical outlooks. Blurring of levels, often made visible through self-referential constructions, seems to be hardly avoidable, yet it leads to considerable even if unnoticed difficulties, which one may expect hampers the straightforward development of theory. Is there a way out? To investigate this question I shall look at two topics as a further elaboration of Bateson's research. The first concerns Curry's paradox, the second Gödel's first incompleteness theorem.

Apart from the blurring of levels, Bateson seemed to believe that also the logical operation of *negation* was responsible for the possible emergence of paradox, for he wrote that 'no paradox can be generated [in iconic[5] communication] because in purely [...] iconic communication there is no signal for "not" ' (Bateson 1972, 291). However, Haskell Curry discovered a paradox, an elaboration of Russell's that does not use negation. Several versions of the paradox exist (e.g. Curry, Feys & Craig 1958, 258-59;

[5] With 'iconic' communication, Bateson means communication in which there exists some direct link between objects and their images, in contrast with what he calls digital communication, where links are arbitrary.

Visser 1989, 643), but they have in common that an object is treated as an equivalent of an implication in which the same object acts as an antecedent. Any arbitrary statement can then be derived using the implication, a paradoxical result since the arbitrariness means that for any derived statement also its opposite can be derived.[6] Curry's paradox only aggravates the difficulties encountered already with Russell's, since the self-inclusion or self-reference of certain objects cannot always be avoided in social life. For example, if the objects and people referred to by the term 'a culture' can be studied at all, it cannot be excluded that people who might be included in the reference of that term study those objects and people themselves (see the discussion of Brumann 1999 above). But then the idea of *explaining* or *deriving* facts by using 'culture' (or similar concepts) as a term in social-scientific theories is doomed to fail because by Curry's paradox such explanations are totally arbitrary. The proposal that 'culture does not exist' does not seem to be helpful because this would mean that the term 'culture' really refers to nothing at all in whatever interpretation, a rather trivial way out of an interesting question of semantics. Viewed in the light of Curry's paradox, the consequence of the existence of such collective items like culture would be that these items are actively preventing the formulation of any straightforward and consistent explanatory theory about themselves. A possibility to be investigated is that one considers certain *parts* of the collective items in question, and proposes (hopefully consistent) theories about those parts. The problem arising then is immediately relevant for our research on the possibility of unifying the social sciences; one would wish to avoid the result that the facts we encounter in social life would all be explainable separately with separate theories while at the same time collectively with an overall theory they would not.

[6] For a set-theoretical version, see Krajewski (1981, 23):

$$\begin{array}{lll} X = \{a \mid (a \in a) \rightarrow p\} & \text{definition} & (i) \\ \text{If } X \in X\text{, then } (X \in X) \rightarrow p\text{, hence } p & \text{from } (i) & (ii) \\ (X \in X) \rightarrow p & \text{from } (ii) & (iii) \\ (X \in X) & \text{from } (i) \text{ and } (iii) & (iv) \\ p & \text{from } (iii) \text{ and } (iv). & (v) \end{array}$$

Here no a priori conditions for the contents of p exist, so the derivation implies that any arbitrary statement can be proved starting from the definition of X alone.

We have seen that Bateson stressed the importance of the theory of types in order to avoid paradoxes in mathematics and theories in the social sciences. There does exist a formal procedure in mathematics, however, which violates the principle that 'the name should not be included in the thing named', but nevertheless does not produce paradoxes or inconsistencies. I am hinting at Kurt Gödel's method of using numbers to *code for* expressions of Peano arithmetic and then use them in that same Peano arithmetic.[7] Peano arithmetic ('arithmetic' for short) is an axiomatic system that defines the natural numbers (0, 1, 2 etc.). It had been a problem for mathematicians to find out whether this system could be shown to be *complete*, i.e. to demonstrate that from the axioms making up arithmetic, all true statements of arithmetic could be derived (and all false statements could be disproved). Gödel showed in 1930 that this is not possible. He demonstrated that there exists at least one statement of arithmetic, which is true but cannot be derived from the axioms (Gödel 1988 [1931]).[8]

A simple example will suffice here to see what Gödel's method entailed. Suppose we write down some sentence in the language of arithmetic, like:

$$2 + 2 = 5.$$

This sentence, which is false, must be considered *not provable* from the axioms of arithmetic. But the statement ' "2 + 2 = 5" is not provable' constitutes a statement *about* the sentence '2 + 2 = 5', not in the language of arithmetic itself, but in a *meta*-mathematical language. Gödel showed, however, that the notion 'provable' could itself be written as an arithmetical operation. Gödel demonstrated further that arithmetical expressions could be coded for using natural numbers (in fact, the coding numbers are now called 'Gödel numbers').[9] Expressions like *provable* and *coding number of* are themselves expressible in

[7] More precisely, Gödel dealt with Peano arithmetic as incorporated in the system of Russell and Whitehead's *Principia Mathematica*.
[8] See e.g. Nagel and Newman (1959); Smullyan (1992); for a less technical exposition see Schultz (1980, 135-42).
[9] Nagel and Newman (1959, 76) give the example of the sentence '0 = 0', which could be rewritten by writing for '0' *6* and for '=' *5*. The Gödel number coding for '0 = 0' is then obtained by using prime numbers, yielding $2^6 \times 3^5 \times 5^6 = 243{,}000{,}000$.

the coding device Gödel established. Gödel then showed there exists a sentence in arithmetic with coding number *h*, its corresponding meta-mathematical statement running like:

> The sentence with the associated coding number *h* is not provable.

The sentence cannot be proved from the axioms of arithmetic, which is what it indeed asserts itself, but meta-mathematically we know that the sentence is true. In this way arithmetic is shown to be incomplete. In fact, if the sentence would be provable, arithmetic would be inconsistent and useless as a mathematical theory.

Gödel's result provides an interesting possible elaboration of Bateson's insightful investigations. In the 'Epilogue 1958' of *Naven*, Bateson introduces the following state of affairs: 'Take [...] a system *S* of which we have a description with given complexity *C*' (1958 [1936], 299) and then writes almost at the end of the book: 'Here is the central difficulty which results from the phenomenon of logical typing. It is not, in the nature of the case, possible to predict from a description having complexity *C* what the system would look like if it had complexity *C* + 1' (1958 [1936], 302). Indeed, prediction is likely to be impossible, but with Gödel one does have a glimpse of complexity *C* + 1 when looking from *C*. Gödel avoided paradox through his ingenious numbering system in which the distinction between the language of arithmetic and the meta-language is kept intact. But by actually showing that such a numbering system is indeed possible, Gödel's method provides an alternative for Bateson's (1979, 229) use of the theory of types as a prohibition to present a name as of equal (or higher) logical type as the object named. Even though Gödel numbers should not be regarded as 'denoting' the arithmetical sentences (Shanker 1988, 216), it is still the case that they both *code for* sentences and can be *incorporated in* sentences. In fact, in Gödel's unprovable sentence one is incorporated in an object of the code for that very object, without any paradoxical consequence of the 'Russell' or 'Curry' sort. There is also the recognition that the expression 'This sentence is not provable' can itself be read as a meta-mathematical judgement about its corresponding arithmetical sentence, which we know, through the coding system, to be stating its own unprovability. The truth of that judgement can be established with meta-mathematical considerations, but not with the devices provided by arithmetic itself. However, when the meta-mathematical language would be formalised, this would in turn contain unprovable sentences (i.e. only provable by yet another more powerful language, and so on).

This recognition has given rise to debates as to whether or not the human brain, or its functioning, can be modelled with fully formalisable theories (see e.g. Penrose 1990 [1989], 407-09). It is impossible to give here a discussion of these debates (which still seem to be undecided), but at least it shows that Gödel's result is relevant for social sciences dealing with cognition and learning. For example, evolutionary psychologists tend to adhere to a view of the human mind as a *computational* device (Samuels 2000, 15), clearly a statement for which Gödel's discovery has relevance. But Gödel is also of interest for the present article in its relevance for what it might have to say about the possibility of unifying the social sciences.

5. Theories and study objects

Judging from the above investigations concerning Bateson's problem, I think it not likely that a total and absolute distinction between theories and study objects in the social sciences is feasible, in so far as such theories are supposed to be unambiguously *about* the study objects. Rather, study objects with powerful enough communication devices are likely to be able to refer to the theories proposed about them. Such theories are consequently somehow incorporated in their own study objects. If this does not lead to the logical chaos of Curry's paradox, then at least it will have to be concluded that any proposed *consistent* theory falls short of providing explanations for all the observed phenomena concerned, if the theory belongs to the observational scope of itself. In fact, intended study objects (e.g. people's cognitive capabilities; human culture) might be considered in some ways to be about the theories just as the theories can be said to be about these study objects, but this recognition leads to the realisation that no formalisable type-theoretical distinction is possible between theory and study object in the way Bateson meant. This casts doubt on the ability of social scientific theories to provide for an adequate and exhaustive explanation of their study objects.

This is not just a matter of the peculiarities of self-reference. The possibility for self-reference as encountered in Curry's paradox, or in incompleteness of the Gödel sort, are themselves possible by virtue of general features of the objects at hand. For example, theories may allow for a mixture of types where other theories do not, or contain a variety of axioms that are absent in simpler theories. Analogously, whether or not social sciences can be unified (in the sense of becoming a single consistent summary encompassing the existing social sciences), might very well be depend on certain characteristics inherent in objects like *culture* and *society* (e.g. anthropology; political science) or *mind* (e.g. psychology, pedagogy).

Perhaps this is not only confined to the social sciences. The issue of distinguishing between object and theory is taken up in physics by John Barrow (1998, 221-230). Since physical theories are mostly written in mathematical formulas, the incompleteness of mathematical theories could possibly imply that nature could not exhaustively be explained using these formulas. For the social sciences the matter seems even more pressing: If the statements *about* the study objects can be *represented* in the study objects, this could well imply that these study objects contain phenomena that are *part of* the study objects but are *not explainable* using information from these very study objects, while *adding extra information* would not ultimately solve the requirement of full explainability. For example, one could not claim that 'culture explains human behaviour' or 'psychological dispositions explain human behaviour' if we would really mean *all* behaviour, but also not when one adds these claims to an assertion 'culture and psychological dispositions explain all of human behaviour'. So Bateson was, I think, right to say 'If social scientists would keep the levels straight, they would not use phrases like "society forces the individual".' My point here is that his comment could be extended to an analysis of what can be said about levels, or blurring thereof, in society itself.

A practical example may illuminate the above. Van Dijk (1991, 44) defines *discourse analysis* as ‘a multidisciplinary approach to the study of language use and communication [which] emerged in the late 1960s and early 1970s from different but related developments in anthropology, ethnography, linguistics, poetics, psychology, micro-sociology, mass communication, history, political science, and other disciplines in the humanities and social sciences’. In discourse analysis, many already existing different disciplines are added up to form a new scientific entity, which is then itself restricted to the study of ‘textual or conversational structures’ of which the ‘explanatory frameworks [...] derive from the analysis of the relationships between “text and context”’ (Van Dijk 1991, 45). Van Dijk himself tries to understand the role discourse plays concerning (the reproduction of) racism (cf. 1991, ix).

That a fully integrated explanatory framework is not easy, however, may be clear from the comments of Condor and Antaki (1997, 320). They distinguish between two approaches in the study of discourse, one in which researchers use psychological mechanisms of individuals as explanans for the perception of themselves and others, and one in which researchers try to understand individuals’ cognition, using social interaction as an explanans. Combining these two approaches, with opposite chains of explanation, may perhaps enlarge the area of investigation, but the distinction between explanatory levels has become quite uncertain. The mutual embeddedness of the explanation chains may result in a circular theoretical apparatus, thereby trivially capable of explaining any result.

Such uncertainty with respect to explanatory levels is only aggravated when it is considered that researchers may actually publicly discuss their results in newspapers or other media that they otherwise use as study objects of their research. A solution to the resulting circularity may be that one acknowledges that discourse analysis cannot deal with all discourse as far as explanation thereof is concerned. Let us consider Van Dijk (2003), who uses aspects of his research to back up his supposition that a certain well-known Dutch author is behind a racist pamphlet pubished in 1990, the writer of which being only known by a pseudonym (cf. Shadid & Van Koningsveld 1995, 13). Van Oostendorp (2003a; 2003b) discusses some arguments of van Dijk that deal with style and word choice of authors and tends to the conclusion that Van Dijk's supposition is unlikely to be true, and this would throw into question the current state of affairs in discourse analysis as applied by Van Dijk. On a factual level, Van Oostendorp's publications could conceivably play a role in the eventual establishment of the truth or falsehood of Van Dijk's supposition. The point here, however, is to determine whether Van Oostendorp's writings can be seen as a genuine study object of discourse analysis. Can they function as confirmations or denials of Van Dijk's theories? If it is a task of discourse analysis to develop theoretical devices for analysing the interplay between texts and discrimination in society, a negative outcome (for Van Dijk's supposition) of Van Oostendorp's discussions that are *critical* about the present state of affairs in discourse analysis dealing with the reproduction of racism, should not be taken as *confirming* Van Dijk's discourse analytical theories as being an example of such reproduction (for example because those critical discussions would hinder research about racism - cf. Van Dijk 2003, 179).[10] I maintain that a critical discussion *about* methodological questions should not be rejected while using arguments of the very methodology that is reviewed, since such rejection would allow the methodology to be self-proving, analogously to the situation we have with Curry's paradox. A (hypothetical) positive outcome (for Van Dijk's supposition) within Van Oostendorp's writings should not be taken as an explanandum of Van Dijk's theory of discourse as reproducing racism either – for, *as* discourse, Van Oostendorp's writings would itself form a counterexample to the theory, since it would be discourse that does not reproduce racism.

In short, apart from mentioning the interplay between text and discrimination, Van Oostendorp's pieces also include references to discourse analytical arguments about that

[10] No other than methodological questions are at stake in Van Oostendorp's 2003a and 2003b pieces.

interplay. Therefore the pieces cannot be considered to be type-theoretically congruent with the usual study objects of discourse analysis in which the approach itself does not play a part. Being about aspects of discourse analysis, Van Oostendorp's writings constitute discourse outside the *explanatory* range of Van Dijk's discourse analytical ideas. Results on the factual level (e.g. Van Dijk's supposition about the identity of an anonymous writer) are not necessarily consistent with results on a higher level (Van Dijk's application of theories *about* facts). We can be quite sure that comparable situations exist for discourse analysis as a whole. This is not a weakness of discourse analysis; on the contrary, when Van Oostendorp's pieces, or similar ones, would actually be explained by discourse analysis this would either be self-proving or self-contradictory. As with Gödel's unprovable sentence, leaving them out of the explananda of discourse analysis, however multidisciplinary, brings the blind spots necessary for the consistency of the approach.

6. Conclusions

I have been discussing aspects of Gregory Bateson's ideas about the application of Russell's theory of types to the social sciences. I intended to show that the problems for which this theory was supposed to be a solution are relevant for the social sciences, even though the solution itself does seem to hold for at least some objects studied within the social sciences. I have applied certain arguments about these matters to the problem of distinguishing study objects and theories and the relationship of this problem with the possibility for unifying the social sciences.

It is quite possible that study objects of the social sciences, like *society* and *mind,* are such that they prevent their own being fully and consistently explained (if we take explanation to mean that all events occurring in these objects should be derivable from some deductively constituted theory). To establish this would itself be a positive discovery about these items, and one might expect fruitful contributions from analytical philosophy when investigating their logical properties. Applying analytical philosophy in social science is certainly not commonplace. For instance, as far as I know earlier applications of Curry's paradox to definitions of 'culture' are absent in anthropology, despite its rather obvious relevance. Many problems are also related to such topics as *reference* and *semantics*, as well as set-theoretical ideas.

As for the unification of the social sciences, one might try to formulate a consistent overall theory derived from them (assuming that these sciences are internally consistent). However, it is possible that this overall theory still leaves research questions uncovered, when its explanatory devices together have formal properties that prevent analytical completeness. On the other hand, it may appear that the combination of, say, anthropology and psychology is providing methods that cover all possible research questions, but turns out to be inconsistent. The requirement of consistency would then lead to a renewed division of research attention into separate parts, resulting in a situation of relativism in which research is separated according to methodological points of view. These viewpoints may then be constituted as traditional academic disciplines or crosscutting research schools.

If the latter situation would arise, it could in the first instance be taken as a 'failure' to unify the social sciences. But there can only be a justified desire for unifying if the research objects allow it. If research objects themselves imply that they can only be approached relativistically, it is not a failure to actually do so. In fact, the peculiar constitution of human *culture* and *mind* may very well be at the base of the possibility to do social scientific research at all. There could have been no theory about them if these objects would themselves 'keep the levels straight' all too drastically. There is also nothing unscientific about a non-unified social science. At least some hypotheses will still be formulatable and testable; it is only unlikely that a general body of axioms will be discovered from which such hypotheses could be mechanically derived for all social scientific research.

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First and second order unification in the social and human sciences

'The unity of a plot does not consist, as some suppose, in its having one man for its subject. An infinity of things befall that one man, some of which it is impossible to reduce to unity; and in like manner there are many actions of one man which cannot be made to form one action. One sees, therefore, the mistake of all the poets who have written a *Heracleid*, a *Theseid*, or similar poems; they suppose that, because Heracles was one man, the story also of Heracles must be one story.'
Aristotle, *Poetics*, 1451a16-21.

1. Theories and disciplines

Any project of unification of the sciences, natural or social or otherwise, requires a prior agreement about the construction to be put on the term 'science'. We might begin by agreeing that science, whatever else it may be, is a theoretical enterprise. Along with the theory usually goes a practice, computational, experimental and the like, so that we have the familiar contrasts between theoretical and experimental physics, for example, or between ethnography and ethnology in anthropology. But it is also possible to look at physics, anthropology and the like not only as theories with their accompanying practices, but also as disciplines, to be studied and professed. 'In a science the ultimate object is knowledge, about the world or about society, and what practice there is follows from the knowledge (or serves it, e.g., in experimentation), whereas in a discipline the object is an activity, carried out, of course, in a suitably disciplined way' (Caws 1993, 351). The assignment of intellectual (and especially of academic) work to various disciplinary categories tends to serve administrative and organizational purposes, as in teaching, publication and the like. The question of how the disciplines are established and how they correspond to distinct bodies of knowledge is itself a challenging one (see Caws 1999).

The problem of unification in science may therefore take two distinct forms: that of theoretical unification and that of disciplinary unification, one of which may be more tractable than the other. In what follows I shall argue that theoretical unification in the sciences is more readily achieved than disciplinary unification, and that the problem of unification in the social sciences can be clarified by the invocation of the concept of the 'human sciences' and its relation to, and contrast with, that of the natural sciences.

A theory can be - in some sense must be - something that an individual knower has. Given the origins of the term in the name of the Athenian *theoros* (the official observer at the games or the consultation of oracles) it represents a 'way of looking' at the world, or part of it, that has a certain formality and status. An isolated theoretician (Newton, Einstein, Darwin, Freud) can come to have a more inclusive way of looking, taking in under a single aspect parts of the world formerly viewed as disparate; this is one of the main historical mechanisms of unification. But a discipline can hardly be sustained by an individual; the disciple needs a teacher, the process of acquiring the discipline involves a collective practice. The two are closely connected, as suggested above, in that one object of the discipline, in the cases that are of interest to us, is the acquisition of the theory. Disciplines ensure the persistence of theories, passing them on from one generation of scholars to the next. And it is usually in a disciplinary matrix that theories meet challenges, which may result in their modification or demise. Disciplines, however, move more slowly than theories - it may be a long time before the theoretician's linking of domains is translated into a merging of disciplinary programs.

Perhaps this need never happen - running a discipline is a very different matter from entertaining a theory. It's not just that there are battles over academic turf, not all of which are worth fighting, but there are also historical and traditional issues to be considered, as well as questions of intellectual *Lebensraum*. The sociologist Randall

Collins has claimed that in the history of a discipline - he is talking about philosophy, but the point is more general - only three to six major positions (and by implication major figures identified with them) can coexist at any one time. He calls this the 'the intellectual law of small numbers' (Collins, 81). If the claim holds it follows that the conflation of two disciplines should demote up to half a dozen major figures to secondary status. The main casualty here would not be their wounded egos but the availability of role models to a new generation, not to mention research funding and all the other appurtenances that go with leadership in a field. In practice we probably need not worry: the outcome of the merging of two theories when a new higher-level theoretical structure subsumes them both is often not one discipline but three - the two original ones and a new one that springs from their intersection. When the physical basis of biology became firmly established both physics and biology survived in their old form, but they were joined by a new speciality, biophysics. To adapt my epigraph from Aristotle: a multitude of things may happen to one theory in disciplinary space, some of which it may be impossible to reduce to unity. We need not make the mistake of supposing that because a theory is one theory, the discipline of that theory must be one discipline.

2. First- and second-order unification

Another way into this question is to think of the unification of disciplines as first-order unification, at academic ground level, and the unification of theories as second-order unification, at a higher level of abstraction and generality. The unity of what there *is* seems unproblematic - we call it the universe, the turning of all things towards one thing. That is as abstract and general as it gets. But the unity of what we *know* - of what we can say about what there is - is not so easily grasped. We come across different bits of the universe at different times, we apprehend them partially, we don't see their interconnections at once or perhaps at all. What we come to know is unified in a rough and ready sense, in that it's always one person's knowledge, but elements of it may conflict with one another. Depending on the knower's tolerance for cognitive dissonance this may not matter very much, but the possibility of achieving a coherent and connected structure of knowledge, an ever more complete picture of what there is, remains one of the lures of human cognition. Henri Bergson used to go Aristotle's dictum ('All men by nature desire to know') one better: 'we should all begin, as mankind began', he would tell his lycée classes, 'with the simple-minded but noble ambition to know everything'.

In spite of its apparent naivete this proposal of Bergson's seems to me quite feasible, on condition of making a few distinctions - such as Galileo's distinction between intensive and extensive knowledge, or the distinction I draw (following George Sarton and no doubt others as well) between direct and indirect knowledge. Extensive knowledge is the knowledge of cases *seriatim*; intensive knowledge is knowledge of principles that subsume many cases at once. God knows everything extensively, and we couldn't aspire to that, but we can know some things intensively as well as God does: mathematical propositions, for example, or principles (as opposed to the details of the cases that fall under them). Direct knowledge is knowledge that we can reliably communicate on demand, without having to look it up or ask anyone. We know only some propositions in this way, but if we're suitably connected or supplied with appropriate resources we can know all the other propositions (all the ones there are to know, which is one plausible construction of 'everything') by looking them up or asking someone who already knows. I call this indirect knowledge. Just as in the Galilean case we can't know it all at once, or, extensively, ever, but there's none of it we couldn't know if we put our minds to it and had enough time. Carrying this off requires a couple of special kinds of direct knowledge, which I call exemplary and fiduciary. Exemplary knowledge is enough direct knowledge of what we're asking about to be able to make sense of the answer, and fiduciary knowledge is knowledge of the reliability of the sources we consult or the people we ask.

The sources and the people will normally belong to disciplines - that's how we know what to look up or whom to call. The resulting knowledge, if it is of anything complex, will come from several disciplines, and the inquiring knower will find herself in command of relevant aspects of each of them, working together to throw light on the object of inquiry. Being able to marshal this knowledge and bring it to bear demonstrates a form of theoretical unification, in the person of the knower. T.H. Huxley used to give a lecture, 'On a piece of chalk', in which he canvassed all the bits of science one would have to know in order to understand the origins and properties of a morsel of chalk from the Sussex Downs. I have adapted this idea for students in interdisciplinary courses as what I call the 'chalk game', the challenge being to specify what resources of what disciplines they would have to call upon to explain as completely as possible some arbitrarily chosen object or event: a cigarette lighter, a piece of money, a ball-point pen. From the point of view of the game the differences between disciplines are less important than their common relevance to the explanatory process. This goes as well for the differences between the natural-scientific disciplines and the social-scientific ones - in the cigarette-lighter case the physics and chemistry of ignition and combustion will find themselves cheek by jowl with agricultural economics and the psychology of addiction, to mention only four out many relevant topics that will eventually have to be called in.

3. Why unification?

Such personal and eclectic gathering-in of the disciplines is unification of a kind, but a far cry from what the unity-of-science movement had in mind. The drive to unification has ancient roots as well as modern exemplars, running from Aristotle and the two Bacons, Roger and Francis, through the French encyclopedists to Auguste Comte and John Stuart Mill, on to the Vienna Circle and Herman Hesse's Glass Bead Game, up to recent attempts to develop a TOE or theory of everything. In its most elaborated form it has tended to concentrate on the physical sciences, for reasons that I will try to spell out (I have not forgotten that my topic is the social sciences, and I promise to come back to them.) But it is not always so obvious what the drive is driving at.

Why have people wanted a unified science? Their motivation may at different times have been intellectual curiosity, or aesthetic satisfaction, or pedagogical economy, or administrative convenience, or even political liberation. The latter in fact seems to have informed the concept of unification most prominent in the literature of the middle of the last century: Neurath and his associates in the Vienna Circle really did think that a unified scientific conception of the world, that would subsume or displace the heterogeneous cultural and metaphysical conceptions that had landed the West in what they rightly saw as an untenable situation in the wake of the First World War, would have the effect of freeing mankind from inhumanity and encouraging the spread of democracy.

The principles of unification on which the Vienna Circle rested its hopes were however strikingly narrow. Carnap's pamphlet of 1934, *The Unity of Science,* ends with the assertion that '... the statements and words / the facts and objects of the various branches of science are fundamentally the same kind. For all branches are part of the unified Science, of Physics' (Carnap, 101). He means to include the social sciences in this claim, which rests however less on a conviction that social facts are merely physical than on an argument about language, namely that the language of physics is the only one that can lay claim to scientific status. *If* there is to be social *science*, *then* it will be part of physics. The argument concludes that there is only one possible intersubjective language, namely what Carnap calls 'the physical language'. 'Science is the system of *intersubjectively valid statements*. If our contention that the physical language is alone in being intersubjective is correct, it follows that *the physical language is the language of Science*' (Carnap 1934, 66-67).

This aspect of the unity of science movement is worth remembering because it was on to something important. We really do need intersubjectively valid statements, and it really is hard to come up with them outside the physicalism of the natural sciences. Also that physicalism goes a very long way towards explaining our world. But it runs up against a fatal limitation (or, from another point of view, a liberating possibility) when it encounters non-physical, non-material, non-perceptual objects. It was a failure to recognize that there could be such objects (as objective, after their own manner of objectivity, as physical objects), or that they could be described in valid intersubjective terms, that hampered the ambitions of the unity of science movement. I shall be calling them intentional and especially 'cointentional' objects, preferring the latter description to 'intersubjective', though the idea of an intersubjective language - that is, one whose statements are valid for many if not all subjects - can be carried over from the logical empiricist program. Curiously enough Carnap had set out, six years before his essay on the unity of science, to construct science on a basis that would have been friendly to the possibility of such objects if only he had been able to carry it through. In *The Logical Structure of the World* he attempted to establish a 'constructional system' on the basis of the 'immediately given', and specified various levels of construction, principally the physical, the psychological, and the cultural. But he insisted on starting from sensory, mainly visual, experiences available to an individual observer (the 'autopsychological basis') and proceeding by an unbroken stepwise ordering that would eventually yield objects at higher levels. All the objects would belong to the same domain: 'there is only one domain of objects and therefore only one science' (Carnap 1967, 9). In the end the project did not succeed - the higher-level object could not be reached in this way - and from this promising, almost phenomenological beginning he reverted to physicalism.

What is appealing about physicalism is the way in which it provides a covering model for almost the whole history of science up to the emergence - as yet tentative and misunderstood - of the human sciences, a model that still functions for the most recent discoveries in the natural sciences. It offers a view of that history as a process of aggregation, starting from modest beginnings. The modesty is important: science has suffered from the immodesty of some of its enthusiasts. Laplace has a lot to answer for with his demon, whose hubris opened the scientific enterprise to charges of arrogance and inhumanity, when it is in fact perhaps the most sublime of human achievements. One modest beginning finds Galileo rolling balls down inclined planes in Padua (not dropping them from towers in Pisa). He has a simple experimental apparatus, in which he has slowed down the vertical component of gravitational acceleration by making the incline sufficiently shallow so that elapsed times are sufficiently long to be measured by his water clock. And he devises a simple mathematical expression that relates the time after release of the ball to the distance covered in that time. That's all: it is the confrontation and reconciliation of two different entities - the behaviour of the rolling ball and the mathematical expression of that behaviour. Not as easy or obvious as it looks - as Galileo makes clear, people had been offering mathematical explanations of physical behavior for a long time, they just hadn’t bothered to do the exact measurements.

Limited episodes like this were repeated by different scientific workers investigating different natural phenomena - by more and more workers looking into more and more remote corners of the world. The process still continues. Some philosophers of science have denied that this represents anything like a linear progress. Certainly there have been and may yet be wrong turnings and retreats, but the pattern is of a steady aggregation of contiguous patches, as it were, with occasional imaginative leaps that bring apparently unrelated partial aggregations into a larger inclusive one. William Whewell called this 'consilience' ('jumping-together'), a term that has been picked up and run into the ground by the sociobiologist Edward O. Wilson (Wilson 1998). Perhaps the most brilliant move in the process of unification by aggregation was Newton's linking together of Kepler's celestial with Galileo's terrestrial mechanics, by his audacious and as it turned out correct conjecture that the moon might be a falling body. The patient matching of smaller or larger elements of the world with smaller or larger elements of theory (which are also in the world) has been the strength of the natural sciences. But the resulting edifice covers only what it covers, and is unitary only to the degree that remote as well as proximate connections have been established and inconsistencies smoothed out.

4. Gaps and continuities in the natural and human worlds

This admission of systematic incompleteness in our knowledge of the natural world is not, however, a concession to the view that that world may be a patchwork of interconnected structures with real gaps, a 'dappled world', to use Nancy Cartwright's evocative phrase. 'Our' world is epistemologically dappled all right, but that does not mean that the natural world (of which our world is only a partial aspect) is ontologically dappled. There may well be some ontological dappling at the quantum level, but the claim that 'for all we know, most of what occurs in nature occurs by hap, subject to no law at all' (Cartwright, 12), seems inconsistent with the overall reliability of ordinary physical things and processes. If *most* of what occurred in nature were haphazard this would surely have repercussions at the everyday macroscopic level, and yet most of the explanations and predictions of the natural sciences in what I have called the 'flat region', in which space is Euclidean, time Newtonian, and causality regular, prove reassuringly dependable. (I cannot mount here a full defense of this claim against the melodrama of the Kuhnian view that these regularities have all been overturned - it is only far from the flat region, in the direction of the very small or the very fast or the very remote, that they have been displaced by their revolutionary successors. For a brief treatment of this point see Caws 2005, 1925.) Where ontological dappling enters with a vengeance is in the domain of the human sciences, of which more below. The parallel between physics and economics with which Cartwright opens the argument of *The Dappled World* seems to me to blur a distinction which, while often taken to be discredited, remains in my view of cardinal importance, although in its familiar form - as a distinction between the natural and social sciences - it is certainly problematic.

When I say that theories are also 'in the world', this mode of being 'in' the world is mediated by the subjectivity of the scientist. Some sociologists of science have therefore concluded that, as Steve Fuller puts it, 'the study of science should be conducted so as to be subsumable under a unified social science, which in its search for regularities and causal mechanisms will provide the basis for science policy' (Fuller, 3). Some ambiguities need to be teased out here. Is the 'study of science' doing physics, for example, or doing the philosophy or sociology of physics? Is science given as an object to be studied or is it the world that is given and 'the study of science' a way of talking about the study of the world? In the first case we would be aiming at knowledge of science, in the second at scientific knowledge of world. 'Knowledge,' as Fuller goes on to say, 'exists only through its embodiment in linguistic and other social practices. These practices, in turn, exist only by being reproduced from context to context, which occurs only by the continual adaptation of knowledge to social circumstances' (Fuller, 4). 'Science' is undeniably a social practice, and the question whether such practices are susceptible of scientific treatment, and if so according to what model, is certainly worth asking. It is a question about social science. But the fact that scientists require to have been 'socialized' (learned language, learned to apply standards, learned to cooperate) does not mean that the objects they study have become infected with the social.

I would be inclined to modify Fuller's second statement in two small but significant ways: 'knowledge exists only through its embodiment in *particular individuals and its expression in* linguistic and other social practices. These practices, in turn, exist only by being reproduced *from individual to individual*'. The process of this reproduction I call a (special case of) instruction, a term I use in a technical sense to mean all the processes (genetic, epigenetic, experiential, experimental, cultural, autonomic) by which we acquire the inner structure that mediates our dealings with the world and one another. It remains the case however that what is embodied and expressed in scientific cases may not all be the same sort of thing, that knowledge of chemistry and knowledge of choreography may involve differences of content and level not easily subject to unification. At the very least there are differences of practice and hence of discipline. The socialized thinker can think what there is and how it is (roughly, the theory), and can also think what we know and how we come to know it (roughly, the discipline); the prior social conditions thus cancel out as far as the contrast between the two is concerned. The whole program of the sociology of science in fact seems to me properly directed to the disciplines, and to leave the theory untouched.

5. Physicalism and its limitations

To revert then to the theoretical unification of the natural sciences, from which we may hope to learn something about how a parallel (but very different) process might go on in the social sciences: this unification has depended, not to be sure on the reduction of everything to physics, but on the acceptance of an underlying physicalism. The Milesian ambition to have everything made intelligibly of one sort of thing was first realized, crudely but correctly, by the old atomists in the fourth and fifth centuries BCE; it was reformulated by Gassendi (1592-1655), given its now familiar form by Dalton (1766-1844), and has survived all the way to contemporary particle physics and molecular biology. The split in physics between relativity and quantum theory (an ironic opposition of the two domains Newton had united) does not affect the efficacy of this unifying principle. What is hard for people to realize vividly enough, and to remember, is the sheer size of the numbers involved. An order of magnitude clue is given by Avogadro's number, the number of atoms in a mole of a given element or compound, a mole being originally the number of grams of a substance equal to its atomic or molecular weight. The standard is now the number of atoms in twelve grams of carbon 12, which is roughly 6×10^{23}, or six followed by 23 zeroes. Twelve grams of carbon is about the equivalent of one piece of well-burnt toast. This proliferation of particles holds everywhere, in the stars and the sun and the ocean and the brain; it has been estimated that the number of particles in the physical universe is on the order of 10^{85}.

Forgive all this numerical stuff - there's method in it! Everything in the world, from neutrinos to neurons, consists of particles, singly or in combination. There seem to be plenty of them to make up all the things there are. But then comes an awkward fact: particles can constitute sound waves but not utterances, texts but not their meanings, works of art but not their appeal, machines but not their uses. They can account for what Kenneth Pike called 'etic' properties but not for 'emic' ones (Pike, 37). They can get us to the top of the physical world, but not even to the bottom of the social world - or that part of it whose constitution and perpetuation depends on the mediation of thought. This is where consilience breaks down. And yet we are still in the same universe, and there is no evidence that there is any other basis for the being of what there is than particulate matter and the spatial fields associated with it. How do we get from the matter to the thought? People sometimes think of this as what has come to be called the 'mind-body problem'. I take there to be two versions of this problem: the 'mind-dead body problem' and the 'mind-live body problem'. The latter dissolves if we suppose - as seems reasonable, given the complexity of the central nervous system, including the brain - that mental activities are natural functions of sufficiently complex live bodies. The former then reduces to what I call the live body-dead body problem - that is, the problem of the biology of complex self-replicating systems. This used to be just as big a problem as the mind-body one, which attracted solutions all the way from theology to vitalism - but hardly anyone thinks of it as a serious problem any more.

The route from matter to thought, then, seems obviously to lie through the stage of organized thinking matter that we call the brain. This does not mean mind-body identity in the philosophical sense - the utterance really is different from the soundwaves or the neurological events they stimulate. Still it is worth dwelling on the complexity of those events. I spoke earlier of 'the sheer size of the numbers involved' in coming to grips with the physical world, and similar considerations come into play here. The average human brain contains 10^{11} neurons, more or less continuously active. 10^{11} is a big number all right but it is the activity whose dimensions it is hard for people to grasp. Every brain event involves the passage of neurotransmitter molecules across synaptic gaps between neurons, all 10^{11} of which are multiply connected to many, many others. Exact numbers are hard to come by, either for the number of connections or the number of neurotransmitter molecules involved in each, but here's a telling anecdote from a lecture I once heard about just one molecule of one neurotransmitter, dopamine, whose docking at a post-synaptic receptor was simulated in a beautiful short film by Svein Dahl, a Norwegian neurologist. Dahl's film lasted four minutes. In his commentary he admitted that the details of this event were still conjectural, but he said he could assure us of two hard facts about it: first, that in real time it would have taken eighty picoseconds, or eight hundred-billionths of a second; second, that it had taken him four hours on the central processing unit of a Cray supercomputer to calculate its details. This is the sort of complexity that has marked every second of our cerebral activity since the womb and is still busily at work, with its inconceivable speed and multiplicity, as we read or speak.

6. Natural sciences, social sciences, human sciences

I would ask you to remember these two levels of complexity, of the physical world at large and of that part of it we are carrying in our heads, as I turn now to the social sciences proper and hence (finally) more directly to the topic at hand. The conventional boundary between the natural and the social sciences, while I take it to be necessary and not easily erased, is I think badly drawn. The social sciences suffer from what might rather melodramatically be called a crisis of identity, aspiring on the one hand to be like the natural sciences, realizing on the other that they are something quite different. Let me make a preliminary distinction between two aspects of the social sciences, which may be called respectively behavioral and intentional. The objects of the behavioral social sciences belong to same domain as the objects of the natural sciences - what in an as yet unpublished paper I have called the first (or materialist) ontology, understanding 'ontology' in Strawson's sense rather than in Heidegger's (that is, as an inventory of beings rather than as a theory of Being). They yield to empirical inquiry and generalization, often statistical but forming a basis for predictions of voting patterns, market behavior, and the like. The objects of the intentional social sciences, on the other hand, belong to - *but do not exhaust* - a different domain, which I call the second ontology. The second ontology is emergent with respect to the first, in a quite specific way: it is made possible by the capacities of the human brain, in particular the powers of apposition and intention, which together construct by reiteration the lifeworld of the individual subject and, in mutual exchanges with other subjects, the human world - a part of which we call the social world.

A whole theoretical development would be required here, going far beyond the bounds of this paper. Perhaps I can illustrate the essential point by a citation from Wilhelm Dilthey's *Introduction to the Human Sciences* (the *Einleitung in die Geisteswissenschaften* of 1883, in which Dilthey borrows from but radically revises Mill's concept of the 'moral sciences' and originates what I take to be a crucial and as yet not fully realized movement towards a developed idea of the 'human sciences'). Dilthey says: 'Mental facts are the highest boundary of facts of nature; facts of nature constitute the lower conditions of mental life' (Dilthey, 85). I called the first ontology materialist and it would be tempting to call the second idealist if it were not for the inevitable connotations of that term - we do live, informally speaking, in a world of ideas as well as things, but the ideas do not have the independent ontological status that 'idealism' suggests. It might be better to speak of 'second ontologies' in the plural, to avoid the error into which the original 'ideologists' fell, of thinking that ideas constituted a new natural kind - if geology, and biology, why not ideology? (There is no need here to go into the unfortunate subsequent history of the latter term. It may however be worth mentioning that the same error of reification was repeated later by the associationist psychologists, and has shown up even more recently among the theorists of 'memes'.) The whole edifice, the material base and the human superstructure (to borrow two terms that also have their old connotations, though I take them to be neutral enough to allow a different use) remains 'material' in the Milesian sense of 'matter', which in Aristotle's rendering is 'that of which all things that are consist, the first from which they come to be, the last into which they are resolved' (*Metaphysics* 983b8).

This Aristotelian formula is worth a moment's reflection: it allows for the possibility that non-material entities might 'come to be', and also for the possibility that the eventual resolution back into the material might be indefinitely postponed. I take it that this is what is happening with social and cultural objects - they require the material base (brains, soundwaves, texts and other artifacts) but can stay aloft, as it were, by being passed from subject to subject, from generation to generation, for a long time without being resolved back into it. (If all this talk of materialism makes people uncomfortable - because it seems to reduce or belittle the higher or spiritual side of human culture - I can only recommend the attitude of a wise old French biologist, Jean Rostand, who when asked whether he believed that thought came from matter replied 'Of course - but I have never pretended to know what matter is'.) The crucial phrase here is 'from subject to subject' - in the singular. I affirm here a radical individualism in the human and social sciences - not methodological, however, but ontological. Margaret Thatcher's *bon mot* - 'there is no such thing as society' - was trivially right but at the same time misleadingly wrong; the ontological base is in the individuals, but if the individuals conjointly construct society, then there it is: for each of them. Each of us has a *whole* language, a *whole* society, etc. - that is the condition for speaking the language, for functioning in the society. No two of us have identical languages or societies. We may take the natural world to be self-identical, so that the natural sciences (which we may grasp just as idiosyncratically) should in the long run be self-correcting. But there is nothing self-identical to anchor the human sciences.

The emergent second ontology obviously looks very different from the material ontology from which it emerges, and it would be remarkable indeed if the theories and disciplines that deal with it could be simply articulated with those that deal with the physical world. That is where the old unity of science went wrong, namely in supposing that there exists (or subsists) a domain of social objects as it were 'out there in the world' analogous to the domain of objects dealt with by the natural sciences, on to which the apparatus developed in those sciences can be transferred, mutatis mutandis, so that work can go on more or less as before. This is a tempting supposition because in our lifeworlds the two domains do interpenetrate and overlap; we move comfortably in both and do not notice profound ontological differences between objects that are familiarly associated with one another. We recognize this time as morning but also think of it as Saturday morning; we notice that we are in a room but also believe we are in London; we meet one another as fellow human beings but also acknowledge one another as colleagues. These are different cases and would require different treatments if their details were to be spelled out: the room is an artifact in a way that days and persons are not, and in all three cases the *names* of the physical objects or states are just as intentional as the day of the week, the city (*civitas* as opposed to *urbs*), or the academic title. But the essential distinction is that some names name objects that do not require human intentionality to be the objects they are, while other names name objects that do. And that is a distinction that blocks the easy transfer of methods from the natural to the human sciences.

7. Unification in the social sciences

What of unification in the social sciences - and pluralism? The remarks I made earlier about theories and disciplines seem to me to apply here: the various social-scientific disciplines have their own histories, their own heroes and founding figures (Adam Smith, Auguste Comte, Ferdinand de Saussure, and a host of others), their own learned societies, their own university departments and journals, just as the natural sciences do. What I have called 'first-order unification' seems to make as much or as little sense here as there, and to be subject to the same mixture of motivations. The question remains, then, as to whether there might possibly be a second-order or theoretical unification, a covering model that would do for the social sciences what physicalism has done for the physical ones.

Let me introduce one last large number - not that large, really, in comparison with the 10^{85} particles in the physical universe, but larger, I think, than we often remember or can fully take in, namely the number of human individuals - roughly 6.4×10^9, *each* with 10^{11} neurons, and also everywhere (in the biosphere anyway). It is what they do that makes the social or human world, just as what the particles do makes the physical world. You will notice that I am hedging here as to terminology - 'social' or 'human' - and I admit to a preference for the latter to describe the scientific theory that offers as I see it the best chance for a radical and unified understanding of the what I have called the intentional social sciences. Some, but not all, of the events in the human domain are mediated by elements of my second ontology; those that are not fall back into the province of the behavioral social sciences, which as I have suggested can be thought of as continuous with the natural sciences. The agents who participate in these events presumably *attend* to what they do, notice that they are doing it, but they often fail to *intend* it, except in the ordinary-language sense of setting out to do something gratifying and hence perhaps predictable. But the elements of the second ontology are through and through intentional, in the Brentano-Husserl sense - they are objects created and requiring to be sustained by human subjects, calling for interpretation rather than measurement and calculation.

The human sciences, on which limitations of time and space prevent me from expanding further, are as I see it conceptually prior to the social sciences. Society is one, but only one, of the objects human intentionality has created. Every step towards society, every step for the modification or improvement of society, every new idea or creation, happens outside society and cannot be explained in its terms. But they are all explainable, along with everything else that happens in the human world, in terms of the powers and acts of individual human beings (along to be sure with their mutual connections to one another). This is not a reductive move - the explanation moves up through levels that are emergent with respect to what underlies them. In a similar way what happens in the physical world is explained by its material constitution (and subject to the same disciplinary limitations). Some writers have tried to exploit the analogy by borrowing physicalist terminology: 'Man, the molecule of society, is the subject of social science', says Henry Carey (1872, 77), while Jacob Moreno develops a typology of 'social atoms' (Moreno 1960, 57). This reading of the analogy is radically flawed: the objects of the human sciences are not human individuals, but all those things that human individuals create and sustain and share: languages, values, institutions, laws, cultures, myths, histories, theories, religions, performances, games, fictions, philosophies, and other practices and discourses. But the reading is not altogether pointless, because it reminds us that the operations of the human sciences are multiple and particular and distributed, and that all these objects come into and are sustained in being by separable and to a degree independent individuals - of whom however there are so many that the cointentional objects they share take on an aspect of almost material objectivity.

The prospect of unifying the intentional social sciences, as theories though not as disciplines, lies in my view in bringing them under the human sciences as a covering model, taking account of the sheer numbers of the subjects who animate them, and acknowledging the radically individual and idiosyncratic character of these subjects. The stability, and hence the possibility of unification, of the natural sciences depends on the plausibility of a realist hypothesis, which attributes material reality to their objects as entries in the first ontology; the objects do not depend for their permanence on their being known by subjects, but are for the most part available for observation and checking to successive generations of subjects. The objects of some of the human sciences - notably linguistics and some aspects of psychology, anthropology, and sociology - seem to enjoy a similar permanence, but a realist hypothesis is not plausible in these cases, and the situation is even less stable in domains involving values, such as literary or aesthetic or moral theory. However in these cases there is an analogue of the realist hypothesis in what might be called the 'other-minds hypothesis': if I am not at a given time sustaining some intentional object - a linguistic form, a social institution - others probably are, and if there are enough of us it will become an element of a familiar world in common, just as natural objects do.

The social sciences, then, deal with those aspects of our common world that are so sustained - and which, if not so sustained, will pass away, as its natural aspects will not. The particular social sciences, corresponding to their very various disciplines, fall under the unifying umbrella of the human sciences (which deal after all with much that is not social in any obvious sense) as special cases. A final note, though: not all of the 6.4×10^9 inhabitants of the earth contribute to social objects and structures to the same degree and in the same way. This fact would seem to guarantee the persistence of pluralism.

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