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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.¹ 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.

2.1. Formal biological social science

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 oversimplification 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. 24-25⁵] 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 units¹⁰ 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 $N_{i-2}, N_{i-3}, \dots, N_{i-n}$ 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-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 protoplasts Has anatomy (cytological)	Composed of definitive cells and tissues Has anatomy (tissues and organs)	Composed of definitive organisms and species 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 limitations of protoplasmic amounts (size, surface-volume ratio)	Has ontogeny (embryology) Has limitations of cell numbers (size, surface-volume ratio)	Has ontogeny (succession) Has limitation of population numbers
Regeneration of parts Division of labor between protoplasts Cycles of protoplasmic behavior	Regeneration of parts Division of labor between cells Cycles of cellular behavior	Regeneration of parts Division of labor between organisms and species Cycles of organismic and species behavior
Self-sustaining organization (dynamic equilibrium) Successful integration of whole determines survival of parts and repetition of parts Homology of cytological parts	Self-sustaining organization (dynamic equilibrium) Successful integration of whole determines survival of parts and repetition of parts Homology of tissues and organs	Self-sustaining organization (dynamic equilibrium) Successful integration of whole determines survival of parts and repetition of parts Homology of phylogenetically related species in different communities
Senescence and rejuvenescence of cell Phylogeny of gene pattern Selection of whole cell unit determines survival of gene patterns Controls internal protoplasmic environment and establishes optima Selects or rejects protoplasmic building materials	Senescence and rejuvenescence of organism Phylogeny of cellular pattern Selection of whole organismic units determines survival of cell pattern Controls intercellular environment and establishes optima Selects or rejects tissue-building materials	Senescence and rejuvenescence of community Phylogeny of species pattern Selection of whole community determines species and organism patterns Controls environment within community and establishes optima 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 benefits¹⁶ 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.

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 exist 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 metamerism 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).

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.

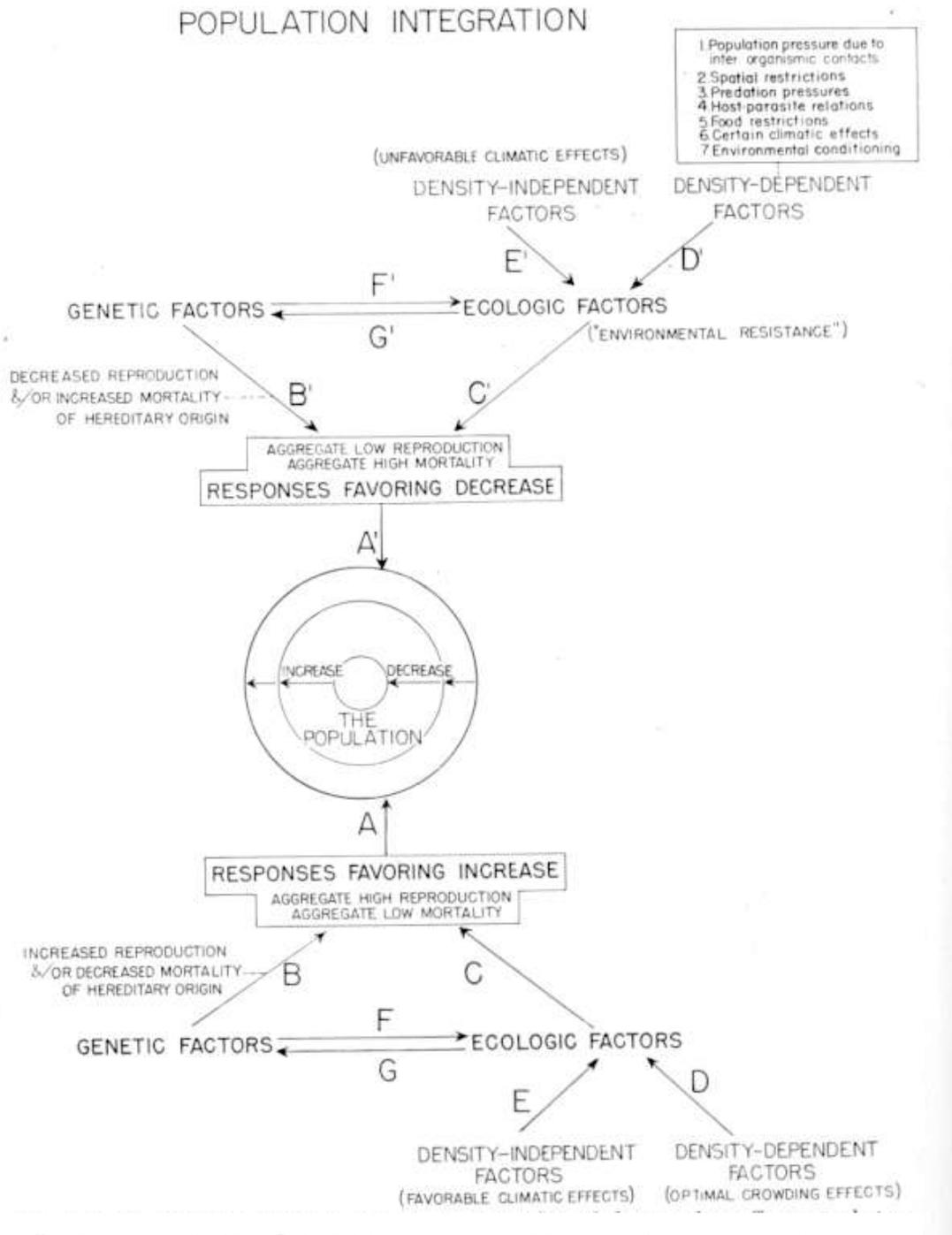


Diagram 2. A schematic representation of the interplay of factors that affect populations.

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 Burgessⁱⁱⁱ

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

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 Beatty, 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 intertwining 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.