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Talking about sociobiology

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From Charles Bazerman's pioneering *Shaping Written Knowledge* to Greg Myers's *Writing Biology;* from Bruno Latour and Steve Woolgar's *Laboratory Life* to Steve Fuller's *Social Epistemology;* and from an entire recent issue of *Rhetorica* to the latest review in *College English,* the available literature on rhetoric of science seems to be undergoing exponential growth.[1] If this burgeoning literature is any accurate indicator of popularity, rhetoric of science appears to be very 'in'.

Just what rhetoric of science *means* is a much-contested point, however.[2] As R. Allen Harris in *College English* points out, both rhetoric and science are two exceedingly unstable, big, and sloppy words with rapidly shifting meanings. Studies of rhetoric of science can therefore vary enormously as users of these terms attach varying and frequently contradictory meanings to them. For these reasons, Harris refuses to attach *the* definite article to this increasingly well-worn phrase.

1. *Howe and Lyne's 'Gene talk in Sociobiology'*

The shifting meanings of the terms rhetoric and science become very apparent in Henry F. Howe and John Lyne's 'Gene talk in sociobiology'. Arguing that sociobiologists have appropriated the language of genetics, Howe and Lyne attempt to persuade the reader that this act constitutes a misappropriation, since the 'talk' (talk is defined as secondary or tertiary usage of the primary language) is wrested from its initially more rigorous contexts of genetics. The appropriated gene talk is especially useful to social scientists and behaviorists, who are not sufficiently versed in this discourse, since it can—and is used to authorize and legitimate particular views of matters of 'grave social importance' such as criminality, sex roles, education, etc., as well as determining social policies. This appropriation takes place as a result of what Howe and Lyne describe as the 'force of suggestion', which can best be understood as the legitimacy that is given to terms, especially scientific terms, as they emerge from their original tightly constrained disciplinary context.

Sociobiologists, according to Howe and Lyne, have drawn from what they categorize as three 'rhetorics of genetics': population genetic rhetoric, biometrical genetic rhetoric, and molecular genetic rhetoric. For them, a 'rhetoric' is a particular form of discourse which binds or intertwines linguistic and nonlinguistic practices. Arguing that

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geneticists' terms like 'fitness', 'gene' and 'selection' have more precise meanings, since they are bound up in mathematical and experimental constraints as developed, defined and reworked by geneticists within these rhetorics, Howe and Lyne point out that these terms and others, are distorted as they are grafted on to other discursive systems whether they be neighboring biological subdisciplines, more distant social sciences, or public discourses. In being appropriated, these terms become converted to metaphors which are substituted for 'clearly defined language' with the end result being that 'woolly thinking' is substituted for 'carefully sculpted concepts'. In the case of the grafting of the genetic terms to sociobiology, the authors contend, the distortions in meanings can vary from the very 'subtle' to the 'spectacular'.

Sociobiologists, according to Howe and Lyne, have also appropriated the 'selectionist rhetoric' that characterized evolutionary biology in the 1930s and 1940s. The appropriated gene talk is therefore also characterized by a 'time-lag'. This time-lag makes sociobiologists exist in what can best be described as a 'time warp' with respect to extant practicing 'real' geneticists whose 'words have been co-opted but whose methods and rigor have been spurned'. In their concluding, very graphic metaphor, Howe and Lyne leave the reader with an image of sociobiologists 'careening down the space age highway in a Model T Ford, hawking antiquated and dangerously obsolete wares'. Rather than concluding that sociobiologists *willfully* appropriate such gene talk to promote 'biological determinism', Howe and Lyne suggest instead that sociobiologists simply do not understand the implications that ensue when gene talk is excised from the disciplinary constraints of genetics. The purpose of their study, as they make explicitly clear, is not to add to the already existing and powerful critiques of sociobiology, but to study the processes of communication or miscommunication in the biological sciences. What 'gene talk in sociobiology' amounts to, is a failure to communicate, which leads to confusion between groups of biological scientists as well as social scientists.

2. *Science as discursive activity*

That science is discursive activity with its practitioners situated in discourse communities, each employing forms of disciplinary discourse, has long been recognized by Charles Bazerman and other composition theorists like Martin Nystrand and David Bartholomae—all, incidentally, involved with writing-across-curriculum programs.[3] How knowledge is altered as it crosses such discourse communities (in this case from the esoteric to exoteric audience) was one focus of Greg Myers's fine-grained analysis of *Writing Biology*.[4]

In one very illustrative example, which is analogous (to some extent) with Howe and Lyne's case study, Myers demonstrates how knowledge claims are altered as they are appropriated, translocated, and recontextualized for different audiences. Here Myers tells the story of an article reporting mounting behavior in a species of parthenogenetic (asexually reproducing) lizard, which was published in a prestigious scientific journal, *The Proceedings of the National Academy of Sciences (PNAS)* under the title '"Sexual" Behavior in Parthenogenetic Lizards (*Cnemidophorus*)'. The knowledge claims made by the workers in the *PNAS* article had been the result of a series of negotiations which arose in response to contestations within the specialist community. Picked up by the popular 'newsmagazine' *Time,* the claims made in the *PNAS* article were published in a section entitled 'The Sexes' under the title: '"Leapin" Lizards: Lesbian Reptiles Act Like Males!' None of the controversy or subsequent negotiations leading to the *PNAS* article were apparent in the *Time* article, as the now 'aberrant' mounting behavior in lizards, clearly viewed from the perspective of human sexual behavior, became reified to a matter of fact. The jump from the claims made in the *PNAS* article to the *Time* article was not the result of sloppy journalism, but an inevitable outcome of appropriating, relocating, and recontextualizing the original claim in the wider context of the exoteric community.

Although Howe and Lyne focus more narrowly on the communication between the 'rhetorics of genetics' and sociobiologists, their study does resonate—at least on the surface—with this case (and others) described by Myers. How meanings of claims are altered as they cross discourse communities and become adapted to different audiences is as clearly demonstrated by the Howe and Lyne paper as they are in Myers's recent treatment. 'Gene talk in sociobiology' does much to highlight the discursive, dialogical as well as the highly contextual, dynamic and rapidly shifting practices within the biological sciences in what is unquestionably the most politically charged biological practice, sociobiology. This makes the Howe and Lyne project timely, interesting and a valuable contribution to the burgeoning literature on the rhetoric of science as well as launching a critique of sociobiology (though they appear not to have this as their primary agenda). But there are some features of their argument which left this reader if not profoundly troubled, then greatly perplexed, especially with respect to some of the rhetoric of science literature.

3. *Rhetoric in Howe and Lyne*

Citing work from Wittgenstein to Fuller, Howe and Lyne make the initial assumption that an 'unconscious "essentialist" bias which holds that a term has transparent or fixed meaning as well as a fixed and transferable information value' has obscured conceptions of communication. In other words, Howe and Lyne wish to make clear that terms do not have fixed and stable meanings, but at the same time wish also to make clear that terms do have validity, and language has clarity within the local disciplinary constraints that gave rise to them. Herein lie some of the problems with their argument.

If one holds to their anti-essentialist rationale, then terms like 'genetics' and even 'sociobiology'—terms used for categories of knowledge or disciplines—have no fixed meaning; nor can their component disciplinary parts be typed or characterized. The use of the terms genetics and sociobiology to describe subjects or fields of study, and the appellations of geneticist or sociobiologist, are evoked for and against points of view depending on the context of use. What counts as 'real' genetics or who counts as a 'real' geneticist is a pressing problem of rhetoric that Howe and Lyne seem to take for granted, since they make only a passing reference to standards of use such as publication in peer-reviewed journals. Just as one cannot attach fixed meanings to the terms of disciplines, one cannot also type or characterize the components, or outline the features, or delineate the boundaries of disciplines (assuming one wishes to be consistently anti-essentialist).

At the same time that they uphold the notion that terms are unstable and lack fixed meaning (which renders them vulnerable to appropriation), Howe and Lyne also wish to uphold the view that meanings are constrained by the local disciplinary context. This is where their notion of a 'rhetoric' (that which binds linguistic and nonlinguistic features of disciplines together), becomes important. It is this binding or intertwining between the linguistic features of the discipline and the nonlinguistic features that fixes meaning and leads to precision and clarity of language. How this binding takes place, as well as further articulation of what constitutes a rhetorics, is not to be found in this paper however; nor is the question of what counts as a rhetorics, and where one rhetorics ends and another begins, ever posed.

Though their notion of rhetoric of science is not clearly articulated (in this article at least), what does come across is a stronger version of the view held by the authors of a recent article in *Rhetorica* entitled 'Some Cautionary Strictures on the Writing of the Rhetoric of Science'.[5] While authors J. E. McGuire and Trevor Melia entertain the importance of the rhetorical features of science, they also urge caution with a full-blown rhetorical analysis. For them, science is not as susceptible to rhetorical analysis as other disciplines, since scientific practice deals not only with, 'textual representations, but extra-textual interventions with nature'. Scientific texts encounter 'a special recalcitrance' with the world they are trying to describe. The 'hard sciences'—astronomy, physics and mathematics—they add, are least likely to surrender to rhetorical analysis than the human or social sciences since they involve heavier involvement of extra-textual practices. Hence for McGuire and Melia, rhetoric of science is a useful analytical perspective but incomplete or inadequate to account for certain scientific practices. Howe and Lyne differ only *in degree* from McGuire and Melia's perspective. Undergirding their argument (as I will discuss shortly) is the belief that genetics is *like a 'hard science'*; and because of this, geneticists can make not only more constrained, but also more value-neutral and truthful claims about the 'real' world. The wish to demonstrate how 'right' the geneticists have been, and how misguided the social scientists have been, forms the central portion of their paper, which articulates the 'tightly' held logic of the three rhetorics. While McGuire and Melia explicitly state *their* wish to avoid scientism (whether they can avoid scientism with their point of view is a point of contention), Howe and Lyne's perspective is as unabashedly scientistic as could be.

4. *Howe and Lyne talk about genetics*

By far the most troubling feature of the Howe and Lyne paper is this scientistic, and privileged, view of genetics they have unwittingly bought into and endorse. Genetics in the Howe and Lyne study, comes off as being the 'cutting edge' (my term) of biological research, and an exacting, rigorous and scientifically precise activity—the only really legitimate or good science from those mentioned in the article. Reasons for this accuracy, as I understand Howe and Lyne, have a great deal to do with, firstly, the experimental nature of genetics and, secondly, with the mathematical nature of genetics. The mathematical aspects of genetics as exemplified in the work of mathematical population geneticists is only secondary since mathematical models may have little bearing on the 'real' world, while the experimental nature of genetics makes knowledge claims replicable, testable and somehow verifiable. In conjunction with mathematics, experimentation in genetics makes genetics more constrained, and genetical theory more tightly logical. Hence, the reader is therefore led to conclude that to Howe and Lyne—as to others—geneticists claims are more value-neutral than others whose practices are nonexperimental or nonmathematical. These knowledge claims become more truthful in time since Howe and Lyne make it clear that genetics has made advances since the 1930s and 1940s. Genetics is, therefore, a science which is progressive and cumulative in its growth.

That Howe and Lyne view genetics as an empirical and progressive science is clear from even a cursory reading of their paper. Their historical perspective, which can best be characterized as the linear, chronological history of the 'victors' to the exclusion of the 'failures' (see especially Tables 1 and 6), is exemplary of what historians and philosophers long ago reviled as 'whig' history. This form of 'whig' history is especially useful to reinforcing the disciplinary solidarity of genetics and historically delineating the boundaries of the discipline, at the same time that it reproduces the notion of the inexorable progress of the scientific enterprise.[6]

In stressing the theoretical and conceptual foundations of genetics and evolutionary theory, moreover, Howe and Lyne have also bought into a view of science which has recently been characterized as 'theory-dominated',[7] since it does not examine closely enough the practical features of the science (this despite their sense of the importance of experimentation). Howe and Lyne's view of science, which also stresses the 'internal logic' of the theory to the exclusion of its 'historicity', has been rendered highly problematic in the wake of powerfully constructed arguments made by philosophers like Richard Burian[8] and others. While it is not my intent to contest the progressive, cumulative 'growth' model of science, or even to examine the proper relationship and the dialogue between theory and practice—I defer to post-positivist philosophers of the ilk of Thomas Kuhn, as well as practice-oriented philosophers such as Ian Hacking, Nancy Cartwright, Peter Galison[9] and others—it *is* my intent to examine carefully, bring into relief, and unmask—if possible—the privileged view of the workings of genetics that Howe and Lyne seem to have eagerly bought into.

The traffic of influence between the genetical 'theory', history and culture, and sociobiology is far more complex and multi-directional than Howe and Lyne believe it to be; nor are there clear and distinct categories such as genetics and sociobiology. Using Howe and Lyne's typology of disciplines, and looking closely at the construction of these disciplines, one may argue that genetics itself has co-opted the language of the 'harder' sciences, especially physics and chemistry. This may very well constitute a miscommunication (by Howe and Lyne's standards of communication) since the biological sciences do not always conform readily with the law-like exemplars in the hard sciences. Genetics, moreover, is deeply embedded—inextricably so—within cultural practices such as eugenics. So deep is the influence between eugenics and genetics, that claims to value-neutrality cannot be sustained. It is through the writing and rewriting of the history of genetics, (through disciplinary histories) that the culturally embedded origins and features of this discipline are removed.

The remaining portion of this paper will examine two features raised by the Howe and Lyne argument. First, I will argue that genetics did not arise as a ready-made scientific discipline which transcended human values, but instead was constructed by the 'talk' appropriated from the hard sciences at the same time that it legitimated cultural practices such as eugenics. Secondly, I will argue that genetics and the origins of sociobiology are closely linked or packed together within the synthetic theory of evolution as it emerged in the 1930s and 1940s. In keeping with the move away from theory-dominated accounts, the synthetic theory can be rethought as sustaining the newly emergent discipline of evolutionary biology. Within the packaging of the synthetic theory—and the linkage is sustained by the belief in the 'internal logic' of theories—sociobiologists are hardly the misguided social scientists that Howe and Lyne believe them to be, but are completely consistent with their version of what counts as *the* synthetic theory.

5. *The place of genetics in the biological sciences: historical perspectives*

Since its initial emergence as a legitimate category of scientific knowledge, genetics has had a privileged position in the biological sciences. The founding father—if we must speak of founding fathers at all—of this new discipline was not Gregory Mendel, but William Bateson. Ever the exacting and precise scientistic thinker, Bateson's excitement with the 'rediscovery' of Mendel was in part due to the fact that Mendel had articulated 'law-like' regularities for the formerly anarchical processes of inheritance.[10] Most pleasing were the features of Mendel's methodology which were mathematical and tracked the progeny of crosses. This led Mendel to construct precise (or roughly so) ratios for the inheritance of discrete characters. This statistical (admittedly) simple treatment of inheritance was most likely an outcome of the training Mendel had received while a student of Doppler in Vienna. The simultaneous articulation of the chromosomal theory of heredity, which singled out the chromosomes as the material carriers of heredity, combined with the statistical tools of agriculturalists made the new science of heredity exact, and an experimental science to rival physics and chemistry.

This had been Bateson's view of the science of heredity when he coined the phrase 'genetics' as the new category to encompass the formerly disparate set of practices which integrated the work of agriculturalists, horticulturalists, civil servants, and medical practitioners. From its original inception, genetics was an '-ic' word which evoked other exact sciences like mathematics, mechanics, and statistics among other sciences. Unlike the other various 'ologies' like biology, geology, and even sociology, which were descriptive sciences, the new science of heredity would emulate the exactness of mathematics and the experimental sciences like physics and chemistry. Hence, genetics, which became the first of the life sciences to be law-like, exact, and completely mechanistic and materialistic most closely resembled physics—it was made to.

But undergirding the new science of genetics and the wish to understand the processes of inheritance was also the accompanying desire to control, in order to 'improve', not only plant varieties and domestic livestock, but also humans. Hence the science of genetics was inextricably linked to eugenics (another '-ic' word) as it emerged as a legitimate scientific discipline. That geneticists at the turn of the century were all—or nearly all—ardent eugenicists has been a point that historians of biology have long pointed out. The extension of this new law-like biological science, which carried the authority of a science like physics to the 'improvement' of humans, became a volatile combination all too quickly. By the 1930s, genetics, law-like and exact, carrying the authority of the 'hard science' claims of value-neutrality, gave rise to, and justified Nazi medicine.

6. *The construction of the synthetic theory*

The traffic of influence and the linkage between eugenics, genetics, Darwinian selectionism and the construction of the synthetic theory is most clearly demonstrated by examination of the work of one of the most rabid eugenicists and selectionists of all time, R. A. Fisher. Driven by his pressing concern to improve British racial stock, Fisher combined the practices of the practical breeder, the statistician and the Darwinian selectionist in a highly potent and influential manner. Publishing in 1930 what became one of the centerpieces of the 'synthetic theory of evolution', *The Genetical Theory of*

Natural Selection, Fisher consciously modeled his selection theory on physics and chemistry. Articulating this 'fundamental theorem of natural selection', Fisher adopted the law which held 'the supreme position among all laws of nature', the second law of thermodynamics. If the physicists and chemists could have such a supreme law, the biological sciences could have one as well. Natural selection, to Fisher, became the biologists' second law of thermodynamics. Though his book was replete with equations articulating his fundamental theorem, the final chapters laid bare the Fisherian eugenicist agenda. These included considerations of: 'Reproduction in Relation to Social Class', 'The Social Selection of Fertility', all leading to his view of the 'Conditions of Permanent Civilization'.

Fisher's adoption of physics and chemistry as exemplar sciences was echoed by J. B. S. Haldane and, to a lesser extent, Sewall Wright. These three, Fisher, Wright and Haldane have been singled out as the 'founding fathers' of mathematical population genetics. Their views, transmitted to nonmathematical and field biologists Theodosius Dobzhansky and E. B. Ford, formed—in classical scientists' histories—the 'core' of the synthetic theory of evolution. Dobzhansky, himself, was aware of the special status given to genetics. He wrote:

> Genetics is the first biological science which got in the position which physics has been for many years. One can justifiably speak about such a thing as theoretical mathematical genetics, and experimental genetics, just as in physics. There are some mathematical geniuses who work out what to an ordinary person seems a fantastic kind of theory. This fantastic kind of theory nevertheless leads to an experimentally verifiable prediction, which an experimental physicist has to test the validity of. Since the time of Wright, Haldane and Fisher, evolutionary genetics has been in a similar position.[11]

In the work of Fisher, Haldane and to a lesser extent Wright, but especially Dobzhansky and other architects (a self-designated term) of the synthetic theory like Julian Huxley, Ernst Mayr, G. G. Simpson, and G. Ledyard Stebbins among others, the link between the new 'evolutionary genetics' and human evolution became powerfully reinforced. From the notion of the 'gene'—a newly constructed entity—to the human and to human culture, all were accounted for in a powerfully selectionist[12] framework within what can be viewed as a master narrative of human origins which was adaptive in nature. This continuum (from the gene to the human) formed the basis for the view most closely associated with Dobzhansky that microevolution (evolution below the species level) and macroevolution (evolution including and above the species level) were subject to the same evolutionary factors. These are some of the fundamental tenets of the synthetic theory as it emerged in the 1930s and 1940s. The 'rhetoric' of these architects, who were actively constructing a science of origins as a unified science grounded ultimately in physics and chemistry, was so persuasive that it led to the emergence of another category of scientific knowledge, now a mechanistic and materialistic science, which came to be called evolutionary biology. Evolution in turn functioned as the 'central organizing principle of the biological sciences', so that it served an integrative function within the larger biological sciences.

At the same time, this selectionist framework painted an optimistic and progressive view of humans and human culture. Views of evolutionary progress (here the human was the great achievement) were linked to social progress through technological change. This was explicitly stated in Julian Huxley's 1942 book *Evolution: The Modern Synthesis*. Views of human 'improvement' were integral features of evolutionary theory in the 1930s and 1940s. Only after the atrocities of World War II shook the world did evolutionists and geneticists purge their vocabularies of the word eugenics. But though they purged their vocabularies of this 'e' word, views of improvement and progress (technological, social and evolutionary) and the continuum between the gene and the human were preserved. With the development of molecular biology and the development of molecular genetics, the continuum between the gene and the human was widened further to stretch from the molecule to the human. Thus, 'Molecules to Man' became one of the overarching themes of the biological sciences after the 1960s.

To conclude, all this history is in the way of pointing out that geneticists from Bateson to Fisher (T. H. Morgan is another good example) appropriated or wrested 'talk' from the even more tightly constrained (since they are more heavily experimental and mathematical) disciplines of physics and chemistry. The 'force' of suggestion of this 'physics talk' easily swayed the 'founding fathers' of genetics and facilitated the construction of a 'rhetoric of genetics' which would reconfigure as it legitimated a diverse assemblage of practices like eugenics. The eugenical origins and agenda of the discipline were ejected only after the atrocities of the holocaust were made apparent; but the continuum between the gene and the human and human culture were preserved and extended further in the 1960s. This continuum, as I argue in the next section, gave rise to, and justifies (within a local context of values) sociobiology.

This 'physics talk' that was appropriated, it should be noted, was also characterized by a noticeable time-lag since the 'rhetoric of physics' appropriated by geneticists was mostly Newtonian mechanics. This appropriation, it may well be argued, constitutes a misappropriation and therefore a miscommunication (by Howe and Lyne's standards) since law-like regularities are not always easy to establish in genetics and other biological sciences. Geneticists, to conclude, may here be viewed as co-opting the 'talk' of physicists in a manner not unlike the manner in which sociobiologists have co-opted the 'talk' of geneticists. The scaffolding for the culturally embedded construction of genetics has been removed by the selective writing and rewriting of the history of the discipline.

7. *The synthetic theory and sociobiology*

The construction of the synthetic theory as it emerged from the 1930s and 1940s, which drew from genetics and hence also eugenics, therefore had built into it notions of progress and the 'improvement' of humans through evoluton. Hence the linkage between human culture, the human, and the gene was part and parcel of the synthetic theory. Just what constituted the 'elements' of the synthetic theory is hard to locate, however, though the architects of the theory appeared to agree that there was one, unified and synthetic theory of evolution. Simultaneous with this agreement (considerations of space prevent me from elaborating further on a discussion of this agreement), there arose the discipline of evolutionary biology in the late 1940s.

The relationship between the emergent evolutionary biology and sociobiology is exceedingly tight; and the view that sociobiology descends from evolutionary biology, as Howe and Lyne recognize, is a point well taken. But while sociobiology can very well be seen as an 'unruly offspring' of the 'parent discipline' of evolution, this offspring is not at all illegitimate. The tightness of the relationship between sociobiology and the synthetic theory as it emerged from the 1930s and 1940s exists, because for E. O. Wilson and others, sociobiology *is* a logical entailment of what they view as *the* synthetic theory.

This connection between the synthesis and sociobiology is clear from the choice of title for his 1975 book, *Sociobiology: The New Synthesis,* the abridged version of which clearly states Wilson's wish to 'codify sociobiology into a branch of evolutionary biology'. The very organization of the book begins with the first and introductory chapter on 'The Morality of the Gene' and concludes with the final chapter 'Man: From Sociobiology to Sociology'. Howe and Lyne rightly point out that the first chapter reveals the sociobiological program's grounding in genetics; but they have not noted that the structure of Wilson's argument, which stretches from the gene to the human (and, therefore, links genetics to sociology), is very much in line with Dobzhansky's continuum between microevolution and macroevolution. In tandem with the strongly selectionist and adaptationist features, Wilson's argument follows within the 'internal logic' of the synthetic theory as it emerged in the 1930s and 1940s. Wilson himself, moreover, was intimately acquainted with evolutionary biology, at least in the 1950s. The first teacher of the first course with the title of evolutionary biology at Harvard University (in 1958), was none other than Wilson himself.

In the 1960s the foundations of sociobiology solidified further when behaviorists (and others), began to draw not only on evolutionary biology (they were embedded within it, of course), but also economic theory to reformulate their views of social and cultural evolution. All this reformulation took place within a view of the synthetic theory, which was selectionist and upheld the continuum between the gene to the human. The reformulation was not only inevitable but worthy from the perspective of the evolutionary biologist turned sociobiologist. Wilson and Dawkins and other sociobiologists, therefore, are hardly the misguided social scientists or mistaken behaviorists that some critics contend. The point here, as Howe and Lyne grow to appreciate, is that so long as the synthetic theory (and within it genetics) are tightly packed together within an 'internal logic' of the theory, it is inevitable that there will attempts to 'synthesize' or bring into line points of view within such an agreed-upon framework. Ever since the synthetic theory was constructed it has functioned as just such a structuring framework. Within the synthetic theory, the location of genetics is privileged because of its experimental quantitative nature (a belief which Howe and Lyne have eagerly bought into). So long as this privileged location is held 'genetic talk' *will* be wrested from its from its more local contexts to explain persistent problems within the wider domain of the synthetic theory. Sociobiology itself and the synthetic theory (and within it genetics) are so closely linked that sociobiology draws from genetics, and is as subject to critique and amendation insofar as the synthetic theory is subject to critique and amendation.

For this reason one of the most powerful critiques of sociobiology comes through a major amendment of the synthetic theory. Such an amendment formed the basis of the debates in evolutionary theory in the late 1970s and early 1980s which led to the critique of the adaptationist program at the same time that it brought into line persistent problems with the tempo and mode of evolution as experienced by practicing paleontologists.[13] The effect was to sunder the continuum between microevolution and macroevolution so that the gene had a limited determination of the human condition.

The critique of the adaptationist program met with controversy, even notoriety, among practitioners of evolutionary biology, in part because it urged a major reform of the synthetic theory. But even this major critique of the adaptationist program did not end the reign of the sociobiologists. Nor have even other amendments of the synthetic theory by workers who identify themselves primarily as population geneticists ended once and for all the appeal of sociobiology. So long as the notion and the authority of *the* or *a correct* synthetic theory which is unvarying and fixed (in an essentialistic sense), persists; and so long as upholders of the theory situate the gene and human in the same evolutionary narrative, there will be attempts to bring together biology (genetics) and sociology (sociobiology) to generate variations on the theme of sociobiology. And so long as genetics is given a privileged locale, groups touching or falling within or bordering on the domain of the synthetic theory will appropriate the rhetoric of genetics, and believe themselves to be completely in the right.

The belief that there is one unified, and unvarying synthetic theory seems to be one of the central assumptions at issue here. Just what constitutes the synthetic theory is a contentious issue for philosophers who have made repeated attempts to outline the 'essential' features of the theory. Philosophers of biology like Richard Burian who have attempted to fix *the* theory at its core, have come to the conclusion that synthetic theory is 'a moving target'.[14] Howe and Lyne's project makes it all the more clear that *what counts* as *the* synthetic theory, and *to whom*, is a pressing rhetorical question.

8. *Closing thoughts*

Howe and Lyne raise an interesting set of issues for a varied audience of readers. Though they hold to the view that science is discursive activity, their position argues that some discourses are more privileged than others. These privileged discourses attain this status through their ability to make value-neutral claims through experimental and mathematical practices which serve to constrain or fix meanings. In the biological sciences the more experimental and mathematical disciplines consist of disciplines like genetics.

Belief in the value-neutrality of genetics (and this holds for disciplines like physics and chemistry as well) is also the result of, and reinforced by, disciplinary histories which remove or conceal the cultural embeddedness of scientific activity. Historically, genetics as a discipline did not emerge as ready-made product but was constructed as a legitimate category of scientific knowledge from 'talk' wrested from other locales like physics and chemistry in order to sustain a disparate set of practices, including eugenics. Genetics itself has been used as a grounding for sociobiology; but genetics has also served as the grounding of practices as diverse and legitimate as the synthetic theory as it emerged from the 1930s and 1940s, to the grounding of such reprehensible practices as Nazi medicine. In so doing, genetics has been altered and transmuted by the uses to which it has been put—and continued to be altered as its disciplinary boundaries—if there are such things at all—continue to be negotiated and renegotiated. The traffic of influence between and within not only genetics but all biological and social practices is multi-directional with linguistic, material, and even social practices dynamically appropriated, translocated and recontextualized by heterogeneous audiences. Such appropriations are ubiquitous between heterogeneous groups of practitioners and are the mainstay of the production of knowledge. Viewed this way, meanings or appropriated terms are not 'converted' to 'woolly metaphors', only metaphors that do not work—for one reason or another—in a given context.

Moreover, the closer, or more proximate, the practices, the more likely and frequent the appropriations. In the case of sociobiology and genetics, proximity is secured within the structuring synthetic theory, which situates the gene (or molecule) and the human within an adaptationist framework. Within the structuring framework of *the* synthetic theory (which version of the synthetic theory is upheld is an issue here) the relationship between sociobiology and genetics is all the more tightly meshed, difficult to disengage, and justifiable within local standards of the synthetic theory. Just whose version of the synthetic theory—and to what extent there is one synthetic theory—is the central rhetorical question for any examination of sociobiology.

The dangers of appropriations reside not only with how the appropriator uses (willfully or unwillfully) the knowledge claims or terms appropriated, but also the critic or commentator who continues to privilege one discourse over another. So long as the notion of a privileged location is upheld, the 'force of suggestion' will be all the more enticing for the appropriation and co-option of the 'talk' of such disciplinary discourse. The more its practitioners and critics paint genetics as a rigorous, precise and value-neutral practice, the more enticing genetics will be as grounding to reprehensible practices such as Nazi medicine. So long as the perception of the privileged locale of genetics is upheld, there will be appropriators like sociobiologists who believe themselves to be on grounding on stable value-neutral ground.

One way to unmask, expose, and deprivilege claims to value-neutrality is through close study of two big, sloppy and unstable words: 'rhetoric' and 'science'. What exactly these words mean remains a greatly contested point, however.

Notes

1. This is by no means an inclusive list of all the literature on rhetoric of science. The most up-to-date review, which includes a taxonomic classification is R. ALLEN HARRIS's review in *College English,* 53(3) (March 1991), pp. 282–307.
2. For a recent and acrimonious debate see JOHN DURANT's review of ALAN GROSS's *The Rhetoric of Science,* 15 March 1991, and the response by Gross, and CHRISTOPHER LAWRENCE and STEVEN SHAPIN in the *Times Literary Supplement,* 19 April 1991.
3. See DAVID BARTHOLOMAE, 'Inventing the university', *When a Writer Can't Write* (ed. ROSE, M.), The Guilford Press, New York, 1985; MARTIN NYSTRAND (ed.), *What Writers Know. The Language, Process and Structure of Written Discourse,* Academic Press, New York, 1982; DAVID JOLIFFE (ed.), *Writing in Academic Disciplines,* Ablex Publishing Corporation, Norwood, NJ, 1988; see also GAYLE ORMISTON and RAPHAEL SASSOWER, *Narrative Experiments. The Discursive Authority of Science and Technology,* University of Minnesota Press, Minneapolis, MN, 1989.
4. See GREG MYERS, *Writing Biology,* University of Wisconsin Press, Madison, WI, 1990.
5. J. E. MCGUIRE and TREVOR MELIA, 'Some cautionary strictures on the writing of the rhetoric of science', *Rhetorica,* 7 (1989), pp. 87–99.
6. For an interesting treatment of the uses of disciplinary histories see: WOLF LEPENIES and PETER WEINGART (eds), *The Functions and Uses of Disciplinary Histories,* Reidel, Dordrecht, 1983.
7. For a clear exposition of the 'move away from theory-dominated accounts of science' see TIMOTHY LENOIR and YEHUDA ELKANA, 'Practice, context and the dialogue between theory and experiment', *Science in Context,* 2(1) (Spring 1988). See alao JAN GOLINSKI, 'The theory of practice and the practice of theory: sociological approaches in the history of science', *Isis,* 81(308) (September 1990), pp. 492–505.
8. See RICHARD BURIAN, 'More than a marriage of convenience: on the inextricability of history and philosophy of science', *Philosophy of Science,* 44 (1977), pp. 1–42.
9. For practice-oriented philosophy see IAN HACKING, *Representing and Intervening,* Cambridge University Press, Cambridge, 1983; NANCY CARTWRIGHT, *How the Laws of Physics Lie,* Oxford University Press, Oxford, 1983; PETER GALISON, *How Experiments End,* University of Chicago Press, Chicago, IL, 1987.
10. See William Bateson's address to the Royal Horticultural Society in 1906 in the published *Report of the Third International Conference on Genetics.* See also the historical treatment of Mendel by the by the Rev. W. Wilks, editor of the volume.
11. From Dobzhansky's Oral Memoir of 1962.
12. Fisher was the strongest supporter of selectionism. Wright became more of a selectionist with time, however, as did the majority of the 'architects' of the synthetic theory. This 'hardening' of the selectionist framework has been noted by Stephen J. Gould and others. See GOULD's article on 'The hardening of the modern synthesis', in *Dimensions of Darwinism* (ed. GRENE, M.), Cambridge University Press, New York, 1983, pp. 71–93. For a detailed development of Sewall Wright's work and how Wright came to uphold a more strongly selectionist perspective, see WILLIAM, B. PROVINE's biography of Wright, *Sewall Wright and Evolutionary Biology,* University of Chicago Press, Chicago, IL, 1986. See also my 'Unifying Biology: The Evolutionary Synthesis and Evolutionary Biology', *Journal of the History of Biology,* 25(1), pp. 1–65.

13. For a critique of the adaptationist programme see S. J. GOULD and R. C. LEWONTIN, 'The spandrels of San Marco and the Panglossian paradigm: a critique of the adaptationist programme', *Proceedings of the Royal Society of London, Series B*, 205 (1979), pp. 581–598.
14. See RICHARD BURIAN, 'Challenges to the evolutionary synthesis', *Evolutionary Biology*, 27 (1988), pp. 247–269.

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