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Definitional Argument in Evolutionary Psychology and Cultural Anthropology

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Argument

Evolutionary psychologists argue that because humans are biological creatures, cultural explanations *must* include biology. They thus offer to unify the natural and social sciences. Evolutionary psychologists rely on a specific history of cultural anthropology, particularly the work of Alfred Kroeber to make this point. A close examination of the history of cultural anthropology reveals that Kroeber acknowledged that humans were biological and culture had a biological foundation; however, he argued that we should treat culture as autonomous because that would bring benefits to the biological sciences as well as the human sciences. Hence, the historical caricature of his work by evolutionary psychology fails. The paper concludes that cultural anthropologists were successful in creating their discipline, at least in part, because they argued by pragmatic definition. Evolutionary psychology, on the other hand, offers an essentialist definition of "culture" and thus offers a much less promising vision of interdisciplinary collaboration.

The role of disciplinary history in the creation and maintenance of disciplinary autonomy and authority has been a target of scholarly inquiry at least since Thomas Kuhn's claim that such histories are key indicators of a reigning paradigm (Kuhn 1970). In the United States, the history of psychology is a recognized subdiscipline of psychology, with histories of psychology serving to inculcate students into psychology as well as to establish and maintain the authority of research programs (Ash 1983; Leahey 1992; Samelson 1997; Samelson 2000). We should not be surprised, therefore, to find evolutionary psychologists appealing to the history of the social sciences when they make their calls for the necessity and value of their nascent discipline.

In this paper I shall examine how evolutionary psychologists use the history of science to bolster their new discipline. Employing a specific account of the origins of American cultural anthropology at the beginning of the twentieth century, evolutionary psychologists offer a particular history of that discipline in order to argue that we now need evolutionary psychology (EP). I shall show that each discipline (EP and anthropology) has attempted to create space for itself by defining a central term, "culture," as a means to define its own scientific program: defining the nature of scientific inquiry by defining the central object of study.

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In the first section of the paper I examine definitional argument, contrasting an essentialist approach to a rhetorical/pragmatic approach. I argue that claims to have found the "real" definition of a term actually hinder productive argumentative exchanges between disputants. I suggest that we should always view definition as an inherently value-laden process and it is best if we treat it as such.[1]

In the next section of the paper, I examine the historical account offered by evolutionary psychologists and exemplified by the "Standard Social Science Model" (SSSM). Evolutionary psychologists claim that the SSSM represents a sharp bifurcation between the natural and social sciences. As Hampton recently argued, "The contrast between the Standard Model and the evolutionary model which Tooby and Cosmides propose legitimises the claim that, as a whole, evolutionary psychology is a new approach" (Hampton 2004, 16). They tell a history of social science in the United States wherein cultural anthropologists and sociologists banished Darwin from their disciplines. A key move here is the claim that social scientists posited concepts of "culture" and "society" that were expressly non-biological. One of the standard villains of this history is the cultural anthropologist Alfred Kroeber (1876–1960), who argued that because culture was "superorganic," biology could play no part in the kind of explanations offered by a cultural anthropologist.

Next I turn to Kroeber's work to see if the history offered by the evolutionary psychologists adequately captures his views. Here, I shall argue that Kroeber was making not an ontological claim about the superorganic but rather a pragmatic claim. In short, Kroeber accepted that humans were biological organisms and were the products of natural selection, but that science would be better off if such biological claims did not play a part in anthropological explanations of culture.

In the final section, I argue that Kroeber's definition of culture was widely adopted because it offered advantages to both the biological and the social sciences. Evolutionary psychologists' claims for the unity of biological and social sciences fail because they offer no such advantages.

Definitional Argument

John Locke distinguished between the "real essence" of a thing and its "nominal essence." The real essence was "the being of anything, whereby it is what it is." The nominal essence, by contrast, was merely a convenient sorting device for categorizing things and stands for "nothing but that abstract idea which the general or sortal . . . name stands for" (Locke 1894, Book III, chap. 3, Section 15). The standard way this

[1] I use "rhetoric" in this paper to refer to the discipline that studies persuasion. I do not use it in the vulgar sense of "empty bombast." In the rhetorical sense, argument is situation and addressed speech and to evaluate argument one must be aware of the rhetorical situation. Because pragmatism is also concerned with argument as related to human ends and needs, there is commonality in rhetorical and pragmatic approaches to argument.

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has been taken up is to agree that to define something by its real essence you need to investigate the thing itself, but to define something nominally, you need to investigate how the word is commonly used (Gupta 2009). A similar usage has been offered by Edward Schiappa who called an argument from *real* definition one that answers the question "What is 'X'?" (Schiappa 2003). According to this view, we should use the word "X" in a particular way because that is what X *really* is. Schiappa, as we will see rejects the notion that we should focus on "real" definitions because when focusing on "real" definitions, other sorts of social values were secondary, if acknowledged at all, to the philosophical quest for the essence of the thing being defined. Arguing from real definitions transforms an argument *about* a definition into an argument *by* definition. David Zarefsky has noted:

> The key definitional move is simply stipulated, as if it were a natural step along the way of justifying some other claim. . . . One cannot help noticing an irony here. Definition of terms is a key step in the presentation of argument, and yet this critical step is taken by making moves that are not themselves argumentative at all. They are not claims supported by reasons and intended to justify adherence by critical listeners. Instead they are simply proclaimed as if they were indisputable facts. (Zarefsky 1997, 5)

If we want our definitions to be productive in controversies, we need to rethink definitional argument by rejecting any form of essentialism. Both of Locke's categories shared a search for essence, although what he meant by that term was much different from the Platonic tradition we often associate with that term. Essentialism has certainly fallen on hard times in the last couple of centuries of philosophy, but more than a trace of it remains in philosophical discussions of definitional argument. Douglas Walton surveyed a number of modern logic textbooks and noted: "This traditional treatment [of definition] has come down to us from Aristotelian roots, and has not changed all that much in basic outline over the years. Typically, the textbook account of definitions contains an outdated and questionable doctrine of essentialism at its core" (Walton 2005, 171). A philosopher might object to Walton's characterization and point out that philosophers have myriad types of definition: explicative, descriptive, persuasive, lexical, and the like. This is true, and draws our attention to the notion that the act of defining is a speech act and that what distinguishes different types of defining acts from one another are the goals of the arguer and other pragmatic concerns. The act of defining, therefore, is best understood within the context of arguing as a process: "The goal of such a speech act is evidently to alter or fix the meaning of a word or phrase for use in the subsequent dialogue between the two parties" (ibid., 172). Viewed this way, an arguer claiming to offer the "real" definition then hinders productive argument among interested parties. "It seems that every proposed definition or theory in philosophy is arguable," writes Walton, "and that there are almost always two sides to a controversial issue of a kind subject to philosophical disputation. Fixing on your own definition or viewpoint as 'real' and those of your opponents as merely nominal or

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'unreal' seems like a dogmatic approach that is contrary to good philosophical method" (ibid. 174). Indeed, the "realness" of a definition cannot serve to be, in and of itself, a reason for accepting the definition. One must engage in argument to support one's definition, and that cannot be achieved by simply stating "and it is true" (Hacking 1999, 81).

The idea that defining is best understood as part of an argumentative dialogue has a long history in rhetorical studies of argument, dating back at least to Hellenistic Greece. Hermagoras, a teacher of rhetoric, taught that definition was a *stasis*, a word borrowed from boxing (think of the *stance* of a boxer). A *stasis* was the question that disputants would take as their central point of contention. The question of definition, according to this tradition, was a separate question from a question of fact or quality (Kennedy 1963, 303–321). In this rhetorical tradition, definitions were seen as points of dispute in social controversy, often law courts. Definitions needed to be argued and there were real social consequences for the winners and losers in the dispute. Writing from this rhetorical tradition, Edward Schiappa has called for a rejection of "real definition" and an embrace of *pragmatic* definition, which takes the form, "How should we use the term 'X'?" In this view, an arguer puts forth reasons for using the term "X" in a particular way. "Instead of presuming to be able to identify metaphysical 'essences,' in definitions," writes Schiappa, "we should acknowledge that definitions emphasize aspects of social realities that serve particular interests" (Schiappa 2003, 169). Reframing definitional argument away from the question, "What is 'X'?" and toward "How should we use the term 'X'?" requires an arguer to put forth reasons for accepting the offered definition. The act of defining has consequences and is always done in a value-laden way; it is not merely a matter of discovering what X really is.

To put this abstract discussion in the context of this paper, I argue that evolutionary psychologists are arguing by real definition: we should view culture as biological because that is what culture really is – and, according to evolutionary psychologists, that reality is precisely what is denied by the modern social sciences. This definitional move frames the argument often made by evolutionary psychology that any explanation of social or cultural phenomena must invoke biological capacities. By contrast, Alfred Kroeber employed the strategy of pragmatic definition, clearly outlining the benefits that would accrue to a variety of scientific disciplines if they would accept his definition of culture as superorganic.

My argument is that evolutionary psychologists proclaim that culture is biological (the stipulated definitional move) in order to demonstrate that, to make psychology a science, one *must* accept their account of the origin of culture.

The Broad Sense of Evolutionary Psychology

John Tooby and Leda Cosmides, often considered the founders of evolutionary psychology, have proclaimed that there are two senses of EP, "Evolutionary psychology

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in the narrow sense is the scientific project of mapping our evolved psychological mechanisms; in the broad sense, it includes the project of reformulating and expanding the social sciences (and medical sciences) in light of the progressive mapping of our species' evolved architecture" (Tooby and Cosmides 2005, 6). This broad vision of EP manifests itself in their claims to provide the necessary tools to unify the social and natural sciences. Philosopher Richard Hamilton recently noted that evolutionary psychologists' "commitments to massive modularity" are matched by their "equally strong commitment to the unity of science" (Hamilton 2008, 107). Evolutionary psychologists' assertions for the unity of science, particularly how those claims rely on specific historical appeals, have seldom been the focus of critique.[2] David Buller, who finds evolutionary psychologists "wrong in every detail," masterfully unpacks those details except for the claim that EP provides a "Grand Unified Theory" of the human mind itself (Buller 2005, 481).

John Dupré, a well-known philosophical opponent of the unity of science does not make EP's assertions of unity a centerpiece of his critique, observing that, "Tooby and Cosmides's rather vague appeals to the unity of science are wholly irrelevant to the plausibility of evolutionary psychology." Dupré also admits "reductionism clearly has a role in explaining the attractions of evolutionary psychology" (Dupré 2001, 73, 75). In other words, while *philosophically* unsupportable, EP's claims to unify the social and natural sciences are nonetheless *rhetorically* successful; that is to say, having found the appeal to unity persuasive, scientific audiences have therefore allowed EP to gain a foothold in the academy.

While philosophers are often scrupulous about distinguishing popular from mainstream versions of sociobiology and evolutionary psychology (e.g., Kitcher 1985), citation patterns indicate that evolutionary psychologists have no hesitation in drawing on popular accounts for their scholarly papers, especially in their invocation of the SSSM and claims to unify the sciences. The popular works of Steven Pinker and Edward Wilson, for instance, are heavily cited in the scholarly literature of EP. As Andrew Winston has noted regarding the historical narrative I will discuss in this paper, "Pinker's popularized version draws on Tooby and Cosmides, but then becomes support for their subsequent academic version of the narrative" (Winston 2006, 84). If EP is gaining a foothold in the academy it may be due to the popular accounts it offers rather than the more technical aspects of the research program. Indeed it has been asserted that "Evolutionary studies of human behavior are receiving extraordinary attention from the popular media" moreover, with "most academics . . .too busy to read primary literature unrelated to their own specialization. . . .we are all increasingly relying on popularized syntheses" (Smith, Mulder and Hill 2001, 129–130).

If EP is gaining stature in the academy, there is some reason to think it is *not* because its technical claims about the Environment of Evolutionary Adaptiveness and

[2] Works that have examined the historical case presented by evolutionary psychologists are Hampton 2004 and 2006; and Winston 2006.

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the massive modularity of the mind have withstood rigorous academic scrutiny; rather we must look to its claims about the nature of science and its promises to transform the social sciences into natural sciences. Maarten Derksen has observed, "One reason for evolutionary psychology's popularity among psychologists must surely be this promise of unification" because "unity is regarded by most in psychology as a good thing" (Derksen 2005, 140). "The appeal of evolutionary theory is its potential to serve as a unifying theory in the human sciences" says Linnda Caporael (Caporael 2001, 621). Nicholson, citing Wilson, claims "The EP goal, as might be wished for all scientific inquiry, is a unity of knowledge that assists the translation between disciplines and across levels of analysis. This is an essential part of the EP project – to restore the link between the social sciences and other bodies of knowledge (Wilson 1998, quoted in Nicholson 2005, 402). Maarten Derksen has concluded:

> One of evolutionary psychology's main selling points is its promise to restore wholeness. Readers of the many popular and semi-popular works by evolutionary psychologists are put back in touch with their inner mammal: their natural propensities to choose youthful and slim-waisted, or mature and well-off mates, to prefer realist over abstract art, and family life over the commune, in short: human nature, from which they have been alienated by decades of political correctness. For the academic audience the message of wholeness is focused on overcoming the disciplinary boundaries that have sustained this alienation. Evolutionary psychology presents itself as the foundation of an integrated social science, tying sociology, anthropology, economics and other disciplines to the anchor of biology, from which they have been drifting away since the 1920's. (Derksen 2007, 189)

Evolutionary psychologists' claims about the unity of science are based on a historical account of the origin and prevalence of the SSSM, particularly as offered by John Tooby and Leda Cosmides's 1992 "manifesto" for EP. As I will show later, this historical account is presented in EP textbooks as well as scholarly articles, and it even has appeared in an article in *History of the Human Sciences* (Dunbar 2007). Seen this way, the massive modularity thesis becomes a warrant for the larger claim that the social sciences must embrace Darwinism and that biology must inform explanations offered in the social sciences. Robert Richardson argued: "The evolutionary explanations offered by evolutionary psychologists are a means to an end, where the end is the reform of psychology" (Richardson 2007, 20).

The Unity of Science in Evolutionary Psychology

Those in the science studies disciplines who have looked for evidence of the unity of science have failed to find much. Peter Galison has written extensively on how difficult communication is even within the limited domain of high-energy physics (Galison 1997). John Dupré explored the difficulty of seeing biology as a unified field of inquiry (Dupré 1993; Dupré 1996, 101–117). John Beatty observed how, even in

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the limited domains of biology, we do not have a unified science; rather "theoretical pluralism and relative significance controversies occur at every level of investigation in biology (Beatty 1997, 434).

Moreover, in a recent survey of evolutionary approaches to the social sciences, of which EP is only one, there was little hope that Darwin could serve to unify the social sciences because "beneath this apparent unity lie serious theoretical and methodological disagreements. Given the diverse backgrounds of the practitioners, it is hardly surprising that evolutionary social science contains several distinct styles of analysis, reflecting the methodological and conceptual habits of the parent disciplines" (Smith, Mulder, and Hill 2001, 128).

In the philosophy of science, the notion of the unity of science has fallen on very hard times. "The Unity of Science Movement is dead," Philip Kitcher declared ten years ago:

> If philosophers ever believed that science could be organized as a hierarchy of theories founded on general principles with the basic generalizations of "higher level" theories derivable from those of more "fundamental" theories, then they do so no more. The doctrine that chemistry is reducible to physics, biology to physics and chemistry, psychology to biology, and the social sciences to psychology has suffered from scrutiny of crucial junctions – particularly those between biology and the physical sciences, and between psychology and biology. (Kitcher 1999, 337)

News from the science studies disciplines has not yet reached the communities of scientists. The views Kitcher describes are still widely held by many scientists, who still hold fast to the idea of a hierarchy of science, with "more basic" sciences (physics, chemistry) serving as a foundation (in some unspecified way) for "more derivative" sciences (biology, psychology). As Ian Hacking has argued, such beliefs are ideological rather than fully articulated arguments in the sciences (Hacking 1996). Evolutionary psychologists tap into the ideology of the unity of science in order to bolster the appeal of their research agenda. To make their case for the unity of science, Tooby and Cosmides start with an ontological claim about the "vast landscape of causation" from the Big Bang forward in order to "account for the types of entities that emerge (pulsars, tectonic plates, ribosomes, vision, incest avoidance)" (Tooby and Cosmides 1992, 20). In argument *by* definition, the key definitional move is often stipulated rather than argued for and Tooby and Cosmides simply stipulate that incest avoidance is the "same sort" of ontological entity as a ribosome or pulsar. If culture is the same type of thing as a pulsar, then the social/cultural sciences are doing the same thing as the natural sciences. As they develop their argument, it looks something like this: because culture *really is* nature, the kinds of explanations offered by the social sciences must look the same as those produced in the natural sciences.

Following from the ontological claim about the nature of culture are claims about the nature of science: since nature is unified, science is unified. As Tooby and Cosmides

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frame it, this is more an empirical claim than a normative one: "Such a history – in its broadest outlines – is well on its way to being constructed" by the natural sciences (ibid., 19). The business of science, in this view, is to reconstruct the causal chain from the Big Bang to human culture. The natural sciences are then unified because the universe is unified. Tooby and Cosmides declare that the natural sciences'

> extraordinary florescence throughout this century has resulted in far more than just individual progress within each field. These disciplines are becoming integrated into an increasingly seamless system of interconnected knowledge and remain nominally separated more out of educational convenience and institutional inertia than because of any genuine ruptures in the underlying unity of the achieved knowledge. In fact, this development is only an acceleration of the process of conceptual unification that has been building in science since the Renaissance. (Ibid., 19)

Other writers urging the unification of the social sciences write admiringly that "One of the great triumphs of twentieth-century science was the seamless integration of physics, chemistry, and astronomy, on the basis of a common model of fundamental particles and the structure of space-time" (Gintis 2007, 1). All this talk is very vague and such claims about the existing unity of the natural sciences are unsupported by footnotes or references. Nonetheless, as they develop their arguments for the unity of science, evolutionary psychologists conceive of "unity" in two different ways: via a consistency thesis or via an "explanatory coherence" thesis.

The Scientific Consistency Thesis

In a weak version of their claim, Tooby and Cosmides invoke the unity of science as a means of asserting that each science must produce knowledge that is not contradicted by what is, in their view, a more basic science. The hierarchy is familiar: physics constrains what chemistry may claim, chemistry constrains biology, and biology should, in principle, constrain what the social sciences may claim. For Tooby and Cosmides, "valid scientific knowledge – whether from the same or different fields – should be mutually consistent" (Tooby and Cosmides 1992, 22). Pinker argues that such mutual consistency can be described as "good reductionism. . . . [that] consists not of *replacing* one field of knowledge with another but of *connecting* or *unifying* them" (Pinker 2002, 70). Jerome Barkow offers the most irenic version of the "consistency" asked for by EP: "What evolutionists are asking is only that sociology and social-cultural anthropological accounts be *compatible* with what we think we know of human evolution and psychology: that is all. Incompatibles indicate errors at one level or the other and must drive research. The aim is never to *replace* sociology or anthropology with psychology and biology" (Barkow 2006a, 29). Barkow even allows that the discovery of an incompatibility might lead to a correction in a more basic science

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rather the assumption that the social science must be erroneous. Much more strident, Tooby and Cosmides assure us that after EP unites the social sciences with evolutionary biology, "the resulting changes to the social sciences are expected to be dramatic and far-reaching" because "the existing superstructure of the social and behavioral sciences . . . will have to be dismantled" (Tooby and Cosmides 2005, 6–7; see also Richardson 2007, 21).

All these writers are vague as to how this unification might occur, and, as noted, science studies scholars might question if such compatibility exists even in the natural sciences. Nonetheless, calls to make the social sciences "compatible" with the natural sciences are prevalent in EP (Barkow 2006b, 348–349; Geher 2006; Gintis 2004; Grosvenor 2002; Mesoudi, Whiten, and Laland 2006). Such claims follow from the often elided definitional move that places culture in the same ontological process that produces the objects of study in the natural sciences. Such claims about the requirement of compatibility among explanatory regimes also lead to a stronger sense in which evolutionary psychologists call for the unity of science: the requirement of explanatory coherence.

The Explanatory Coherence Thesis

According to many evolutionary psychologists, each level of science *must* incorporate the more basic science in its explanation. Tooby and Cosmides claim that while the natural sciences have been "weaving themselves together through accelerating discoveries of their mutual relevance" the social sciences have been clinging to the "doctrine of intellectual isolationism" (Tooby and Cosmides 1992, 22). Afraid of being "reduced to more basic sciences," social scientists have seen "conceptual unification . . . [as] an enemy, and the relevance of other fields a menace to their freedom to interpret human reality in any way they chose. Thus, despite some important exceptions, the social sciences have largely kept themselves isolated from the crystallizing process of scientific integration" (ibid., 21). It is this isolation that results in the stagnation of the social sciences, claims Jerome Barkow: "Sociocultural anthropology clearly has not progressed in the cumulative fashion of evolutionary biology" (Barkow 2006b, 347; see also Mesoudi, Whiten, and Laland 2006, 330). Such a stance has been adopted wholesale in the textbooks that have started to appear in evolutionary psychology. Bruce Bridgeman writes, "All of the natural sciences are tied together – facts of biology are explained in terms of chemistry, chemical processes are understood in terms of physics, and so on. . . . The social sciences in contrast talk to one another much less; each works independently of the others" (Bridgeman 2003, 6).

The upshot of this isolation is that explanations found in the social sciences are "incoherent." Tooby and Cosmides argue that social scientists, because they refuse to incorporate any biology into their explanations for human behavior, produce incoherent explanations. All explanations generated by the social sciences are strictly

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environmental and make no mention of biological capacities for social behavior. This, Tooby and Cosmides argue is incoherent:

> Incoherent environmentalists . . . are those who propose theories of how environments regulate behavior or even psychological phenomena without describing or even mentioning the evolved mechanisms their theories would require to be complete or coherent. In practice, communities whose rules of discourse are governed by incoherent environmentalism consider any such trend toward explicitness to be introducing vague and speculative variables and – more to the point – to be in bad taste as well. The simple act of providing a complete model is to invoke evolved design and, hence, to court being called a genetic or biological determinist. (Tooby and Cosmides 1992, 37)

Other evolutionary psychologists are even more outspoken in their condemnation of social or cultural explanations. David Buss argues that an explanation for a cultural trait *must* include biological mechanisms if that explanation is in fact to explain anything:

> Without these mechanisms, the "cultural differences" literally could not occur. There are two profound implications that follow: (a) Cultural variability, far from constituting evidence against evolved psychological mechanisms, depends on a foundation of evolved mechanisms for its very existence; and (b) cultural variability is not explained merely be invoking "culture" (which merely mystifies the actual causal processes involved) but rather represents phenomena that require explanation. (Buss 1995, 13)

In this view social scientists are unworthy to claim the mantle of "science." Irwin Silverman claims, "The epistemology of science is rooted in the search for definable causes. The concept of culture has no explanatory value unless it includes the ultimate question of how and why cultural traits evolved in the ways that they did (Baumeister 2005; Tooby and Cosmides 1992)" (Silverman 2007, 542).

With these claims for explanatory coherence pushing far beyond the minimal requirement that the social scientist not contradict the "more basic" natural sciences, it is worth noting the strength of the assertion: to explain cultural or social phenomena, the social science is *required* to include the biological capacity for culture in the explanation because culture, by itself, explains nothing. Buss is quite clear on this point: "'Culture,' 'learning,' and 'socialization,'" he writes, "do not constitute explanations, let alone alternative explanations to those anchored in evolutionary psychology. Instead, they represent human phenomena that require explanation. The required explanation must have a description of the underlying evolved psychological mechanisms at its core" (Buss 1995, 13). Silverman agrees: "Until psychologists become involved in questions of ultimate causation, they will continue to function on a comparable level to tribal weatherman and children. It is noteworthy that none of the natural sciences other than the behavioral sciences ignore their ultimate questions. Biologists of all stripes are concerned with the origins of life, physicists with the origins of matter, and progress on these questions is a mark of the progress of the disciplines" (Silverman 2007, 544).

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The invocation of the word "ultimate" in these claims is significant. The problem with this formulation is that given the ontological unity and causal chain posited by evolutionary psychologists that there is nothing "ultimate" about evolutionary explanations, other than Ernst Mayr's clever appropriation of the word "ultimate" for them (Mayr 1961). It is not as if the causal chain begins with biology: how was the earth formed in order to allow for life to evolve? The writings of evolutionary psychologists seem completely unaware of the burdens imposed on them by their own standards of explanatory coherence. They write as if Mayr's "ultimate" explanations in evolutionary biology are "ultimate" in some metaphysical sense of the word. On the contrary, a geologist could rightfully argue that evolutionary biologists must include geology in their explanations of adaptation for it is the environment that allowed life to begin. The notion that biological explanation must include inorganic causes was a live topic of investigation in Kroeber's time. Lawrence J. Henderson, for example, argued that biological explanation must include chemical and geological components that allowed organic life to exist at all. "Fitness of environment is quite as essential a component as the fitness which arises in the process of organic evolution," he declared (Henderson 1913, v). Following the logic strictly, we would be left with the notion that any explanation of human behavior must begin with cosmology. Kroeber recognized the near-infinite regress of this formulation and I show in the next section his pragmatic solution to the problem.

At the end of this paper, I will return to the idea that an explanation for social/cultural behavior *must* have a basis in biology. At this point, however, let us examine the context in which this claim is made. Evolutionary psychologists' claims for unification come into clearest view when they offer an account of the history of the social science disciplines and how those disciplines banished Darwin in the early part of the twentieth century. Such a narrative sets the stage for evolutionary psychologists to sweep to the rescue, transforming the incoherent social sciences into truly scientific natural sciences.

The Standard Social Science Model

Evolutionary psychologists have a ready explanation for why the social sciences hold themselves apart from the natural sciences: the Standard Social Science Model. The SSSM, according to Tooby and Cosmides, rejects the idea that the social sciences must rest on a foundation of biology because the SSSM portrays the mind as a blank slate (*tabula rasa*), and the only possible impressions on that slate were social/cultural ones:

> The mind could be seen as complex, but its procedures were still assumed to be content-free. As long as environmental input could enter and modify the system, as it clearly could, environmental input was presumed to orchestrate the system, giving its functional organization. It doesn't matter if the clay of the human mind has some initial shape

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> (tendencies, dispositions), so long as it is soft enough to be pounded by the external forces into any new shape required. (Tooby and Cosmides 1992, 29)

Thus, according to the SSSM, biology had no part in culture and the social sciences had no duty to acknowledge that humans are the products of evolution. The result was the complete autonomy of the social sciences from the natural sciences, which Tooby and Cosmides argue "has been a conscious, deeply held, and strongly articulated position, advanced and defended since the inception of the social sciences." In cultural anthropology, whatever their methodological differences, "the founders of American anthropology, from Kroeber and Boas to Murdock and Lowie, were equally united on this point" (ibid., 22).

Evolutionary psychologists delve into the history of science in order to show the terrible mistakes made by the founders of American cultural anthropology. Here we can hand the bulk of the exposition over to Tooby and Cosmides's colleague, Stephen Pinker, and his bestselling book, *The Blank Slate* (Pinker 2002). Pinker recounts a history of the growth of cultural anthropology and behavioral psychology in the twentieth century. Both of these disciplines, Pinker maintains, hold that the mind is a blank slate, and hence, anything the mind contains must be explained by reference to culture, which is taken to be a free-floating, autonomous, metaphysical, mysterious *something*.[3]

Pinker explains that at the dawn of the twentieth century, the doctrine of the blank slate was used to great effect against the racist and sexist sciences of the time. The key term was "culture," which was severed from its foundations in race most effectively by Franz Boas. Boas used idealism to "lay a new intellectual foundation for egalitarianism. The differences among human races and ethnic groups, he proposed, comes not from their physical constitution but from their *culture*, a system of ideas and values spread by language and other forms of social behavior" (ibid., 22). This explanation for the rise of Boasian anthropology is both a compliment and a condemnation. On the one hand, Pinker praises the social and political impact of racial egalitarianism. On the other, his clear implication is that, because Boas's success was political, it could not have been scientific. The notion that the SSSM was triumphant for ideological rather than scientific reasons, is an important theme in EP's presentation of their foe. That Boasian anthropology was *both* a scientific and a political success Pinker never even offered as a possibility.

According to Pinker, Boas argued that unless the contrary can be proven, we must assume that all differences are socially determined rather than hereditary. Boas's students went further. Relying on Carl Degler,[4] Pinker points the accusing figure straight at

[3] Space does not permit an examination the EPists' claims that behaviorism was an anti-Darwinian psychological program but Christopher D. Green has critiqued that claim by showing how behaviorism was rooted in Darwin (Green 2009).

[4] Pinker gives no indication he has ever read any works by Kroeber, indeed he even gets Kroeber's name wrong, referring to him as "Albert" Kroeber (see Pinker 2002, 23). Every quotation of Kroeber in *The Blank Slate* is

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Boas's first doctoral student at Columbia, Alfred Kroeber. "Boas had created a monster," Pinker writes,

> His students came to dominate American social science, and each generation outdid the previous one in its sweeping pronouncements. . . . Kroeber did not just deny that social behavior could be explained by innate properties of minds. He denied that that it could be explained by *any* properties of minds. A culture, he wrote is *superorganic,* it floats in its own universe, free of the flesh and blood of actual men and women. (Ibid., 23)

Pinker argues that because Kroeber wrote at a time of rampant scientific racism, with the vestiges of vitalism in biology still apparent, he could perhaps be forgiven for thinking that biology had nothing do to with culture. However, Pinker continues, we now have enough evidence from evolutionary psychology to put aside the notion that culture can only be explained by reference to culture. Evolutionary psychology has arrived, and it is time for social scientists to fall into line with the program.

Such a view of the SSSM and its history are ubiquitous in EP. In their manifesto Tooby and Cosmides are reticent about ascribing the SSSM to any particular social scientist; rather they claimed to be distilling several views into a model that was not necessarily embraced by any one individual. Thirteen years later, however, they have lost such diffidence:

> For almost a century, adherence to the Standard Social Science Model has been strongly moralized within the scholarly world, immunizing key aspects from criticism and reform (Pinker 2002; Tooby and Cosmides 1992). As a result, in the international scholarly community, criteria for belief fixation have often strayed disturbingly far from the scientific merits of the issues involved, whenever research trajectories produce results that threaten to undermine the credibility of the Standard Social Science Model. (Tooby and Cosmides 2005, 7)

In an EP textbook, Workman and Reader argue that Boas used the SSSM to fight racism and classism: "From these honourable beginnings, Tooby and Cosmides argue that the SSSM became the dominant approach to the study of human behaviour and tended to stifle alternative approaches (see Pinker 2002). Many social scientists developed an almost pathological fear of biological explanations of human behaviour, a disposition that sociologist Lee Ellis (1996) termed *biophobia*" (Workman and Reader 2004, 14). In his EP textbook, Bruce Bridgeman concurs: "What [the social sciences] have in common is the SSSM, the idea that the critical variables for understanding human behavior, experience, and social structure are primarily environmental and cultural rather than biological. Human nature in this view is reduced to not much more than a capacity for culture" (Bridgeman 2003, 6).

taken from Degler 1991, and none of Kroeber's 600 or so publications appear in Pinker's bibliography. For an analysis of Pinker's misuse of Degler's work, see Winston 2006.

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Such claims can begin to sound conspiratorial – evil social scientists have blocked free inquiry and shut off fruitful avenues of exploration (Winston 2006). Consider Gandolfi, Barash, and Gandolfi's claim that the SSSM "has tremendous influence on the way late twentieth century Americans think about social problems. Even those who have never taken a formal course in anthropology, psychology, or sociology use some version of it in their thinking. It dominates all the social sciences (except for economics), including history and political science" (Gandolfi, Barash, and Gandolfi 2002, 10). Like Pinker, they asserted that Alfred Kroeber deserves a share of the blame for this, with his "immensely influential article published in 1917" on the superorganic (ibid.). Somit and Peterson, who press for an evolutionary approach to public policy, also cite Tooby and Cosmides when they argued that "For at least the past fifty years, the so-called Standard Social Science Model (SSSM) has dominated the behavioral sciences, colored popular thought, and, by its pervasiveness, directly and indirectly influenced public policy" (Somit and Peterson 2003, 3). Against this "hegemonic SSSM," which has been "long the reigning paradigm," they claim that the evolutionary approach is a "near 180-degree shift in orientation" (ibid., 4).

Far from being an ancillary claim unnecessary to the advancement of the program, the historical portrayal of the dominance of the SSSM forms the central appeal in the call for EP in the broad sense. It allows evolutionary psychologists to tap into the ideology that there is only one way of doing science because there is only one universe in which we live. Thus they can assert that social scientists are not really scientists at all: social scientists offer descriptions of the social world "any way they please" which means that their descriptions are completely unmoored from reality. All explanations for culture or social behavior offered by social scientists are incoherent because they leave out necessary parts of the explanation: in particular the biological grounding for social behavior. Finally, social scientists hold to these views, not from any scientific motive, but because of political and ideological views, perhaps justified a century ago, which are now antiquated and unjustifiable.

All of this flows from the stipulated definitional move that culture *really is* nature and that the scientific task is to trace the causal chain that gave rise to culture out of our biology. Let us now turn to Alfred Kroeber, the key figure in the history of American cultural anthropology, whose call for a superorganic view of culture has been condemned for promoting the view that biology played no part in human culture.

Alfred Kroeber and the Superorganic

Like their claims about the unity of the natural sciences, evolutionary psychologists' claims about the dominance of the SSSM and the absence of biological explanations in the social sciences simply are not true. Apart from scattered quotations from celebrated social scientists like Kroeber or Durkheim, no evidence is ever offered to show how biology has been banished from the social sciences. Science studies scholars who have

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actually researched the history of the social sciences come to the opposite conclusion arguing instead that biological explanations have remained in the social sciences throughout the twentieth century. If, as Kuhn has claimed, we know a paradigm by its textbooks, then surveys of textbooks in twentieth-century social sciences have demonstrated that biological explanations have never disappeared even on such hot-button issues as race and gender (Littlefield, Lieberman, and Reynolds, 1982; Morning, 2008; Winston, Butzer, and Ferris, 2004; Winston 2006). Even eugenics, supposedly banished from proper thought by World War II, persisted in biology textbooks until the 1970s. Tracing the presence of eugenic thought in American biology textbooks, Mark Largent has noted that "biologists' advocacy of eugenics began in the 1960s. The percentage of textbooks that advocated eugenics continuously increased until the end of the 1960s. . . . Throughout the 1960s and 1970s there was no critical discussion of eugenics or of compulsory sterilization in American biology textbooks" (Largent 2008, 128). In short, when historians look for evidence of the hegemony of the SSSM, it is not forthcoming. It is therefore worth looking at the rise of cultural anthropology and the superorganic concept in some detail to see if the specific claims of the evolutionary psychologists have any basis in the historical record.

Franz Boas and the founding of American cultural anthropology is a topic that has spawned a vast literature (Baker 1998; Barkan 1992; Briggs and Bauman 1999; Cravens 1978; Darnell 1990; Degler 1991; Gilkerson 1986; Gossett 1963; Handler 1990; Helbring 1994; Hoover 1981; Risjord 2000; Stocking 1968; Williams 1996). Boas's project for an autonomous cultural anthropology was predicated on freeing anthropology from Herbert Spencer's stadial theory of culture, which, in the context of late nineteenth-century America carried classist, racist, and sexist implications. George Stocking discussed how Boas's own conception of anthropological science was influenced by the "traditional distinction in German thought between the *Naturwissenschaften* and the *Geisteswissenschaften*, between the sciences dealing with physical nature and those dealing with human spiritual activity" (Stocking 2001, 37; see also Bunzl 1996; Lyman and O'Brien 2004). This enabled him to insist on cultural anthropology as an interpretive inquiry, and to assert its autonomy by distancing it from the notion of evolutionary progress, which was, at the same time, being undermined by Weismannism.

Boas, however, is not the villain of the set piece offered by the evolutionary psychologists, indeed Pinker has relatively kind words for Boas. It is Alfred Kroeber, who is blamed for exiling Darwin from cultural anthropology. The "first Boasian," Kroeber received his Ph.D. under Boas's direction in 1901 and moved to Berkeley, California, to build a museum and an anthropology department (Jacknis 1993; Jacknis 1996; Jacknis 2002). Historians have focused on Kroeber's work with American Indians (e.g.. Buckley 1989; Buckley 1996) because Kroeber, never considering himself a "formal theoretician" of the culture concept, claimed that his thoughts on the theory of culture were a "by-product" of his empirical work in anthropology "sweated out piecemeal and slowly over fifty years" (Kroeber 1952, 3). I will focus on a series of

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works Kroeber published in the first two decades of his career (Kroeber 1901; Kroeber 1915; Kroeber 1916a; Kroeber 1916b; Kroeber 1917; Kroeber 1918; Kroeber 1923), where he most fully articulates his belief that anthropology is a separate discipline from biology.

In the next sections, I will show that unlike his teacher, Kroeber believed division of culture and biology had less to do with European traditions, which turned on the possibility of reducing the "human spirit" to a materialist base, and much more to do with a pragmatic definition of culture that was not ontological in nature (although it could appear to be so). For Kroeber, the actual division of biology and culture was not at issue; the crux for him was what phenomena should be invoked in our cultural explanations and what phenomena should be accepted as part of our background assumptions. Despite evolutionary psychologists' claims that Kroeber was hostile to Darwin and biology, Kroeber was in fact a staunch Darwinist and, what is more, embraced Weismannian doctrines of hard heredity. Evolutionary psychologists are right that Kroeber was seeking to give cultural anthropology a subject matter of its own; they err in claiming that he did so by turning his back on Darwin. Given Kroeber's embrace of Darwin's defender, August Weismann. I agree with Maria Kronfeldner who argues, "we can make historical sense of [of Kroeber's work] only if we acknowledge its Weismannian underpinning" (Kronfeldner 2009, 107). Although he spoke not of "mental modules" but instead of "instincts" and "tendencies" of the human mind, nevertheless Kroeber believed in the very "psychological universals" denied him by evolutionary psychologists. Kroeber offered a pragmatic definition of culture in which biology and psychology had no place, *despite accepting the reality of humans as products of natural selection with psychological universals*. Far from seeing Darwin as a threat to the autonomy of his discipline, Kroeber actually saw Darwin as a scientific hero, and took arms against those who proclaimed themselves Darwinians without ever really understanding the master's work.

Eugenics, Race, and Heredity

Despite evolutionary psychologists' claims about the century of hegemony of the SSSM, the early twentieth century was marked by heated debates about the relationship of biology to social life: eugenics (Kevles 1985), the instinct debate in psychology (Hampton 2004; Hampton 2006), functionalism and Darwinism in psychology (Green 2009), and debates about the racial basis of civilization (Barkan 1992; Cravens 1996; Jackson and Weidman 2004).[5] Kroeber in particular faced off with the American eugenics movement, specifically the hard core of racialist anthropologists exemplified

[5] Kroeber also needed to evade the evolutionism inspired by Herbert Spencer. According to Spencerian views of culture, there was a natural hierarchy of civilization and a certain inevitability of ranking of societies. Cultures in the Spencerian tradition were more evolved or less evolved and could be understood by reference to an inner

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by Madison Grant. Civilization was a product of biological race according to Grant and hence the subtitle of his most famous work was "The Racial Basis of European History" (Grant 1916). Kroeber detested these views, "So far as civilization is concerned, there is no such thing as an Anglo-Saxon breed or a white man's burden," he declared in 1914 (Anon. 1914, 1). One motivation for Kroeber was to carve out a place for anthropology as a discipline with a subject matter of its own precisely to stop the kind of academic racism represented by Grant. Writing to Edward Sapir in 1917, Kroeber declared, "I'm tired of anthropologists being a charity orphan allowed to pick up a profusion of scraps until biologists or geographers or psychologists or Madison Grants take a fancy to having them again. . . . We don't get respect now: we get kindliness and tolerance. And I'm fed up on it" (quoted in Golla 1984, 234).

Kroeber had two targets for his definitional work, which was dedicated to a clear explication of culture's relationship to biology. His first enemies were the racialist anthropologists of the eugenics movement typified by Grant. His second target was the social scientific community – a community that he felt was not fulfilling its obligations to fight back the eugenics movement. Kroeber believed that audacious racists and timorous social scientists were confused about a central scientific matter: the inheritance of acquired characteristics, which he referred to as "use inheritance." By the early twentieth century, Mendel had been rediscovered, and Weismann's work was making its way into the United States; however, Lamarckian doctrines still held significant sway among many scholars and in most of the debates about race formation and instincts in psychology that Kroeber was involved in. Kroeber believed that by clarifying the notion of use inheritance, he could both create a space for anthropology as a non-biological science and clear away mistaken ideas about evolution itself. As George Stocking has observed, Kroeber "seems to have been virtually alone among social scientists in realizing what had been the implications of Lamarckianism for the independent development of the social sciences" (Stocking 1968, 259; see also Kronfeldner 2009). The roots of Kroeber's attack on use inheritance can be found in his very first publications; indeed, he sets the stage in his doctoral dissertation.

Kroeber and Universal Psychological Mechanisms

In telling critiques of evolutionary psychologists' account of the SSSM, Simon Hampton points out that they are often quite ambiguous about exactly how long the SSSM has been dominant: sometimes they claim sixty years; sometimes a century; sometimes they date the SSSM back to Locke or Descartes. Hampton argues that one major debate completely missed by the evolutionary psychologists is that about the

drive that made for progress. In the interests of shortening this already too-long paper, I will not discuss this aspect of Kroeber's work.

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existence of psychological instincts in the first third of twentieth century psychology. After a detailed account of the instinct debate, Hampton concludes that

> psychological and behavioural thinkers have for long periods been immersed in the implications of Darwinism. It is plainly and factually incorrect for evolutionary psychology to deny this. And it is disingenuous to down-play it. Evolutionary psychologists who use the term "Standard Social Science Model" and rhetorical equivalents such as "the neo-behaviourist tradition" (Nicolson 1999, 5) and the "the tabula rasa view" (Crawford 1998, 4) undermine their own much-vaunted rigor. (Hampton 2004, 38)

Indeed, Hampton finds that much of the current debate about psychological adaptations mirrors the earlier instinct debate quite closely (Hampton 2006).

Hampton's discussion of the instinct debate informs my examination of Kroeber's ideas about culture because Kroeber needed to frame his ideas against these widely discussed notions about psychological universals. Clearly Kroeber was willing to admit that such universals existed; he simply denied that they had a part to play in explanations of culture. For example, Kroeber's doctoral dissertation examined decorative artwork among the Arapaho Indians. Although in this early work Kroeber did not fully embrace the cultural relativism that would later be a calling card of self-respecting Boasians, he wrote in terms of "higher" and "lower" civilizations – terminology that would soon disappear from the repertoire of the Boasians. Other aspects of the work did bear the marks of Boas: close attention to detail, ample description of Arapaho artwork, and an insistence that the researcher have contact with the culture under study.

Beyond providing a rich description of Arapaho artwork, Kroeber was interested in examining the cultural function of art: was primitive art meant to represent reality or to be purely decorative? Kroeber answered this question by refusing to accept the binary; he argued that primitive art was bound by aesthetic convention but, within those conventions, was meant to be realistic. So, "the main characteristic of Arapaho art [is] its fusion (which is more truly an undifferentiation) of the realistic and decorative tendencies" (Kroeber 1901, 324).

Kroeber had a universalist point to make about "all primitive art" (ibid.). After cataloging examples of the undifferentiation of the decorative and realistic functions of art from all over the world and while warning of the danger of generalizing from "selected examples such as these," his dissertation nonetheless concluded:

> This fusion of two differing tendencies is not merely a frequent or widely distributed occurrence, as are a great many special ethnic phenomena, such as circumcision or doctoring by sucking or angularity of ornament, but this fusion is a rule practically without exceptions. It is universal because it is necessary. Both the representative tendency and the decorative tendency are deep rooted in the human mind, so that it must be virtually impossible to suppress them for any length of time or among any considerable number of men. (Ibid., 326)

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"Every culture" Kroeber continued, "must contain among its motive forces more or less of every tendency, because the tendencies are in the human mind and hence ineradicable" (ibid., 327). But while admitting that the origins of art could indeed be found in the universal psychological mechanisms of the human mind, Kroeber denied that such mechanisms could play an explanatory role in anthropological science. Those searching for the origins of art, Kroeber argued, were faced with intractable problems. If the origin of pictorial representation was very old and emerged gradually over long periods of time, then the origin was lost to the investigator in the mists of time. "But if it, therefore, were comparatively recent in origin," Kroeber continued, "there must until a certain time have been no art among the Arapaho, while at that moment it sprang up full-blown, not as a crude undifferentiated thing, but a highly-specialized pictorial art. Such an event would be extremely remarkable, not to say marvelous, and more in need of an explanation than the phenomenon it explained" (ibid., 329). There would be no principled reason to stop the causal chain that led to art and declare it as *the* origin of events. Hence, the search for origins was futile, because "no myth, no artistic convention, nor any other thing human, ever sprang up from nothing" (ibid., 333). Understanding the pattern of culture, a phrase later made famous by another Boasian, Ruth Benedict, was the goal of anthropology, not a search for the origins of culture. Hence, since the causal chain of any cultural event was, more or less, infinite, Kroeber rejected the idea that one could explain culture by pointing to a previous event and declaring it *the* cause.

A second problem Kroeber identified was what we would call the "overdetermination" problem. In examining rival accounts for the origins of mythology, Kroeber noted that each account was rooted in a psychological capacity, or "tendency," of human beings. Although each theory captured a tendency of human behavior, none of them could stand as *the* explanation of the origin of myth: "This multiplicity of tendencies or causative forces necessarily refutes any explanation that uses and allows only one of them" (ibid., 332). Hence, Kroeber defined away the psychological as outside the province of anthropological inquiry. "These tendencies," Kroeber argued, "being inherent in mind, are everlasting." However, universal psychological tendencies, which Kroeber admitted were "at the root of all anthropological phenomena," were not themselves the object of study for anthropologists (ibid., 332). For, while the psychological mechanisms of the mind were fixed, the cultural patterns they produced were infinitely varied; the *products* of mind were the object of study, not the mind itself. "The phenomena of activity have changed as these tendencies and their relations to one another have become modified." Kroeber concluded: "Therefore the products of mind (the phenomena studied by anthropologists) are, like mind itself, beginningless (for us)" (ibid., 333). The last two words here are significant: it is not that culture is a mystical force without beginning; it is that *for us* it is so. The definition here is a pragmatic one, drawing a boundary around the proper domain of inquiry for cultural anthropologists.

Kroeber thus rejected the very idea of science that the evolutionary psychologists claim as the hallmarks of science – explaining events by reference to previous

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events – and thus escaped the trap that evolutionary psychologists have unwittingly laid for themselves. Kroeber argued that such an explanatory scheme would doubly fail: first, because the causal chain stretched back forever, and, second, because there were a multiplicity of causes for a cultural trait and no absolute way to privilege one over any other. Yet, Kroeber clearly offered a demarcation between the mind and the products of the mind. The mind he relegated to psychology and biology; but the products of the mind were the province of cultural anthropology. As he would develop this idea, it was culture that he would label *superorganic*. This definitional move, however, was not an ontological claim. His definition of the superorganic was a pragmatic one: he argued that there were good reasons for treating culture as separate from biology, and it was irrelevant that culture was biological in origin. Such a pragmatic move, he hoped, would rid the social sciences of the specter of Lamarck and rid the biological sciences of the hated eugenicists.

Kroeber and the Pragmatic Definition of the Superorganic

In the early part of the twentieth century, every industrialized country embraced some form of eugenics (Dikötter 1998; Kevles 1985; Largent 2008; Nye 1993; Paul 1998). Eugenics was a science characterized as "not so much a clear set of scientific principles as a 'modern way' of talking about social problems in biologizing terms" (Dikötter 1998, 467). For Kroeber, the eugenics movement was worse than a political mistake; it was a scientific blunder because it confused biological phenomena with social phenomena. Invoking the idea that one could conceivably trace a causal chain infinitely, he noted that, "Chemists do not feel impelled to expound the rise of genius in chemical terms or explain the variety of moral codes by valences and atomic weights. They therefore leave civilization alone, or if they pronounce judgments in its field, do so avowedly as laymen. But biologists view the province of the social from their very doorsteps" (Kroeber 1916a, 38). Chafing at the eugenicists' encroachment into the province of the cultural and social, Kroeber was one of the most outspoken scientific critics of eugenics in the United States in the 1910s. At the time, few, if any, other American scientist said things like this:

> [Eugenics] is more refined but no less vain than the short cut which the savage follows, when, to avoid the trouble and danger of killing his foe in the body, he pierces, in the safety and amid objurgations uttered in the convenience of his own home, a miniature image addressed by the name of the enemy. Past ages have had their dragons of superstition to fight. Our battles against this ever re-arising brood dawn no smaller and as unceasing; and it would be shallow to try to defer or soften the inevitable conflict by withholding from this movement its true designation. Eugenics . . . is a fallacy. It is a mirage like the philosopher's stone, the elixir of life, the ring of Solomon, or the material efficacy of prayer; and to those who are led by its learned modernity to receive it earnestly, it is a destructive snare. (Kroeber 1917, 188–189)

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Kroeber took this position not because he rejected evolutionary theory but because he embraced it. Evolution, Kroeber insisted, was an ancient idea, and he offered evolutionary myths of many cultures to prove his point. Darwin was certainly not the first evolutionist; rather, Kroeber argued, Darwin's genius was to combine three ideas – variability, heredity, competition – into the *process* of natural selection.[6] While Kroeber was sure that Darwin's ideas might undergo some further modification as new developments shed further light on the process, "but the world must probably forever believe that natural selection is of some influence in the shaping of life" (Kroeber 1916a, 25).

It was left to August Weismann and his doctrine of hard heredity, to complete Darwin's theory, according to Kroeber, because Darwin did not break with the "older pseudo-process of Lamarck" of use inheritance (ibid.). Kroeber maintained that Weismann "was as clear a thinker as Darwin; and his accomplishment will in the end be rated in proportion" (ibid., 26). Under Weismann's "onslaught," Kroeber declared, "the Lamarckian structure" proved to be "absolutely hollow. Experiment failed to produce even a scrap of positive evidence in its favor. Renewed examination demonstrated that there was not a single alleged instance which was more than logically possible. Practically every case of use inheritance was explicable by selection" (ibid.).

The power of Darwinian natural selection combined with the overwhelming evidence for hard heredity left Kroeber with a puzzle: although biologists had stopped talking about use inheritance, few were trying to stamp it out as a "pernicious heresy" as he thought they should (ibid., 28). Indeed, "Scarcely anywhere since Weismann," Kroeber noted, "is there any zeal against the doctrine of acquired heredity as something radically and vitally and destructively wrong. Biology... scarcely professes a cardinal article of faith on acquired heredity. What brings it about that there exist so much weak condemnation, half tolerance, and hankering?" (ibid., 28–9)

The reason, Kroeber argued, was that there were actually two evolutionary processes: a biological one, in the Darwinian/Weismannian mode, and a social/cultural one. In this second mode, "use modification is permanent and transmittal of the acquired exists" (ibid., 31). Civilization was inherited, Kroeber argued, but strictly in a non-organic process:

> Speech, knowledge, arts, learning, and all our activities except the bare substratum of physiological abilities, are not inborn. Heredity gives us the slate and the pencil in good working order. Our individual kinds of slates and the sharpness of the pencils are also wholly from heredity. But with the writing on the slate, which is the part we play in civilization, heredity has nothing to do. That comes from social situation, in other words the existing civilization into which we are born. (Ibid.)

[6] Kroeber understood what the modern evolutionary psychologists appear not to understand: evolution is a study of *processes* rather than *products*. This point is made ably in Buller 2005, 428–429.

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Unlike organic evolution, civilization was cumulative and progressive. It was the fundamental confusion between social inheritance and biological heredity that perpetuated the mistaken doctrines of use inheritance in the scientific and public minds and, simultaneously, led to the mistaken doctrines of eugenics. "The entire doctrine of eugenics is an endeavor to attain moral ends by biological means," Kroeber argued. "Moral of course is social; and yet the open protests have come – strange partnership! – from the orthodoxly religious and the professedly skeptical, but rarely from the enlightened camp of science" (ibid., 34).

While he raged against the popularizers like Madison Grant in his letters, Kroeber also found plenty of academic targets to criticize in his professional publications. An example from Kroeber's "Superorganic" article was Gustave Le Bon, who, in his *Psychology of People*, took as his task "to describe the psychological characteristics which constitute the soul of races, and to show how the history of a people and its civilization are determined by these characteristics" (Le Bon 1912, xvii). Kroeber was clearly disgusted by this because "as a scientific concept or tool, a race soul is as intangible and useless as any phrase of mediaeval philosophy" (Kroeber 1917, 185). "If," Kroeber argued, Le Bon

> had said spirit of civilization, or tendency or character of culture, his pronouncements would have commanded less appeal, because seeming vaguer; but he would not have had to rest his entire thought upon a supernatural idea antagonistic to the body of science to which he was trying to attach his work; and if non-mechanistic, his efforts at explanation would at least have earned the respect of historians. (Ibid.)

Worse, seeming not to understand the science upon which his work was supposedly based, Le Bon argued that the progress of civilization depended on the accumulation of racial traits. Once again, Kroeber leapt to the defense of Weismann by distinguishing organic heredity from civilizational inheritance. Kroeber argued that characteristics don't accumulate in natural selection and only arise out of response to a local environment. Civilization – considered as superorganic, non-biological culture – *does* accumulate as knowledge progresses over time. "If there is anything that heredity does not do," Kroeber declared, "it is to accumulate. If, on the other hand, there is any one method by which civilization may be defined as operating, it is precisely that of accumulation" (ibid., 186). By refusing to understand the difference between biology and culture, Le Bon had produced a work that was neither scientific nor historical. Only by keeping each discipline in its own realm, Kroeber concluded, could each produce worthy work.

It was not just returning biologists to the organic realm that interested him, however. For Kroeber, social scientists had a greater responsibility than the biologists for repairing the damage done by the eugenics movement:

> . . . biology has been born in the last century or two. It has forged its weapons, taught itself their use, conquered a territory and stands forth a young giant of prowess. What wonder

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> that it has proceeded by the divine right of power to annex the antiquated realm of history that lay adjacent, and to impose its rule and laws without inquiring whether they were fit? The greater fault is not with the biologists who have explained historical phenomena by organic processes, but with the sociologists who have accepted and welcomed these alien explanations. (Kroeber 1916a, 34)

Kroeber's exemplar here was Lester Frank Ward, first president of the American Sociological Association, who spoke out forcefully against Weismannism in a number of influential publications (Ward 1891a; Ward 1891b; Ward 1903; Ward 1907). Ward was perfectly willing to accept that Weismann and his followers had disproved the existence of use inheritance in animals, but "when the human species is to be treated, the tables are, in a manner, turned" (Ward 1891b, 315). Ward looked to use inheritance as the means of transmitting those intellectual and moral traits that make us human:

> The whole point at issue is whether there is a causal relation between the cultivation of these faculties and their development; in other words, whether the increment gained by their exercise is transmitted to posterity. Professor Weismann and most of his followers, constituting what is now generally known as the school of Neo-Darwinians, deny such transmission. If they are right, education has no value for the future of mankind, and its benefits are confined exclusively to the generation receiving it. (Ibid., 319)

George Stocking has noted that Ward's embrace of use inheritance was an attempt to keep the biologists at bay. "In the absence of a concept of culture severed from all biological connections, to abandon Lamarck and accept Weismann would be to yield up the social sciences to an unrestricted biological determinism" (Stocking 1968, 256).

Kroeber's solution to the problem was to offer up just the concept of culture that, Stocking says, was needed. Pointing out that Ward argued forcefully for heredity by acquired characteristics because that is the only way we can get "permanent progress for humanity," Kroeber held, "It is, if not a deep view, a common one; and for that reason Ward's formulation is, however worthless intrinsically, representative and significant" (Kroeber 1917, 187). Ward, Kroeber continued, was simply following the traditions of sociological inquiry begun in the nineteenth century by Spencer and Comte. Indeed, Kroeber had borrowed the term "superorganic" from Spencer himself, but "in spite of his happy coinage of the term which has been prefixed as a title to the present essay, he did not adequately conceive of human society as holding a specific content that is non-organic" (ibid., 188). All these writers failed to adequately recognize that culture could be treated as a completely separate entity. "Civilization," Kroeber wrote (echoing back to his dissertation), "is not mental action but a body or stream of products of mental exercise. Mental activity, as biologists have dealt with it, being organic, any demonstration concerning it consequently proves nothing whatever as to social events" (ibid., 192).

Thus, Kroeber was not putting forth a metaphysical claim about the reality of the superorganic, as Pinker has charged. Rather, he was arguing for a pragmatic

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and pluralistic approach to science. For him, although the successes of biology and psychology were certainly impressive, they hardly provided the only ways by which one could achieve an understanding of humans. "Mechanistic science has accomplished wonders in a brief space by adhering ever more rigidly to its own peculiar methods, and allowing no limits to be set to its application of these methods," Kroeber admitted. "Yet that a tool has proved its service for a purpose, does not affect the value of other purposes or the utility of other tools for these other purposes" (Kroeber 1917, 207). The view one took of humanity, Kroeber concluded, depended entirely on the goals one has for inquiry: "The applicability of science to any and all domains of human cognizance must be expressly affirmed. But the same phenomenon can after all be viewed with different ends" (ibid., 208). Or, as he expressed it privately to Edward Sapir,

> I don't give a red cent whether cultural phenomena have a reality of their own, as long as we treat them as if they had. You do, most of us do largely, but most of [us] hang back and fear to avow it and let geographers and biologists . . . walk over us. If we're doing anything right, it deserves a place in the world. Let's take it, instead of being put in a corner. That's not metaphysics: it's blowing your own horn. (Quoted in Golla 1984, 244).

Indeed, when Sapir took to print to criticize aspects of Kroeber's proposal, the title of his article was not "Is There a Superorganic?," but rather "Do We Need a Superorganic?" (Sapir 1917). The question was, therefore, not an ontological one, but a pragmatic one.

Conclusion

Nothing I have written here has anything to do with empirical adequacy of the accounts produced by EP: they may be absolutely correct about the modular theory of the mind, our maladaptedness to modern environments, their claims about gendered behavior of the sexes, and our preferences for landscape art calendars (a topic given a whole chapter in Pinker). However, even if they turn out to be correct (and I personally have my doubts they will), it simply does not follow that such empirical claims would require a reorganization of other branches of human inquiry to take those claims into account. Whatever else they are, scientific disciplines are rhetorically and socially constructed (Gieryn 1999; Taylor 1996). They are human creations that allow for interested investigators to gather and pursue investigations of the natural and social world. The formation of disciplines is not a simple mapping of human organization onto the organization of nature. Derksen has recently argued: "As numerous studies in the sociology, history and philosophy of science have shown, the organization of science is not a straightforward consequence of natural relations, nor can the ideal of a unity of science be deduced from the unity of reality" (Derksen 2005, 147–8). Such work in the science studies disciplines appears to be unknown to evolutionary psychologists, who

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naively proclaim that the natural sciences are unified and that such unification flows naturally from the unity of nature. Kroeber enjoyed a much clearer understanding of how modern disciplines function. He understood that by limiting cultural explanations to cultural factors, he could create autonomy for cultural anthropology as well as for the biological sciences. Whether or not culture *really was* superorganic was not the issue for him, it was that if we pragmatically treat it as if it were, both the social and biological disciplines would flourish.

My argument here should not be read as a brief against interdisciplinary collaboration, indeed such a paper would be inappropriate for an interdisciplinary journal such as *Science in Context*. Rather, my argument is that the specific claim of evolutionary psychologists to unite the sciences relies on a naïve view of the nature of inquiry, an outmoded view of the unity of science, a poorly researched and argued history of science, and is insulting to practitioners in the human sciences. In her rhetorical study of "interdisciplinary inspirational works of science," Leah Ceccarelli argued that a successful call to interdisciplinary collaboration had to be strategically crafted as to promise benefits to autonomous group of researchers (Ceccarelli 2001). Her example of a failed interdisciplinary inspirational work was E. O. Wilson's *Consilience*, which unfortunately is the model for evolutionary psychologists as evidenced by the citation patterns I have shown in this paper. They have adopted the colonialist stance of Wilson's book, charged uninvited into established disciplines, declared what they found worthless, and specified that the workers there had better toe the line with one particular view of evolutionary biology, and then wondered why the natives are insulted.

Alfred Kroeber offered a disciplinary demarcation that promised benefits to both his own discipline of anthropology and to the biologist by clarifying Weismann's doctrines of heredity and banishing eugenics as the delusion it truly was. The imperialist and essentialist rhetoric of EP holds no such promises for disciplinary growth. By yoking their discipline to a claim that culture *really is* biology, evolutionary psychologists attempt to absolve themselves of any promises to bring benefits to the colonized social sciences. Moreover, despite their claims that they will bring "coherence" to social/cultural explanations, they embrace such explanations in order to argue *for* their discipline. Evolutionary psychologists proclaim that cultural/social factors cannot explain anything: "That it is done, all the time," write Leiter and Weisberg, "is, alas, a problem for claims like these. (Impossibility claims are always defeated by the actual!).... All behavioral phenomena may have a biological dimension; and all biological phenomena may have a physical dimension. But there is *no evidence* that in order to explain and predict behavioral phenomena you need biology, or that to explain and predict biological phenomena you need physics" (Leiter and Weisberg 2009).

Indeed, despite their normative claim that cultural explanations *require* biology in their account of the rise of the SSSM, evolutionary psychologists point to nothing except cultural factors: social scientists fearing reductionism, loathing of racism and

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sexism, etc. One searches in vain through their accounts for the mental module that created the capacity for early cultural anthropologists to engage in the behaviors they did. And yet, despite being incoherent by evolutionary psychologists' own normative standards of explanation, the purely social/cultural explanation for the rise of the SSSM is accepted and repeated (and repeated and repeated). It seems that despite their claims to the contrary, evolutionary psychologists may understand that cultural explanations, absent biology, are perfectly coherent after all.

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