

THE POTENTIAL RELEVANCES OF BIOLOGY TO SOCIAL INQUIRY

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■ **Abstract** Sociologists often react with hostility to explanations that evoke biology, and some critics of the discipline contend that this “biophobia” undermines the credibility of sociology and makes it seem increasingly irrelevant in larger public debates. The negative reactions are many times diffuse and undiscerning of the different endeavors lumped together whenever one speaks broadly of biological (or “biosocial”) explanations. We seek to introduce greater awareness of these distinctions with a review organized in terms of some of the distinct ways that the biological can be asserted to be relevant to the conduct of social inquiry. The review has three sections. First, we discuss assertions of the relevance of the human evolutionary past for understanding the character of human nature, for which evolutionary psychology currently receives the most attention. Second, we consider the work of behavioral genetics and the assertion of the relevance of genetic differences between persons for understanding differences in behaviors and outcomes. Third, we consider assertions of the relevance of particular proximate bioindicators for understanding how the biological and social interact, focusing particularly on studies of testosterone and the prospects of developments in neuroscientific measurement. We do not believe that developments in these fields will force sociologists to acquire considerable biological expertise to pursue questions central to the discipline, but we do advocate further efforts from biologically minded sociologists to articulate understandings of the relationship between sociology and biology that will continue to push us past the commonplace view that biological and sociological explanations are inevitably opposed.

INTRODUCTION

Although sociologists vary greatly in their thoughts on biology and social behavior, there is little question that “biology” can dramatically affect the behavior of sociology’s journals. Recently—and unprecedentedly—the *American Sociological Review* published not just three hostile replies to an earlier articles, but also a statement by its former editor explaining and justifying the original decision to publish the article in the first place (Firebaugh 2001). Not long earlier, *Social Forces* made the unusual move of permitting a book’s authors to respond to a

critical review of their book with a rejoinder of nearly equal length (Fischer et al. 1998). “Biology” was central in both cases. In the former, the provocative article was about biology and the adoption of gendered attitudes and behavior; in the latter, the provocative review concerned biology and racial and class inequality. In no way do we offer these examples to second-guess these journals’ editors; instead, we invoke them to illustrate that few things provoke sociologists as easily or strongly as the perceivedly improper invocation of “biology” as an explanatory device, especially when done on sociologists’ own turf.

Karp (1996, p. 9) declares that he has “preached in his introductory sociology class” that “one of the central messages of my discipline is that culture, rather than biology, is destiny.” To many sociologists, “biology” and the “social” are locked in an explanatory zero-sum game in which any ground ceded to the former diminishes the value of sociology (and the need for sociologists). Yet, even if sociologists did banish “biological” explanations of social behavior from their own forums, swelling interest in the topic would still exist elsewhere in the academy, as would a strong flourishing of curiosity among the general public. In his recent presidential address to the American Sociological Association, Massey (2002, p. 1) lamented that sociologists “have allowed the fact that we are social beings to obscure the biological foundations upon which our behavior ultimately rests.” More strongly, critics within sociology have warned that if the discipline continues its “biophobia,” sociology will be regarded as increasingly irrelevant in public debates (Ellis 1977, 1996; Lopreato & Crippen 1999; Udry 1995; van den Bergh 1990). Indeed, the contention that many sociologists are categorically unwilling even to consider some kinds of biological explanations has already been used as grounds for suggesting that the discipline is not credible and that many of its skeptical reactions can be dismissed as politically motivated (e.g., Alcock 2001, Lopreato & Crippen 1999, Pinker 1997, Thornhill & Palmer 2000).

When discussing these matters with others, we have been recurrently surprised at how some can express a strong hostility toward “biological” explanations without necessarily evincing a coherent conception of what they mean by the phrase. This is the reason we put scarequotes around “biology.” When intended to represent a potential explanatory idiom for understanding social behavior, “biological” actually encompasses a host of various research projects that are regularly confused with each other, even though they sometimes operate with much different assumptions and modes of analysis (which is not to deny either areas of overlap or various efforts at integrative explanation). Our goal in this review is to provide an overview of some of the varying ways in which the specific materiality of the human actor—our “biology”—can be asserted to be relevant toward understanding why we behave as we do or why human societies are organized as they are.

We divide our consideration into three parts: the potential relevance of our evolutionary history, the potential relevance of genetic differences, and the potential relevance of more proximate indicators of human physiology. Within each, we review some of the ongoing research that seems to be among the most intriguing for sociologists to engage, whether as consumer, contributor, or critic. We attempt to

provide a balanced portrait of some particularly contentious areas, and in so doing, we reserve some of our own criticisms in favor of presenting research programs on their own terms, although we include numerous references to both original research and critiques that readers can further explore.

THE RELEVANCE OF OUR EVOLUTIONARY PAST

The “historical turn in the social sciences” might be characterized as an increasing appreciation of how “the history and development of a thing . . . can tell you something fundamental of its nature” (Somers 1998, p. 731). Does this reasoning suggest also that reflection on the history of our species can tell you something fundamental about the nature of the human actor? One way of asserting the relevance of “biology” for understanding human social behavior is to propose that our understanding of human activities can be greatly enhanced by specific consideration of humans as evolved species shaped by processes of natural selection. Sociologists have long recognized that individual actors must be understood as “pregnant with history” (Bourdieu & Wacquant 1992, p. 124; Rubenstein 2001, p. 166); in this vein, evolutionary perspectives can be seen as merely pushing the period of relevant gestation back a few million years.

Serious scientific debates about the neo-Darwinist synthesis as the overarching explanation of the origins of our species are, in the larger scheme of things, disagreements over details. Consequently, if sociologists are really not the implicit cognitive creationists that some critics have claimed (Ehrenreich & McIntosh 1997, Pinker 2002, Shermer 1996), then what separates Darwinian social science from its more conventional alternatives is not whether evolutionary theory is correct but how useful specifically “evolution-minded” thinking is regarded for understanding behavior. One can, for example, believe natural selection has produced a human species whose behavior can be understood in terms of a relatively simple metanarrative of mind (e.g., rational choice theories, simple learning theories), such that further consideration of our evolutionary history is not useful, or one can believe that solid knowledge about our evolved past is so sparse that one should stick with building from what is proximate and observable, rather than trying to tie patterns to a past about which little is decisively known.

Alternatively, one can posit that explicit thinking in selectionist terms can yield discoveries of patterns of behavior otherwise overlooked or unknown to social science. To give one example, Daly & Wilson (1988, 1999) have proposed that selection would have favored parents who restrained potentially harmful anger toward their children, but those placed in parent-like positions toward children not their own—namely, stepparents—would lack this evolutionary incentive for caring, restrained behavior. Their subsequent research has indicated that living with a stepparent, as opposed to two biological parents, may be the largest known risk factor for being a victim of child abuse. Buss (1999, p. 203) writes that “hundreds of previous studies of child abuse failed to identify stepparents as a risk factor for child abuse until Daly and Wilson approached the problem with an evolutionary lens.”

Recent enthusiasm for Darwinian explanations has been inspired mainly by “evolutionary psychology.” Book titles like *The Moral Animal: The New Science of Evolutionary Psychology* (Wright 1994) and Buss’s (1999) *Evolutionary Psychology: The New Science of the Mind* present evolutionary psychology as a fresh way of looking at social behavior, even though to others it might look like what used to be called “sociobiology.” Indeed, some rivals complain that evolutionary psychology is only new if one caricatures earlier sociobiology (e.g., Alexander 1990) or that other researchers were around “doing evolutionary psychology before evolutionary psychology was cool—or had a name” (Smith 2000, p. 38). Complicating matters is that many who see evolutionary psychology as a “new science” use the term only to apply to a particular set of theoretical commitments articulated most importantly by Tooby & Cosmides (1992) (see also Buss 1995), whereas others—partly because of the various negative connotations that sociobiology has accrued—want this specific program to be seen as just one way of doing evolutionary psychology.

Even in its more restricted sense, evolutionary psychology has attracted scholars well beyond psychology, but the name does accurately point to a stronger psychological focus than preceding programs of Darwinian social science. Evolutionary psychology resolutely argues that any connections of behavioral patterns to our genetic evolution must be strictly mediated by theories of how evolution has shaped psychological mechanisms. Among evolutionary psychologists, the Swiss Army knife serves as a popular metaphor for the mind (Cosmides 1994). The knife “contains separate tools—each designed to perform a particular task effectively. The human brain also appears to come equipped with cognitive tools designed to carry out specific functions” (Buss & Kenrick 1998, p. 991). Each of these cognitive tools is asserted to have come into existence in response to specific selection pressures and to have acquired its particular form and rules of operation as a result of natural selection. The mind has been described as having highly specific adaptations—akin to “a confederation of hundreds or thousands of functionally generated computers” (Tooby & Cosmides 2000)—operating in such domains as social exchange (Cosmides 1989), mate selection (Buss 1994), language (Pinker & Bloom 1990), imputing intentions to others (Baron-Cohen 2000), and friendship selection (Tooby & Cosmides 1996). Evolutionary psychology now has a number of alternative visions, including some that call for a much more minimalist approach to the proliferating adaptations of orthodox evolutionary psychology (e.g., Tomasello 1999; see also essays in Scher & Rausher 2002).

Partly because of its emphasis on psychological mechanisms, evolutionary psychology resonates most strongly with sociological traditions based on methodological individualism. Sanderson (2001) and Lopreato & Crippen (1999) provide descriptions of what they regard as the core individualistic principles of a Darwinian sociology and follow with lengthy discussions of evidence they interpret as supporting these principles. Kanazawa (2001a) suggests that evolutionary psychology may be able to provide rational choice theories with a theory of values. Important interdisciplinary efforts in recent years have tried to use evolutionary

perspectives to understand both the problem of unsecured commitments for rational choice theories (Nesse 2001) and the disposition of many actors not only to reciprocate but also to be willing to incur costs to punish nonreciprocators (Bowles & Gintis 2000, Henrich et al. 2001, Henrich & Boyd 2001). Meanwhile, others discuss how evolutionary thinking may illuminate why we systematically deviate from rationality in contemporary environments because our minds employ heuristics that were sufficient for “ecological rationality” in the environments of our ancestral past (Gigerenzer 2000).

Sociologists often think that an evolutionary perspective implies that human beings have been programmed to behave as if they are striving to maximize their inclusive fitness. Indeed, some early sociobiology did at least implicitly take this view, and current work in the most prominent alternative to evolutionary psychology in Darwinian social science, human behavioral ecology, often generates models based on a theoretical “gambit” that effectively presumes this (for reviews, see Barrett et al. 2002, Cronk 1991, Winterhalder & Smith 2000). Evolutionary psychology, however, asserts that our cognitive mechanisms evolved mainly during or before the Pleistocene (approximately 2 million to 10,000 years ago) and that modern environments have been too recent and variable to have had much effect on their design (Foley 1996, Wilson 1994). A favorite slogan is that humans are “stone agers in the fast lane” (Eaton et al. 1988). Because our cognitive mechanisms are adapted for life in past environments, they cannot be assumed to produce evolutionarily optimal behaviors in the radically different environments of contemporary developed societies, and some theories propose to explain social phenomena as consequences of this mismatch (e.g., Archer 1997 on pets, Buss 2000 on depression). Richerson & Boyd (1999), especially, offer an especially intriguing theory of the “work-arounds” that sustain effective forms of complex, contemporary social organization staffed by humans whose minds were originally adapted for the different conditions of our evolutionary past.

A noteworthy implication of evolutionary psychology’s position that consequential psychological adaptation preceded the separation and worldwide migration of humans is that evolutionary psychology posits that our evolved psychological mechanisms do not qualitatively differ across races. In this way, evolutionary psychologists argue that they agree with the conventional social scientific position of the “psychic unity of humankind” (Tooby & Cosmides 1992). Recent evolution-based work that has provoked charges of racism (Rushton 1995) and anti-Semitism (MacDonald 1998) are based on premises much different from standard renditions of evolutionary psychology and are considered marginal in the area.

Like sociobiology before it, evolutionary psychology is regularly accused of genetic determinism (Lewontin et al. 1984, Rose & Rose 2000). Evolutionary psychologists have strongly objected and can point to many places in programmatic statements in which the essential codetermining roles of genes and environment in constructing the human phenotype are noted (Kurzban 2002, pp. 99–101). That said, many evolutionary psychological theories posit that the relevant psychological mechanisms operate with similar consequences across the widely divergent

environments provided by existing cultures, which is hard not to see as a practical determinism of some sort. For example, evolutionary psychological discussions of Buss's (1999) comparative study of 37 cultures emphasize the putative uniformity in mate preferences (but also see Eagly & Wood 1999). Evolutionary psychology can be chided for sometimes drawing premature conclusions about psychological universals. For example, many overviews of evolutionary psychology have confidently promoted the idea of a universal male preference for females with low (sometimes even specifically 0.7) waist-to-hip ratios [based on the research of Singh (1993, 1995)]. Subsequent work, however, has given reason to doubt claims about the universality and specificity of this preference (Freese & Meland 2002, Tassinary & Hansen 1998, Wetsman & Marlowe 1999, Yu & Shepard 1998).

Evolutionary psychologists certainly propose that psychological mechanisms can evolve to produce different outputs (e.g., behaviors) given different inputs from the environment. For example, contingent response would be part of any specifically evolved mechanism that yielded preferential treatment of biological children over unrelated children. Another example is the Trivers-Willard hypothesis, which proposes that, because reproductive variance is higher for males than females, higher-status parents will invest relatively more in their sons and lower-status parents will invest relatively more in their daughters (Trivers & Willard 1973). The applicability of this hypothesis to the contemporary United States has recently been the subject of recent sociological debate (Freese & Powell 1999, 2001; Kanazawa 2001b).

Some evolved mechanisms are proposed to be "set" by conditions of early childhood and have enduring effects on behavior thereafter. One theory proposes that cues of resource instability in early environments place girls on different "tracks" of sexual development, with unstable environments evoking a short-term strategy marked by earlier first intercourse, more partners, and shorter pair-bonds over the life course (Belsky et al. 1991). Also, Sulloway (1996) has proposed that children have an evolved tendency to develop personality traits, attitudes, and behavioral propensities that will maximize the resources they receive from their parents, and systematic differences in the maximizing strategies of firstborns and laterborns result in many different kinds of birth-order effects among adults (but see Freese et al. 1999). Theories of mechanisms that are sensitive to environmental conditions give evolutionary psychology a greater consonance with conventional social scientific thinking, and such theorizing also provides a means of accounting for individual differences as products of Darwinian evolution while maintaining a commitment to the psychological unity of humankind.

Even so, it might be more accurate to say that what evolutionary psychologists actually endorse is the psychic unity of "mankind" and "womankind"; for when one looks at what is actually published under the banner of evolutionary psychology, theories and studies of innate, evolved sex differences predominate. Nearly 70% of the pages in the substantive chapters of Buss's (1999) evolutionary psychology textbook contain at least some discussion or mention of a putatively innate behavioral difference between men and women. As just some examples,

studies have claimed support for Darwinian theories of intrinsic differences between the sexes in terms of jealousy (Buunk et al. 1996), performance on spatial reasoning and memory tasks (Silverman & Phillips 1998), parental investment (Biblarz & Raftery 1999), how television viewing affects satisfaction with friendships (Kanazawa 2002; but see Freese 2002), how occupation affects the risk of marital dissatisfaction and divorce (Kanazawa & Still 2000), aesthetic preferences sought in mates (Etcoff 1999, Symons 1995), contents of sexual and homicidal fantasies (Ellis & Symons 1990, Kenrick & Sheets 1993), task-related self-esteem (Hopcroft 2002), interpretations of workplace actions as sexual harassment (Studd & Gattiker 1991), strength of drive to attain high status and preferred method of status pursuit (Buss 1981, Browne 2002), risk-taking behavior (Johnson 1996), responses to panhandlers (Goldberg 1995), content of conversations (Dunbar et al. 1997), and predicted response to sexual assault (Thornhill & Palmer 2000).

Why the overwhelming attention to sex differences? For one thing, common statistical methods are much better at establishing differences than uniformities, and using experimental or questionnaire studies to test if a hypothesized mean difference exists between male and female participants is a relatively straightforward means to high publication fertility. However, theories of sex differences may actually be where the form of evolutionary psychology's reasoning most closely matches the classic positivistic ideal, in which a small number of premises deductively generate a large number of testable empirical implications. Roughly, because the absolute minimal investment required to bring a child into being is higher for women than men (compare gestation to ejaculation), females have more of their reproductive potential tied up in any one child. Combine this with the possibility of uncertain paternity for males, and females can be predicted to be the sex that invests more in offspring (Trivers 1972). The sex that invests more serves as the limiting resource for reproduction, which implies that mating competition should be keener for men and that the risks of bad mating decisions are greater for women.

Chains of reasoning that begin here provide the basis for most of the Darwinian explanations of sex differences in the aforementioned areas. Even so, deriving theoretical propositions about social life is rarely an uncontested business, and feminist (and other) critics have provided a sustained critique of evolutionary psychology's theories of sex differences (e.g., Fausto-Sterling 2000, Rose & Rose 2000, Travis 2003). Many also charge that evolutionary psychology exaggerates the breadth, magnitude, and specificity of intrinsic psychological differences between the sexes (e.g., Eagly & Wood 1999). Although efforts have been made to foster dialogue between evolutionary psychologists and mainstream feminists (e.g., Gowaty 1997), just how much common ground can ultimately be achieved is an open question (Campbell 2002). The work of evolutionary psychology on gender is likely the most provocative and visible to sociologists, although the subject matter of the field stretches far beyond studies of sex differences. Two interesting issues to watch as the field develops are the extent to which studies of sex differences continue to dominate its empirical work and the extent to which the field continues or abandons the particular commitments to cognitive architecture

that originally marked the launching of evolutionary psychology as distinct from sociobiology and other predecessors.

THE RELEVANCE OF GENETIC DIFFERENCES BETWEEN PERSONS

Identical twins are more similar in many ways than are fraternal twins or nontwin siblings, who, in turn, are more similar than adopted individuals who are biologically unrelated but reared together. Such phenomena are commonly attributed to the varying extents to which these different types of pairs share genes. More generally, another way of asserting the relevance of “biology” for the study of social behavior is to propose that understanding the variation among individuals in traits and behaviors of interest requires reference to differences in genetic endowments. Note that when evolutionary psychologists consider behavioral variation, they often do so with the idea that the evolved genetic programs that all humans share respond differently to various environmental inputs. Behavioral genetics, meanwhile, pursues questions with implications for how genetic differences between persons can yield behavioral variation when environmental inputs are the same.

Discussions of behavioral genetics are often framed in terms of the question of how much variation in a trait is explained by “genes” versus “the environment.” If one accepts certain assumptions, one can simply double the difference between correlation coefficients for identical and fraternal twins on a trait and obtain an estimate of that trait’s heritability, the proportion of population variance in the trait that is attributable to genetic differences between individuals (Plomin et al. 2001). Adoption studies and other kinds of family designs, in combination with sophisticated statistical models, can also be used to estimate heritability. With such methods, behavioral geneticists have offered evidence of nontrivial heritability of many traits, including cognitive ability (Neisser et al. 1996), personality (Loehlin 1992), attitudes (Martin et al. 1986), schizophrenia (Gottesman 1991), sexual orientation (Pillard & Bailey 1998), fertility (Kohler et al. 1999), age at first sexual intercourse (Rodgers et al. 1999), years of schooling (Behrman & Taubman 1989), divorce (McGue & Lykken 1992), depressive symptoms (Kendler et al. 1994), vocational interests and satisfaction (Gottfredson 1999), delinquency (Rowe & Osgood 1984), addictive behaviors (Crabbe 2002), as well as numerous others (see Gilger 2000, p. 233; Plomin et al. 1994).

The meaning of such estimates of heritability is easily misinterpreted. First, heritability is often treated as some transcendent biological parameter, but it is instead specific to a population (or subpopulation) and may vary across times and environments (see Plomin et al. 2001). Second, heritability estimates may contradict common-sense ideas about inheritance. Traits that have evolved to be virtually universal features of the human species, such as being born with two eyes, have heritabilities of nearly zero because of the lack of population variation, even though the importance of genes in producing this regularity is not doubted (e.g., Ehrlich

2002). Third, high heritability does not imply resistance to environmental change (Goldberger 1979, Maccoby 2000), and so claims that the heritability of IQ implies anything about the possibilities of compensatory education are misleading. Guo & Stearns (2002) suggest their findings indicate that public policies can help children in disadvantaged environments realize their genetic potential for intellectual development. For that matter, one can point to height, cognitive test performance, and some psychopathologies as instances of traits that have undergone general population increases despite their having substantial heritabilities, presumably because of changing environments (Neisser 1998, Rutter & Smith 1995). Finally, heritability is a statistic concerning individual differences, not group differences, and so heritability can have explanatory power for understanding differences within groups but be irrelevant for understanding differences between them, as has been pointed out many times in debates about racial differences in IQ and other traits (Plomin et al. 2001, p. 89).

Even when correctly understood, the accuracy of heritability estimates is regularly contested. Debates often focus on the usual assumptions made in identifying parameters to estimate heritability: that genetic effects are additive, that different kinship relations in a model experience equally similar environments when reared together, that mating is effectively random with respect to the trait under consideration, and that there are no gene-environment interactions or correlations. For example, studies suggest that the assumption of equal environments—most often, that identical twins do not experience more similar environments than fraternal twins—is often violated, yielding inflated heritability estimates (for specific citations, see Rutter et al. 2001, p. 294; for one defense, see Hettema et al. 1995). Maccoby (2000, p. 11) speculates that violations of this assumption might explain why heritability estimates are usually higher in twin-based studies than adoption-based studies.

Attempts to confront seriously these assumptions pose a daunting methodological problem, although some researchers have attempted to tackle it (e.g., Jencks et al. 1972; but see Taylor 1973). Although some quantitative geneticists, such as Falconer & Mackay (1996, p. 131), have advised that violations of assumptions normally do not seriously affect conclusions under experimental conditions, the solution by Jencks et al. (1972) yields a substantially lower estimate of heritability of IQ (45%) than what is commonly reported in the literature (approximately 75%). Such divergences suggest that heritability estimates from conventional statistical models in behavioral genetics might be more biased than is sometimes presumed. Meanwhile, studies of twins reared apart, which have received considerable media attention and academic admiration (e.g., Udry 1995, p. 1271), have also been strongly criticized for methodological problems that could greatly overstate heritability (Kamin & Goldberger 2002).

Behavioral geneticists have attempted to partition not only trait variation into genetic and environmental components, but also environmental variation into shared and nonshared components. Shared environmental components are presumed to make those who experience them (e.g., siblings) more similar, whereas nonshared

environmental components make these same individuals more different (Rutter 2000, p. 377). Puzzled by the low correlations among biologically unrelated children reared together, Plomin & Daniels (1987) conclude in their review that non-shared environmental effects far outweigh shared environmental effects. This directly challenges sociological research on the effects of family background, as most of the variables encompassed by this phrase involve characteristics shared by siblings reared together. It has also led to some dramatic claims about the inconsequentiality of parental behavior or family conditions more generally for human development (Harris 1998, Rowe 1994). For example, Harris (1998, pp. 300–5) specifically attacks the work by McLanahan & Sandefur (1994) on the effects on children who are raised by a single parent. She argues that heredity explains half of the variance in inferior outcomes for these children and that the other half is attributable to nonshared environmental factors that have nothing to do with the absence of a parent *per se*. Others cogently dispute the conclusions by Plomin & Daniels (1987) about the relative importance of shared and nonshared environments (Rutter et al. 1999, 2001), and the more extreme claims about the virtual inconsequentiality of differences in parental treatment have been widely criticized (e.g., Cherlin 1999, Vandell 2000).

On a practical level, the concept of heritability has also been challenged for being uninformative for designing environmental interventions and policy decisions (e.g., Baumrind 1993, Feldman & Lewontin 1975, Goldberger 1979) and for taking attention away from efforts to understand the various kinds of interplay between genes and environments. Almost half a century ago, Anastasi (1958) urged researchers to move beyond the question of how much heredity and environment contribute to individual differences and instead to focus more on this interplay. Gene-environment interactions may explain different individual responses to similar environmental conditions (see Rutter & Silberg 2002). For example, the diathesis-stressor model of psychopathology proposes that individuals possess genetic vulnerabilities that cause them to be more likely to develop psychopathological symptoms under stressful environmental conditions (see Gottesman 1991, Silberg et al. 1999).

Genetic differences may result in different responses to similar environments, but they can also be associated with selective exposure to different environments, which, in turn, affect traits (Kendler & Eaves 1986, Plomin & Bergeman 1991, Wachs 1992). Such gene-environment correlations are commonly divided into three types (Plomin et al. 1977). Suppose not only that intelligence is heritable, but also that more intelligent parents provide their children with environments more conducive to learning (e.g., by more strongly encouraging reading). This would be a passive gene-environment correlation in which parents' genes influence the child's environment. Suppose now that children who evince high intelligence early in school are given special opportunities (e.g., through tracking) that widen the gap between them and other students. To the extent that the children's early intelligence directly reflects genetic factors, this would be a reactive (or evocative) gene-environment correlation, as manifestations of a genetically influenced trait

evoke different environments that further affect the development of the trait. Finally, suppose further that more intelligent children seek out more intellectually challenging activities and place greater value on high intelligence in friends. If this contributes to further differentiation in intellectual development, this would be an example of an active gene-environment correlation. One hypothesis is that, over the life course, the influence of passive gene-environment correlations gradually diminishes, whereas that of active correlations increases and that of reactive correlations remains fairly constant (Scarr & McCartney 1983). Jencks (1992, p. 107) has suggested that, because of gene-environment correlations, heritability measures should be regarded as “a lower bound on the explanatory power of the environment, not an upper bound.”

Those critical of the concept of heritability can cite the potential biases introduced by gene-environment correlations and interactions, but these same correlations and interactions can compromise estimates for conventional sociological models of individual outcomes that typically fail to consider the possible effects of genetic influences. If genes associated with an outcome are likewise correlated with environmental variables in the model, then estimated “effects” of the environmental variable could be entirely spurious artifacts of selection. The basic issues of selection are familiar to sociological methodology (see, e.g., Berk 1983); behavioral genetics merely calls attention to genes as a potentially dramatic source of selection biases. Caspi et al. (2000, p. 338) examined the effect of neighborhood deprivation on children’s mental health using a British twin sample:

If parents’ problem behaviors are passed genetically to their children, and if parents’ problem behaviors interfere with their capacity to earn sufficiently to secure housing in a desirable neighborhood, this would create a correlation between neighborhood conditions and children’s behavior in the absence of any causal influence from neighborhoods.

The genetically informative design by Caspi et al. (2000) suggests that neighborhood deprivation has a genuine but only modest effect on children’s problem behaviors. Consonant with other studies that employ similar designs, their work strongly suggests the importance of controlling for genetic endowments when pursuing unbiased estimates of environment effects. Unfortunately, this specific study by Caspi et al. does not indicate how divergent the estimates of environmental effects would have been in the absence of the genetic controls.

The methodological problem posed by consequential genetic differences is one of unobserved heterogeneity among observations. Unlike sociologists’ tendency to rely on econometric methods to attempt to address unobserved heterogeneity statistically, behavioral geneticists have emphasized developing solutions through the designs of their sampling and data collection, which would seem a more desirable strategy even though it requires greater resources (Freedman 1991). Indeed, whereas sociologists may associate behavioral genetics with depictions that seem to celebrate the putatively massive and ubiquitous influence of genetic endowments while downplaying environmental influences (as in, e.g., Hamer & Copeland

1998), behavioral geneticists have worked hard to develop designs for attempting to understand more precisely the role of specific environmental conditions in determining specific traits (see Rutter et al. 2001 for an excellent review, as well as essays in Finch et al. 2001).

In addition, behavioral genetics is becoming increasingly linked to molecular genetics. Some studies have identified particular genes linked to certain behavioral traits, and then using quantitative genetic models, researchers have estimated the corresponding effects at the population level (e.g., Lesch et al. 1996). Investigators have also started to incorporate measures of particular genes into studies to examine how they interact with specific environmental factors (e.g., Caspi et al. 2002). Increasingly sophisticated research designs and the possibilities afforded by genotypic measures likely mean that behavioral genetics will be an even more formidable enterprise in the study of human development in the years to come.

THE RELEVANCE OF PROXIMATE BIOLOGICAL VARIABLES

Both Darwinian behavioral science and behavioral genetics are more diverse undertakings than is commonly credited, and yet what we wish to combine together in this last section is far more varied still. If one accepts that human beings are material entities all the way through, then all our thoughts and actions are embodied, imply thoroughly physical processes, and are “biological” activities in the sense of being part of our ongoing constitution as organisms. Even so, the various idioms with which social scientists typically consider behavior rarely require explicit reference to the materiality of human actors; that is, humans can be disembodied abstractions in the language of theory, even if unrelentingly embodied in actual practice. Proximate physiological mechanisms and processes are thus effectively treated as a black box in much social scientific thinking, but another way of asserting the relevance of “biology” is to assert the necessity or value of opening this black box and extracting information about the physical workings of our bodies and minds.

Various kinds of biomeasurement continue to become more sophisticated and offer insight into a deeper array of internal processes. Thinking about the sociological use of such measurements has long centered on behavioral endocrinology, and, of the many hormones in our body, testosterone has stimulated the most interest among sociologists. We can use testosterone as a running exemplar to think about some of the various ways that proximate bioindicators can enter into social scientific discourse. Testosterone has been the subject of longstanding interest, which, given its fundamental role in biological sex differentiation, is perhaps not surprising. Moreover, because testosterone is commonly posited to behaviorally differentiate members of the same sex in ways similar to whatever differences it creates between the sexes, proposals about innate sex differences in behavior can be easily reposed as hypotheses about possible within-sex differences. In other words, testosterone might be an exogenous variable that explains why some men (and women) are more masculine than others.

Using measurements from either saliva or blood, researchers have reported that high testosterone levels are associated with risk-taking (Booth et al. 1999) and dominance (Schaal et al. 1996), as well as aggressiveness and tendencies toward violence (Harris et al. 1996, Sanchez et al. 2000; but see Archer et al. 1998). Higher baseline testosterone is linked to both juvenile delinquency (Booth et al. 2000; but see Drigotas & Udry 1993) and adult criminal behaviors (Dabbs & Hargrove 1997). Likewise, higher testosterone has been linked in males to a syndrome of “antifamily” outcomes: lower likelihoods of marriage; lower marital satisfaction; fewer numbers of children and worse relationships with children; and higher rates of adultery, partner abuse, and divorce (Booth & Dabbs 1993, Gray et al. 2002, Julian & McKenry 1989, Mazur & Michalek 1998).

At the same time, interpreting correlations involving testosterone is complicated by its “trait” and “state” duality: Some individuals appear more disposed to higher testosterone levels than others of the same sex, yet testosterone levels also fluctuate within an individual. Such systematic variations within individuals allow testosterone to be posited as a mechanism implicated in regularities of behavior over the life course. For instance, that testosterone declines with age has been proposed as a central cause of the age-related decline in criminal behavior (Dabbs & Hargrove 1997). In addition, this decline, along with the seemingly independent decline in testosterone associated with marriage, has been speculated to be “crucial to successfully enacting the caring spousal and parent roles” (Booth et al. 2000, p. 1029; Gray et al. 2002).

Testosterone fluctuates in response to social stimuli in ways that allow the construction of theories of reciprocal causation where social circumstances affect testosterone, testosterone changes affect behavior, and behavior affects subsequent social circumstances (Kemper 1990, Mazur & Booth 1999). A most cogent example is presented in the finding that status competitions affect testosterone in males: Testosterone increases prior to various kinds of competitive matches (Booth et al. 1989, 1999) or in response to symbolic challenges like insults (Nisbett & Cohen 1996). Testosterone often remains high in winners and drops in losers. Demonstrating the human capacity for identification with others, similar vicarious effects of winning and losing have been observed among fans watching sporting events (Bernhardt et al. 1998). Kemper (1990) even suggests that the testosterone surges that men experience from their identification with sports teams may help keep them content with subordinate positions in the social hierarchy. In addition, the effects and responsiveness of testosterone levels bears striking resemblance to quasi-psychophysiological concepts central to some sociological theories [see, e.g., Collins’s (1998) discussions of “emotional energy”]. In its responsiveness, testosterone is like other bioindicators in that it can be viewed as part of the material means by which experiences of social life come to shape later behavioral dispositions and life outcomes.

The influence of testosterone on behavior can be seen as mediated in various ways by social conditions that attenuate or accentuate its causal potency. For example, religiosity may temper a tendency for boys with higher testosterone

levels to engage in greater levels of sexual activity and to have more sexually permissive attitudes (Halpern et al. 1994). Various social controls may keep testosterone levels from evincing the same effects on sexual interest and activity among white girls as it does on white boys (Udry & Billy 1987). Likewise, research suggests the relationship between testosterone and antisocial behavior may be weakest among those with high socioeconomic status (Dabbs & Morris 1990, Dabbs et al. 1990).

Yet just as social conditions can be seen as affecting testosterone's ultimate causal potency, so too can testosterone—through its prenatal influence on the organization of the brain—be seen as affecting the ultimate potential of social influence on individual development. Udry (2000) found that prenatal levels of the testosterone-binding hormone SHBG were associated not just with the “femininity” of women aged 27–30, but also with the relative success of maternal socialization efforts toward femininity in these women. For women with high prenatal SHBG, maternal encouragement of femininity appeared ineffective, whereas for women with low prenatal SHBG, maternal encouragement of femininity was solidly associated with adult femininity. Udry (2000, p. 452) concluded that high prenatal SHBG results in an “immunizing” of the brain against feminine socialization (but see, e.g., Miller & Costello 2001).

The preceding paragraphs suggest only some of the ways testosterone measurement can be cast as relevant to understanding social affairs. Of course, as we already noted, testosterone is by no means the only hormone to provoke social scientific interest. For example, a longer review could similarly trace the relationship among women between low estrogen and outcomes such as poor psychological well-being and diminished performance on verbal and memory tasks (Halbreich & Kahn 2001, Morrison & Tweedy 2000, Senanarong et al. 2002, Wolf & Kirschbaum 2002). Dementia in both women and men has also been related to low estrogen (Green & Simpkins 2000, Morrison & Tweedy 2000, Senanarong et al. 2002). Meanwhile, elevated levels of cortisol (sometimes called the stress hormone) have been suggested as central to some pathways through which race and socioeconomic status are related to adverse health outcomes (Epel et al. 1998, Seeman & McEwen 1996).

More importantly, we want to emphasize that potentially interesting measures of proximate biology extend well beyond hormones. Neurotransmitters are an obvious example: Low levels of serotonin are related to depression, difficulty with affect regulation, violence, and suicide (Funder 2001, Holden 1992, Kruesi & Jacobsen 1997, Moffitt et al. 1997, Robinson & Starkstein 1989). As a less well-known example, low resting heart rate appears to be related to violent behavior, whether in terms of self-reports, teacher reports, or convictions. This relationship is believed perhaps to reflect relationships among resting heart rate, fearlessness, and subsequent propensities for risk-taking behavior (Farrington 1997). As with testosterone, the effects of both serotonin and low resting heart rates interact with external social conditions in ways that seem to exacerbate consequences for individuals of low socioeconomic status and so invite biosocial

explanatory models (Farrington 1997, Rothenberg & Heinz 1998). Biosocial models can also posit that environmental conditions alter biology in ways that then have enduring effects for individual life outcomes. For example, interest continues in how the differential exposure of lower socioeconomic status individuals to environmental toxins like lead may explain part of the association between social standing and either negative health outcomes or socially undesirable behaviors (Masters 2001). Low birth weight is associated with various negative outcomes, including lower educational attainment, and thus might serve as another example. However, Conley & Bennett (2000) show that estimates of the effect of environmental conditions on the probability of giving birth to a low-birth-weight child are reduced dramatically when the birth weight of parents is controlled.

Indeed, we expect that in the coming years the most excitement will not surround salivary swabs and hormone measurement at all, but instead will center on the unprecedented opportunities that functional magnetic resonance imaging (fMRI) and related techniques offer for examining what is happening in the brain as persons experience and respond to stimuli. In 1994, Nielsen (p. 290) described the sociology of emotions as “ripe for an evolutionary takeover,” but although there certainly has been interesting work on emotions from a strictly evolutionary perspective (e.g., Frank 1988), more interesting observations have come from neurology or efforts to ground evolutionary theorizing in knowledge from neurology (for efforts by sociologists, see Franks & Smith 1999, Hammond 2003, Turner 2000; for particularly influential general-audience treatments of neurology and emotion, see Damasio 1994, LeDoux 1996). To date, sociologists have had little connection with those conducting fMRI studies, although a collaborative effort is currently underway using one of the most prominent samples for longitudinal research in sociology—the Wisconsin Longitudinal Study—to examine the connection between different kinds of self-reports of psychological well-being and various fMRI measurements. Along these lines, we expect much interest in the years ahead in the relationship between neurological measurements and the questionnaire self-reports that social scientists have long relied on to measure variously conceptualized internal states and traits.

In addition, we expect neuroscience to contribute to breaking down the implicit dualism that guides commonplace thinking, leading to greater recognition that “there is no mind separate from and independent of the body” (Lakoff & Johnson 1999). Perhaps soon, findings like that even a relatively short (8-week) practice of meditation can result in observable differences in brain activity (Davidson et al. 2003) will be no more surprising than learning that an 8-week regimen of disciplined exercise can result in observable differences in an individual’s physique. This research may link behavioral genetics to the sociology of emotions by providing details about how genetic differences may be implicated in differences in emotional responses to similar stimuli, as when fMRI measurements reveal an association between the response of the amygdala to a fear-inducing stimulus and genetic variation at a particular locus (Hariri et al. 2002). Neuroscience

might also contribute to understanding areas where the interests of sociologists and evolutionary psychologists overlap, as in the study of sex differences in emotions (Canli et al. 2002) or the propensity of humans to cooperate with others (Rilling et al. 2002). More generally, however, we anticipate that the lessons from cognitive and affective neuroscience about the particulars of the brain—its operation, structure, and especially its plasticity—will help behavioral scientists understand more precisely how individuals are influenced by the immediate and past social contexts in which they participate.

CONCLUSION

Not only have the above sections provided just the barest sketches of their respective areas, but we have also hardly exhausted the kinds of studies of “biology” available to sociologists. We have not considered the effects of biology “from the outside in” (Piliavin & LePore 1994): how observable differences between our bodies affect our treatment by a social world that assigns meanings to them (e.g., Persico et al. 2001 on height; Snyder et al. 1977 on physical attractiveness). We have not considered studies of cleavages in public beliefs about how biology is implicated in the determination of various outcomes (e.g., Schnittker et al. 2000). We have given no attention to work that wrestles with the social implications of our rapidly advancing biological knowledge (e.g., Evans 2002, Rothman 1998). For that matter, we do not discuss work that contemplates the social implications of the biological changes afoot in our society, such as the consequences of our lengthening life expectancy (e.g., Matras 1990, National Research Council 2001).

What is plain from all we have and have not talked about, however, is that the biological sciences are moving quickly and there exists a panoply of different kinds of inquiries that sociologists can pursue. Daunting perhaps are the depths of the extradisciplinary literatures or collaborations into which one must wade to pursue some kinds of inquiries. Even worse, part of what may have discouraged greater sociological participation in some of these areas is that work is often cast in languages that make the potential offerings to and by sociologists less than apparent, except as something for the discipline to persistently oppose. However, the relative absence of sociologists at the table may be precisely why some of the language is as it is, or at least why more congenial alternative idioms remain to be formulated. Although much sociology can and will proceed without any reference to the specific materiality of human actors, the discipline should not opt itself out of participating in or with intellectual enterprises that will likely continue to excite enormous attention from the public, other social sciences, and major funding agencies. As science continues to reveal more about the biology of behavior—in all of its various senses—sociology should seek and support ways of understanding the interrelationship of biological and social influences that will allow our discipline to gain strength from these new developments rather than be diminished by them.

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