METHODOLOGICAL INDIVIDUALISM AND VERTICAL INTEGRATION IN THE SOCIAL SCIENCES

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ABSTRACT: This paper argues against the false dichotomy between reductionism and holism in the social sciences. I make the points that reductionism is the mark of a mature science, that the social sciences will never progress until they drop their opposition to reductionism, that higher-level explanations, even when more appropriate and coherent than reductionist explanations, must not violate principles established at lower levels of explanation, and that reductionist explanations almost always absorb the explanatory efficiency of broad social categorizations and add incremental validity to them. I demonstrate the validity of these points by exploring them in the context of the four most frequently used variables in social science (gender, race, age, and social class). In each case it is demonstrated that such categories fail to capture the causes of the phenomena social scientists explore, and that by failing to consider more elemental explanations lead to reliance on hypothetical “social facts” which are at best incomplete, and are often demonstrably wrong. The history of science reveals that all disciplines at one time or another have resisted the incursions of the more fundamental sciences, then showed a grudging acceptance of them, and finally became fully integrated with them. The social sciences must do the same with regard to integration with the relevant biological sciences.

Professor Jones (1996) recently described in the pages of this journal what I consider to be a false dichotomy between methodological individualism (reductionism) and methodological collectivism (holism). He correctly points out that social scientists have steadfastly opposed the reduction of Durkheimian “social facts” to psychological or (heaven forbid!) biological principles. Jones wisely does not claim that social events cannot be reduced to propositions about individuals participating in them, but he does declare that nonindividualistic accounts are preferable and that “we have no good grounds for preferring individualistic theories.” He also claims—quite wrongly—that methodological individualists “claim that only accounts using individualist accounts are legitimate” (1996, p. 125 [emphasis in original]). This dichotomy is unnecessarily divisive. I will attempt to demonstrate four points in response to Professor Jones:

1) That reductionist explanations are the mark of a mature science.
2) That social science will never develop into true science as long as it maintains its opposition to reductionism.
3) That even when holistic explanations are more appropriate and more coherent than reductionist explanations they must not violate principles established at lower levels of explanation, and that such violations frequently occur in social science.

AUTHOR’S NOTE:
I wish to thank Professors Robert Corbin and Martin Orr, as well as three anonymous reviewers, for their helpful comments and criticisms. Please address all correspondence to: Anthony Walsh, Department of Criminal Justice, Boise State University, Boise, Idaho 83725.
4) That reductionist explanations generally absorb the explanatory efficiency of broad social categorizations and add incremental validity to them.

Greedy versus Good Reductionism and the Role of Methodological Collectivism

Professor Jones conflates the distinction that several philosophers have called “good reductionism” and “greedy reductionism” in his examples of English table manners and why square pegs don’t fit into round holes. Daniel Dennett defines good reductionism as “the commitment to non-question-begging science,” and greedy reductionism as that which skips over several layers of (higher level) complexity in the rush to fasten everything secure and neatly to a solid foundation (1995, p. 82). Using his now famous metaphors, Dennett describes antireductionists as folks who “yearn for skyhooks” (a sort of deus ex machina that miraculously lifts us out of scientific difficulty) and who “call those who settle for cranes [solidly grounded devices that also function to lift us out of scientific difficulty] ‘reductionists’” (1995, p. 80).

A greedy reductionist is a person who sees a chain of causal events running up the ladder from molecules to human behavior, and who may make claims such as: “Our behavior is controlled by molecules—by nothing else” (Applewhite, 1981, p.1). The claim is probably based on the fact that we are composed of molecules and that there is no “we” apart from the molecules that compose us; thus our behavior must be the result of some molecules moving around other molecules. Viewing behavior as the movement of molecules conceals higher-order explanations that have greater coherence, generality, and utility, and is a kind of reductionism rarely seriously advanced by anyone today. Few would argue with the point that many phenomena cannot be sensibly explained by reference to greedy reductionist propositions. For example, the meaning of a poem cannot be deduced from an examination of the individual words and letters from which it is composed. While there can be no poem without letters or the sounds they denote, the meaning of the poem lies in their distinctive relationship to one another. On another level, I know that when the water molecules gyrating in my kettle reach 100 degrees Celsius, I shall have my cup of tea, but I would lose coherence were I to try to translate this information about temperature (an emergent property of aggregate molecules) into the motion of individual molecules. The conflation of greedy reductionism with the kind of “crane-built” reductionism that has elevated the physical and life sciences haunts sociology, and if not exorcised could lead to its demise as intellectually respectable discipline (Crippen, 1994; Ellis, 1996; Neilsen, 1994; van den Berghe, 1990; Walsh, 1995).

Physicist Percy Bridgman maintained that the first and most important step in understanding any system—atomic particles, chemical compounds, cells, individual organisms, societies—is to understand the elementary units comprising the system. The ultimate problem in the social sciences is similar to that of the physical sciences but is more complex. “The elementary units of the social sciences are men and the corresponding ultimate problem is to understand the individual human being” (1955, pp. 49-50). Bridgman is not denying that social phenomena can be explained on their own terms, or saying that lower-level “elementary units” explanations are necessarily
always superior to holistic ones. Rather, he is saying that social phenomena can be more fully and accurately understood if their explanations maintain consistency with what we know about the more elementary units—the biology and psychology of the actors in the social drama. Cosmides, Tooby, and Barkow (1992, p. 4) refer to this principle of conceptual consistency as vertical integration, explaining that this term refers “to the principle that the various disciplines within the behavioral and social sciences should make themselves mutually consistent, and consistent with what is known in the natural sciences as well.” The false dichotomy between methodological individualism and methodological collectivism is a stumbling block to the attainment of this consistency.

When scientists write about the methodologies employed in their disciplines they typically refer to hierarchically arranged “levels of analysis.” These levels serve to organize knowledge within a field of inquiry along manageable lines, albeit artificially discrete ones. The term “reductionism” is used disparagingly by social scientists to mean that an inappropriate unit of analysis has been used to explore, describe, or explain what they claim to be a “social fact.” In some instances they may be right, but in others they are demonstrably wrong. Examining the phenomenon of crime, for instance, our level of analysis is unquestionably “society” when we ask questions like “Why is the United States a more criminogenic society than Japan?” To ask such a question is to seek a reply from the social scientist because it is couched in broad “macro” terms. It is asking for an explanation of the difference in crime rates (number of reported crimes per 100,000 population) between two different societies, and being so couched we need to look for social, cultural, political, and economic factors that differentiate the two societies in such a way as to provide a satisfactory explanation. Those more attracted to individualistic explanations may also attempt to answer the question using the vocabulary of psychology to indicate that the United States produces a different kind of mind-set than Japan as it relates to such crime-related factors as conformity, competitiveness, aggression, hedonism, temperament, and so forth. In doing so, they reduce the explanation of a question couched in terms of one unit of analysis (whole societies) to a lower unit of analysis (individual mind-sets). Most sociologists would probably consider this inappropriate; most psychologists would probably beg to differ.

The sociologist need not use the vocabulary of psychology to answer the question as posed, for it does not inquire about “mind-sets.” But the psychologist must necessarily use the vocabulary of the sociologist. “Mind-set” is a dependent as well as an independent variable, and thus he must delineate the nature of the social milieu producing the mind-sets so that the question and its answer maintain consistency. That is, the psychological explanation of the phenomenon requires not only the use of psychological terms, but also of specifically sociological, political, economic, and historical terms that do not typically appear in psychology’s vocabulary. However, in finding that the culture of the United States produces individuals who tend more than individuals in Japan to be competitive, aggressive, hedonistic, or distempered, and that those possessing such traits tend to be more antisocial than those who do not, the psychologist has added a useful dimension to sociological explanation. He or she has not detracted from the sociologist’s
explanation as long as the cultural conditions associated with the development of the enumerated character traits are acknowledged.

Unlike the psychologist, however, a biologist could not reply to the question if limited solely to the vocabulary of biology. There are genetic and neurohormonal explanations for why some people are more distempered, competitive, aggressive, or hedonistic than others, but these explanations say nothing about why such traits apparently translate into criminal behavior more in the United States than in Japan, nor why it is that one can be competitive, aggressive, distempered, and hedonistic, and be perfectly law-abiding. Crime rates are emergent properties of sociocultural systems that cannot be deduced from the biological properties of individuals within them. To assert that they can, a biologist would have to show that properties of the whole (society) can be deduced from biological properties of its constituent parts (individuals), and would have to show that these properties differ between the two societies. In other words, the biologist must possess a suitable theory making it possible to analyze the form and nature of the whole as having been derived from the biological properties of individuals. No such theory exists. Thus the phenomenon which is to be explained (crime rates) can be adequately explained sociologically, adequately explained psychologically given the proviso that sociocultural variables are acknowledged, and not explained at all by a theory relying strictly on biology. This does not mean, however, that an understanding of the biological bases of the individual characteristics associated with criminal behavior may not be extremely useful if it is determined that those characteristics are found more frequently in one culture than in another.1

A Short History of Antireductionism in Science

Because discipline lines may be sharp with regard to some questions, it does not mean that cross-disciplinary lines of communication are closed with regard to others. Eminent philosophers of science such as Rudolf Carnap and Otto Neurath always maintained the position that there is no defensible scientific reason why the social sciences should not be continuous with biology in the same way that biology is continuous with chemistry, and chemistry with physics. Reacting to reductionism by drawing sharp lines and erecting high fences against the intrusion of biology into the social sciences only retards their development as mature disciplines. According to E. O. Wilson (1990), each science at one time or another has served as an antidiscipline (a discipline perceived as an intellectual threat) to the more immature higher-order science adjacent to it. Initially there is much tension between the two, but also a creative interplay of ideas and theories which eventually leads to full complementarity. As difficult as it may be for some social scientists to accept, biology is the antidiscipline of the social sciences.

It is not generally known that most of the sciences have had their own Durkheims, who, conscious of the emergent nature of their discipline’s subject matter, attempted to disassociate their science from the more basic sciences. As the most basic of the sciences, physics alone has been free of the reductionist phobia. The maturity of this discipline may be gauged from its lack of ambivalence in viewing matter in both its ultimate reductionist form (quantum mechanics) and in its
ultimate holistic form (the cosmos).

Chemistry, the erstwhile antidiscipline of physics, was the first to suffer and to be cured of antireductionism. Chemistry’s Durkheim was a nineteenth-century chemist named Benjamin Brodie. Fearful of the intrusion of physicists and their new-fangled ideas about things called “atoms” into his discipline, Brodie made a case for limiting chemical explanations to qualitative (transformation of substances such as a solid to a gas) and gravimetric (weight changes due to transformation) changes in chemical compounds on the grounds that there are emergent properties possessed by those compounds considered not to be predictable a priori from their constituent parts (Harré, 1967, p. 290). Brodie wanted an “atomless” chemistry, and the overwhelming majority of the London Chemical Society of the 1870’s was in agreement. Yet it was not long thereafter that physics revolutionized chemistry, and needless to say, there are no Brodian chemists today formulating theories or hypotheses about chemical phenomena that in any way violate the elegant laws of physics.

Biology has a similar history of attempting to maintain disciplinary autonomy, particularly from the encroachment of molecular biology, whose founders brought with them the arcane methods of physics, their parent discipline. In the 1940’s, J. H. Woodger (1948) argued that biologists should only use biological facts to explain other biological facts, and that they certainly should not reduce them to the concepts of physics and chemistry. Woodger probably had a stronger claim than Brodie; the step from the inorganic to the organic is the most momentous discontinuity in all science, for surely life is the quintessential example an emergent phenomenon. Life is certainly more than the arrangement of amino acid chains wound around the fine symmetry of the carbon atom, and to describe it requires a specific biological vocabulary not found in physics or chemistry. But the decoding of the “language of life” by Watson and Crick was not achieved by adhering to the various doctrines of biological ontological autonomy, nor could it possibly have been. Just as Brodie’s discipline was revolutionized by the atoms he wanted to ignore, Woodger’s biology was revolutionized by an exploration of the atomic structure and chemistry of the DNA molecule. Indeed, the central concepts of these disciplines (ions in chemistry, genes in biology) have their origins in what was once dismissed as reductionist science.

Readers of this journal will doubtless know of many examples of psychological Durkheims sternly counseling that psychological facts should only be explained by other psychological facts. Psychology has not yet embraced biology as intimately as biology has embraced chemistry, or chemistry physics, but it is at least on very friendly terms, as a perusal of modern psychology textbooks will reveal. A major shift has taken place over the past two decades with respect to integrating biology into psychological theories. Evolutionary psychology is a rapidly expanding subdivision of psychology, and many of the leading behavioral geneticists are psychologists. In other words, there is much evidence that psychology is rapidly distancing itself from naive antireductionism. Many of the new generation of psychologists appear to agree that not to consider evolutionary, genetic, or neurohormonal evidence is to neglect the guidance of theoretical compasses that have elegantly demonstrated their predictive power time and again.
On their lofty perch atop August Comte’s hierarchy of science ladder sit the acrophobic sociologists, afraid to look down. Alone among the sciences, sociology has decided that the path of least resistance lies in ignoring the more basic sciences. Not only are most sociologists oblivious to biology, “they are militantly and proudly ignorant” (van den Berghe, 1990, p. 177). Like ancient cartographers, they draw bad maps delineating discipline boundaries, “beyond which there be monsters.” It is feared that these reductionist monsters want to cannibalize the discipline and leave it adrift without role or substance. Nothing could be further from the truth. Biological principles will breathe badly needed new life into the contradictory stew of ungrounded theories that now constitute sociological thinking. The discipline will find it profitable to emulate the more mature sciences and take whatever it can from its antidiscipline. No crane lover disputes that even a complete description and understanding of the genetic, hormonal, and neurological bases of complex behavior would constitute a complete understanding of that behavior absent knowledge of cultural context and of motives, purposes of the individual actor. Propositions about genes, hormones, and neurons do not contain terms that help define the human condition at its most meaningful level, terms such as “love,” “hate,” “justice,” “good,” and “evil.” Contrary to Jones, only a greedy reductionist (if such exist) would assert that biological propositions are the only ones needed to make moral, psychological, or social statements containing such terms. This observation, however, does not disqualify biological propositions from participating in efforts to elucidate the substance of those terms.

Why Reductionist Explanations Are More Powerful

Professor Jones makes a statement with which I agree when he writes “. . . a complete scientific understanding of the world requires us to uncover high level generalizations even when low level ones can explain the phenomenon in question” (1996, p. 126). I would add two stipulations, however. First, the higher level explanation must add coherence and utility; if it does not then it is a waste of effort. Second, and more importantly, even if making no reference to the reductionist principles comprising the lower level explanation, the higher level explanation must not violate them. Unfortunately, such violations occur frequently in the social sciences, although this may be more because of adherence to political correctness than to anything else. Whenever scientific statements are made about the individual characteristics of certain broad categories of people which some may construe as negative, there is an annoying tendency for social scientists to respond with invectives such as “racist,” “sexist,” and “classist” (see the works of Gordon, 1980; Scarr, 1981; and Whitney, 1995, for many examples, particularly when it comes to efforts to publish individual-level explanations of behaviors with genetic overtones). Such labels are not substitutes for thought, add nothing to the debate, and probably account for much of the reason why sociology remains primarily a descriptive discipline.

Lubinski and Humphreys (1997) note the similarity between medical epidemiology and social science. Both disciplines deal with broad categories of people (races, genders, classes, ethnic groups, and other subpopulations) and both
seek to determine the location, prevalence, spread, and distribution of their respective phenomena of interest. Unlike social scientists, however, epidemiologists do not believe that they have uncovered causes when at-risk groups have been identified. Rather they continue to peel off layers of clues provided by specific behaviors and characteristics of individuals belonging to the identified group; membership in a gross social category explains nothing *per se*. Medical epidemiologists continually work to narrow (reduce) the focus of inquiry until they identify a specific pathogen responsible for the disease. Each step of the way may identify a method of intervention (behaviors, situations, or environments to avoid) which may lead to a reduction of the spread of the disease, and no epidemiologist would dispute the value of the early steps anymore than he or she would dispute the value of the later steps.

It is unfair to state that social scientists always believe that broad categories and concepts are causes *per se*, but they often do attempt to explain observed relationships between social categories and other measures (say between gender and income) by appealing to something assumed but not measured, such as “sex discrimination.” The fact that there is a relationship between gender and income unfavorable to women is *in and of itself* sufficient evidence of sex discrimination for many social scientists (see, e.g., Henslin, 1995, p. 295). Similarly, phenomena such as unfavorable arrest rates or test scores are routinely viewed as *ipso facto* evidence of racism and/or classism (again, see Henslin, 1995, p. 213). Invoking higher level categories as causes may be faithful to Durkheim’s dictum that only social facts should be used to explain other social facts, but it is terrible (and dishonest) science.

I will use the four most often used categories in social science (gender, race, socio-economic status [SES], and age) to illustrate that explanations which include lower-level explanations of social behavior are almost always superior to solely higher-level explanations. These examples are necessarily simple ones given that the purpose of this paper is to argue the superiority of always including lower-level explanations in sociological analysis, not the substance of the examples.

**Gender:** Several years ago I engaged a number of sociologists in debate regarding the origins of sex differences in behavior. The impetus for the debate was Alice Rossi’s presidential address to the American Sociological Association, in which she implored her colleagues in sex-role sociology to remove their Durkheimian blinders and realize that sex roles and gender differences are largely driven by genes and hormones and have their ultimate origin in the evolutionary history of the species. She warned that if they continued to focus only on “social facts” such as capitalism, patriarchy, and sex role socialization as explanations they would not only misidentify causes, they and their theories would become irrelevant in the world of science (1984, p. 1). Although our debate occurred a number of years after Rossi’s address, not one of the sociologists knew that the male brain is changed by fetal androgen activity from its inherent female form, or about the various sex-based brain differences neuroscience has uncovered over the past three decades. Ignorance of basic scientific knowledge and principles having relevance for one’s own discipline is inexcusable, yet it is pervasive in sociology because of its militantly unrealistic antireductionism.
Lubinski and Humphreys (1997) provide the example of gender differences in engineering, noting that engineering school enrollments are consistently compared unfavorably with law and medical school enrollment. “Discrimination,” “sexism,” “non math-oriented socialization,” and other fuzzy (but properly “social”) concepts are invoked to explain why females are underrepresented by a factor of about 6.3 among Ph.D.s in engineering but overrepresented by a factor of 2.2 among psychology Ph.D.s. In their haste to hook this pattern to a social fact, social scientists overlook the facts that the fields in which females are underrepresented and overrepresented involve visual-spatial and verbal and nurturing skills, respectively, and that there are identifiable neurohormonal reasons for sex differences in these skills (reviewed in Walsh, 1995). Every time sociologists assert that gender is entirely a social construction they contradict cascades of evidence to the contrary. Ignorance of relevant crane-built evidence leads social scientists to violate established principles of the more fundamental sciences and to hang spurious explanations on skyhooks.

Suppose there were no engineering schools and no legal injunctions against using group criteria for hiring. Under these circumstances, should gender be used as an explicit criterion for awarding apprenticeships in an engineering company? Hiring all males would be a better tactic than hiring all females, or hiring half males and half females. However, there would be many false positives because although males have higher group means on a variety of engineering-relevant spatial, mechanical, and practical tests, there is a great deal of overlap, with many females scoring above the male mean. False positives (males with poor engineering-relevant skills) would be replaced by females with good engineering-relevant skills if such tests were administered to all applicants, because measures of individual difference predict job performance better than gross categorization such as gender. Couched in statistical terms, the introduction of an individual measure of visual-spatial skills into a regression equation containing gender will reduce the impact of gender on job performance to zero. The reverse is not true. Introducing gender into an equation already containing the individual measure will necessarily weaken the impact of that measure (because of the covariance shared by gender and the test), but will still leave significant variance in job performance accounted for by variance in the test measure. That is, the individual difference measures of visual-spatial skills absorb the predictive ability of gender and add incremental validity as a bonus, as Lubinski and Humphreys (1997, p.177) clearly show.

**Race:** Lubinski and Humphreys point out that when the annual reports of births and deaths come out, we typically find that the rates of infant death and low birth weight births are about double among blacks than among whites, and that commentators “readily conclude that the data constitute evidence of continuing discrimination in the United States” (1997, p. 179). There are unquestionably differences in the availability of prenatal medical care between blacks and whites, and certainly such availability affects reproductive outcomes. But once again this “social fact” (discrimination) explanation is offered without consulting all the relevant evidence. Statistically equalizing black and white mothers on a large variety of characteristics (age, percentage of out-of-wedlock births, education, fetal gestation...
period, when prenatal care was started, number of prenatal visits, smoking, drinking, and so forth), black rates of infant death and low birth weight births remain about twice the white rates. Black rates are also about twice those of Native Americans, Alaskan natives, and Mexican Americans, groups that may also claim discrimination. In fact, Native Americans were found to be more deprived than blacks on the many of the same measures that document the black-white differences, particularly late prenatal care (Lubinski & Humphreys, 1997, p. 183).

Obviously there is a missing factor (or factors) not included in the analysis that accounts for the different rates. Lubinski and Humphreys do not identify it, but they found the demographic and medical-related variables listed above were unlikely causal determinants. This is not to say that these variables do not contribute to an increased probability of low birth weight or infant mortality within groups, it simply means that they do not account for the difference in rates between the black and white groups. In ruling out these factors as likely explanations for group differences, Lubinski and Humphreys thereby rule out the more general construct of “discrimination,” particularly given that Native Americans suffer more deprivation than blacks yet have infant mortality and low birth weight rates more in line with white rates. It is the black community that will benefit most from uncovering the “X” factor(s) responsible for these tragic rates, but these factors will not be discovered by adhering solely to hypothetical constructs like discrimination that conceal more than they reveal.

A major stumbling block to uncovering such factors is the insistence of many scientists that race is nothing but a social construct. The depth of their commitment to this position can be gauged by their reactions to the Human Genome Diversity Project, which has as its major goal the collection of DNA from all the world’s indigenous peoples. The usual invectives were trotted out, plus some new ones such as “genetic colonialism.” The unfortunate thing about this is that virtually all human genome research is done on white samples, which worries medical people who care about the health concerns of other races (Kahn, 1994). One black immunogeneticist stated that genetically “there is no magic bullet that works on all groups. . . . If we want to give all groups the same quality of treatment, we must study all groups. And African-Americans must participate in these studies if the knowledge is to be applied to us” (quoted in Kahn, 1994, p. 721).3

**SES:** SES is perhaps sociology’s core concept, and it is frequently conceived of as the independent variable which will make sense of everything. SES plays an important part in many criminological theories, including the long-lived anomie/strain theory which traces its lineage back to Durkheim himself. The theory basically states that crime is committed by lower class individuals pressured by cultural values to achieve occupational success and middle-class status, but who cannot do so because they are systematically denied access to legitimate means of attaining it (Messner & Rosenfeld, 1994). Systematic denial (i.e., denied by the “system”) of legitimate opportunities is thus a social fact explanation for both SES and crime. No specific systemic source of opportunity denial is ever identified, and should an anomie enthusiast attempt to find one he will be swamped with a plethora
Perhaps the relationship between criminality and SES hides some more fundamental relationship involving a third variable causally related to both. What could cause variation in occupational success, and thus in SES? Anomie/strain theory appears to claim that one SES level (low) is the cause of not attaining another (middle), which implies a rigid, self-perpetuating social hierarchy of the kind that has not existed in any industrialized nation for generations. Being born to high SES parents doubtless confers considerable advantage in achieving that status one’s self, and being born to low SES parents confers considerable disadvantages. What social scientists fail to realize, however, is that these advantages and disadvantages are genetic as well as environmental, since parents provide their offspring with both. Even then, “in open societies with high degrees of occupational mobility, individuals with high IQs migrate, relative to their parents, to occupations of higher SES, and individuals with lower IQs migrate to occupations of lower SES” (Bouchard & Segal, 1985, p. 408). There is a strong consensus on this within the vocational literature (Gottfredson, 1986, 1997; Gordon, 1997) and a moderate one among the American Psychological Association’s (APA) task force on intelligence (Neisser, et al., 1995). However, my purpose is not to debate the substance of SES, crime, or IQ, but rather to show that a lower level variable (IQ) is superior to a higher level variable (SES) in explaining criminal behavior.

Sociologists have traditionally viewed IQ as being caused by SES; i.e., “socially constructed,” rather than the other way around. The environment does effect intellectual development, but the influences of shared or common environment (influences shared at the family level) has been shown time and again to have almost zero effect on cognitive and other traits and abilities manifested in adulthood. The effects of shared environment (and thus shared SES) on IQ for individuals reared together, while modest in childhood (interpair correlations of between .15 and .30) are essentially reduced to zero in adulthood (see Plomin & Petrill, 1997; Rowe, 1997, for reviews). Environmental effects on adult IQ are specific or nonshared effects resulting from idiosyncratic responses to situations, opportunities, and circumstances both within and outside the family. The overwhelming consensus among behavioral geneticists and the APA task force (Neisser, et al., 1995) is that genes are the major source of variance in IQ, followed by specific environmental effects, with almost no common environmental effects that last beyond adolescence (see the special issue of Intelligence, guest edited by Gottfredson, 1997).

If IQ has greater explanatory power than SES vis-a-vis criminal behavior, we should observe the same statistical effects noted in our discussion of gender and test scores. That is, the introduction of SES will weaken the effect of IQ (to the extent that SES is a product of IQ, IQ is functioning as a control for itself) but not eliminate it, but if IQ is entered into a model containing SES, SES will drop out completely. A review of the evidence by Hirschi and Hindelang (1977) found that IQ was stronger than race or (parental) SES in explaining delinquency, but that it is ignored by sociologists as “threatening to the integrity of the field and to its moral commitment” (1977, p. 572). In a review and analysis of over thirty different data sets containing both SES and IQ data, Gordon (1987) pitted one against the other in a variety of
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creative ways, with IQ always absorbing the explanatory power of SES and adding incremental validity to it. Gordon, a sociologist, was led to conclude that many of sociology’s valued explanatory concepts, such as SES and/or any of its composites, are surrogates of the more fundamental and more general lower level concept of intelligence. Indeed, IQ is implicated in so many areas of social life that the fact that social scientists often fail to even consider its role has been termed “scientific malpractice” (Lubinski & Humphreys, 1997, p. 195).

Age: Because we have all been young, and presumably most of us will become old, age is one of the few broad social categories that social scientists can write objectively about without the danger of being called a nasty name. The second decade of life is a period of great interest to social scientists because it is a period of intense experimentation and deviance among the young. In this age of AIDS and ever-increasing rates of unwed motherhood, adolescent sexuality has become an issue of concern to policy makers. A study which reported sociological and biosocial models of adolescent sexuality (a composite measure which included behavioral and attitudinal variables) is particularly instructive for our purposes (Udry, 1988). Five variables (including age) entered the sociological model predicting sexuality levels at < .05 level of significance, and jointly accounted for 49 percent of the variance. In a biosocial model, three variables (testosterone, pubertal development, and permissiveness) jointly accounted for 59 percent of the variance, with all demographic and attitudinal variables except permissiveness dropping out of the equation. Thus, the lower level measures utilized in the biosocial model not only assimilated the explanatory power of age (as well as other demographics), they explained 10 percent additional variance, and they did so utilizing fewer variables than the sociological model.

Apart from the gender effect, the age effect on delinquency is probably the most robust effect found in sociological criminology. This effect has remained robust, controlling for a host of demographic and situational factors across numerous studies across time and national boundaries; that is, no higher level measure or measures have been known to reduce the age effect to zero. But is age a proxy for some lower-level mechanism that may have more predictive power? In another “sociological vs. biosocial” analysis Udry (1990) found, as expected, that no combination of higher order control variables had much effect in reducing the predictive power of age on delinquency. In the biosocial model, however, when testosterone levels were introduced into the regression equation age dropped out. Udry (a sociologist) concluded that, “the age effects on problem behavior is shown to be a spurious consequence of the causal relationship between age and testosterone” (1990, p. 7). Udry is not saying that testosterone levels are all we need to know to explain delinquent behavior. What he is saying is that a demographic measure (age) appears to serve largely as a proxy for a lower level measure (testosterone) in sociological models of delinquency utilizing age. He is also pointing out that not only does this lower level measure illuminate the mechanisms by which age exerts its influence, it also adds explanatory power and does so in the context of a more parsimonious model.
All four examples I have presented show that demographic categories fail to capture the causes of the phenomena social scientists explore. Failure to explore more elemental explanations, or to even admit the possibility that such exist, leads to sole reliance on hypothetical social facts (mostly on some form of “discrimination” or some other wrong) which at best are incomplete and which are often demonstrably wrong. Sociologists would benefit greatly by adopting the methods of medical epidemiologists who also, as a first step, seek to identify group differences, but who then mine successively lower levels of analysis for further enlightenment. It is one thing to purposely ignore lower level explanations in the name of parsimony, but it is quite another to contradict them. Social scientists contradict them every time they assert that social categories and individual traits such as IQ are entirely “socially constructed,” and thus have no objective reality for the more fundamental sciences to probe.

Conclusion

I conclude that Professor Jones’ attack on methodological individualism is a red herring. The real issue in the reductionist/holistic debate is “which questions are best approached at one level rather than the other?” Questions about people and their behavior, I contend, are always best approached individualistically. If lower level measures 1) absorb the predictive power of higher level measures, 2) add incremental validity to them, and 3) illuminate the mechanism by which higher level variables work, there can be no scientific reason for preferring higher level measures when dealing with individuals and their behavior. Such an approach does not preclude studying the environmental effects on behavior. Indeed, any meaningful and coherent explanation demands it, but we cannot ignore the fact that people bring different traits and characteristics with them to identical situations that lead them to behave quite differently from one another. Many of these traits are highly heritable, and the fact that such traits are correlated with demographic categories clearly implicates biology in broader social concerns such as those we have explored here.

This essay should not be construed as dismissing the role of culture in human behavior. Because heritability estimates fluctuate among different populations and in different environments, behavioral genetic studies allow us to assess the relative importance of the mix of genetic and environmental factors over time and place in a way that studies focusing only on higher-order variables never could (Plomin, 1995). Culture and social institutions do mold individuals in many diverse ways, and to some extent they are autonomous “higher order” entities. As Jerome Barkow put it: “Culture is not simply psychology, not even evolutionary psychology, writ large” (1991, p. 635). But as he further points out, “psychology underlies culture and society, and biological evolution underlies psychology” (1991, p. 635). Thus, while not denying the power of culture, there is more than enough evidence to require social scientists to consider noncultural, lower-level explanations for their data whenever they are examining individuals and their behavior.

Questions best approached armed with higher level explanations include social change, war, and economic cycles. Although such theories would benefit from incorporating evolutionary insights about conflict, xenophobia, deception, and self-
interest, I personally would give short shrift to an explanation of the two world wars that focused on “the German character,” or for an explanation of the Great Depression that put it down to “human greed.” I would not favor higher level explanations of anything, however, just because “they are more easily taught and more easily learned” (Jones, 1996, p. 126). While it is true that sociology is more easily taught and learned than behavior genetics or neuroendocrinology, this seems a particularly impoverished reason for the continued neglect of these disciplines among social scientists. They will continue to be neglected, however, as long as social scientists see a discrete and unbridgeable gap between methodological individualism and methodological collectivism rather than the kind of continua that scientists in the more mature sciences see. Sociology, psychology, and biology are as integral to one another as chemistry and physics are. We will know that the social sciences have matured when naïve accusations of reductionism will be consistently met with Dennett’s mocking answer: “That’s such a quaint, old fashioned complaint! What on earth did you have in mind?” (1995, p. 81).

REFERENCES


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NOTES

1. This does not mean that such a theory is impossible. Temperamental differences are consistently found among infants of different racial groups at a very early age. Asian infants are more adaptable, quiet, stoic, and serene than Caucasian and black infants (Freedman, 1984). Many psychologists today stress the importance of constellations of personal characteristics, especially temperament, that collectively place people at high risks for antisocial behavior (Lykken, 1995). To the extent that temperament is a predictor of antisocial behavior, we should expect, ceteris paribus, somewhat lower rates of crime in Japan than in the United States. In fact, we note a black > white > Asian pattern of crime rates world-wide (Eysenck & Gudjonsson, 1989; Rushton, 1990). Nevertheless, a theory based solely on racial temperament cannot suffice as a complete answer to the question of different national crime rates.

2. The history of science shows that higher-level theories and explanations of many phenomena were in place before the mechanisms underlying them were discovered and elucidated. Useful observations and hypotheses now go in both reductionist and emergent directions in many fields as scientists come to recognize the complementarity of reductionist and holistic explanations. Cell biologists, for instance, know that at bottom they are dealing with sub-atomic “energy packets” and seek to understand their properties. But they also know that there are properties of the cell that cannot be deduced from them a priori, and that we require functional explanations of the whole cell, of how that cell fits into a network of other cells to form a larger whole (the organism), and even of how cellular function is effected by the organism’s transactions in its environment. Science is eclectic by nature, and it can pose questions and offer explanation at several levels of understanding.

3. One might ask why it is the case that higher-level explanations must not violate lower-level principles, but not the reverse. Almost by definition, the more basic and fundamental sciences have a broader and more firmly established body of knowledge. This body of knowledge has been achieved by intradisciplinary work and by interdisciplinary work with the adjacent lower level science. Physicists do not need chemists like chemists need physicists, and chemists do not need biologists like biologists need chemists. Sociological phenomena such as those discussed in this paper are generic terms for a plethora of biological and psychological phenomena as well as for social relationships. Sex, age, race, and class certainly have many empirical referents, and are thus worthy of study in their own right. However, each of those constructs is composed of people with brains, genes, and hormones which impose restrictions on what we may reasonably conclude about them. Although present knowledge of brains, genes, and hormones is far from complete, more is known about them with greater certainty than is known about sociological constructs, simply because brains, genes, and hormones are more amenable to verifiable and replicable experimentation and observation. Further, biology has made itself consistent with chemistry, and chemistry with physics, which provides biology with the firmest of scientific anchors. Phenomenal regularities are continuously found in biology which lead fruitfully to further discoveries, while sociology cannot even agree on the nature of its constructs, or even if they exist! For instance, many individual and groups traits such as IQ, temperament, and dominance are considered to be mere epiphenomena of social forces; that is, “socially constructed.” Not only does adherence to such thinking render sociological explanations incomplete, it violates and contradicts much of what is known biologically about those traits. I do not think that even the most zealous Durkheimian would argue that human biology should make itself consistent with sociology.

4. I am aware that hormonal secretion patterns and hormonal levels are not independent of the environment, and that hormones facilitate rather than “cause” behavior. Yet within the context of the “thermostatic” nature of hormone activity, male and females brains respond differently because of the early organizing effects of fetal androgens on the developing brain (Ellis, 1986; Nyborg, 1984). Additionally, males and females differ dramatically on the amounts of the various facilitating hormones they secrete (Udry, 1990).

5. As is the case with gender, the charge that race is socially constructed is true to some extent because racial boundaries are ambiguous and shifting. The case is made that there are no “pure” races and
therefore it is more fruitful to think in terms of clines (a graded series of physiological and morphological species changes along lines of geographic transition). But showing that physiological and morphological differences can be analyzed clinally does not mean that they cannot be analyzed by discrete categories (the form in which practically all racial data come to us) also, regardless of the “purity” of these categories. For instance, studies of genetic distance (differences in allelic frequencies across populations) show that the English differ from Danes by 0.21 percent of their polymorphic genes, but by 22.88 percent from African Bantus (Miller, 1994). A study of differential gene-based susceptibility to disease would certainly make more sense comparing Englishmen to Bantus rather than to Danes. Forensic anthropologists regularly classify skeletons by race, and other forensic experts are increasingly able to identify a perpetrator’s race by body fluid and hair samples (Rushton, 1995).

6. As coincidence would have it, Merton’s article “Social structure and anomie” (1938) introducing anomie/strain theory was followed by an article by Clark and Gist (1938) examining IQ and occupational choice. This article found that IQ was highly correlated with occupation, and that IQ served to funnel people into their various occupations, not “social structure.” Clark and Gist were not saying that IQ is the only cause of SES any more than I am, but they did conclude that it was perhaps the most important cause.

7. Because shared environments account for little or no variance in cognitive, temperament, and personality measures does no mean they have no effects on behavior. Shared environment are only those features of the environment that are constant across all siblings, such as parental vocabulary, income, education, religion, and culinary preferences, number of books in home, school attended, and so forth. These things serve to make siblings similar. Nonshared environment environments serve to make siblings different, and include birth order, differential treatment by parents, siblings, teachers, and peers, sicknesses, accidents, and the idiosyncratic ways different children experience the same environments. Among other behavioral outcomes, shared environment does have an effect on sibling similarity for propensity to become delinquent or criminal, which may index SES effects beyond those accounted for by IQ (Rowe, 1994).

8. “Heritable” does not mean “genetically determined”; most traits and characteristics have been found to be less than 50% heritable. Rather it means that genes participate in social behavior, that they and their environments are correlated in passive, evocative, and active ways, and that they bias individuals’ responsiveness to environmental opportunities in different directions (Scarr & McCartney, 1983; Rowe, 1997). Genes, in other words, largely determine the environments we experience and what aspects of the environment will be meaningful and rewarding to us and which will not (see Bouchard, Lykken, Tellegen, & McGue, 1996 for a discussion of this couched in terms of evolutionary psychology). Human beings are not blank slates at birth; they bring traits with them to their environments, and what they experience in those environments have further effects on those traits.

9. For instance, heritability estimates are always lower for most traits in disadvantaged environments, and biological factors play a smaller role in the probability of delinquency in disadvantaged than in advantaged environments (Venables, 1987; Walsh, 1992).