

I have always argued that the psychiatric commitment to a categorical sphere of diagnosis is fundamentally wrong and has to be replaced by a dimensional system, if we are to be governed by factual evidence (Eysenck 1960; 1970). The low reliability of categorical diagnoses, resulting from overlapping classifications, is notorious, and the claim that the intersituation has been remedied by successive editions of the APA Diagnostic and Statistical Manual has not been found to be justified (Kirk & Kutchins 1992). Even here, sanity is breaking in; DSM-IV agrees that a dimensional approach is scientifically preferable, although refusing to use it in actuality because of old, established habits of medical diagnosis.

Mealey suggests an absolute contrast between primary and secondary sociopaths but reports no evidence that these could be separated diagnostically with any acceptable reliability. From experience, I would be surprised if agreement between professional observers would be greater than 0.3 or thereabouts; clearly insufficient for scientific or practical purposes. I believe a much better picture would be one in which individuals were represented by points in a hollow, three-dimensional globe, the three diameters of which would represent Extraversion, Neuroticism, and Psychoticism (Eysenck & Gudjonsson 1989); the group of primary and secondary sociopaths would then appear as clusters of points in the $E + N + P$ octant, but certainly not as two quite separate and distinct clusters. The differences Mealey notes would locate primary sociopaths further out toward the periphery, and possibly closer to P than secondary sociopaths, who might be closer to the centre, and nearer to E and N . But all differences would be dimensional, with no absolute demarcations. And, clearly, a system of diagnosis referring each point in this globular universe to the three dimensions, as a three-digit number, would be more reliable and more valid than a verbal type of construct, as suggested by Mealey.

It would be possible, of course, to rotate these three reference axes to accommodate the theories of Gray and Cloninger, as Mealey suggests. I believe that both have made important contributions to the psychophysiological interpretation and understanding of personality-related behaviour, but I am not convinced that the evidence presented by them is adequate to suggest the rotation of the three primary axes. The latest study of the Gray system (Carver & White 1994) suggests to me that the activity of the BAS system aligns it with extraversion, that of the BIS system with neuroticism. But however that may be, and even if future research should force some degree of rotation, the principle of dimensional diagnosis would remain unaffected. If nature has not in fact produced some 300 separate psychiatric illnesses, as DSM suggests it has, then no human effort to force diagnoses into this Procrustean bed is likely to be successful. Nor will efforts to force behaviours into a two-type system be any more successful. Primary and secondary psychopaths may occupy different positions in our globe, but there are innumerable gradations observable in the points lying between pure examples of these points. If the notion of categorical disease entities breaks down completely when such time-honoured groups as schizophrenics and manic-depressives are concerned (Kendell & Brockington 1980), what hope is there for primary and secondary sociopaths?

Primary sociopaths are said to be mainly activated by *genetic* factors, secondary ones by *environmental* ones, but surely no one would argue that the ratio of these two factors is not infinitely variable, and at present incapable of measurement. All the genetic and environmental correlates and possible causes of both conditions (and criminality as well) are continuously variable; they cannot conceivably give rise to two categorically distinct groups. Indeed, it is not even clear just how Mealey would diagnose her primary sociopaths phenotypically. She says that "there will always be a small, cross-culturally similar and unchanging baseline frequency of sociopaths and a certain percentage of sociopaths . . . will always appear in every culture, no matter what the socio-cultural conditions" (sect. 3.1.1,

para. 1). But how would we ever test such an hypothesis, in the absence of methods of diagnosing the genetic make-up of a given individual? And is it really suggested that each and every one of these primary sociopaths in fact had the identical genetic make-up? Surely there would always be a more-or-less and a gradual fading into the genetic make-up of the secondary sociopath, with no clear-cut boundary between them. And it is of course quite doubtful whether the proportion of sociopaths would indeed be *identical* from group to group. Rushton (1994) has reported evidence of very marked differences in criminality between racial groups; it is not unlikely that this portends similar differences in the number of primary sociopaths.

Mealey also seems to suggest that treatment, whether preventive or later in life, should be *categorically* different for primary and secondary sociopaths. This again seems to go counter to fact. Unless diagnosis was well above the 0.70 level, any scheme of allocating treatment differentially to primary and secondary sociopaths would be doomed to failure. Considering that the great majority would fall between the two extreme *pure* groups, what should we do with them? Again, a graded, dimensional system would seem much better able to accommodate the facts of human diversity. Fortunately, it should not be difficult to translate the numerous psychological, hormonal, and physiological characteristics of Mealey's two types into a dimensional language, thus transforming an unnatural typological system into a much more natural dimensional one.

The epigenesis of sociopathy

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Abstract: Mealey distinguishes two types of sociopathy: (1) "primary," or obligate, and (2) "secondary," or facultative. Either sociopathy evolved twice, or one form is derived from the other, e.g., through: (1) genetic assimilation generating polymorphism in the relative strength of biases favoring the development of otherwise facultative strategies, or (2) independently heritable but strategically relevant characteristics biasing the optimal selection of facultative strategies.

Mealey's target article represents an important contribution to the study of sociopathy. She is to be congratulated on a theoretically insightful synthesis and creative reinterpretation of a wide-ranging assemblage of scientific findings on the topic. This kind of work is a sure sign that evolutionary psychology is rapidly maturing in its ability to incorporate and accommodate otherwise fragmented and disparate empirical content from the traditional social sciences by providing a meaningful functional context and a powerful interpretive framework. Mealey's major claim is that sociopathy can be productively seen as an evolved adaptive strategy for intraspecific social parasitism, or "cheating." Although this identity remains far from conclusively established, the case for functional equivalence is too compelling to dismiss as mere coincidence. A less persuasive secondary claim, however, is the proposed distinction between "primary" and "secondary" sociopathy in both phenomenology and etiology. It is about this second and more minor claim that I harbor certain reservations.

Although I have never worked explicitly on sociopathy, I have recently been involved in various collaborative research projects dealing with the deviant social and sexual strategies of competitively disadvantaged males. A major and recurring problem in this line of research concerns whether the deviant behavior patterns represent what behavioral biologists distinguish as "alternative" versus "conditional" adaptive strategies (Alcock 1989; cf. Mayr 1974). An "alternative" strategy can be defined as an adaptive strategy that is *obligate* for the individual,

"Just So" stories and sociopathyAndrew Futterman^a and Garland E. Allen^b^aDepartment of Psychology, College of the Holy Cross, Worcester, MA 01610; ^bDepartment of Biology, Washington University, St. Louis, MO 63130. futterman@hacad.holycross.edu; allen@biodept.wustl.edu

but for which the genetic predisposition is *polymorphic* within the population. A "conditional" strategy can be defined as an adaptive strategy that is *facultative* for the individual – contingent for its phenotypic expression upon environmental circumstances – but for which the genetic preparedness is *monomorphic* throughout the population.

In a study of domestic violence, Figueredo and McCloskey (1993) found that a structural equations model, based on 21 manifest indicators that were theoretically related to the depressed mate quality of the perpetrator, predicted 60% of the variance in spouse abuse and an associated 25% of the variance in child abuse. In a clinical sample of juvenile sex offenders, Russell et al. (in preparation) found that a series of moderated multiple regressions predicted a sequential progression from psychosocial deficits to sexual deviance to nonsexual criminality and, finally, to sexual criminality, with psychosocial deficits functioning as significant upregulators of that sequential progression. In both of these path models, the development of deviant strategies was driven by individual failure at mainstream social and sexual strategies, seemingly favoring the "conditional" strategy hypothesis.

In an explicitly behavior-genetic study, however, Rowe et al. (in press) found that the single most important predictor of juvenile delinquency, a measure reflecting the relative allocation of reproductive effort into mating effort (as opposed to parental effort), was highly heritable. In this study, individual failure at mainstream social and sexual strategies seemed more a consequence of this behavioral deviance than its initial precondition, seemingly favoring the "alternative" strategy hypothesis. Regrettably, we have never measured sociopathy per se in any of these studies but it would be surprising if it were not among the major contributors to these deviant behaviors. Nonetheless, Mealey's proposal that sociopathy is discriminable into two distinct types, based primarily on degree of genetic preparedness (cf. Garcia & Ervin 1968; Garcia et al. 1974; Seligman 1970; Seligman & Hager 1972), would greatly mitigate the apparent discrepancies between these various findings.

I am nevertheless reluctant to accept that formulation. For one thing, conditional and alternative strategies are not necessarily mutually exclusive mechanisms. Certain microevolutionary processes, such as genetic assimilation (Waddington 1957) may produce developmental biases favoring the development of certain phenotypes over others in response to specific environmental contingencies. Thus, even within the context of conditional strategies, genetic polymorphism in the relative strength of these developmental biases could be generated in a population of otherwise facultative strategists. In addition, it is possible that the chance individual possession of independently heritable (but strategically relevant) characteristics may bias the selection of adaptive strategies (i.e., "reactive heritability"). For example, an individual with a sexually attractive phenotype will be more effective at pursuing certain mating strategies than others and would, thus, be rationally expected to select them preferentially.

In the absence of more conclusive evidence I am personally biased toward the belief that it is more parsimonious to postulate a continuous trait than a discontinuous typology. This is not to say that such discrete typologies do not exist, as there are numerous nonhuman animal examples (Alcock 1989). In this case, however, it may be necessary to postulate that sociopathy arose not once but twice in human behavioral evolution; otherwise it is necessary to derive one form secondarily from the other, as the mechanisms proposed above purport to do. These and other plausible alternative hypotheses should be more fully explored, if not exhausted, before accepting the reality of a more complex state of affairs. Nevertheless, I must acknowledge that Mealey's theory is at least consistent with all of the data in this line of research that I have had direct involvement with, and holds the further promise of reconciling certain seemingly discrepant results.

Abstract: Sociobiological explanation requires both a reliable and a valid definition of the sociopathy phenotype. Mealey assumes that such reliable and valid definition of sociopathy exists in her "Integrated Evolutionary Model." A review of psychiatric literature on the diagnosis of antisocial personality disorder clearly demonstrates that this assumption is faulty. There is substantial disagreement among diagnostic systems (e.g., RDC, DSM-III) over what constitutes the antisocial phenotype, different systems identify different individuals as sociopathic. Without a valid definition of sociopathy, sociobiological theories like Mealey's should be viewed as entirely speculative.

Sociobiologists who study human behavior are confronted with a daunting task. They seek to provide a compelling evolutionary explanation for the emergence of behavioral phenotypes such as altruism, sociopathy, alcoholism, homosexuality, or any other trait that seems at first glance to be individually maladaptive. In order to do so, they must demonstrate that the phenotype advances the fitness of individuals, or relatives who demonstrate it. Darwinian fitness is claimed first by demonstrating that the behavioral phenotype in question is stable and consistent across environments, and then, by demonstrating that this behavioral phenotype in some manner confers an adaptive advantage over rival traits. In circumstances in which they cannot demonstrate consistency of phenotype or little is known about genetic and developmental mechanisms, sociobiologists often introduce evolutionary "models" to "explain" the trait's persistence. Another term for these models is "just so" stories, taken from the Rudyard Kipling book in which animal curiosities are fancifully and allegorically explained for the amusement of children (e.g., "How the Leopard Got His Spots"). However troubling or important these behaviors may be, such behavior-genetic explanations amount to little more than speculations that may sound reasonable but for which there is little or no evidence.

In the case of the "just so" story to explain the persistence of sociopathy developed by sociologist Linda Mealey, not only are the mechanisms and developmental processes underlying the sociopathy trait not specified, but the stability and consistency for the sociopathy phenotype itself has not even been demonstrated. And unless it can be demonstrated empirically that a sociopathy phenotype remains stable over a range of environments, a study of its genetic base becomes impossible.

One reason for this is that the classification scheme for sociopathy is not firmly fixed so that we should have any confidence that descriptions of the trait are either reliable or valid. Unlike the classification of Kettlewell's moths as speckled, dark, or white in rural and industrial Britain – a classification scheme that is broadly understood relative to selection effects and remains largely unchanged and unquestioned after 90 years – the validity of the classification scheme for sociopathy has changed frequently, and the properties that comprise the syndrome have been and still are assessed in a multitude of different ways. Moreover, unlike judgments of moth color, identification of sociopathy requires judgments of properties such as "lack of remorse," "impulsivity," "reckless disregard for others," and other social behaviors (American Psychiatric Association 1994) that are subjective and of highly variable description. Thus, that Mealey treats sociopathy "inclusively" should not prompt us to concede that she has identified sociopathy in any meaningful way or that she or other independent observers can assess the disorder reliably. As Kitcher (1985, p. 124) rightly points out: "Those who engage in studies of evolution of behavior need to do some work before they can reach the point from which Kettlewell began." [See also *BBS*, multiple book review of Kitcher's