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Is Crime in the Genes? A Critical Review of Twin and Adoption Studies of Criminality and Antisocial Behavior

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This paper performs a critical review of twin and adoption studies looking at possible genetic factors in criminal and antisocial behavior. While most modern researchers acknowledge that family studies are unable to separate possible genetic and environmental influences, it is argued here that twin studies are similarly unable to disentangle these influences. The twin method of monozygotic–dizygotic comparison is predicated on the assumption that both types of twins share equal environments, and it is argued here that this assumption is false. Adoption studies have been promoted as a better way of separating genetic and environmental influences. However, there is good reason to believe that adoption studies of criminal and antisocial behavior were confounded by selective placement factors. In addition, these studies suffered from bias and serious methodological errors. In spite of these problems, no adoption researcher claimed to have found evidence of a genetic predisposition for violent crime. It is concluded that the weight of the evidence from family, twin, and adoption studies does not support a genetic basis for any type of criminal or antisocial behavior. The historical background of genetic theories of criminality is also discussed.

The debate over the causes of crime is an old one. The potential for criminal behavior has been viewed by some as being stamped on people at birth for reasons of heredity, while others have maintained that “criminals are made, not born.” Still others have taken an interactionist stance, holding that both genes and environment are important. This paper focuses on twin and adoption studies of criminal, “antisocial,” and “psychopathic” behavior. Topics beyond the scope of this paper include the “XXY” controversy of the 1960s and 1970s, studies linking abnormal behavior with specific genes (e.g., Brunner, Nelen, Breakefield, Ropers, and van Oost, 1993), and studies looking at possible biological components of criminality. Although conflated in the minds of many,

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the finding that a condition (mental or physical) is influenced or caused by biological factors does not mean that the condition has a *genetic* basis. Many environmental factors such as stress, prenatal experience, diet, pollution, and radiation, can lead to biological abnormalities. This does not necessarily imply that genetic factors are operating. For a critique of biological explanations of criminality, the reader is referred to Katz and Chambliss (1991).

It is necessary to look at the genetics of criminality and antisocial behavior in light of the increasing influence of behavior genetic thinking on the role of genes in human behavioral differences. When members of the general public read about a supposed genetic link to crime, the conclusion often reached is that "crime is genetic." This leads to the belief that criminality is the result of genetic makeup — and not of racism, poverty, and other oppressive social conditions — thereby influencing public attitudes toward ethnic groups having a relatively high conviction/incarceration rate (such as African-Americans).

As a prelude to a discussion of twin and adoption studies it is worthwhile to look at the historical background of ideas on the genetics of criminal behavior. This is the subject of the following section.

Historical Background of the Genetics of Criminality

More than any other socially disapproved behavior, a belief in the inherited basis of criminality was widespread long before adoption, twin, or even family (consanguinity) studies had been performed. In the nineteenth century, Cesare Lombroso argued that criminals could be identified by their physiological features and that they represented, to use Gould's description in *The Mismeasure of Man*, "evolutionary throwbacks in our midst" (1981, p. 124). According to Lombroso,

Many of the characteristics presented by savage races are very often found among born criminals. Such, for example, are: the slight development of the pilar system; low cranial capacity; retreating forehead; highly developed frontal sinuses . . . the thickness of the bones of the skull; enormous development of the maxillaries and the zygomata . . . greater pigmentation of the skin; tufted and crispy hair; and large ears. To these we may add the lemurine appendix; anomalies of the ear; dental diastemata; great agility; relative insensitivity to pain; great visual acuteness; ability to recover quickly from wounds; blunted affection; precocity as to sensual pleasures . . . laziness; absence of remorse; impulsiveness; physiopsychic excitability; and especially improvidence, which sometimes appears as courage and again as recklessness changing to cowardice. (1911/1968, pp. 365–366)

For Charles Davenport, an important early figure in the American eugenics movement, criminals represented "our . . . ape-like ancestors" from "animalistic strains," who should be rooted out of the American breeding stock:

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The acts of taking and keeping loose articles, of tearing away obstructions to get at something desired, of picking valuables out of holes and pockets, of assaulting a neighbor who has something desirable or who has caused pain or who is in the way, of deserting a family and other relatives, of promiscuous sexual relations — these are crimes for a twentieth century citizen but they are the normal acts of our remote, ape-like ancestors Imbecility and “criminalistic” tendency can be traced back to the darkness of remote generations in a way that forces us to conclude that these traits have come to us directly from our animal ancestry and have never been got rid of If we are to build up in America a society worthy of the species *man* then we must take such steps as will prevent the increase or even the perpetuation of animalistic strains. (Davenport, 1911, pp. 262–263)

Today, the publicly stated views of Lombroso and Davenport are an embarrassment to researchers of the genetics of criminality, but the view of criminals as “evolutionary throwbacks” remains in some quarters. For example, behavior geneticist David Lykken (1995) described different criminal types as a “zoo of caricatures” (p. 17), distinguished by their various “species and subspecies” (p. 24). Lykken lamented the fact that National Socialist genocide and the “cruel experiments conducted by Dr. Mengele” at Auschwitz prevented “an entire generation of civilized people” from looking at the role of inherited factors in crime. “One minor but significant casualty of the Hitler period,” continued Lykken, “was the loss to our language of the innocent and useful word *eugenics*” (p. 71).

The role of genetic factors in crime was widely accepted during the late nineteenth and early twentieth centuries (see Fink, 1938). For example, Hall (1914) wrote, “That a criminal father should beget a child pre-destined to criminality is a foregone conclusion. The father exerts a hereditary influence equal to all the previous ancestors in the paternal line” (p. 87). A physician at “Sing Sing” prison in New York held this opinion:

My own observations, which have been practically unlimited along lines of information connected with the male offender, have led me to believe, in the last few years, that criminal character depends in the first instance on heredity So the born criminal is the product, mind and body, of the forces of heredity. Not only his body, but his mind is deeply impressed with the character of the parentage. And few indeed are the criminals who come to our prison at Sing Sing with minds that were at birth *tabula rasa*, whose mental powers at birth were not already thickly sown with seeds of crime. (Irvine, 1903, p. 750)

There were frequent calls for the sterilization of criminals on eugenic grounds during this period. Vasectomy was proposed in the late nineteenth century (Ochsner, 1899) as a “humane” alternative to castration. Ochsner justified the procedure on the grounds that it was “demonstrated beyond a doubt that a very large proportion of all criminals, degenerates and perverts have come from parents similarly afflicted” (p. 867). He claimed that the large-scale sterilization of criminals “would do away with hereditary criminals

from the father's side," and recommended the same treatment for "chronic inebriates, imbeciles, perverts and paupers" (p. 868).

In 1907 Indiana became the first of many states to pass a law permitting compulsory eugenic sterilization. The law sanctioned sterilization "to prevent procreation of confirmed criminals, idiots, imbeciles, and rapists" residing in a state institution who had been judged as "unimprovable" by a panel of physicians (quoted in Reilly, 1991, pp. 46–47). A prison physician, Harry Sharp, had performed vasectomies on inmates in Indiana since 1899. According to Sharp, "There is no disputing the fact that mental as well as physical defects are transmitted to the offspring . . . The decidedly defective individual is very easily recognized, as the mental abnormality is usually accompanied with prominent physical defects, described by Lombroso" (1909, pp. 1897–1898). In a discussion of Sharp's article in the pages of the *Journal of the American Medical Association* we find the following comment by Dr. J.N. Hurty, who had recently visited the estate of a wealthy family:

I was standing near the man who was in charge of the beautiful collies at the kennels; one of them (a female) came up to me, and she looked so pleasant that she seemed to me to have a laugh on her face. I patted her on the head, and she was duly grateful for the attention. I asked him, "Do you have any vicious dogs here?" He said, "Do you suppose that we would breed from viscous animals? *If a vicious animal appears here we kill it* [italics added]; we have nothing to do with them at all; and the result is that we have no biting animals, but only those amenable to instruction." *Why cannot we apply this to the human family?* [italics added]. (see discussion in Sharp, 1909, p. 1901)

While the call to kill (and thereby, as imagined, eliminate) "vicious" strains in the "human family" was an extreme view even for that time, a glance at the bibliography in Fink's 1938 review of the period's literature on the causes of crime demonstrates the common concern over the procreation of "criminals" and "defectives." The consensus of the time was that society could eliminate criminality and antisocial behavior by means of controlled breeding.

The period 1877–1919 saw the publication of several histories of "degenerate families" (see Rafter, 1988), the most well-known being *The Jukes* (Dugdale, 1877/1910) and *The Kallikak Family* (Goddard, 1912/1927). For many years these studies were cited as proof that criminality and "feeble-mindedness" were hereditary conditions. For Lombroso, Dugdale's book provided "the most striking proof of the heredity of crime" (1911/1968, p. 161). According to Davenport (1911), Dugdale showed that criminal family pedigrees could be the result of "a single focal point" of bad heredity, which in this case was "traced back to Max [Juke] living in a lonely mountain valley" (p. 183). Davenport created pedigrees of his own and concluded, "The foregoing cases are samples of scores that have been collected and serve as fair representations of the kind of blood that goes into the making of thousands of criminals in this country" (1911, p. 92).

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Today, "studies" like the Jukes and the Kallikaks are largely discredited because, among other reasons, it is now acknowledged that poverty, criminality, and illiteracy can run in families for social and environmental reasons. That being said, the historical record should be set straight regarding the conclusions of the authors of these investigations, since both recognized the importance of environmental conditions. According to Dugdale:¹

Where the organization is structurally modified, as in idiocy and insanity, or organically weak as in many diseases, the heredity is the preponderating factor in determining the career; but it is, even then, capable of marked modification for better or for worse by the character of the environment Where the conduct depends on the knowledge of moral obligation (excluding idiocy and insanity), the environment has more influence than the heredity For instance, where hereditary kleptomania exists, if the environment should be such as to become an exciting cause, the individual will be an incorrigible thief; but if, on the contrary, he be protected from temptation, that individual may lead an honest life, with some chances in favor of the entailment stopping there. (1877/1910, p. 65)

And Goddard wrote, "there is every reason to conclude that criminals are made and not born," while adding that the "best material out of which to make criminals, and perhaps the material from which they are most frequently made, is feeble-mindedness" (1912/1927, p. 54).

Few modern proponents of the hereditary basis of crime believe that there are specific "crime genes." Rather, they argue that people inherit predispositions for personalities that make them more likely to commit crime. As Goldsmith and Gottesman have written,

Notions such as "genes for crime" are nonsense, but the following notion is reasonable: There may be *partially* genetically influenced *predispositions* for basic behavioral tendencies, such as impulsivity, that in certain experiential contexts make the *probability* of committing certain crimes higher than for individuals who possess lesser degrees of such behavioral tendencies. (1996, p. 9)

Still, the general conclusion drawn from the results of genetic research is that criminal behavior is caused by heredity.

Any person looking into the causes of crime is confronted with the problem of how crime and criminality are defined. Not surprisingly, political and moral considerations are bound up with the definition of criminal and antisocial behavior. As eugenicist Paul Popenoe pointed out in an article written during the Great Depression, "A few years ago the man who had a bag of

¹Commenting on Dugdale's book, Fink (1938) wrote: "Perhaps no one book in the field of criminology in America has lent itself to such partisan interpretation as has Richard Dugdale's *The Jukes*, published in 1877. Unread, misread, or willfully distorted, it has been used by hereditarians and environmentalists alike to assert and supposedly to prove their respective positions" (p. 179)

gold in his safe was a thrifty and praiseworthy citizen; today he is a criminal" (1936, p. 388). Even Lombroso would have likely applauded the deeds of his "criminal men," had their "savagery" only been directed at the opponents of the Italian army's colonial campaigns.

Another difficulty in determining who should be studied is that being a registered or convicted criminal is dependent on one's being apprehended. As Rutter observed, "Many surveys have shown that, at some time, almost all boys commit acts that fall outside the law and which could have led to prosecution if they had been caught" (1996, p. 2). Surveys in Scandinavian countries such as Norway, Sweden, and Denmark, where some of the larger twin and adoption studies were performed, show that most young men, when answering anonymously, admitted to committing one or more criminal offenses at some point (Hurwitz and Christiansen, 1983, pp. 42–43). If these surveys are accurate, one could conclude that almost all Scandinavian males are genetically predisposed to commit criminal acts. It might be argued that genetic studies of registered or convicted criminality look at possible genetic factors in *the apprehension for*, rather than the commission of, criminal acts. P. Taylor (1996) noted that something that is illegal one day can become legal the next, and that the reporting of certain types of crime varies widely. He also noted that wealthy people usually have better legal representation than the poor. His conclusion: "In using any sort of recorded crime figures, researchers are taking on something that is very messy indeed, and that is one reason why confusion will follow" (p. 137).

This review is limited to an analysis of the two research methods most frequently cited in support of a genetic basis of criminal and antisocial behavior: twin and adoption studies. Consanguinity studies can, at best, demonstrate "scientifically" what society already knows — that the relatives of criminals are more likely to be criminals than are the relatives of non-criminals. These studies prove nothing about genetic factors, as Pam (1995) has succinctly noted:

The most serious breach in inductive logic . . . [is the] use of kinship concordance rates to determine genetic transmission of psychopathology. We have already noted that no family inheritance study can control for environment in human research; such data, therefore, are nowhere near "suggestive" — they are at best inconclusive and at worst misleading . . . This inferential limitation holds with respect to any consanguinity finding, even if the design and technique employed in the investigation were scientifically impeccable. (p. 19)

These sentiments are shared by several modern genetic investigators. As noted by adoption researchers Brennan, Mednick, and Gabrielli (1991), "In terms of genetics, very little can be learned from . . . family data alone," because "The parents have a major influence on the child's environment as well as on his/her genetic makeup; family studies cannot disentangle these hereditary and environmental influences" (p. 232).

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Criminal Twins: Blood Brothers or Partners in Crime?

The Twin Method and the Study of Criminality

The "classical twin method" (more commonly known as "the twin method") compares the concordance rates or correlations of reared-together identical twins (also known as monozygotic or MZ) to the same measures of reared-together same-sex fraternal twins (also known as dizygotic or DZ). A significantly greater similarity or concordance of MZ twins (100% genetic similarity) when compared with DZs (who average a 50% genetic similarity) is usually cited as evidence in favor of the genetic basis of the trait or condition under study. Most criminal twin studies have employed this method.

As noted elsewhere (Joseph, 1998a, 1999b, 2000a), it is a fruitless task to subject individual twin studies to a detailed examination of their numerous methodological flaws, because it is likely that monozygotic twins are more similar or concordant than dizygotics for most traits. Twin researchers have concluded that this difference demonstrates the operation of genetic factors, often failing to point out that this conclusion is based on a highly questionable theoretical assumption. What follows, therefore, is a brief review of the larger twin studies as a prelude to a discussion of whether the twin method is a valid instrument for the detection of genetic influences.

The first criminal twin study was performed by Johannes Lange (1930) in Germany, only a few years after the twin method had been developed. Lange's study was one of the first popular expositions of the twin method. He identified 13 MZ and 17 DZ pairs where one twin had been imprisoned. Because Lange found that ten of the MZs but only two of the DZs had a co-twin who was also imprisoned, he concluded that genetic factors "play a predominant part" (p. 46). In keeping with the zeitgeist, Lange proposed eugenic measures as a way of eliminating or reducing crime. In a carefully worded recommendation in his final chapter, Lange (who published his study during the Weimar period) stated that "our most important task" is to use "preventative measures," and that "We must try to make it impossible for human beings with positive criminal tendencies to be born" (p. 242). The implication was that criminals or those carrying "criminal genes" must be prevented from reproducing, although Lange stopped short of calling for sterilization. After the National Socialist seizure of power the regime passed the 1933 "Law for the Prevention of Genetically Diseased Offspring," which established "genetic health courts" deciding on cases of compulsory sterilization for eugenic purposes. The National Socialists passed another law in 1933 allowing the castration of sex offenders (Proctor, 1988). Lange's goal of "making it impossible" for criminals to be born was now, it seemed to him, being realized, and he wrote in favor of these laws (see Lange, 1934).

Rosanoff, Handy, and Rosanoff (1934) studied 97 pairs of twins, finding male concordance rates of 22/33 MZ (67%) and 3/23 same-sex DZ (13%). The investigators concluded that criminal behavior is caused by “pre-germinal [genetic] rather than germinal” factors (p. 929).

Two criminal twin studies were published in National Socialist Germany in the mid-1930s (Kranz, 1936; Stumpfl, 1936). Stumpfl (1936) studied 18 MZ and 19 DZ pairs, finding concordance rates of 65% and 37% respectively (cited in Christiansen, 1977b, p. 72). He concluded that some criminality is hereditary and should lead to sterilization. Among Kranz’s (1936) same-sex twins, he found concordance rates of 66% MZ (21/32) and 54% DZ (23/43). This comparison is not statistically significant ($p = .21$, Fisher’s Exact Test, one tailed), which did not prevent Kranz from concluding, “we must ascribe to heredity a more important role in the production of crime, than has hitherto been the case” (quoted in Popenoe, 1936, p. 390). While recognizing that environmental factors are sometimes important, Kranz (1936) supported compulsory sterilization:

One could ascertain so far on the basis of twin concordance rates that have been found that the imbecile criminals are undesirable in terms of racial hygiene [*rassenhygienisch unerwünscht*]; furthermore, some types of criminal psychopaths are borderline psychotics and severe alcoholics. All of these are already being recorded to a large extent through the sterilization law. (pp. 250–251, my translation)

Kranz wrote that the genetic impairment of sex offenders “can hardly be questioned,” while adding that “the castration law is simultaneously fulfilling the racial hygienic task [*rassenhygienische Aufgabe*]” (p. 251, my translation).

A few smaller studies appeared between 1937 and 1976. Then, two major Scandinavian criminal twin studies were published in the 1970s (Christiansen, 1977a; Dalgard and Kringlen, 1976). These studies were the first to obtain pairs from an unselected and complete series of twins. Typically, the older studies identified persons found in prisons or in mental institutions who were identified as being criminals and who also had a twin. This method of sampling has been criticized on the grounds that it creates a sample biased in favor of concordance (Rosenthal, 1962). The newer Scandinavian studies, on the other hand, were based on national registers and studied an entire population of twins unselected for criminality.

Dalgard and Kringlen studied 49 MZ and 89 DZ pairs, finding no significant MZ/DZ concordance rate difference using either a “broad” or “strict” definition of crime. The MZ pairwise rate for broadly defined crime was 22%,

²The term “racial hygiene” was used in Germany, while “eugenics” was used in most other countries. The terms are essentially synonymous. The German Society for Racial Hygiene was founded in 1905 by, among others, Alfred Ploetz and Ernst Rüdin (Proctor, 1988).

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and 26% for strictly defined crime. On the basis of these results and of the more similar environments experienced by monozygotic twins, the authors concluded, "These findings support the view that *hereditary factors are of no significant importance in the etiology of common crime*" (1976, p. 231).

Christiansen (1977a) studied 85 MZ and 147 same-sex DZ pairs and found pairwise concordance rates of 33% MZ (28/85) and 12% same-sex DZ (17/147). Christiansen's sample included all twins born between 1881 and 1910 in the Danish islands east of the Little Belt in which both twins were alive past the age of 15. These twins were then checked against Danish police records. The final results of the study were reported by Cloninger and Gottesman (1987), who found a pairwise MZ rate of 48% (56/116) and a same-sex DZ rate of 28% (56/202). They concluded (on the basis of proband-wise concordance figures) that the results supported a 54% heritability of liability to crime (p. 97).

Lyons et al. (1995) studied pairs obtained from the Vietnam Era Twin Registry (VETS). Diagnostic interviews were administered by telephone to 3,226 pairs of male twins. The researchers concluded that several antisocial traits were "significantly heritable" (p. 906). However, the method of determining antisocial and criminal behavior by self report is certainly questionable. For example, most of the respondents in a companion study (Lyons, 1996) denied any "early criminal behavior" such as "swip[ing] things from stores or from other children or steal[ing] from your parents or from someone else" (p. 63). As one critic asked, "Who hasn't swiped something before the age of 15?" (M. Daly, discussion in Lyons, 1996, p. 71). In fact, two-thirds of the respondents denied *all* antisocial behaviors (p. 71). The very title of Lyons's 1996 investigation, "A Twin Study of Self-Reported Criminal Behavior," illustrates a serious problem with the study. Unless answering anonymously, people tend to refrain from telling others about their past and present criminal behavior — including telephone interviewers who know their identity.

There have been other twin studies of antisocial behavior performed in the last 20 years (e.g., Eley, Lichtenstein, and Stevenson, 1999; Rowe, 1986), which will not be discussed here. As mentioned previously, the crucial question is whether the underlying assumptions of the twin method are valid.

Twins Reared Apart

There are no systematic studies of *reared-apart* criminal twins in the literature, although there a few published individual case histories. Grove and colleagues (1990) performed a reared-apart twin study of antisocial behavior, and claimed to have found "significant heritability" for childhood and adult antisocial behavior. However, this and other reared-apart twin studies (e.g., Bouchard, Lykken, McGue, Segal, and Tellegen, 1990; Juel-Nielsen,

1965/1980; Newman, Freeman, and Holzinger, 1937; Shields, 1962) are plagued by methodological errors, bias, and the failure to control for the common environmental influences shared by reared-apart twins (see Farber, 1981; Joseph, 2001b; Kamin, 1974; H.F. Taylor, 1980)

An example of the problems with case histories of reared-apart twins is evidenced in the report of Schwesinger (1952), who discussed 18-year-old Mexican–American identical twins Esther and Elvira. Although the girls were separated at nine months, Elvira soon returned to her mother’s home, while Esther was sent to live with another relative. For eight years following their separation the twins lived “in neighboring houses, perhaps a couple of hundred feet apart” (p. 40). Their mother reported that the girls played together, had the same type of clothing and toys, and “were aware of their twin relationship” (p. 40). The girls became geographically separated only at the age of nine, but even then the girls had contact with each other, including yearly visits in the summertime. As an adolescent, Elvira was beaten by her “brutal” (p. 41) stepfather, who threw her out of the house. She subsequently ran the streets and was sent to a correctional institution because of her delinquency. Around this time, Esther returned to her mother’s home determined to be reunited with her twin: “Her one desire was to be with her twin, just as the latter’s was to be with her” (p. 42). Schwesinger noted that “the bond between the girls was a close one” (p. 42). Soon, Esther became delinquent and was committed to a correctional school different from the one to which Elvira had been sent. She was able to escape from this school, while Elvira was on release from her school “very much upset, and determined to find Esther” (p. 42). Soon, the girls were picked up and sent to the same school where they “proved devoted to each other” (p. 42). At the age of 18 Esther, who unlike Elvira, “had not basked in warm mother love throughout her formative years” (p. 46), was found dead of a morphine overdose in the hallway of a big city hotel.

As is often the case in pairs of reared-apart twins reported in the literature, these girls were raised in different branches of the same family and had considerable contact and a strong twinship bond. Farber (1981) considered this pair typical of the “dubious separation” (p. 51) found in many cases of reared-apart twins and concluded, “There is no substantive evidence in the twin reared-apart data to support the claim that genetic determination is significant in this area [crime], and there is much to support the idea that environment is potent” (p. 229).

Does the Greater Similarity or Concordance of Monozygotic vs. Dizygotic Twins Point to a Genetic Predisposition to Crime?

The fundamental theoretical assumption of the twin method holds that the environments of reared-together MZ and DZ twins are roughly the same.

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On the basis of the "equal environment assumption," most twin researchers have concluded that significantly higher MZ concordance points to the operation of genetic factors (for a detailed critique of the equal environment assumption, see Joseph, 1998b; Pam, Kemker, Ross, and Golden, 1996). Twin researchers believe that environmental factors are held constant because both types of twins are born at the same time, and must therefore experience similar environments. However, the evidence suggests that identical twins are treated more similarly and experience more similar environments than fraternal twins (Kringlen, 1967; Scarr and Carter-Saltzman, 1979; Smith, 1965; Wilson, 1934). In addition, MZs experience a level of association and "ego-fusion" (Jackson, 1960) unmatched, perhaps, by any other type of human relationship. It is therefore plausible that the higher concordance rate of MZ twins is *entirely* the result of environmental factors (plus methodological error and bias). Although modern twin researchers such as Kendler (1983, 1993) argue that the twin method is only minimally biased by the greater environmental similarity of MZ twins, this argument does not hold up to critical analysis (see Joseph, 1998a, 1998b; Pam et al., 1996).

The doubtful validity of the twin method has been acknowledged by criminal twin researchers themselves. According to Dalgard and Kringlen, the twin method requires that "The environmental conditions are in general similar for MZ and DZ pairs," but the researchers concluded, "This assumption is obviously not true" (1976, p. 214). Later, they wrote that the equal environment assumption is "an assumption which today cannot be accepted" (p. 223). In his well known schizophrenia twin study, Kringlen published data (1967, p. 115, Table 46) demonstrating that monozygotic twins grow up in more similar environments and experience a far higher level of closeness and identity confusion than dizygotic twins. Kringlen's error lies in his belief that the falsity of the equal environment assumption means only that genetic factors will be exaggerated, when in fact it indicates that, like family studies, the twin method cannot reliably distinguish possible environmental and genetic influences.

Dalgard and Kringlen found that 86% of their MZ pairs had felt an extreme or strong interdependence, while only 36% of DZs felt this way (1976, p. 224).³ When the researchers grouped twins on this basis, there was virtually

³In a discussion of the Dalgard and Kringlen paper, Cloninger and Gottesman (1987, pp. 98-99) wrote, "As expected, a slightly greater proportion of MZ twins than of DZ twins were psychologically close (84% of 31 vs. 74% of 54)." However, they were looking at the concordance figures of twins' level of "intra-pair interdependence" in Dalgard and Kringlen's Table 12 (1976, p. 224), which compared concordance rates among a select group. When information was reported on the entire sample of interviewed pairs (Dalgard and Kringlen, 1976, p. 224, Table 11), we find that 42 of 49 MZ pairs (86%) had an "extremely strong" or "strong" level of closeness, which was true for only 32 of the 89 DZ pairs (36%). Seven MZ pairs (14%) were as close as ordinary siblings, which was true for 57 (64%) of DZs.

no concordance rate difference among pairs with an “extreme or strong” level of interdependence (MZ: 6/26, or 23.2%, vs. DZ: 3/14, or 21.4%; p. 224).

According to Christiansen,

The fundamental assumption underlying conclusions about heredity and environment that have usually been drawn from criminological twin studies is that the relevant environment of the two twins is (and has been) equally similar or equally different, regardless of zygosity. Stated in another way: intrapair environmental variations must be the same for MZ co-twins as they are for DZ co-twins. *It is open to serious doubts whether this condition is fulfilled with respect to social behavior* [emphasis added]. (1977a, p. 93)

Christiansen acknowledged that “it is generally accepted that the experienced environment of MZ twins is more similar than that of DZ twins” (p. 94), citing several studies as evidence. Thus, the authors of two large population-based Scandinavian criminal twin studies had serious doubts about the validity of their research method.

The findings of behavior geneticist Gregory Carey confirmed the doubts of Dalgard, Kringlen, and Christiansen. On the basis of an analysis of the Danish criminal twin study data, Carey (1992) concluded,

The assumptions of the traditional twin method may be violated for phenotypes related to externalizing antisocial behavior If MZ twins influence each other more than do DZ twins — a hypothesis that cannot be rejected in this analysis — genetic effects for criminal liability may actually be small. (p. 21)

Carey, a proponent of the twin method in general (see Carey and DiLalla, 1994), believed that the equal environment assumption was likely invalid for criminal and antisocial phenotypes.

Lyons (1996) also recognized that monozygotic twins experience more similar environments:

The twin method does not assume that the environment is the same for both members of the twin pair What the twin method does assume is that the environment is not more similar for MZ twins than it is for DZ twins in a way that would inflate the observed resemblance for MZs relative to DZs The evidence is quite supportive of the “equal environments” assumption. (p. 72)

Lyons is describing the “trait-relevant equal environment assumption,” which holds that MZ and DZ environments need only be equal *in respect to etiological factors shown to be relevant to the trait in question* (Gottesman and Shields, 1972). Although most contemporary twin researchers are proponents of the trait-relevant definition and acknowledge that monozygotic twins experience a more similar environment than dizygotic twins, few realize that they have thereby transformed the twin method into little more than a special type of family study (Joseph, 2000a). In both family and twin studies, common genes

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and common environment are correlated, yet twin researchers argue that family studies are confounded by environmental factors, whereas the twin method is not. However, if MZ twins (like members of the same family) share a more similar environment, they are more likely to be exposed to relevant etiological factors, even if these factors are not always known.

The following case illustrates the likely effect of the twinship bond on MZ concordance rates. Wheelan (1951) chronicled the lives of a pair of reared-together British identical twins whose personalities differed greatly. The "patient" and his twin brother (called "Twin A" and "Twin B" here) had been raised in an abusive and alcoholic family environment. Twin A, who later was diagnosed as an "aggressive psychopath" and was convicted of larceny, was the acknowledged "leader of the twins" from an early age (p. 134). The twins were investigated at age 27, and their divergent personalities are described as follows:

The patient [Twin A] has no friends, and he soon tires of acquaintances whom he makes easily; he is described as being cold-hearted, selfish and unpredictable; he never heeds advice; borrows money, is dishonest, and [is] a shiftless worker; he shows no affection or consideration for anyone — even his four children, for whom he has never accepted responsibility. His brother [Twin B] is steady, stable, modest, less quick tempered and more ambitious. In contrast to his brother's agnosticism, he recently joined the R. C. Church. (p. 136)

What makes this pair interesting from the standpoint of monozygotic twins concordant for criminality is the fact that Twin B, upstanding citizen that he was, nonetheless was convicted as an accessory to jewelry theft. His brother Twin A, who was living in a different part of the country, had asked him to pawn stolen jewelry for him. Twin B agreed to do this and was subsequently arrested and convicted for the deed. Thus, vastly different monozygotic twin brothers were concordant for criminality on the basis of the criteria used in most family, twin, and adoption studies. We should recall that Twin A was the leader of the pair, and it was probably difficult for Twin B to turn down his request in spite of his likely distaste at being involved in illegal activities. Like most of the twins recorded in the various studies, concordance for criminal behavior in this case was the probable result of association, not common blood.

There is a marked difference between DZ same-sex (DZss) and DZ opposite-sex (DZos) concordance rates in the various studies. In the Rosanoff and associates 1934 study, the adult criminality concordance rate for DZss twins was 18% (5/28), but only 3% (1/32) for the DZos pairs. Kranz (1936) reported DZss rates as 23/43 (54%), and DZos rates as 7/50 (14%). In Stumpf's study (cited in Christiansen, 1977b), the rates are DZss 7/19 (37%), and DZos 2/28 (7%). Large differences were found in the final report of Christiansen's study

(Cloninger and Gottesman, 1987, p. 99). The rates were: DZss 56/202 (28%), and DZos 14/228 (6%). The Eley et al. (1999) study also reported higher DZss vs. DZos correlations.

Dalgard and Kringlen noted that the higher rate among DZss twins "emphasizes the significance of environmental factors" (1976, p. 217). This statement is true but requires clarification: the DZss/DZos concordance rate difference emphasizes the role of environmental factors *affecting concordance rates*, which suggests that the MZ/DZ concordance rate difference is also affected (or explained entirely) by environmental factors.

In his 1983 study of teenage twins, Rowe attempted to test the validity of the equal environment assumption, and concluded that the assumption is valid. According to Rowe, the equal environment assumption is supported because delinquency among the twins was not predicted by their degree of association, and because MZs did not commit delinquent acts together more frequently than DZs. Due to the problems with the study, however, these findings have little meaning. These problems include: (1) The study depended on mailed responses from twins, who were asked to self-report delinquent acts. It is unlikely that these teenage respondents were willing to honestly report antisocial (and sometimes criminal) acts. (2) Only 50% of the twins returned questionnaires, which likely biased the respondent sample in the direction of better behaving pairs. (3) A statistically significant association between shared activities and a greater similarity of DZ males' delinquent behavior *was* found. However, as Rowe described it, he inspected the scatterplot, removed an outlier, and declared the association non-significant (p. 478). (4) Rowe's test involved only a few environmental variables, while many others went unchecked. The shared activities questionnaire contained a nine-item Likert scale. Rowe calculated mean scores from the Likert scale responses, which potentially obscured important associations between answers on the ends of the scale and concordance.

Evaluation of the Criminality Twin Study Literature

The genetic basis of criminal or antisocial behavior cannot be established on the basis of a greater concordance rate or correlation among monozygotic versus dizygotic twins, because MZ twins share a more similar environment and a greater association than DZ twins. Thus, there is no reason to accept that the twin method records anything more than the greater environmental similarity of MZ versus DZ twins, plus bias and methodological error. This was Jackson's (1960) basic argument in his discussion of the schizophrenia twin studies, and none of Jackson's major points has been refuted by twin method proponents (Joseph, 2001a). The twin method and consanguinity studies share a common problem: both are confounded by the correlation

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between genetic and environmental similarity. In an attempt to disentangle these potential influences, several researchers have studied adopted children, a topic to which we now turn.

Adoption Studies of Criminality, Psychopathy, and Antisocial Behavior

Overview

Adoption studies have been promoted as an ideal way of separating nature from nurture, since an adoptee receives his or her genes from one set of parents but is raised in the family environment of another. According to Mednick and Kandel (1988), family and twin studies suffer from possible environmental contamination: "To address this problem, adoption studies have been utilized. These are natural experiments in which the effects of genetic and rearing influences may be separated to a relatively high degree" (p. 103). The most famous psychiatric adoption studies were the Danish-American schizophrenia investigations of Kety and associates (Kety, Rosenthal, Wender, and Schulsinger, 1968; Kety, Rosenthal, Wender, Schulsinger, and Jacobsen, 1975; Rosenthal, Wender, Kety, Welner, and Schulsinger, 1971; Wender, Rosenthal, Kety, Schulsinger, and Welner, 1974).

There have been five adoption studies of criminality or antisocial behavior: two minor North American studies and one minor Danish study, plus two major Scandinavian investigations (Bohman and colleagues in Sweden, and Mednick and colleagues in Denmark). Before undertaking a critical examination of these investigations it is important to note that *in no adoption study do the researchers claim to have found evidence in support of a genetic basis for violent crime*. The investigators claimed only to have found a genetic component for "petty" or "property" offenses, or for a vaguely defined notion of antisocial or psychopathic behavior. While the twin study discussion was concerned more with the problems of twin studying in general than with the individual studies, this section looks at each adoption study individually, with a more detailed discussion reserved for the large and influential Scandinavian studies. With this in mind, let us now turn to the North American studies of Crowe and Cadoret.

Crowe's Iowa Studies

The 1972 study. Crowe (1972) looked at 52 adopted-away biological children of 41 women incarcerated in Iowa prisons (index adoptees — Crowe called them the "proband group" — consisting of 27 boys and 25 girls) and compared their arrest rate to a group of control adoptees matched for age, sex, race, and age at adoption. Apart from arrest records, Crowe had little information on the biological parents of either group. He found that there

were significantly more index adoptees than controls having an arrest record in Iowa (8/52 vs. 2/52; $p = .046$), a record of conviction (7/52 vs. 1/52; $p = .03$), and a record of incarceration (5/52 vs. 0/52; $p = .028$). However, there were no significant differences for adult arrests or for adoptees with two or more arrests. Crowe did not conclude that he found evidence for a genetic basis of crime, but did state, "the results are . . . in line with those of other [non-adoptive] studies which have reported an increased prevalence of psychopathy among the relatives of criminals" (1972, p. 603).

Critique. There are some glaring inadequacies in this study. Of the 41 biological mothers, only *one* was convicted of a violent offense (assault). There were 15 cases of forgeries and check felonies, five cases of prostitution, four cases of larceny, three cases of desertion, three cases of breaking and entering, two cases of lewdness, and one each of the following: conspiracy, aiding prisoners to escape, bigamy, transmitting a venereal disease, contributing to the delinquency of a minor, and exposing a dead body (p. 601). Several of these women were therefore convicted on morals charges and bizarre or questionable offenses such as "exposing a dead body" and "desertion" (although none of the offspring of women convicted of lewdness, adultery, or bigamy was among the offender adoptees). Of the seven convicted adoptees only *three* were felons, and none committed a violent offense. One was arrested for "lascivious acts," and another for "delinquency."

Crowe's study looked only at criminal records in Iowa, which is surrounded by seven other states. It is likely that many index and control adoptees lived or spent time in these states, yet no arrest records were looked at outside of Iowa (Robins, 1975). In the Swedish and Danish adoption studies, it was not likely (though possible) that residents of these countries crossed international borders to commit crimes, but it is much more likely that people would cross the Iowa state border and commit crimes. Iowa is a landlocked state whose borders come within 150 miles of such major cities as Chicago, Kansas City, Milwaukee, Minneapolis, Omaha, and St. Louis.

Crowe also failed to consider the likelihood that index and control adoptees were raised in dissimilar environments. As noted elsewhere,

Adoption studies are based on a critical theoretical assumption: that factors relating to the adoption process (including the policies of adoption agencies) did not lead to the placement of experimental (index) adoptees into environments contributing to a higher rate of the condition or trait in question. The placement of adoptees is assumed to have been random, meaning that children were not selectively placed into homes correlated with the status of their biological family. (Joseph, 1999b, p. 133)

Crowe stated that "although there are no known data on the homes . . . there is no known reason why the homes should have differed" (1972, p. 603), and admitted that he did not know whether prospective adoptive parents were

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aware of the background of the child's biological family. This is an important piece of information because genetic ideas were strong in Iowa during the years that these adoptees were placed.

In 1911 Iowa became one of the first U.S. states to enact a eugenic sterilization law, which passed by a vote of 32-0 in the state Senate (Reilly, 1991, p. 46). Iowa subsequently created a state Board of Eugenics to oversee the law and to enforce sterilization. Penitentiary wardens were required, in the words of Iowa state law, to submit quarterly reports with the names "of all persons, male or female, living in this state, of whom he or she may have knowledge, who are feeble-minded, insane, syphilitic, habitual criminals, moral degenerates, or sexual perverts who are a menace to society" (Reichmann, 1939, p. 436). Persons judged by the Board of Eugenics to have an "inherited tendency" to these conditions were subject to a sterilization order. The law was enacted in order to "protect society from the acts of such a person, or from the menace of procreation by such a person" (Reichmann, 1939, p. 437). Laws permitting the eugenic sterilization of criminals were in force during the entire period in which Crowe's (and Cadoret's) adoptees were placed (see Lindman and McIntyre, 1961, Chapter 6; Robitscher, 1973, Appendix 4), which supports the idea that the biological offspring of convicted criminals in Iowa were viewed as "hereditary taint carriers."

The 1974 study. Crowe's next paper (1974) used the same group of adoptees, but this time they were rated on the basis of whether they were diagnosed with antisocial personality. Because Crowe was unable to interview the 41 biological index mothers, he simply *assumed* that they were *all* antisocial personalities on the basis of one study claiming to show that 65% of female offenders were sociopathic. This is quite an assumption when we realize that, (1) because the incarcerated women were not interviewed, their true rate of antisocial personality was unknown, and (2) even if Crowe's assumption was valid, about 35% of the incarcerated women were not antisocial. The study is therefore invalidated on this basis alone; one cannot simply assume a personality type on the basis of a criminal conviction. For example, if the women convicted of "desertion" had left their families because of spousal abuse, they still would have been diagnosed by Crowe as antisocial.

Crowe conducted structured psychiatric interviews with index and control adoptees, who were rated for antisocial personality by three blinded judges. As Crowe acknowledged (p. 790), the fact that the interviewer was not blind to the status of the adoptees could have biased the data used by the judges to rate the adoptees. The results showed that six index and zero controls received a diagnosis of "definite antisocial personality," which reached statistical significance. The biological mothers of these six cases were convicted of larceny (2), assault (1), prostitution (1), contributing to the delinquency of a minor (1), and desertion (1). Crowe listed cases of "probable antisocial per-

sonality" and "borderline antisocial" but did not count these cases in his final determination that the index group had a significantly higher rate of antisocial personality. Ironically, the Danish-American schizophrenia adoption studies depended on a broad and vaguely defined "schizophrenia spectrum of disorders" to obtain statistically significant results, whereas Crowe required a strict definition to obtain the same result — and for neither study is there evidence that the researchers determined their diagnostic criteria *in advance*. Had Crowe (like Kety, Rosenthal, and associates) counted the "probable" and "borderline" cases, the index/control difference would not have reached statistical significance (6/46 index vs. 4/46 control; $p = .37$, Fisher's Exact Test, one-tailed. See Crowe, 1974, p. 787, Table 4).

Finally, there is evidence that the antisocial adoptees suffered more deprived early environments than either non-antisocial index adoptees or controls. Let us look at the figures from Crowe's Table 7 (1974, p. 789). As noted by Katz and Chambliss (1991), the antisocial adoptees were more often raised in broken homes than non-antisocial or control adoptees, and were placed later than the other groups (mean age at placement: 18.8, 7.7, and 7.1 months respectively). More importantly, antisocial adoptees spent an average of 14.2 months in an orphanage, whereas non-antisocial index and control adoptees spent only 4.0 and 3.4 months in such an environment. The fact that the antisocial adoptees spent so much time in an orphanage suggests that, (1) their subsequent behavior was influenced by the deprivation they suffered in these institutions, and (2) the status of their biological mother may have been known to prospective adoptive parents, which meant that they remained in the orphanage longer because they were less desirable adoptees, and that the family eventually adopting them provided a less psychologically healthy environment than experienced by the other adoptees. While Crowe (1974, p. 789) acknowledged that the "age of adoptive placement and length of time spent in temporary care (orphanages and temporary foster homes) showed a clear association with antisocial outcome," he downplayed the suffering of children living in institutions, claiming that Iowa orphanage nurseries were "adequately staffed [and] the quality of physical care was good and, although personal attention was lacking, the babies were not subjected to the degree of deprivation that has been described in some orphanages of that day." Attachment theorists would argue that the rupture of the child-caregiver bond experienced by infants is crucial to their later development, and the antisocial adoptees were clearly more deprived of love and nurturance than were the other adoptees in Crowe's study. Even Heston recognized the damage done to children in the Oregon foundling homes where some of the adoptees he studied were placed:

The actual extent to which these children were deprived of maternal or emotional nurture beyond that implicit in group care must be inferred largely from indirect evi-

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dence The later history of these children strongly supports the hypothesis that significant deprivation did occur. Random observations recorded in school or nursery records and recollections of foster parents and subjects describe several children as shy, withdrawn, demanding excessive attention, or sad; few as happy, spontaneous, or normal. Some were requiring sedation at night It is certain that most of these children were unhappy and probable that there was significant deprivation of emotional nurture. (Heston and Denney, 1968, p. 366)

While there are other problems with Crowe's investigation, it is clear that his study contains invalidating flaws and therefore does not support the role of heredity in causing crime or antisocial behavior. As noted by other commentators, "The Crowe study is so far from minimum standards of scientific adequacy that it deserves only minimal comment" (Gottfredson and Hirschi, 1990, pp. 59-60).

Cadoret's Iowa Study

Cadoret (1978) looked at the biological offspring of 28 parents who had given up their children for adoption and were regarded by Cadoret as antisocial on the basis of adoption agency records. Of these 28 biological parents, eight were diagnosed as antisocial because they were convicted felons, one was diagnosed for having a record of several arrests (like Crowe, Cadoret *assumed* antisocial personality on the basis of conviction), and the remaining 19 parents (all female) were diagnosed as antisocial if the records showed that they demonstrated two or more antisocial behaviors described by Robins (1966, Appendix D, pp. 342-343). According to Cadoret's use of Robins's criteria, a parent could have received a diagnosis of antisocial personality on the basis of exhibiting two behaviors such as "poor work history," "poor marital history," "sexual promiscuity or perversion," "suicide (attempts)," "public financial care," "many somatic symptoms," "lack of friends," or "vagrancy." Thus, a parent with somatic symptoms and a poor marital history could have been diagnosed as antisocial by Cadoret. Like Crowe's method, Cadoret's method of diagnosing biological parents as antisocial was inadequate (Walters and White, 1989).

Biological offspring were diagnosed on the basis of a structured questionnaire administered over the telephone. Although the research assistant was blind to the status of the adoptees, the reliability of information obtained in this way is questionable. The results showed that there were significantly more psychiatric diagnoses among the biological offspring of antisocial parents, versus a control group of adoptees whose biological parents did not have a recorded behavior or psychiatric diagnosis. However, this finding depended on adoptee diagnoses which included "mood swings," "other personality disorders and deviating traits," and "other psychiatric diagnoses" (Cadoret, 1978,

p. 178). Of the 12 spectrum offspring of antisocial biological parents, only one was diagnosed with antisocial personality, while three of the six adoptees of parents with "retardation with antisocial behavior" were diagnosed as antisocial. Cadoret combined these adoptees and stated that a rate of four cases out of 18 (22%) was significantly greater than the control group rate of 0/25 (p. 179). However (leaving aside methodological errors for the moment), this rate also must be higher than the population base rate before the results can be generalized to the non-adoptee population, and the difference between the rate of antisocial adoptees with antisocial biological parents is not significantly higher than the population expectation, even if we say that it is zero of 18 (4/18 vs. 0/18; $p = .052$, Fisher's Exact Test, one-tailed). Cadoret's Iowa adoption study, like Crowe's study, contained serious methodological flaws and biases, and the data failed to support the conclusions of its author.

Schulsinger in Denmark

Schulsinger (1977; originally published in 1972) selected 57 Danish index adoptees diagnosed as psychopathic and a group of 57 matched control adoptees with no history of psychiatric diagnosis.⁴ Schulsinger utilized the Adoptees' Family method, which locates affected adoptees and compares diagnoses among their adoptive and biological relatives. Schulsinger, who was diagnosing blindly, found a 3.9% psychopathy rate among index biological relatives compared to a control biological relative rate of 1.4%, a difference barely reaching statistical significance ($p = .049$, Fisher's Exact Test, one-tailed). However, the difference between the biological fathers is not statistically significant (5/54 index vs. 1/56 control; $p = .094$, Fisher's Exact Test, one-tailed). Schulsinger created a broad "psychopathy spectrum" which included diagnoses such as "probable psychopathy," "character deviations," and "hysterical character deviation" (1977, p. 119). He found a significant biological relative difference on the basis of this broad spectrum. Schulsinger concluded that "genetic factors play an important role in the etiology of psychopathy" (p. 122).

Schulsinger's definition of psychopathy is open to question. He provided little justification for the validity of his "psychopathy spectrum," and even his criteria for "core psychopathy" are questionable. According to Schulsinger, it was sufficient to have a record of drug or alcohol abuse to receive this diagnosis (p. 115). As Walters and White observed, "Criterion measures such as these are too vague, idiosyncratic, and detached from criminal involvement to be very useful in investigating the biological correlates of

⁴This investigation can be criticized on the basis of the selective placement of adoptees. For a detailed discussion of how selective placement factors likely confounded adoption studies in Denmark and other Scandinavian countries, see Joseph (1999a, 1999b, in press-b), and the upcoming analysis of the Bohman et al. and Mednick et al. studies of registered criminality.

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criminal behavior" (1989, p. 473). Thus, Schulsinger's study provided inconclusive evidence in favor of hereditary factors in psychopathy, and no evidence for genetic factors in criminal behavior.

The Bohman and Associates Swedish Studies

Bohman and colleagues have produced one of the two major criminality adoption investigations. In his 1978 study, Bohman reported on the rates of criminality and alcoholism in a large group of Swedish male and female adoptees given up by their biological parent(s) and placed into nonrelative adoptive homes during the first three years of life. The names of these adoptees and their biological and adoptive parents were checked against the records of the Swedish Criminal Register and of the Excise Board, which maintained records on people fined for intemperance.

The purpose of the study was to determine whether alcoholism and criminality rates were elevated among adoptees who had a criminal or alcoholic biological parent. Bohman found that the adopted-away sons of biological parents with a criminal record were themselves criminal at rates comparable to the expected population rate. He also performed a control study of 50 male and 50 female adoptees whose biological fathers "were among those with the most serious criminal records Most of these men had been sentenced to long terms in prison" (Bohman, 1978, p. 272). Only 8% (4/50) of the male adopted-away biological children of these men, and 8% of controls (4/50), had criminal records. Bohman (1978, p. 274) concluded, "The results suggest that there is a genetic determinant for alcoholism but not for criminality (defined as repeated offenses with long prison sentences)."⁵ Thus, Bohman found no evidence of a genetic predisposition for criminality, while stating that the results "must be regarded as preliminary" (p. 276).

The year 1982 saw the publication of a reanalysis of the study by Bohman and his associates (Bohman, Cloninger, Sigvardsson, and von Knorring, 1982). In this paper, Bohman et al. reported the criminality rate of 862 Swedish men from the same adoptee cohort as the 1978 study. Bohman and associates reanalyzed the data and argued that, while there was no association between criminality in the biological parents and their adopted-away sons, genetic influences were detected if the type of offense and the use of alcohol were controlled for. The investigators claimed that "nonalcoholic petty criminals had an excess of biologic parents with histories of petty crime but not alcohol abuse" (Bohman et al., 1982, p. 1233). The researchers con-

⁵Because the present study looks at the genetics of criminality, Bohman's conclusions about alcoholism will not be the subject of critical analysis. See Peele (1986) for an analysis of the alcohol genetic literature.

cluded that they found a genetic influence for petty offenses — a claim that cannot go unchallenged — but *not* for violent crime.

Critique. As noted, the 1982 Bohman et al. study looked at 862 male adoptees, but in 1978, Bohman studied 1,125 male adoptees. Because the 1982 paper was essentially a reanalysis of the 1978 data, one must ask why the male adoptee group was reduced from 1,125 to 862, a drop of 23.4%. The investigators offered an explanation:

The subjects included all 862 men born out of wedlock in Sweden from 1930 through 1949 who were adopted at an early age by nonrelatives. Other subjects included by Bohman in preliminary analyses (Bohman, 1978) were excluded because of incomplete data, late placement, or intrafamilial adoption. The age at adoption was less than 3 years in all cases and 8 months on the average. The adoptees ranged in age from 23 to 43 years at the time of last information. (Bohman et al., 1982, p. 1234)

The dramatic reduction of the male adoptee group is puzzling because, as Bohman reported in his 1978 paper (p. 270), *all* 1,125 male adoptees had been placed with nonrelatives before the age of three, and were checked against the Criminal Register. Thus, there is no indication that the 1982 adoptee group was subject to more stringent criteria than the 1978 group, and no more specifics were given by the authors. Nor was it stated that the researchers were unaware of the Criminal Register status of the 263 adoptees (and their relatives) who were dropped from the study.

In Table 1 of Bohman and associates' 1982 paper we see that the criminality rate of the adopted-away biological sons of parents with a record of criminality (with or without alcohol abuse) is 13%. This figure is comparable to the Swedish male population risk of 11% (p. 1235). Contrary to genetic predictions, the fact that an adoptee had a criminally-registered biological parent did not lead to an elevated crime rate when compared to the population rate. The authors presented several pages of complex statistical procedures which they claimed showed a genetic predisposition for petty offenses. In addition to the problems already discussed, there are two factors receiving insufficient attention from the investigators: (1) the rate of index petty criminality was not stated as being significantly higher than the population expectation, and, (2) the selective placement of adoptees.

In any adoption study, researchers must demonstrate that the index rate of the trait in question is significantly higher than the general population expectation, or more accurately, than the non-adoptee population expectation (Boyle, 1990). This is true even if a control group is utilized. For example, Bohman stated that the population risk for male registered criminality in Sweden was 11%. Suppose that Bohman, in his 1978 study, had found that 11% of his male index group adoptees had criminal records, while only 2% of the male control adoptees had a criminal record — a statistically significant

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difference. Contrary to the beliefs of most adoption researchers, no conclusions about genetic factors could have been reached from these figures because the index adoptees had the same criminality rate as one would expect to find in a randomly selected group of Swedish males. The significant index/control difference could indicate that controls experienced environments less conducive to producing criminality, but the difference would not provide evidence for the operation of genetic factors. In the Bohman et al. 1982 reanalysis there is no indication that any group of adoptees had a rate of petty criminality higher than the population prevalence, which could have been determined through the records of the Criminal Register. Groups of adoptees are compared with each other, but the population base rate comparison was not examined.

The Bohman and colleagues 1982 study was essentially a post-hoc reanalysis of Bohman's 1978 data, from which the authors drew a set of conclusions more in line with genetic thinking on the subject. Ironically, the opposite approach was utilized by the Danish-American schizophrenia researchers. In the well-known Adoptees' Family series of Kety, Rosenthal, Wender, and associates, there were no statistically significant elevations among index biological relatives versus controls for any one diagnostic category (which is especially true if the comparisons are limited to first-degree relatives). This is evidenced by the words of Kety himself, in a paper co-authored with Ingraham:

At the prototypical end of the spectrum, chronic schizophrenia is found exclusively in the biological relatives of chronic schizophrenia patients where it occurs at a low prevalence (approximately 3%), whereas the prevalence in the biological relatives in the normal controls is negligible. The same is true for uncertain chronic schizophrenia. Latent or borderline schizophrenia was found at a 4–5% prevalence in the biological index relatives and 1% to 1.5% in the biological relatives of controls. This is also true where the symptoms are less distinct and the diagnosis is designated uncertain. *Since neither in chronic nor in latent schizophrenia the results for the definite or uncertain diagnoses are statistically different, it appears justified to combine them* [italics added]. (Ingraham and Kety, 1988, pp. 121–123)

In order to claim that his study found significant results, Kety needed to combine his spectrum diagnoses into one total — and his investigation is considered (albeit incorrectly) to have produced the most compelling data in favor of a genetic basis for schizophrenia (Joseph, in press-a). Contrast this to Bohman and associates' 1982 reanalysis. In this case the definition of crime was *narrowed* in order to find a significant genetic effect. And in neither the Kety and colleagues schizophrenia investigation nor the Bohman et al. criminality study were the criteria for inclusion or exclusion published *before* the appearance of papers stating how the dependent variable would be defined. Thus it is fair to assert that in both cases, the investigators performed a post-

hoc reanalyses of the data — using opposite approaches — in order to find a statistically significant effect for the trait under study. And in both studies, the researchers could just as easily have concluded that they found no genetic influences on schizophrenia or criminality.

Regarding selective placement, there is reason to believe that the biological relatives of registered criminals were not placed into the same types of environments as adoptees lacking such a family history. Adoptees in the Swedish study were born between 1930 and 1949 — a period which coincided with the widespread belief in the inherited nature of most mental abnormalities, including antisocial behavior (Broberg and Tyden, 1996). Sweden passed its first eugenic sterilization law in 1934, and another law was passed in 1941 permitting eugenic sterilization for those demonstrating “an anti social way of life” (cited in Broberg and Tyden, 1996, p. 108). The existence of such laws suggests that children with a criminal family background, easily checked through the use of registers, would not have been attractive candidates for adoption because of their perceived “hereditary taint.” As noted by Bohman et al., Swedish children “thought to be at high risk for heritable disorders were unlikely to be considered eligible for adoption” (1982, p. 1234). But what about children thought to be at moderate genetic risk? They might well have been included in the adoption process but would have been considered less desirable adoptees. According to Bohman et al. (1982, p. 1234), Swedish “adoptee[s] and the adoptive parents were never informed [by adoption authorities] about the identity or behavior of the biological parents,” although in an earlier paper Bohman indicated that a child’s biological background *was* a factor influencing the adoption process. In a discussion of a subgroup of children from his 1971 study, Bohman wrote,

This group may be regarded as a negative selection from the primary series, in that many of the children were considered at birth, or while at the infants’ home, to be difficult to place on account of retarded development, *poor heredity* [italics added], or somatic complications. *It often took longer to place these children in their ultimate home environment than it did the other children in this study* [italics added]; more than half of them spent over nine months at an institution before being placed. (Bohman, 1971, p. 6)

It appears that a potential adoptee’s “poor heredity” was a factor in the adoption process. This could have been manifested in two ways: (1) prospective adoptive parents were informed of the biological background of the child, or (2) the authorities were reluctant to place children with “poor heredity” into the homes of “good” adoptive families. In any case, in 1971 Bohman clearly indicated that these children were placed later and spent more time in an institution than other children.

An interesting set of data supports this idea. In Bohman’s control study (Bohman, 1978, p. 274, Table 6), the biological male children of fathers and

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mothers registered for criminality *but not for alcohol abuse* are recorded alcohol abusers at rates significantly higher than controls. Among these adopted-away biological offspring of registered criminal fathers, nine of 50 (18%) were registered alcohol abusers, versus two of 50 controls (4%; $p = .026$, Fisher's Exact Test, one-tailed). Among the offspring of registered mothers, the rate of alcohol abuse is nine of 48 (19%), versus two of 48 controls (4%; $p = .025$, Fisher's Exact Test, one-tailed). Combining both sets of offspring versus controls, the significance level reaches .001. Of course, one could argue that these figures suggest a genetic relationship between criminality and alcohol abuse, but a far more plausible explanation would be that the children of criminal parents were placed into more psychologically damaging environments than were control adoptees, leading to significantly more alcoholism.

According to Bohman and Sigvardsson, "children were selected and placed . . . according to the social and occupational status of their biological parents" (1980, p. 348), and Bohman (1978) wrote that adoptees with antisocial or alcoholic biological parents were placed, on average, two to three months later than controls. He conceded the possibility that "later placement is associated with selective factors that contributed independently to poorer social adjustment later in life and hence to an increased risk of appearing in the registers" (p. 275). If true, then placement policies might account for any possible significant elevation in so-called petty offenses.

To summarize, the Swedish adoption study of Bohman and colleagues, as acknowledged by the investigators themselves, found no evidence for genetic influences on violent crime. The finding of a genetic influence for petty offenses was based on a post-hoc reanalysis of the data, and the study suffers from several methodological problems and potential environmental confounds.

Mednick and Associates' Danish Study

The Mednick and associates Danish adoption study (Mednick, Gabrielli, and Hutchings, 1984) is perhaps the most well-known and most often cited study in support of a genetic basis for criminal behavior. The investigators utilized a register of 14,427 Danish adoptees placed between 1924 and 1947. This register had been compiled by Kety and associates (1968), who studied rates of schizophrenia among adoptees and their relatives. Mednick and colleagues (like Bohman et al.) utilized the Adoptees' Family method, and identified 13,194 adoptees (6,129 male, 7,065 female). Like most genetic studies of criminality and antisocial behavior, Mednick et al. concentrated on the results among male subjects, which will be the focus of attention in this critique.

As discussed by Hutchings and Mednick (1975), criminality was determined by checking the names of adoptees against the records of the Danish

Police Record Office and a separate criminal record, called the *Personalia Blad*, which kept information on all persons who had been convicted of offenses. Criminal behavior was defined as those subjects with a record of court conviction. The investigators checked the conviction records of the biological and adoptive relatives of convicted adoptees. According to Mednick et al. (1984):

The size of the population permits segregation of subgroups of adoptees with combinations of convicted and non-convicted biological and adoptive parents in a design analogous to the cross-fostering model used in behavior genetics. If neither the biological nor the adoptive parents are convicted, 13.5 percent of the sons are convicted. If the adoptive parents are convicted, and the biological parents are not, this figure rises only to 14.7 percent. However, if the adoptive parents are not convicted and the biological parents are, 20.0 percent of the sons are convicted. If the adoptive parents as well are convicted, 24.5 percent of the sons are convicted. These data favor an assumption of a partial genetic etiology. (p. 892)

Mednick and colleagues also performed a "sibling analysis," which compared rates among split-up half- and full-sibling pairs placed into different adoptive homes. Genetic theory predicts that full-sibling pairs would be more concordant because they are more similar genetically. Of the 126 male-male *half-sibling* pairs, 31 had at least one convicted member and four pairs were concordant (concordance rate = 4/31, 12.9%). Of the 40 male-male *full-sibling* pairs, 15 had at least one convicted member and three of these pairs were concordant (concordance rate = 3/15, 20%). Although the full- versus half-sibling difference is not statistically significant, Mednick and colleagues concluded, "The numbers are small but indicate that as the degree of genetic relationship increases, the level of concordance increases" (Mednick et al., 1984, p. 893).

The authors believed that although the evidence pointed to a genetic predisposition for property offenses, "This was not true with respect to violent crimes" (Mednick et al. 1984, p. 891). This finding was elaborated upon in a subsequent paper:

A significant relationship exists between parents' convictions and property offending. A significant relationship does not exist for violent offending . . . Genetic factors predispose to property offending but not to violent offending. *If a biological predisposition to violence does exist, then it must be a result of other postconception factors [italics added].* (Mednick, Brennan, and Kandel, 1988, p. 29)

Like Bohman and associates in Sweden, the investigators believed they had found evidence of a genetic component for petty or property offenses, but not for violent crime.

Critique. As in the other criminality adoption studies, there are major problems with the Danish investigation. To begin, Danish adoptees in gen-

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eral were significantly more susceptible to criminal conviction than non-adoptees. According to Mednick et al. (1984, Table 1), 84.1% of the identified male adoptees had no record of being convicted, meaning that the adoptee conviction rate was 15.9%. The researchers stated that the prevalence rate for male conviction in Denmark was about 9% (Hutchings and Mednick, 1975), while Hurwitz and Christiansen (1983, p. 41) put the figure at 8%. We have seen that Mednick and associates found a conviction rate of 13.5% for male adoptees whose biological and adoptive parents had no record of conviction (1984, p. 892), which means that about 827 such adoptees were registered criminals (13.5% of 6,129). This figure can be compared to the expected rate among 6,129 members of the Danish male general population, yielding an expected figure of 490 (8% of 6,129). Thus, Danish adoptees placed during the era under study were far more likely to be convicted when compared to the general population prevalence, and thereby constituted a distinct population with regard to criminal conviction.

An 8% Danish population prevalence is actually a high figure to compare with the 13.5% rate among adoptees lacking a convicted biological or adoptive parent, for at least two reasons. The first is that the 8% population prevalence rate is slightly inflated by the inclusion of adoptees, who have a higher rate. The second reason is the likelihood that a fair percentage of the non-adoptee convicted criminals had a parent with a conviction record, yet Mednick and colleagues found a 13.5% prevalence among adoptees whose adoptive (and biological) parents *were not convicted*. Although the Danish figures are not available, it is probable that the conviction prevalence rate among non-adoptees whose parents had no conviction record was well below 8%.⁶ Thus, Danish adoptees placed between 1924 and 1947 who had non-convicted biological and adoptive parents were at *roughly twice the risk* of being convicted versus non-adoptees lacking a convicted parent.⁷ Therefore, with regards to criminality, *findings among Mednick and colleagues' Danish adoption cohort cannot be generalized to the non-adoptee population, Danish or otherwise*. Conclusions can be applied only to the population of Danish adoptees.

A controversy in the schizophrenia twin study literature is relevant to this discussion. In his devastating critique of these studies, Jackson (1960) theorized that the identical twinship itself might lead to higher rates of

⁶In another paper by the investigators (Kandel et al., 1988), which looked at a consecutive series of Danish children born between 1936 and 1938 who were *not* adopted away, 39% of the sons of "severely sanctioned" criminals were themselves recorded criminals by 1972, while 7% of the sons of non-registered fathers had been registered (p. 225).

⁷The higher rate among adoptees is consistent with the results of a recent paper from the investigators (Raine, Brennan, and Mednick, 1997) which found a correlation between early maternal rejection and adult violence.

schizophrenia, due to problems of identity confusion found among MZ twins. If this theory were true, then MZ schizophrenia concordance rates would have been inflated and MZ/DZ differences would not be generalizable to the single-born population. In response, Rosenthal (1960) and others attempted to show that monozygotic twins were no more likely to receive a schizophrenia diagnosis than members of the single-born population, and that the schizophrenia twin studies were therefore valid (see Joseph, 2001a). As we have seen, Danish adoptees were about twice as likely to be registered criminals as non-adoptees, meaning that the argument used by schizophrenia twin researchers in defense of their studies limits Mednick's generalizations to the Danish adoptee population.

The next serious problem with Mednick and colleagues' conclusions is the likelihood that, as we saw in the Iowa and Swedish studies, the biological children of convicted criminals were placed into inferior rearing environments when compared to other adoptees (Kamin, 1985). To begin, it is necessary to understand the social and political climate in the period when these adoptees were placed (1924–1947). In 1929 Denmark became the first European nation to enact a eugenic sterilization law, which was passed with the agreement of most Danish political parties (Hansen, 1996). In 1935 a law was passed allowing the compulsory sterilization of mentally "abnormal" people on eugenic grounds. In the same year, a panel of leading Danish medical authorities reviewed the results of the 1929 law, writing,

[The psychopaths] are often — to a larger extent than for example, the mentally retarded — asocial or antisocial (criminal); and their erotic activity and inventiveness, considered together with their fertility — often extramarital — is considerable With respect to hereditary tainted progeny the psychopaths are comparable to the more well-defined mental diseases, even though the pattern of inheritance is still unknown. (Sand, 1935, quoted in Hansen, 1996, pp. 42–43)

As Hansen's account suggests, the children of criminals and "psychopaths" were regarded as "hereditary tainted progeny," and Hansen noted that "the leading [Danish] medical experts in 1935 were ready to go very far in their pursuit of eugenic goals and social control of the marginal groups of society" (p. 43). He pointed out that "there were no adverse reactions" to the 1935 document and that "the medical world seemed to agree with the conclusions" (p. 43).

Between 1940 and 1945 Denmark was under German occupation, which represents about a fifth of the period when the adoptees in Mednick's study were placed. This period saw a dramatic rise in the Danish conviction rate (Hurwitz and Christiansen, 1983), and these convictions are counted in the study. The occupation was likely marked by an even greater emphasis on eugenics, since Denmark was under the occupation of the armies of the

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German government of "applied biology." Mednick has noted on several occasions (e.g., Mednick and Volavka, 1980) that the twin studies of Stumpfl and Kranz were "tainted by their origins in Nazi Germany" (p. 92), which he rightly considered "a politically unfortunate period" (p. 95). However, he typically failed to discuss the importance of the fact that about 20% of the period during which his Danish adoptees were placed, and criminals were registered, occurred during a similarly "unfortunate period" in Danish history.⁸

The purpose of this discussion is to establish that the period when adoptees were placed in the Mednick and colleagues study (1924–1947) coincided with a period of Danish history when undesirable traits were seen largely as the product of bad genes; so much so that the authorities passed laws in an attempt to prevent the alleged carriers of these genes from reproducing. As we will see, the evidence suggests that the Danish adoption process was heavily influenced by this social atmosphere.

According to Mednick and Hutchings (1977), a "potential problem with the adoption method is the possibility that the adoptive family is informed by the adoption agency of deviance in the biological family," and they noted that a social worker who had read the older adoption journals "formed the impression that serious deviance in the biological parents *was routinely reported* [italics added] to the prospective adoptive parents unless they refused the information" (p. 161). In addition, information was obtained for other members of the biological family, and was used in determining the suitability of adoptees. According to an annual report issued by the leading Danish adoption agency of the time,

Before a child is cleared for adoption, it is investigated with respect to health, and an attempt is made to obtain detailed information on the child's family background and to form an impression of its developmental potential. Not only for the adoptive parents, but also for the child itself, these investigations are of great importance for its correct placement. Information is obtained on the child's mother and father; *on whether or not there are serious physical or mental illness in the family background* [italics added]; criminal records are obtained for the biological parents; and in many cases school reports are obtained. By means of personal interview with the mother an impression of her is formed. Where information is uncovered on *convicted criminality or on mental retardation, mental illness, etc. in the family background* [italics added], the case is referred to the Institute of Human Genetics of Copenhagen University, with whom there exists a valuable cooperation for advice on the advisability of adoption. (Mother's Aid Organization for Copenhagen, Copenhagen County and Frederiksborg County. Annual Report for 1946–47; quoted in Mednick and Hutchings, 1977, p. 163)

⁸Not all researchers of the genetics of criminality considered studies published during the National Socialist period to be "tainted." For example, Schulsinger wrote, "The very special eugenic ideas of the Third Reich involved some German psychiatric geneticists in the classical type of family studies on relatives of psychopathic probands . . . Their work was carried out in the same neat way as other, respectably intended, genetic work from the famous Munich school. The results of these studies unanimously indicated that heredity plays a role in the etiology of psychopathy" (Schulsinger, 1977, pp. 112–113)

As we see, the adoption agency obtained the criminal records of the parents and others "in the family background." The biological background of a potential adoptee was "of great importance for its correct placement" because it was believed to determine the child's "developmental potential" — that is, its *genetic potential*.

Mednick et al. (1984) stated that 37% of the biological parents had their first conviction before their child was adopted, whereas 63% were convicted after the adoption. They produced figures showing that there was no difference between the conviction rates of adoptees placed before and after their biological parents' conviction, and concluded, "The fact that the adoptive parents were informed of the biological parents' criminality did not alter the likelihood that the adoptive son would be convicted of a crime" (p. 893). However, this comparison looks only at the parents and leaves out knowledge of the criminal status of other family members which, as seen in the Mother's Aid Organization document, was of interest to the adoption authorities.

There were many more available Danish children than there were families wanting to adopt them during much of the period when adoptees were placed. As described by Mednick, Gabrielli, and Hutchings:

Many of these adoptions took place during the Great Depression and World War II. It was more difficult to find willing adoptive homes in these periods owing partly to the relative unavailability of adoptive parents and to the additional number of adoptees available. (1987, p. 78)

In other words, the adoption process was very much a buyers' market during this period of Danish history. Mednick read over some of the case reports of children who were put up for adoption, but were never placed: "Every weekend (at least in the 1930s), Danish people who wished to adopt would visit the orphanages and pick children . . . Children whose selection by an adoptive parent is delayed may be less attractive physically and behaviourally" (1996, p. 134). To this description it should be added that many children were less attractive *genetically*. Crudely put, Danish prospective adoptive parents arrived at the orphanage each weekend and picked children in much the same way as they might have selected apples at the market. Children who were healthy, attractive, and were not "hereditarily tainted" were picked by the most qualified parents in the same way that they would have selected the reddest, shiniest, and least bruised apples. The "best" families would have gotten the "pick of the litter," leaving the less desirable children either unadopted or placed into homes providing a less favorable environment than the other adoptees.

In this sense Mednick and associates failed to understand the significance of the differing environments of desirable and undesirable children. For these investigators, the prospective adoptive parents' knowledge of the biological

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family's criminality might cause the adoptee to be labeled as being expected to commit criminal acts on the basis of his or her biological background, which would thereby affect parental treatment. But what is more important is that a child's background, under the conditions of Danish society during the era in which he or she was placed, meant that the child was not a desirable adoptee in the eyes of the most qualified adoptive parents who, as we have seen, were routinely provided information on the criminal records of the biological parents. Into what kinds of adoptive families were the adopted-away biological children of families with convicted criminals eventually placed? This is an important question. It is likely that these adoptees were placed into the homes of people who, for various reasons, were not qualified to adopt the most desirable children. These homes may have been more chaotic or potentially exploitative than the others. In a control study reported by Hutchings and Mednick (1975, p. 109; discussed by Kamin, 1985), the biological and adoptive fathers of criminal adoptees (N = 143) were compared to the biological and adoptive fathers of non-criminal adoptees (N = 143). Among the criminal adoptees, 33 (23%) had a registered *adoptive* father, whereas only 14 (9.8%) of the control adoptive fathers were registered. The comparison is statistically significant ($p = .002$, Fisher's Exact Test, one-tailed), and Hutchings and Mednick added that the difference "was also reflected in the various indices of criminality such as number of recorded cases and total length of sentence" (1975, p. 109).

The rate among these adoptive fathers further illustrates the different family environments experienced by the criminal adoptees. In fact, a major aspect of the Adoptees' Family model compares the prevalence of the trait in question among index and control *adoptive* relatives. According to Rosenthal's (1970) description of how the model is used with schizophrenia, "A higher incidence among the adoptive relatives of index cases than of controls supports the view that rearing by, of, or with schizophrenics contributes to the development of the disorder" (p. 57). Commenting on the results of the 1968 Adoptees' Family schizophrenia study of Kety and associates, Rosenthal noted that

among adoptive relatives, there was no appreciable difference between the two groups Genetic theory would predict such a finding. Most environmentalist theories would predict a higher incidence of such disorders among the adoptive parents of the index cases as compared with the controls, but the findings do not support this prediction. (Rosenthal, 1970, p. 127)

It appears that the conviction rate difference between index and control adoptive fathers in Mednick's study is, as Rosenthal outlined, consistent with the environmental prediction.

Returning to the analogy of selecting apples, we might expect that all types of buyers would at least have an equal chance to select the best apples. It is not likely that this was the case in the Danish adoption process, however, because the authorities had the ultimate decision of where children would be placed. It is probable that children with the best "developmental potential" were placed with more stable and nurturing available adoptive families, leaving the rest to less stable families. As Mednick acknowledged, "In Denmark, the national adoption agency had a policy of selective placement" (1996, p. 134). Mednick claimed, however, that "the agency was not successful" (p. 134), but the evidence suggests that it was "successful" in many respects. Such a policy would have influenced placements in the previously discussed Mednick et al. "sibling analysis" comparing concordance rates among adopted-away half- and full-sibling pairs. Because the full-siblings had a more similar biological family background, it is likely that they were placed into more similar environments than were the half-sibling pairs. As noted, the concordance rate difference between the pairs was not statistically significant. Unfortunately, Mednick and colleagues (1984, p. 893) claimed that a non-significant difference, based on an admittedly "small" sample, showed that a relationship existed between genetic similarity and concordance.

In a footnote to their 1984 *Science* article, Mednick et al. pointed out,

Among males, there was a statistically significant association between adoptee criminality and the amount of time spent in the orphanage waiting for adoption. This effect, which was not true for females, may be due to institutionalization. Or it may be a function of selection bias ("less desirable" boys adopted later and also being convicted). [p. 894]

Like Crowe and Bohman et al., Mednick and colleagues found an association between time spent in an institution and subsequent conviction. They acknowledged that the association might be due to selection bias or the effects of institutionalization. The psychological damage inflicted on children spending significant periods of time under the typically appalling and nurtureless conditions of an orphanage (particularly under the conditions of the Great Depression, foreign occupation, and war) was greater for convicted than non-convicted adoptees, and was not controlled for in this study.

I noted earlier that the investigators, by their own admission, found no evidence in support of a genetic basis for violent crime. Unfortunately, the papers published by this group typically emphasize the theme of alleged genetic influences on criminality in general. The investigators could have given their papers titles such as, "No Genetic Basis for Violent Crime," or "Environmental Causes of Violence Must be Identified," but instead they used titles such as "Genetic Influences in Criminal Convictions: Evidence

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from an Adoption Cohort" (Mednick et al., 1984), "Genetic Correlates of Criminal Behavior" (Gabielli and Mednick, 1983), and "Predisposition to Violence" (Mednick et al., 1988).

The evidence suggests that Mednick's study was confounded by selection factors in the Danish adoption process, and it is therefore unlikely that children with a criminal family background were placed into the same types of environments as children lacking such a history. Thus, Mednick and colleagues interpreted their findings as demonstrating the operation of genetic influences on criminality, when selective placement is a far more plausible explanation (Kamin, 1985, 1986).

Discussion

We have seen that the idea of criminality as a hereditary condition is an old one, whose "scientific" basis dates back at least to Lombroso. In the United States, the sterilization of criminals on eugenic grounds was permitted in several states. Long before the publication of Lange's German twin study, the idea of "crime as destiny" was widespread in the North American population, including especially its most educated layer. Studying the genetics of criminality and antisocial behavior fell into relative disfavor in the years after World War II, with the revelations of National Socialist genocide in the name of "racial hygiene."

Since the late 1960s, however, genetic theories for most human traits have made a comeback. So too have ideas about genetic factors in criminal behavior, although statements by the proponents of the genetic position are more cautious than for other behaviors, in part because of the commonly held view that social conditions such as racism, unemployment, and poverty contribute greatly to criminal behavior. Most genetic researchers also recognize that these and other environmental factors play a role. Why then is it necessary to study the genetics of criminality? According to Brennan, Mednick, and Gabielli,

We must try to identify the specific biological mechanisms through which heritable predispositions toward criminal behavior are expressed. By identifying these mechanisms we can learn how to successfully treat and prevent criminal behavior. (1991, p. 243)

The authors failed to discuss what types of treatment and preventive measures they had in mind, or to distinguish these from the "treatments" of previous generations. Perhaps Brennan and colleagues had in mind relatively benign measures such as early intervention programs (as discussed in Gabielli and Mednick, 1983), but they rarely mention the possibility of *improving the socioeconomic environment* as a way of eliminating crime which

— even if criminality is genetically predisposed — would still require an environment conducive to crime. Why change the person and not the environment? This is not a scientific but rather a political question.

The call by Brennan, Mednick, and associates for treatment is even more puzzling when we realize that one of the main findings of their adoption study was that *there is no genetic basis for violent crime*, which is the type of offense of greatest concern to the general public. Thus, they might have concluded that social and family conditions must be changed in order to reduce or eliminate violent crime. One might ask how there could be a genetic predisposition for property crime but not for violence. How can people be predisposed to steal but not to shoot; to write bad checks but not to rape? The explanation becomes clearer when the invalidating flaws of the research are exposed, which is the purpose of the present review.

But suppose that future researchers present compelling evidence in support of a genetic predisposition for criminal behavior. What significance would this have? One could argue that this would not be an important finding, because the proper environmental conditions would eliminate most criminal behavior (even among the genetically predisposed), just as the expression of a genetic disorder such as phenylketonuria (PKU) is prevented with the proper diet. While the practical significance of finding a genetic predisposition for any psychiatric condition or behavioral trait is open to debate (Joseph, 2000b), the finding of a genetic predisposition for criminal and antisocial behavior would accomplish little more than diverting society's attention from eliminating the environmental conditions leading to these behaviors.

Several authors have calculated or have attempted to popularize heritability estimates of criminal or antisocial behavior (e.g., Cloninger and Gottesman, 1987; Dinwiddie, 1996; Eley et al., 1999; Gjone and Stevenson, 1997; Hamer and Copeland, 1998; Jacobson and Rowe, 2000; Lykken, 1995; Mason and Frick, 1994). Aside from the fact that these estimates are derived largely from the flawed studies and methods discussed in the present review, heritability statistics for criminality and other traits are misleading. Contrary to popular belief, a heritability coefficient is not a "nature/nurture ratio" of the relative contributions of genes and environment (Hirsch, 1997); rather, it is an estimate of the variance of a trait in a population attributable to genetic factors.

Whether heritability is 10% or 90% says nothing about the efficacy of a particular environmental intervention; a heritability estimate is applicable only to a specific population in a specific environment at a specific point in time (Feldman and Lewontin, 1975; Plomin, DeFries, McClearn, and Rutter, 1997). The heritability statistic was developed in agriculture as a means of predicting the results of a program of selective breeding for economically desirable traits (Lush, 1945). Unfortunately, the invalid extension of the her-

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itability concept to human populations has led to a great deal of misunderstanding about the possible role of genes in shaping human psychological trait differences. Thus the question of “how much” of the variation of a phenotype (such as criminal or antisocial behavior) is attributable to genes is irrelevant because, like phenylketonuria, a simple environmental intervention could eliminate a condition with a heritability of 100%.

Conclusion

The title of a review article on genetic studies of criminality (Walters and White, 1989) asks whether the body of evidence records “bad genes or bad research?” The present review emphatically concludes that we have a body of bad research — not only because of the way the research was carried out, but because the methods used are hopelessly confounded by environmental factors. Quite erroneously, the twin method has been seen as a “natural approximation to a controlled experiment for estimating the genetic involvement in a trait” (Wilson and Herrnstein, 1985, pp. 90–91), and adoption studies have been promoted as “natural experiments in which the effects of genetic and rearing influences may be separated to a relatively high degree” (Mednick and Kandel, 1988, p. 103). On paper, an adoption study might appear to be just such a natural experiment. In the real world of selective placement, socioeconomic differences, and attachment disturbance, these studies are typically confounded by environmental factors — to a lesser degree than family and twin studies perhaps — but confounded nonetheless. For this reason, Cassou, Schiff, and Stewart (1980) referred to adoption studies by their proper designation: *the study of abandoned children*.

Goddard’s study of the “Kallikak” family traced two lines of the descendants of Martin Kallikak. The “good” line began with the progeny of Martin and his “good Quaker wife,” whereas the “defective strains” were produced by Martin’s liaison with a “feebleminded tavern girl.” For Goddard, a comparison of these two lines was pure science: “We have, as it were, a natural experiment with a normal branch with which to compare our defective side” (1912/1927, p. 68). Few people of Goddard’s day disagreed with the notion that family pedigrees (compiled by non-blinded investigators) were a true “natural experiment” of the influences of heredity and environment. One day, we will look upon twin studies and most adoption studies in much the same way as we now view the folly of Goddard’s logic.

In summary, the evidence from family, twin, and adoption studies does not support the existence of a genetic predisposition for any type of criminal, psychopathic, or antisocial behavior, however it may be defined at any given time or in any given society. The idea of a genetic predisposition for these behaviors, therefore, finds little basis of support.

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