LETTERS

Criminality and Adoption

Sarnoff A. Mednick, William F. Gabrielli, Jr., and Barry Hutchings conclude in their study of an adoption cohort (25 May 1984, p. 891) that "the findings imply" genetically transmitted "biological predispositions are involved in the etiology of at least some criminal behavior" (p. 893). While a study in which adoptees were placed randomly into adoptive households would be informative about the presence or absence of genetic transmission, such random placement never in fact occurs. The study by Mednick *et al.*, like others, is made uninterpretable by selective placement effects. These are easily demonstrated by reference to an earlier report by Hutchings and Mednick (*1*), based on 1145 of the 6129 adopted sons included in the more recent study.

The earlier report provides statistics (*1*, pp. 218 and 226) from which it can be calculated that 40.3 percent of criminal adoptive fathers, but only 29.5 percent of noncriminal adoptive fathers, received the children of criminal biological fathers as adoptees. Their more recent report does not provide the requisite statistics, but it seems likely that a similarly significant selective placement effect characterizes the enlarged sample. That is a predictable consequence of "an attempt by the adoption agency to match certain characteristics of the two sets of parents." The selective placement could have been accomplished by a more subtle process than crudely matching pairs of parents with overt criminal records. For in fully 63 percent of cases the first conviction of a biological parent did not occur until *after* the child had been adopted. Thus, the biological children of parents later to be convicted of crime could have been selectively placed into "criminogenic" adoptive homes. This of course would not have been a malign policy of adoption agencies, but an attempt to "fit" the particular child into an "appropriate" home.

The only reference to selective placement made by Mednick *et al.* involves a correlation in socioeconomic status (SES) between biological and adoptive parents. They stress, however, that "the relation between biological parent and adoptee criminal convictions exists at each level of adoptive parent SES. . . . independent of SES, biological parent criminality is significantly related to adoptee criminality." That fact, however, may merely reflect the crudeness of a seven-point SES scale relative to the subtlety of adoption agency decisions that control placement. That is, even at fixed levels of adoptive parent SES, social workers may place the children of present and future criminals into more criminogenic adoptive homes.

There is in fact clear evidence that *adoptive* parent criminality can be significantly related to adoptee criminality, independent of measured SES. The earlier report by Hutchings and Mednick (*1*, p. 218) included a comparison of 143 criminal adoptees to 143 noncriminal control adoptees. The control adoptees had been deliberately matched to the criminals on the basis of adoptive fathers' SES. Despite this, adoptive fathers of the criminal adoptees were more than twice as likely to be criminals than were adoptive fathers of the controls (*1*, p. 220). Recall that biological children of criminals had been selectively placed with criminal adoptive fathers; clearly, a correlation between biological parent and adoptee criminality could have been the result of such placement.

The results from the earlier reported partial sample, however, differ from those more recently reported for the full sample. For the partial sample, there was "an association between the criminality of the sons and their fathers . . . on both the biological and adoptive fathers' sides" (*1*, p. 218). For the full sample, "Adoptive parent criminality was not found to be associated with a statistically significant increment in the son's criminality, but the effect of biological parent criminality was." Mednick *et al.* do not refer to the earlier report and do not explain why adoptive parent and adoptee criminality were associated in the partial, but not the full, sample. We can note that, among other differences, the partial sample was based entirely on the adoption file for the Copenhagen area. The full sample added to this an adoption file for the rest of Denmark.

From data presented in the two reports, it can be calculated that the criminality rate among adoptive fathers in the partial sample was more than twice that among the subsequently added adoptive fathers. However, criminality rates among adoptees in the partial and full samples were similar. Thus, as the sample expanded, the association between adoptive parent and adoptee criminality diminished. That does not mean, however, that among cases subsequently added to the partial sample criminal and noncriminal adoptees had been placed into equally criminogenic adoptive homes. Mednick *et al.* note that "simply knowing that an adoptive parent has been convicted of a crime does not reveal how criminogenic the adoptee's environment has been." Similarly, knowing that an adoptive parent has *not* been convicted does not indicate that the home provided by that parent is not criminogenic. Although adoptive parents with criminal records were rare outside of Copenhagen, the placement process could still have assigned children of criminal biological parents to more criminogenic adoptive homes (*2*).

By ignoring selective placement effects, Mednick *et al* appear to have been able to interpret correlations between biological parent and adoptee as "genetic," even though such correlations are confounded with, and may be entirely mediated by, correlations between biological and adoptive parents, and between adoptive parents and adoptees. Regrettably, even before the issue of *Science* publishing their report appeared, national newspapers were quoting the incautious "genetic" interpretation of Mednick *et al.*

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References and Notes

1. B. Hutchings and S. A. Mednick, in *Genetic Research in Psychiatry*, R. R. Fieve, D. Rosenthal, H. Brill, Eds. (Johns Hopkins Univ. Press, Baltimore, 1975), p. 215.
2. This does not imply that the placement process outside Copenhagen used different criteria to assess the fit of an adoptive home to a particular child. Rather, the correlation between such environmental criteria and overt criminal record of adoptive parent could have differed between regions. (The same logic would apply if some factor other than region is responsible for the different criminality rates of adoptive parents in the partial and full samples.)

It is my reasoned opinion that Mednick *et al.* reach conclusions that cannot be trusted because of deficiencies in statistical analysis and overgeneralization from the data.

First, on page 893 the authors report an important finding of statistical significance based on stepwise multiple regression. They do not tell us how significance was assessed, but we do know that typical printouts carry "*P* values" that are believable in certain well-defined circumstances, which, however, do *not* include stepwise choice of regressors. Thus the authors' significance statement cannot be accepted at face value; if they performed some valid simulations to assess significance, there is no account of those in the report.

Second, much of their argument rests on concordance between sibling or half-sibling adoptees placed in different

homes. Three bodies of data are offered here. Tables 1 and 2 report on different children; Table 3 has data that overlap the first two.

Table 1. Male-male half-sibling pairs placed in different homes.

	Number convicted			Number of pairs
	Neither	One	Both	
Observed	95	27	4	126
Expected	93.43	30.14	2.43	126

$$\hat{P} = \frac{27 + 2 \times 4}{2 \times 126} = \frac{35}{252} = .139 \quad \chi^2\,(1) = 1.37$$

Table 2. Male-male full-sibling pairs placed in different homes.

	Number convicted			Number of pairs
	Neither	One	Both	
Observed	25	12	3	40
Expected	24.02	13.95	2.02	40

$$\hat{P} = \frac{12 + 2 \times 3}{2 \times 40} = \frac{18}{80} = .225 \quad \chi^2\,(1) = .79$$

Table 3. Male-male separately placed offspring of biological fathers with one or more convictions.

	Number convicted			Number of pairs
	Neither	One	Both	
Observed	32	9	4	45
Expected	29.61	6.89	1.61	45

$$\hat{P} = \frac{9 + 2 \times 4}{2 \times 45} = \frac{17}{90} = .189 \quad \chi^2\,(1) = 4.39$$

Standard methods (widely used in genetics, incidentally) produce the estimates of P, the probability of conviction hypothesizing independence between pairs, and also the chi-square goodness-of-fit statistic for assessing that hypothesis. Neither Table 1 nor Table 2 gives reason to question independence. Table 3 does produce a χ^2 (1) equal to 4.39; this corresponds to a P value of slightly less than 0.04 and might be regarded as significant if, apparently contrary to fact, no problems of multiple significance tests needed to be taken into account. But even if the statistic were unambiguously significant, it would not necessarily indicate a genetic factor. Any other correlate of the biological father that is also connected to the probability of offspring's conviction could suffice to produce such an effect. An example shows the idea. Suppose that both offspring of fathers from low-conviction neighborhoods were differentially placed for adoption into low-conviction neighborhoods, and that both offspring of fathers from high-conviction neighborhoods were placed into high-conviction neighborhoods. Then perfect independence in each stratum would be reflected in rows 1 and 2 of Table 4, but the *combined* (pooled) data produce the expectations in the last line with a strong fallacious indication of "genetic" effect.

Table 4. Pooling data from strata with perfect independence.

	Number convicted			Number of pairs
	Neither	One	Both	
Neighborhood conviction rate = .1	180	18	2	200
Neighborhood conviction rate = .3	98	84	18	200
Pooled data	278	102	20	400
Expected values for pooled data	270.6	116.8	12.6	400

$$\hat{P} = \frac{102 + 2 \times 20}{2 \times 400} = .1775 \quad \chi^2\,(1) = 12.8$$

The authors examine the data separately by number of convictions of adoptive parent, but this would be only a partial control for the factor just described.

It is likely that conviction rates changed appreciably between 1927, when the adoptions started, and 1980 or so, when the convictions stopped entering the data. If this is true, then historical changes in conviction rates may masquerade as other phenomena. For example, a strong time trend of decreasing conviction rates in the population would produce high rates for biological fathers, who were early in the series, and also (although less markedly) for their early sons; late biological fathers, and sons, would both tend to have low rates. With no other forces operating, then, a correlation, or trend, between "criminality" of biological father and sons would be seen. It might be supposed that this could be ruled out, since the same reasoning should produce a similar artifactual correlation between adoptive father and adoptee; but the exclusion of persons with convictions in the previous 5 years (footnote 5 of the report by Mednick *et al.*) would seriously diminish the expression of this artifact for adoptive fathers. The authors regrettably give no indication of the influences of time trends, although they suggest the presence of them in their text at the point where they offer footnote 3.

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Kamin expresses concern about selective placement, that is, matching of the biological and adoptive parents on certain conviction-related dimensions. This is an issue that has engaged the attention of all adoption researchers. Indeed, in this and in other adoption studies, selective placement usually *has* been noted. Teasdale (*1*) reported a correlation of .15 and Ho, Plomin, and DeFries (*2*) reported a correlation of .13 for occupationally defined socioeconomic status (SES) between biological and adoptive parents in Colorado. These correlations are statistically significant but small. When we have accounted for them (for example, by considering only a limited range of SES or by hierarchical regression techniques), the relations between biological parent and adoptee convictions remain unchanged.

Kamin conjectures that perhaps the adoption agency selectively placed adoptees according to indicators that are described as "subtle." He then hypothesizes that "the biological children of parents who would later be convicted of crime could have been selectively placed into criminogenic adoptive homes." This hypothesis requires that an employee of the adoption agency make a judgement regarding the biological parents that (unwittingly) would select persons who would have *future* court convictions. Since the male biological parent is responsible for the lion's share of the court convictions, and since the contact of this parent with the adoption agency is, in most cases, nonexistent, the adoption agency employee must be sensitive to extremely minute and often almost nonexistent signs. Kamin's hypothesis would also require the adoption agency employee to be able to identify characteristics of adoptive parents that are relevant to their adoption concerns and would be related to *future* criminogenic child-rearing practices. The employee must be able to do this despite the fact that the adoptive parents most often are childless.

There are a number of things that speak against the likelihood of selective placement. (i) During the period in which these adoptions took place, interviews with the biological mother and the adoptive parents were each conducted by two different agency employees. Pairing of adoptees and adoptive parents were made from notes by understaffed personnel. (ii) Regarding the ability of the agency personnel to do the matching required by Kamin, note that a school of social work was not established until 1937; before that time the personnel were not

trained in psychology, sociology, or social work. (iii) A large portion of these adoptions took place in the 1930's during the Great Depression. There was considerable press on the agency to place children, since prospective adoptees were in oversupply, and willing adoptive homes were far less available. Our conversations with individuals who worked for this agency during these years revealed that they made some effort at selective placement on the basis of hair and eye color and height and would have preferred to have been able to match for social characteristics; but they were more likely simply to be content to find a decent home for children who were otherwise slated to be raised in institutions. These factors speak against the possibility that selective placement played a significant role in explaining the relation between adoptee and biological parent convictions.

Kamin compares the data of our *Science* report with a pilot study we published in 1975 (*3*) on an urban-born portion of this same adoption population. He notes differences in rates of criminal registration between the subpopulation and the total population. However, he does not note that the pilot study data were based on *police* registration while the data in the *Science* report were drawn from a *court* conviction register. Kamin notes correctly that the pilot study (police data) reported a significant relation between male adoptees' and adoptive parents' criminal registration; this relation, while in the same direction, is not significant in the total population (in which court convictions were used). Kamin ascribes the differences in results to a complex form of selective placement. It seems to us more likely that the difference in results is a function of the source of the crime data. We use court convictions or police records as indicants of antisocial behavior. Both indicants are distorted as records of antisocial behavior. The police record, however, is closer to the behavior than the court record. Judgments and acts of defense attorneys, prosecutors, judges, and often juries interpose between police involvement and a record of court conviction. If there are genetically transmitted characteristics influencing antisocial behavior, then we would expect them to be more closely related to police than to court records. Indeed, as Kamin notes, the police data of the pilot study *do* show a significant relation between adoptee and adoptive parent registration, while the court records data are in the same direction but do not show a *statistically significant* relation. Our interpretation (degree of proximity to actual antisocial behavior) would be supported if we found a closer relation between the adoptee and *biological* parent for police records than for court records. This is, in fact, what we observe. (Compare table 8 of the pilot study with the last paragraph in the first column on page 892 of our *Science* report.) The relation between both adoptive and biological parents' crime and adoptees' crime is a bit better when derived from police, rather than court, records. It is thus highly unlikely that the biological parent-adoptee relation is influenced by selective placement.

In summary, we appreciate Kamin's comment but do not agree that selective placement explains our findings.

Moses raises four issues. First he asks about the results of our stepwise multiple regression analyses by which we assessed the independent contributions of convictions and SES of biological and adoptive parents to convictions in the adoptees. When we stated that we "varied the order of entry of biological parents' convictions and SES and adoptive parents' convictions and SES," we were apparently not clear enough. Stated another way, our analyses were hierarchical; *we* defined the stepwise choice of regressors. Significance was determined by the F value (and associated P value) that reflects the *improvement* in prediction with the inclusion of each additional regressor. The analyses could be thought of as testing the significance of a regressor (for example, the biological parents' convictions) with the residual of the regression of adoptive convictions upon the covariate regressors (that is, adoptive parents' convictions and SES and biological parents' SES). The results of these analyses indicate the strength of the independent contribution of the regressor of interest (biological parents' convictions) to the prediction of adoptees' convictions (appropriate adjustments for changes in degrees of freedom having been made).

In a small number of cases, we included siblings and half-siblings who were adopted and reared in separate homes. In three short paragraphs of our *Science* report, we pointed out that "the numbers are small," but the results of some interest. Moses concludes that "much of [our] argument rests on concordance between sibling or half-sibling adoptees placed in different homes." We believe the sibling analyses are, at best, of subsidiary interest and note with gratification that Moses does not find fault with our major analyses and results concerning the total population.

In our report, we observed concordance for convictions in 4 of 31 half-sibling pairs, 3 of 15 full-sibling pairs, and 4 of 13 half- and full-sibling pairs with convicted biological fathers. Moses presents "estimates of P" and also "the chi-squared goodness-of-fit statistic." For the critical concordance cells the expected frequencies are low: 2.43, 2.02, and 1.61. We believed they were too small for analytic statistics: simply by increasing the number of concordant pairs by two cases in Moses' tables 1 and 2, the chi-squared values become statistically significant. Moses reports one test as significantly favoring our hypothesis, and two as not significant but in the right direction. We would hesitate to impute excessive reliability to chi-squared tests involving such small expected values.

More interesting is Moses' demonstration, in his table 4, that concordance effects can be artifactually produced if pairs of siblings from low-conviction neighborhoods are adopted together into correspondingly low-conviction neighborhoods, while pairs of siblings from high-conviction neighborhoods are adopted together into high-conviction neighborhoods. (We should point out that there is an error in his table 4; the line "Neighborhood conviction rate" does not equal .1 but, according to the figures he gives, should actually be .055.) Teasdale and Owen, who identified the siblings in this adoption cohort, have examined this problem and state (*4*): "From the addresses of the adoptive parents at the time of adoption it appears that the median distance separating the homes of the reared-apart sibling pairs was 58 kilometers and only six pairs, all in the capital city, lived within four kilometers of each other." This should reassure the reader regarding the neighborhood question. If one uses the neighborhood as an example of any correlation in rearing experience for the reared-apart siblings, table 1 in a paper by Teasdale and Owen (*5*) offers further reassurance. They examine the intraclass correlations for these same sibling pairs for social class of their adoptive homes. For full siblings and half-siblings reared apart the intraclass correlation for the SES of the adoptive homes or for the adoptive fathers' income of the two members of the sibling pairs equals zero. Further, the "reared-apart siblings were not placed in their respective adoptive homes simultaneously (in 74 percent of cases the older of the pair had been adopted away before the birth of the younger), nor with any intention that contact between the siblings should be

established. We are informed by the then-director of the major adoption agency that adoptive parents were not told of the existence of any sibling to the child they had adopted" (*4*). It seems clear that there was no correlation in rearing SES, adoptive fathers' income, or neighborhood for the members of the sibling pairs. They were placed independently. Thus Moses' concern is without empirical basis.

Moses raises one final point: in analyses which stretch over several generations he fears that changes in conviction rates over the years might produce artifactual correlations between convictions of the adoptees and the biological and adoptive parents. It is also true that during this timespan this area of the world was faced with the Great Depression, World War II, and industrialization. It is conceivable that the influence of genetic factors might interact with or be affected by these social upheavals as well as the changes in conviction rates. We examined this possibility in an earlier publication (*6*). The analyses reported for the entire cohort were repeated for each of five shorter intervals: 1924–1928, 1929–1933, 1934–1938, 1939–1943, and 1944–1947. "The results were virtually identical for all of these periods and virtually identical to the analyses of the total sample. The changes across these years did not interact with the relationships between biological parent and adoptee crime" (*6*).

As in any area of science, this research project does not stand or fall alone. What is ultimately most important about a research result is its replicability. There is a considerable literature on the genetics of antisocial behavior (*7*).

1) Eleven twin studies from 1929 to the present have uniformly shown much higher rates of concordance for convictions or arrests for identical than for fraternal twins. Christiansen, in a study of a total population of twins (n = 3586 pairs), found 52 percent concordance for criminal convictions for identical (male-male) pairs and 22 percent concordance for (male-male) fraternal twin pairs (*8*).

2) Two U.S. adoption studies show concordance between crime in biological parents and crime in their adopted-away offspring (*9*). An investigation of crime in a major Swedish adoptee cohort (*10*) yields findings that agree with ours in just about every detail. These authors point out that "there are no genes for criminality, but only genes coding for structural proteins and enzymes that influence metabolic, hormonal, and other physiological processes, which may indirectly modify the risk of 'criminal' behavior in particular environments."

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References and Notes

1. T. W. Teasdale, *Behav. Genet.* **9**, 103 (1979).
2. H. Ho, R. Plomin, J. C. DeFries, *Soc. Biol.* **26**, 1 (1979).
3. B. Hutchings and S. A. Mednick, in *Genetic Research in Psychiatry*, R. R. Fieve, D. Rosenthal, H. Brill, Eds. (Johns Hopkins Univ. Press, Baltimore, 1975).
4. T. W. Teasdale and D. R. Owen, personal communication.
5. T. W. Teasdale and D. R. Owen, *Nature (London)* **309**, 620 (1984).
6. S. A. Mednick, W. F. Gabrielli Jr., B. Hutchings, in *Prospective Studies of Crime and Delinquency*, K. T. Van Dusen and S. A. Mednick, Eds. (Kluwer-Nijhoff, Hingham, Mass., 1983). (Also, there are two typographical errors in our *Science* report; the adoptions began in 1924, not 1927. In the third column of page 893, the number 15.6 percent should have been 19.8 percent, and 16.1 percent should have been 20.4 percent.)
7. S. A. Mednick *et al.*, in *Human Development: An Interactional Perspective*, D. Magnusson and V. Allen, Eds. (Academic Press, New York, 1983).
8. K. O. Christiansen, in *Biosocial Bases of Criminal Behavior*, S. A. Mednick and K. O. Christiansen, Eds. (Gardner, New York, 1977).
9. R. Crowe, *Arch. Gen. Psychiatr.* **27**, 600 (1972); R. J. Cadoret, *ibid.* **35**, 176 (1978).
10. M. Bohman, C. R. Cloninger, S. Sigvardsson, A.-L. Von Knorring, *ibid.* **39**, 1233 (1982).

Jumping Frog Genes

In the article "Frog genes jump species" (Research News, 23 Nov., p. 955), Roger Lewin describes the second and more likely origin of the *Rana ridibunda* individuals with mitochondrial DNA of an *R. lessonae* type as being a cross between an *R. lessonae* female and an *R. ridibunda* male. However, this cross would produce an *R. esculenta* hybrid, as described previously in the article. The original authors refer (*1*) to a cross between an *R. esculenta* female and an *R. ridibunda* male.

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References

1. C. Spolsky and T. Uzzell, *Proc. Natl. Acad. Sci. U.S.A.* **80**, 2290 (1983).

Erratum: In the report "Antibodies to peptides detect new hepatitis B antigen: Serological correlation with hepatocellular carcinoma" by A. M. Moriarty, H. Alexander, G. B. Thornton, and R. A. Lerner (25 Jan., p. 429), the legend of table 1 should have begun, "Reactivity of human serum samples with peptide 99 and peptide 142," not "Reactivity of human liver samples. . . ."