finds it strange that "the genetic interpretation of mental differences" has been viewed as a "counsel of despair," we recommend that he read the history of eugenics as applied to IQ in the early part of L. J. Kamin's book (*10*). Perhaps then he will see how much "educational and social advancement" has been achieved as a result of such counsel.

The intemperate tone of Morton's letter, in which he accuses us of cultivating "obscurity" and "clumsy harrying of biometrical genetics" is understandable, since he has spent so much of his own scientific energy in developing the methodologies that we question. Unfortunately his letter provides no substantive support for his polemic. Morton offers as his example of a case where genetic knowledge has improved risk prediction, of all things, hemophilia! But hemophilia is the result of a single recessive sex-linked mutation with complete penetrance. As we point out in our article, this is *precisely* the one situation in which genetic information is useful in predicting risks. The question is whether any genetic hypothesis *more complicated* than one or two Mendelian loci with high (Morton correctly points out our slip of the pen here) and constant penetrance, improves risk estimation. Rather than suggesting that those who are forced to use empirical risk calculations are "charlatans" and "quacks," Morton might have helped us by giving us the evidence that the complex pedigree analyses in which he engages have, *in fact*, improved the practice of genetic counseling. The absence of such evidence and the question of what constitutes first- or second-rate service to patients must remain open (*11*).

Morton claims that "flogging" broad heritability is unnecessary. He need only read any issue of *Behavior Genetics*, not to mention numerous textbooks on genetics and behavior. As to whether any geneticist supposes that the heritability of group differences can be predicted from intragroup heritability, he might try comparing notes with Plomin and DeFries, who also have a letter to the editor in this issue of *Science*. We agree that there was nothing in our article that any competent geneticist does not know. But *knowing* and *saying* appear to be two quite different things.

Genetic counseling has an important function in serving to avoid human suffering. We must not reject any knowledge that will make such counseling more accurate; but we must not pretend to knowledge that we do not have nor assume that very complicated and impenetrable mathematical formalities are necessarily closer to the truth by nature of their being farther from our understanding.

Plomin and DeFries make two points worth commenting on. First they offer the demonstration of heritability of schizophrenia as a counterexample to our claim that genetic analyses of "complexly determined behavior" are not useful. But they do not reveal what the use of this demonstration has been either in counseling or treatment. Perhaps it is their belief that the existence of such a heritability argues against psychotherapeutic treatment and in favor of some sort of physical intervention. The heart of our argument is that the existence of heritability is irrelevant to the possibility and form of therapy.

In their second point Plomin and DeFries persist in that incorrect claim that the formula connecting within-population and between-population heritabilities has some content. They seem to believe that any formula involving two variables (h^2_B, h^2_W) provides them with a meaningful connection. For example, let the variance in amount of manure produced by bulls in Iowa be σ^2_B and the variance in the number of words in letters to the editor of *Science* be σ^2_S. We then form the ratio $B_S = \sigma^2_B/\sigma^2_S$. By a simple rearrangement we have $\sigma^2_B = B_S \sigma^2_S$. Have we really shown that there is some meaningful relationship? This argument is logically identical to that which connects h^2_B and h^2_W. That is, their ratio is used to define the intraclass correlation, and then each by an algebraic rearrangement, h^2_B, is made to appear as a function of h^2_W.

Frankel raises the entirely spurious issue of scientific freedom and openness of inquiry. He tells us that "No person has a right to legislate . . . social attitudes for others, much less for a whole scientific community" and that "Scientific advocates of eugenics have the same right . . . to express their views as do Feldman and Lewontin." But these are red herrings. Nowhere in our article do we "legislate" anything or speak about depriving anyone of the right to express any idea or view. What we have done is to point out that some "ideas" are incorrect, some even nonsense, and that scientific concepts have been *misused* and sometimes blatantly misrepresented for political ends. We reiterate that "in our opinion geneticists ought to dissociate themselves utterly from eugenics" for the reasons given in our article. Frankel implies that we wish to bury objective truth or prevent its discovery because we dislike or fear the social consequences. This is an often repeated error in discussions of genetics and race. We neither fear nor dislike any objective truth. What we fear and detest is the misuse of scientific concepts in order to justify misrepresentation of objective reality. The right to express views does not include the "right" to twist scientific concepts, the "right" to illogical reasoning, and the "right" to misrepresent data. On the contrary the community of scientific workers has the obligation to expose falsehood and to demonstrate the limitations that assumptions place on the applicability of conclusions.

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"Pregnancy Prevention"

Healey's letter (9 July, p. 98) suggests that the incidence of gonorrhea has declined more rapidly in Sweden than in Denmark because the Swedes refer to protectives devices by a shorter word. Not to be outdone by the Swedes, the Danes also use the word *kondom*. The Danish term *svangerskabsforebyggende middel* is a general one that also refers to IUD's, diaphrams, and pills. Furthermore, even though a purchaser would not ask for *kondoms* by the general term, it would be no more difficult for him to say than the equivalent, "pregnancy preventative," is for English-speaking people.

I am sorry Healey's theory does not hold water; it would be a great advance in medicine if diseases could be controlled by the introduction of new words into vocabularies.

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