Separated Twins: Data and Their Limits

Identical Twins Reared Apart. A Reanalysis. SUSAN L. FARBER. Basic Books, New York, 1981. xvi, 384 pp., illus. $26.50.

Six decades ago, a note (*1*) in the *Journal of Heredity* reported a brief case history of Jessie and Bessie, identical twin sisters separated at two weeks owing to maternal illness, adopted by unrelated families on the death of their mother at 8 months, and reared apart until age 18 years. A photograph accompanied the note to prove that "they still look alike," and the twins' own report suggested that "they still think as much alike as they look." Jessie commented that "an intelligence test would find our capacities very similar, and I surely would like to try the experiment if the opportunity presented itself." H. J. Muller, later a Nobel laureate, reported results of that experiment in 1925. Jessie was right: despite significant differences in their schooling, the twins' scores on two early IQ tests were virtually identical (*2*). Four years later, H. H. Newman, a Chicago biologist, reported three new cases of monozygotic twins reared apart (MZA's), and, with the lure of the World's Fair, he eventually located 19 pairs (*3*). A BBC telecast enabled the late James Shields to identify 44 pairs in England (*4*), and Juel-Nielsen added a dozen Danish cases in 1965 (*5*).

By 1973, 121 pairs of MZA's were in the literature, and these cases form the basis for Susan Farber's book. After elimination of cases with insufficient information or uncertain zygosity and those in which co-twins were not separated until age 4 or later, 95 cases remain for reanalysis. Farber describes her book as "more a chronicle than anything else." And, as a chronicle, it is interesting and valuable. An international literature, much of it obscure, some of it out of print, has been collated with sensitivity and skill, important new material on several cases of psychiatric interest is presented, and some fascinating heuristic conclusions are drawn from cataloging MZA data on behavioral development and disease outcome.

But, as its subtitle reveals, Farber's book is not merely a chronicle. It is also a reanalysis of MZA data. And the reanalysis is highly speculative, often misleading, occasionally in error. Central to it is Farber's concern with a psychodynamic variable she calls "twinning." She suggests that the generalizability of conventional twin studies must be questioned because of experiences unique to identical twins reared together (MZT). MZA studies are necessary because, in Farber's psychodynamic theorizing, the psychology of twins differs from the psychology of singletons. This is an old issue and one that long ago was answered. The hypothesis that "having a partner who is an identical image of oneself may be peculiarly stressful for separation-individuation" predicts that MZ twins experience more than their share of personality problems. There is no evidence for that claim. The prevalence of personality problems in twins in general, and MZT twins in particular, is the same as that for singletons. Nor are twins distinguishable from nontwins on any measurable dimension of adult personality.

The test for all inferences drawn from twin data is whether they are corroborated in independent data sets of singleton subjects. For both personality and cognitive ability, extended-family data are fit by the same biometric models that fit twin data. Such findings provide no evidence whatever for the ubiquitous twinning phenomenon advanced by Farber. Yet she suggests that the effects MZ co-twins have on one another rival that of any social or perinatal variable. On what does this extraordinary claim rest? It derives from an impressionistic analysis of 47 MZA cases selected from the work of Shields, Juel-Nielsen, and Newman. Each case was assigned two three-point ratings, one, from the original investigator, for global similarity in personality, and the other, by Farber, for degree of social contact between co-twins from birth to time of interview. Farber reads into her analysis an inverse relationship between social contact and personality resemblance, such that MZA's with least contact turn out most similar. No statistical test of the association is made, but none is necessary: clearly, the differences reported are not significant. Were twins most similar when most separated, MZA's would be more alike than MZT's. They are not.

I can find no evidence for Farber's assertion that her analysis of MZA data "raises serious questions about using twins reared together as subjects for personality studies." On the contrary, I agree with Shields that analyses of MZA data vindicate assumptions necessary to conventional twin studies.

Farber concludes that "everything in these data points toward the massive and perhaps predominant influence of family and culture on attitudes and psychological traits." This reviewer finds nothing in these data pointing toward that influence. The most challenging fact from twin-family studies of the past quarter-century is that the results provide no evidence that shared environmental factors influence personality development. This is not a bias of the twin method itself, since the same analyses applied to IQ provide, indeed demand, that a significant portion of the variance be attributed to common environmental effects. Yet, in both twin and family studies of personality, parallel effects are absent. Unrelated individuals reared together exhibit significant resemblance in verbal ability, none in personality. In vocabulary tests, the resemblance of MZA's is significantly less than that of MZT's; in personality tests, they do not differ.

The fundamental rationale for adoption studies, of which the study of MZA's is one illustration, is to test environmental effects on behavior development or disease outcome. Because all differences between MZ co-twins must, by definition, be nongenetic, pairs that are separated early and reared in different environments provide a measure of sensitivity to environmental effects. It is co-twin differences rather than similarities that are, or ought to be, of central interest. Muller (*1*) so presented the first MZA pair, and Slater introduced Shields's work as "a controlled experiment set up by nature to test the effects of the environment." A recent report from Finland (*6*) suggests the potential power of this "controlled experiment." Nearly 500 pairs of separated twins have been identified from a national twin registry and graded according to degree and timing of separation; comparisons with appropriate control groups will permit sensitive appraisal of the influence and timing of environmental differences on identical genotypes.

Regrettably, this focus is lost in Farber's analysis. Similarities observed in small samples of MZA's, biased by self-selection and demonstrably unrepresentative of the general population, are used to estimate heritabilities of metric traits. In one of many misapplications of

MZA data, Farber presents a crude analysis of blood pressure levels on 15 sets of MZA's and concludes that blood pressure is strongly influenced by heredity. At best, that conclusion merely reflects what is well established from very much larger, more representative, and more informative sets of twin, family, and adoption data. In principle, separated identical twins permit a direct estimate of heritability; in practice, MZA data are so limited that, as Farber documents, any generalization is suspect. These rare cases generate many hypotheses but rarely if ever confirm them.

Similarities in reunited MZA's have been dramatized in the popular media and in a recent account in this journal (*7*). Such drama makes good show biz but uncertain science. Resemblances of MZA's in habits, interests, and likes and dislikes are described as "provocative," "astonishing," "uncanny," and "astounding." Are they? A colleague (*8*) suggests that we cannot know without necessary control data on similarities found in pairs of age-matched strangers, and he has observed many similarities in idiosyncrasies of everyday life among paired college students brought together to discover their resemblances. Were one to capitalize on cohort effects by sampling unrelated but age-matched pairs, born, say, over a half-century period, the observed similarities in interests, habits, and attitudes might, indeed, be "astonishing."

Two analyses highlighted by Farber may represent such cohort effects. The first deals with dental similarity, the second with similarity of menstrual functioning and distress. Farber recognizes probable cohort effects in her analyses of similarity in dental history and dental structure, suggesting that MZA's are not so much similar to each other as they are similar to people of their eras. But that argument is by no means limited to dental health. Cohort effects will be operative in interests, attitudes, and education, and Farber's box-score analysis of twin pairs born from the 1890's to the 1950's may be severely confounded with cohort effects.

The analysis of IQ is the central chapter of the book. It is also the most curious. The chapter begins with arguments that lead the author to conclude that pooling of the data is not legitimate, that no analyses are, in fact, justified. Yet, the chapter continues, less cautious investigators would not hesitate to pool data with less conservative tests for bias, and besides, these are the only MZA data available. And they *are* provocative! Accordingly, the author goes ahead and performs the analysis said to be illegitimate and ill advised. What then is new about this analysis of IQ in separated twins? No change in substantive conclusions from those reached by others is obtained. It is estimated that heritability is about 0.5 and that 20 to 25 percent of the variance in tested IQ is attributable to the effects of common environments. Analyses of conventional twin data and adoption studies suggest the same conclusion. The resemblance of unrelated individuals who have been reared together provides a direct estimate of the effects of common environment, and the correlation of such individuals is on the order of .20 to .25.

Farber's detailed analyses suggest a sex by degree-of-contact interaction, but the sample sizes for these analyses are vanishingly small. The author (with Noel Dunivant, who performed the statistical analyses) concludes, "In our opinion, these results suggest a complex pattern of environmental effects on IQ which have not been detected by previous investigators." That opinion is uninformed. Modification of the expression of a genotype by environmental experience is well documented, and the documentation comes not from MZA data but from extended twin-family data for which innovative path analytic solutions have been formulated. Resolution of genetic and environmental sources of behavioral variation can be made from conventional data with tools and techniques now being developed. It is not necessary to rely on the MZA data, which, by all accounts including Farber's, may not warrant generalization.

Susan Farber's chronicle of separated twins will introduce to a wide audience data of great intrinsic interest and potential value. One hopes that serious scientists in that audience will read the original sources (*1–5*) listed below.

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Claims for Newton

The Newtonian Revolution. With Illustrations of the Transformation of Scientific Ideas. I. BERNARD COHEN. Cambridge University Press, New York, 1981. xvi, 404 pp. $37.50.

The publication of Isaac Newton's *Principia* in 1687 inaugurated a new scientific era. To mention only some of its most notable achievements, Newton here introduced a rigorous concept of force, formulated the laws of motion, mathematically demonstrated the properties of the motions of bodies under a wide variety of forces, deduced the existence of universal gravitational attraction, and applied these results to explain a spectacular range of celestial and terrestrial phenomena. For more than two centuries this magisterial achievement, the so-called Newtonian synthesis, was taken as the model for the development of all the sciences, and mechanics reigned supreme. To historians of science this represents the Newtonian revolution.

In his latest book I. B. Cohen does not so much reject this view as move beyond it. Starting from 18th-century assessments of the Newtonian revolution, he locates the truly revolutionary feature of the *Principia* in what he calls the "Newtonian style." By "Newtonian style" Cohen means the successive development of mathematical models which are compared with experiment and observation and continually refined until a model corresponding sufficiently accurately with nature is obtained; at this stage the model is applied to explaining natural phenomena; and only then are physical causes examined. Since this is essentially a description of the practice of much of contemporary science, Cohen is in fact making the rather strong claim that to Newton is due not only the development of classical mechanics but also the very way in which we do much of our science.

Cohen's concept of Newtonian style will undoubtedly prove to be a useful one for exploring the development of Newton's science and that of his era. It brings to the fore the essential role of mathematics in Newton's physics, and it clearly separates Newton's mathematical-physical analyses from his mainly subsequent concern for the physical origins of force, thereby redressing the undue emphasis in the recent literature on Newton's speculations on the various forces of nature. The concept also makes it clear that it was the *Principia*'s style and not just its content that served as a model for 18th-century science. Howev-