Letters

Burt's Data: Dorfman's Analysis

Rubin and Stigler (Letters, 21 Sept., p. 1204) suggest that I engaged in broad "rounding and collapsing" of Burt's data followed by inappropriate χ^2 tests in my original article "The Cyril Burt question: New findings" (29 Sept. 1978, p. 1177). In fact, I did not use that illogical paradigm in my article, and in my reply (20 Apr., p. 246) to their earlier letters (20 Apr., pp. 242 and 245), I did not follow rounding and collapsing with χ^2 tests, since that is obviously inappropriate (*1*). The reader will find that in my original article I *never rounded* before performing χ^2 tests on Burt's data and collapsed only the tails, which in all cases contained less than .3 percent of the total—a procedure in no way comparable to the broad collapsing and rounding performed by Rubin and Stigler in their demonstration. Moreover, collapsing tails with small theoretical probability is a rather standard procedure.

Rubin and Stigler further suggest that they used my techniques on a table of unquestioned integrity and that I would have concluded that the data were fabricated on the basis of their evidence. They did not use my techniques, and I would not have concluded that the Scottish tailor had fabricated his data. I would never consider such a hypothesis unless all qualitative and quantitative evidence converged on that conclusion—that is, only if the *prior* evidence of fraud were extremely strong. In fact, Hearnshaw's conclusions were already public before my letter was written (*2*). The only conclusion I would draw from their demonstration is that the Scottish data are a rather good example of a naturally occurring approximate bivariate normal distribution. This conclusion is consistent with the fact that Quetelet collected striking examples of distributions of human traits that conformed to his theory that the normal curve—or the Quetelet curve as it was once called—was a law of nature (*3*).

Here is how our techniques differ. Let us consider the regression equation. As I reported in my article and again in my reply, I inferred the equation for the relation between Burt's means from Conway's (an invented assistant) 1959 predictions (*4*). Conway predicted (*5*, pp. 6-7): " . . . assuming that the I.Q. of the parents in the professional class averaged about 130, we should expect the I.Q. of their children to average about 115. Similarly, if the intelligence of the 'unskilled workers' averaged about 90, then we should expect that of their children to average about 95." I predicted Burt's 1961 data with the unique straight line that satisfies Conway's 1959 predictions of (90, 95) and of (130, 115). I present Fig. 1 to remove any further ambiguity about my technique of predicting Burt's 1961 regression equation. In contrast, Rubin and Stigler's regression equation is merely *fit* to the Scottish data. Moreover, I did not round and collapse in any way before computing the reported correlation of .999. I did round the correlation to three significant figures, which is certainly a rather standard procedure.

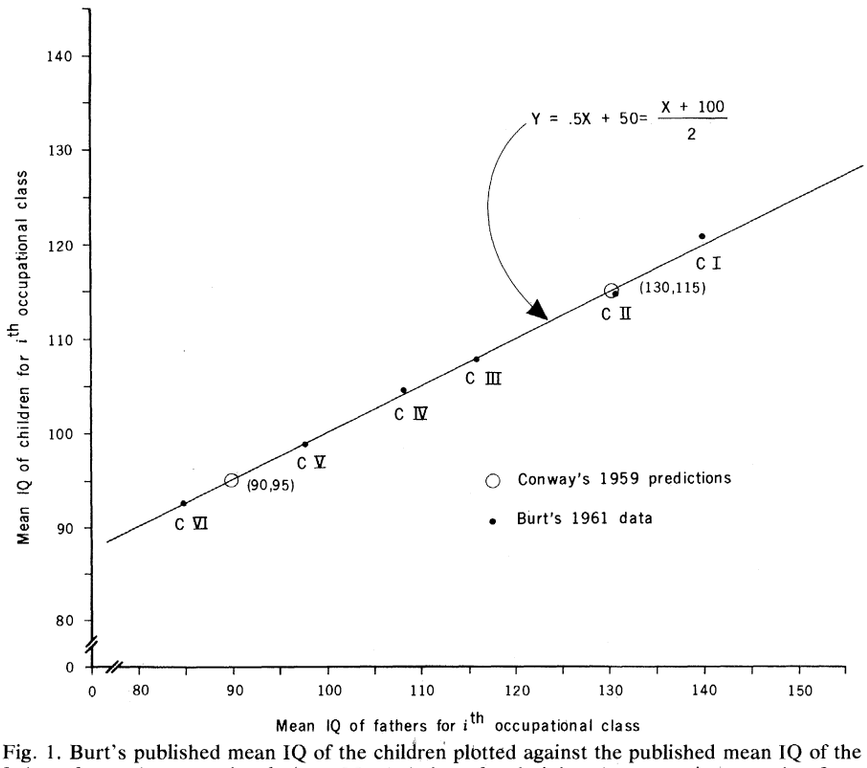

Fig. 1. Burt's published mean IQ of the children plotted against the published mean IQ of the fathers for each occupational class. My technique for deriving the regression equation from Conway's predictions (*5*) is also shown. No rounding and collapsing have been performed.

But most important, the proportion of the variance of the means of the children accounted for by the a priori equation

$$Y = {}^1/_2(X + 100)$$

is .997, as opposed to .992 for the proportion of the variance of the chest-circumference means accounted for by Rubin and Stigler's a posteriori regression equation.

In summary, my technique was in no way comparable to that of Rubin and Stigler. For it to be comparable, they would have to show that an invented assistant predicted the regression of chest-circumference on height for the 11 Scottish regiments in a report published about 2 years before the data were compiled.

I will now discuss the collapsed and rounded tables published in my earlier reply to Rubin and Stigler. I did not present those tables as evidence of fabrication. The *qualitative* and *quantitative* evidence for fabrication were presented in my original article and I stand by that article. The reader will find that, in my earlier reply, the "collapsing and rounding" were stimulated by the following assertion in Stigler's first letter (20 Apr., p. 245):

> Contrary to Dorfman's implication, it is not a simple matter to fill in either a 6 × 10 or a 6 × 6 table with predetermined marginal totals and get a plausible correlational pattern.

Thus, I accepted Stigler's challenge. Now I find that he meant that it would be "laborious" to do and that "published tables are ill-suited to this purpose." In fact, the tables of volumes of the normal bivariate surface given in Pearson (*5*) are well suited to this purpose, and they were issued by University College, London, where Burt had been professor. The collapsed and rounded tables were presented as informal support for my model of how he fabricated data, not for

whether he did so. The evidence for the fabrication was presented in my original article, which I ended thusly (p. 1184):

> The final judgment on Burt's honesty as a scientist will rest with future historians of behavioral science. It is hoped that the foregoing analyses will contribute to a fair and reasoned judgment in this matter.

Finally, I believe that Hearnshaw has provided us with a fair and reasoned judgment in this matter.

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References and Notes

1. Some other corrections to the letter from Rubin and Stigler are the following: (i) I did show that rescaling the IQ assessments to fit a normal curve and then reweighting along rows does not imply that the column totals will fit a normal curve; (ii) since Burt did not rescale his assessments of social class to fit a normal curve, there is no a priori reason why his tables should be "approximately multivariate normal." Moreover, marginal normals do not imply linear regression.
2. P. E. Vernon, *Intelligence: Heredity and Environment* (Freeman, San Francisco, 1979).
3. Indeed, Quetelet presented that distribution of Scottish chest-circumferences as a striking example of a normal curve in his Lettre XX of the work cited by Rubin and Stigler [A. Quetelet, *Lettres . . . sur la Théorie des Probabilités Appliquée aux Sciences, Morales et Politiques* (Bruxelles, 1846)] and fitted it to the normal curve. The fit was striking.
4. According to Hearnshaw, the Conway of 1959 was an invention of Burt's [L. S. Hearnshaw, *Cyril Burt: Psychologist* (Hodder and Stoughton, London, 1979)].
5. J. Conway, *Brit. J. Stat. Psychol.* **12**, 5 (1959).
6. K. Pearson, Ed., *Tables for Statisticians and Biometricians,* Part II, (Biometric Laboratory, University College, London, 1931).
7. I am deeply indebted to Roger Milkman for invaluable assistance and to Robert Hogg for some statistical assistance.

The Future of Education

In his editorial, "Education for the 21st century" (14 Sept., p. 1087), Philip H. Abelson addresses one of the most fundamental issues of our time—how to reshape and relate our educational system to the future condition and needs of American society and "somehow manage to avoid enormous trauma during the transitions that lie ahead." He singles out for discussion the threat of "a shortage of trained people [scientists and engineers] capable of meeting society's physical needs" and shows how the educational system suffers from mediocre performance "in counseling the young." He highlights various aspects of this particular educational problem and suggests appropriate solutions.

This is excellent as far as it goes; but the title of the editorial calls for much more. The tremendous changes that lie ahead for our society will be traumatic and, as Abelson says, "Education stands out as the best basis for hope. . . ."

In addition to the future physical needs of society that will require more