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ABSTRACT

In his book, "I.Q. in the Meritocracy," Richard J. Herrnstein (1973) calls on a classic article by Barbara S. Burks (1928) to support his position that a large part of the variation in intelligence can be accounted for by variation in heredity, as distinguished from variation in environment, and from covariation of heredity and environment. But Herrnstein's report of the Burks study is substantially inaccurate. In Chapter Four of his book, after reviewing other empirical evidence on heritability, Herrnstein turns to the Burks study. His presentation, pages 182-184, is reproduced in this document in its entirety. Burks' study focused on adoptive families. Herrnstein cites, in particular, the low correlations of children's IQs with their adoptive parents' IQs and with environmental variables as evidence that the role of environment is small. It is found in this document that some of Herrnstein's figures cannot be found in the Burks study, that her sample was extremely selective, that her environmental measures were limited, and that widely different estimates of heritability can be obtained from her data. Herrnstein's report cannot be taken at face value: to find out what the Burks study contains, it is necessary to read Burks. Further, the Burks study cannot support strong conclusions about the relative contributions of heredity and environment to the determination of intelligence. (Author/JM)

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MYSTERIES OF THE MERITOCRACY

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ABSTRACT

We critically examine three pages of Richard Herrnstein's book, I.Q. in the Meritocracy. Herrnstein uses Barbara Burks's 1928 study of adoptive families to support his position that intelligence is largely inherited. In particular, he cites the low correlations of children's IQs with their adoptive parents' IQs and with environmental variables as evidence that the role of environment is small. We find that some of Herrnstein's figures cannot be found in the Burks study, that her sample was extremely selective, that her environmental measures were limited, and that widely different estimates of heritability can be obtained from her data. We conclude that Herrnstein's report of the Burks study is substantially inaccurate.

MYSTERIES OF THE MERITOCRACY

Arthur S. Goldberger

Glendower: "I can call spirits from the vasty deep."

Hotspur: "Why, so can I, or so can any man; but will they come when you do call for them?"

— *Henry IV, First Part*.

1. INTRODUCTION

In his book, *I.Q. in the Meritocracy*, Richard J. Herrnstein (1973) calls on a classic article by Barbara S. Burks (1928) to support his position that a large part of the variation in intelligence can be accounted for by variation in heredity, as distinguished from variation in environment and from covariation of heredity and environment.

But Herrnstein's report of the Burks study is substantially inaccurate.

2. HERRNSTEIN'S REPORT

In Chapter 4 of his book, after reviewing other empirical evidence on heritability, Herrnstein turns to the Burks study. His presentation, pages 182-184, is reproduced below in its entirety. For ease of reference, I have italicized and numbered selected passages:

A still more persuasive case for the relative unimportance of home-genetic covariance can be found in a study published in 1928 by Barbara Burks of Stanford University. The study compared 214 foster children and their adoptive parents (the "experimental" group) to a carefully matched collection of 105 children being raised by their own parents (the "control" group). The control group was chosen to mimic the experimental group for the age and sex of the children, and for the locality, type of neighborhood, occupation, and ethnic characteristics of the family. Moreover, enough was known about the true fathers of the adopted children to show that there was little if any selective placement as regards fathers. There was no correlation between the occupational level or the cultural rating of the foster fathers and the occupational level of the true fathers. All the children in the experimental group were adopted before the age of twelve months and more than 60 per cent of them did not know they were adopted at the time of the study. The distribution of intelligence-test scores covered about the same range, with close to the same average, for the foster parents and the control

(1) parents raising their own children. *To the extent possible for naturalistic studies of human beings, Burks succeeded in crossing a broad range of genetic endowments with a broad range of home environments.* If covariance were crucial, the study would have shown it.

First, in keeping with the studies of foster
(2) children summarized earlier, *the foster children's I.Q.'s correlated with their natural parents' I.Q.'s more than with their foster parents'.* Even though the natural parents and the foster parents were uncorrelated as regards cultural or
(3) social-class characteristics, *the true father-child or true mother-child correlations were in the .5 range.* In contrast, the foster father-child correlation was essentially zero, while the foster mother-child correlation was about .2.

(4) *The control-group correlations, for parents raising their natural children, were only slightly higher than the true parent-child correlations in the experimental group, comprising adopted children.* The study clearly and unequivocally showed that the home environment, when disentangled from the genetic connection between ordinary parents and their children, accounts for relatively little of the variation in children's I.Q.'s.

(5) *Subjecting Burks's results to the statistical procedures of quantitative genetics yields an estimate of heritability in the familiar .8 range,* even though the design of her study carefully eliminated the covariance of genetic endowment and home environment. From this, and from other comparable results, it can be concluded that covariance of this variety accounts for little concerning the I.Q. in most circumstances. That, of course, is again not to say that in a radically different world, covariance might not be highly significant, or even that in certain limited instances in our own society, it is unimportant.

(6) *From her analysis, Burks could properly say that "nearly 70 per cent of schoolchildren have an actual I.Q. within 6 to 9 points of that represented by their innate intelligence." But unusually good or unusually poor environments, so rare as to affect something less than 1 per cent of the total population, might be promoting or retarding the I.Q.'s of the people encountering them as much as 20 points.*

(7) *Burks's sample was drawn from the white, primarily native American or western European population living around San Francisco and Los*

(8) *Angeles. The families spanned all social classes, from those of unskilled laborers to successful*
(9) *professionals and businessmen. Nevertheless, in racial, ethnic, linguistic, and, no doubt, cultural terms, the study omitted significant parts of the vastly diversified American population.* Since heritability measures a population trait, it is quite possible that the estimates are off somewhat.
(10) *It could well be that there are sources of environmental variation left out of Burks's study*, or, for that matter, the other studies in the literature reveiwed here. Including them might reduce the heritability estimate. Much of Jinks and Fulker's [1970] analysis is, for example, based on data collected in England by Cyril Burt. If one assumes that the intellectual environment in England is more homogeneous than in America, then Burt's data will set too high a value on heritability for the American population.

These uncertainties inhere in any population statistic--birth and mortality rates, crime rates, and so on--not just in the estimation of the genetic contribution to tested intelligence. Population statistics are not like the timeless constants of physical science, fixed by properties somehow inherent in nature. They are, rather, more like the actuarial data of the insurance business--more or less approximate, contingent, and, above all, changeable features of populations. Both insurance companies and quantitative geneticists are well advised to keep taking soundings.

The fact that a number of independent studies point to a particular narrow range of values for the heritability of I.Q. suggests a robustness to the estimate that should not be overlooked. However, it should not be overinterpreted either, for, while independent, the studies may nevertheless
(11) share common methodological weaknesses. *For example, the poorest, most culturally deprived sectors of the population tend to be omitted, or at least underrepresented, in most assessments of heritability.* If those unfortunate people happen to show most fully the impact of environment, their omission from the population sampled raises the heritability.

3. A MAJOR MYSTERY

In passages (2), (3), and (4), Herrnstein refers to the correlation between the IQs of the foster (= adopted) children and the IQs of their natural (= true) parents. In (2), he says that it is larger than the correlation between the IQs of the foster children and the IQs of their foster parents. In (4), he says that it is less than the correlation between the IQs of own (= nonadopted) children and the IQs of their parents. And in (3), he says that it (or rather each component of it) is approximately one-half.

What makes these statements mysterious is the fact that the Burks study contains no information on the IQs of the natural parents of the foster children. Burks's research group did not meet these parents and did not test them, nor was their intelligence tested by anyone else.

Where then did Herrnstein's figures come from? Burks, on pages 314 and 316, gives .45 and .46 as the correlations of the IQ of "true child" with the IQ (strictly speaking, "mental age") of father and of mother respectively. These numbers are indeed in the .5 range and larger than the .07 and .19 reported for the foster-child--foster-father and the foster-child--foster-mother correlations on pages 313 and 315. However, the .45 and .46 clearly refer to the own, nonfoster, children, that is, to the control group. It seems that Herrnstein mistook these control group figures for foster group figures. Furthermore, on page 285, Burks gives .55 and .57 as the control group correlations, corrected for attenuation, of the IQ of child with the IQ of father and of mother respectively. These numbers are indeed slightly higher than .45 and .46, but of course they refer to the control group. It seems that Herrnstein also mistook a difference between corrected and uncorrected correlations in one group for a difference between the control and foster groups.

It is obvious that Herrnstein's report of Burks's findings on the resemblance between the intelligence of adopted children and the intelligence of their natural parents is untrue; Burks had no such findings.

4. ANOTHER MYSTERY

In items (1) and (8), Herrnstein suggests that Burks's sample was fairly representative of the United States population; this is qualified to some extent in (7), (9), (10), and (11).

Turning to Burks (p. 236), we find that the foster sample was confined to white, non-Jewish, English-speaking, adoptive couples, who were American, British, or north-European born, with husband and wife both alive and living together, resident in the San Francisco, Los Angeles, and San Diego areas.

Proceeding in Burks's article, on p. 267 we find that more than half of the adoptive fathers were professionals, business owners, or managers (while 2 percent of them were unskilled laborers), on p. 268 we learn that 83 percent of them were home-owners, and on p. 270 we find that one-third of the boys and one-half of the girls had private tutoring outside of school in "music, dancing, drawing, etc."

On the 25-point "Whittier index" of home quality, the foster families' average score was 23.3 points (p. 269).[1] In intelligence, the foster parents averaged a full standard deviation above the population at large (p. 305). As for "the total complex of environment," Burks's own conservative estimate was that the foster homes averaged somewhere between one-half and one standard deviation above the general population (p. 306).

Surely Burks's families were not representative of the population at large. It should come as no surprise that children were placed for adoption with families located in the upper socioeconomic brackets. What is mysterious is Herrnstein's decision to regard this sample as though it covered a broad range of environments, being merely limited with respect to racial, ethnic, linguistic, and cultural characteristics. His concession that some environmental variation may have been omitted and his hint that the very poorest groups were underrepresented, hardly do justice to the facts.

Since Burks's sample was so highly selective, the variation in environment must have been much less than in the population at large. If so, the explanatory power of environment in the sample will also have been limited, compared to that in the population.[2]

5. QUANTITATIVE GENETICS

Next, we turn to Herrnstein's item (5), which says that the statistical procedures of quantitative genetics applied to Burks's data yield an estimate of about .8 for heritability (= proportion of variation in intelligence accounted for by variation in heredity). He cites no source, but two possibilities suggest themselves.

The first is the Burks article itself. For the foster group, a multiple correlation of R = .42 was obtained when child's IQ was regressed on father's IQ, father's vocabulary score, mother's vocabulary score, and income (p. 287). Interpreting those explanatory variables as environmental measures, Burks takes the multiple R^2, namely .17 (= $.42^2$), to be the proportion of variance in child's IQ that is due to home environment. She then arbitrarily adds .05 or .10 to this to allow for "the possible 'random somatic effects of environment'" and, subtracting the total from 1, produces the conclusion that "probably, then, close to 75 or 80 percent of IQ variance is due to innate and heritable causes" (p. 304).

It is not clear why Herrnstein would feel that a regression of foster child's IQ on three test scores and income involves the statistical procedures of quantitative genetics. Be that as it may, it is not clear why we must accept that particular combination of variables as the relevant measure of home environment.[3]

The second possible source of Herrnstein's heritability estimate is a detailed analysis of Burks's data by the distinguished geneticist Sewall Wright. Wright (1931) works with five correlations drawn from Burks:

<u>Foster group</u>: $r_{CP} = .23$ $r_{CE} = .29$

<u>Control group</u>: $r_{CP} = .61$ $r_{CE} = .49$ $r_{EP} = .86$

Here C denotes the child's IQ, P denotes the midparental IQ, and E denotes "environment." For environment, Wright uses Burks's "Culture Index," a 25-point scale reflecting cultural characteristics of the parents.[4]

Wright introduces a variable H to represent the child's heredity, which is of course not directly observed. He develops a simple model in which the basic equation is a regression of C on H and E. He supposes that H is correlated with P and E in the control group but uncorrelated with them in the foster group. In this model, the set of observed correlations given above produces an estimate of .90 as the "path coefficient" (= standardized regression coefficient) running from H to C. And the square of this, namely .81, measures the proportion of the variation in IQ that is attributable to variation in heredity.

This calculation of Wright, then, may provide a basis for Herrnstein's statement that a heritability estimate in the .8 range results when Burks's data are subjected to "the statistical procedures of quantitative genetics." As such, it merits our attention.

As Wright clearly indicates, his model attributes to heredity H, which is not directly measured, all effects that cannot be attributed to measured environment E. If so, the heritability estimate may be sensitive to the choice of a measure for E. To see this, let us subject Wright's (1931, p. 160) formulas to the quantitative procedures of elementary algebra. We find that his estimate of the path coefficient, say p, running from H to C, is calculated as

$$p = \sqrt{1-q^2}\,\left(-qr + \sqrt{q^2r^2 + 1-2q^2}\right)/(1-2q^2),$$

where q and r are respectively the foster-group and control-group correlations of child's IQ with environment. Thus the estimate of p is completely determined by the two r_{CE}'s, and is quite independent of the r_{CP}'s and r_{EP}. The environmental measure used by Wright is the Culture Index, a single variable reflecting certain aspects of the parents' speech, education, interests, home library, and artistic taste: with that measure of E, we have q = .29, r = .49, and the formula above indeed gives p = .90.

But there is nothing sacred about the Culture Index as a measure of the environmental influences on intelligence. For example, we have already seen that Burks found a foster-group multiple correlation of .42 between C and a set of four environmental variables. As it happens, she found a control-group multiple correlation of .61 between C and a set of four environmental variables.[5] For illustrative purposes, we can take q = .42 and r = .61, instead of q = .29 and r = .49 as values for the correlations of child's IQ with environment. When the new values are inserted in the formula above, we find p = .82 instead of p = .90. That is, we get $p^2 = .68$ rather than $p^2 = .81$ as our estimate of heritability.[6]

It is hardly surprising to find that, in Wright's model, a more refined measure of environment leads to a lower estimate of heritability. After all, that model attributes to heredity all effects that are not attributable to measured environment. What is mysterious is that Herrnstein chose to cite only "the .8 range" for heritability.

What deepens the mystery is the fact that in the same brief article, Wright (1931) himself obtains a different estimate of heritability from Burks's data. The alternative comes from a different model. In this second, more elaborate, model, environment is still measured by the Culture Index alone, but the effects not attributable to measured environment are now allocated between G (additive genotype) and M (a residual that includes unmeasured environment, along with any non-additive genotype and interaction effects). The path coefficient running from G to C is estimated as .71; squaring this yields Wright's second estimate of heritability, namely .49.

To some extent, the reduced value arises because of the shift from broad to narrow heritability. But Wright does not explain it away in that manner. Rather (p. 162) he clearly states that the first estimate is intended as an upper bound, the second as a lower bound. On at least two subsequent occasions, in re-reporting his analysis of Burks's data, he emphasized the same point:

> [The first model is] doubtless too simple since heredity is represented as the only factor apart from the measured environment. Any estimates of the importance of hereditary variation will thus be maximum. ... [In the second model, we] attempt at obtaining a minimum estimate of heredity. ... The path coefficient for influence of hereditary variation lies between the limits +.71 (if dominance and epistasis are lacking) and + .90.
>
> — Wright (1934, pp. 185, 187, 188)

> The results are reasonable [for the first model] except that H undoubtedly includes more than heredity...
>
> - Wright (1954, p. 23).[7]

With all this in mind, it seems fair to conclude that Herrnstein's item (5) is not an accurate statement.

6. ENVIRONMENTAL EFFECTS

Now let us turn to Herrnstein's item (11), which purports to give the effects of environmental change upon intelligence, measured in IQ points. Here Herrnstein is accurately reporting these items from Burks's summary of her conclusions (pages 308-309):

> 3. Measurable environment one standard deviation above or below the mean of the population does not shift the IQ by more than 6 to 9 points above or below the value it would have had under normal environmental conditions. In other words, nearly 70 percent of school children have an actual IQ within 6 to 9 points of that represented by their innate intelligence.
>
> 4. The maximal contribution of the <u>best</u> home environment to intelligence is apparently about 20 IQ points, or less, and almost surely lies between 10 and 30 points. Conversely, the least cultured,

> least stimulating kind of American home environment may depress the IQ as much as 20 IQ points. But situations as extreme as either of these probably occur only once or twice in a thousand times in American communities.

Burks, in turn, was summarizing her calculations on pp. 306-308. Her basic estimate is that a standard-deviation change in environment will change IQ by 6 points. It was obtained as follows. The correlation of foster child's IQ with "environment," namely the previously reported multiple R of .42, was viewed as a standardized regression coefficient: a standard deviation change in environment produces a .42 standard deviation change in IQ. Multiplying this by the standard deviation of IQ, namely 15 points, yielded 6 points.

This 6-point figure was then <u>tripled</u> to give "about 20 points" as the change in IQ produced by a <u>three</u>-standard deviation change in environment, that is by a movement from an average environment to the "best" environment. Her higher estimates for the effects of one- and three-standard deviation changes in environment, namely 9 and 30 points respectively, were calculated in the same manner except that .62 was arbitrarily used instead of .42 for the IQ-environment correlation. Finally, "nearly 70 percent" and "once or twice in a thousand" are simply Burks's descriptions of the respective probabilities with which a normally distributed variable lies within one standard deviation, and beyond three standard deviations, of its mean.

Let us focus on Burks's basic estimate, namely that a standard-deviation improvement in environment would raise IQ by 6 points. In constructing this estimate she uses the environmental standard deviation in the sample, but her conclusion refers to the environmental standard deviation in the population. Her logic is invalid when the sample is systematically different from the population. In particular, if environmental variation is substantially less in the sample than it is in the population, Burks's method will lead to a substantial underestimate of the effect of environmental change upon IQ.

Recall the various respects in which the foster group is non-representative of the population at large, having been drawn from the upper reaches of the socioeconomic scale. The variation of environment within those upper brackets is no doubt less than the variation of environment over the population at large. Consequently the sample standard deviation of environment is no doubt less than the population standard deviation of environment. An environmental difference that is large when measured in sample standard deviations--and rare in the sample--may well be small when measured in population standard deviations- and common in the population.

To suggest orders of magnitude, we interpolate in a table given by Lord and Novick (1968, p. 141). If we select the top 38 percent of a normally distributed population, we get a group whose mean is one standard deviation above the population mean; the standard deviation within this group is 54 percent as large as that in the population. Recall Burks's guess that on "the total complex of environment," her sample may have averaged one standard deviation above the population average. Consequently, a fair guess is that the standard deviation of environment in her sample was about half as large as it was in the population at large. If so, we are free to double her estimates of the effects of environmental change.

To sum up this section: In item (11) Herrnstein accurately reports Burks's conclusions; what is puzzling is that he believes that her conclusions were properly drawn from her data.

7. REMARKS

What lessons are to be drawn from our critical reading of Herrnstein? First, his report cannot be taken at face value: to find out what the Burks study contains, it is necessary to read Burks, not Herrnstein. Second, the Burks study cannot support strong conclusions about the relative contributions of heredity and environment to the determination of intelligence.

Throughout the IQ controversy, the advocates of high heritability have, to a considerable extent, developed their case by reporting on several studies of adopted children along with several studies of separated identical twins: see Herrnstein (1973, Chapter 4) and Jensen (1972b, pp. 121-130, 307-326; 1973, Chapters 7 and 8). My own assessment is that those reports cannot be taken at face value, and moreover that the studies themselves cannot support strong conclusions.

But a thoughtful reader will hardly take _my_ word for it, and is advised to consult the original studies.[8] To assist in this detective work, Bronfenbrenner (1972) and Kamin (1974, Chapters 3-5) provide many helpful clues.

FOOTNOTES

[1]This "Whittier Scale for Home Grading" is the sum of scores on five 5-point items: necessities, neatness, size, parental conditions, and parental supervision. The respective mean scores (with standard deviations in parentheses) in the foster group were 4.7 (0.4), 4.5 (0.6), 4.7 (0.5), 4.8 (0.4), 4.7 (0.4). To convey the meaning of such averages we give the verbatim descriptions of the type of conditions that led to scores of 4 and 5 on each item:

Necessities

4 = income, salary and tips of head waiter in a large hotel. Clothing neat, well-kept, apparently made to last. Good table set. Half modern bungalow. Furniture good quality, plentiful. Wicker and reed chairs, piano, rugs, good pictures. Rather poor lighting from windows, but modern electric fixtures. Running water, modern sanitary conveniences. Rear porch bedroom, couch in living room.

5 = Architect, well-to-do. Well-dressed. Table ware indicates abundant food. Large modern bungalow, frame construction, well finished. Furniture fine quality, plentiful. Fine carpets, rugs and pictures. Modern conveniences, built-in cupboards, electric fixtures, plumbing.

Neatness

4 = Rooms clean, but dark, closed and stuffy most of the time. Furniture neatly arranged and kept in good order. Exterior cleanliness good. House somewhat in need of paint. Lawn well-kept. Considerable attention given to home when possible.

5 = Interior clean and sanitary. Furniture neatly arranged, good order. Yards and grounds clean, no outbuildings. House well-kept. Yard clipped close, small, neat garden. General neatness good. Considerable attention apparently given to care of home.

Size

4 = Seven rooms, all rather small. Two-story house. Rooms convenient, although small. Propositus [= foster child], three younger children, mother and step father.

5 = Seven rooms, two-story house. Good sized rooms. Plenty of room conveniently arranged. Two adults, father and mother, propositus and younger sister. Rather small front yard, good open porch. Large back yard as city yards go.

Parental Conditions

4 = Father a painter, in good health. Mother probably normal. Harmonious most of the time. Mother nags father some on account of irregular work. No separation. Father away at work during day.

5 = Father normal, has average success as a carpenter. Mother keeps home in fair condition. So far as known, there is harmony between the parents. Mother at home all of the time, father away at work most of day. (In practice we never assigned a rating as high as 5 to this item if either parent tested with a mental age below 12.0).

Parental Supervision

4 = Father apparently interested in welfare of boys. Fairly good control. Equally fair treatment as far as known. Father a colored preacher. Good habits and reputation.

5 = Parents interested in health, education and welfare of children. Kind and intelligent discipline. Complete fairness as far as known. Parents of good reputation and character, good example to children. Children kept at home evenings as a general rule.

The full frequency distributions were not given by Burks, but can be reconstructed approximately from the means and standard deviations. For example, to have a mean $\bar{x} = 4.7$ and a standard deviation $s = 0.4$ on a 5-point scale virtually requires that two-thirds of the families score 5, one-third score 4, and none score 1, 2, or 3. (To obtain this conclusion, let p_i = proportion of sample scoring i (i=1,...,5). Then use the equations $\Sigma_{i=1}^{5} p_i = 1$, $\Sigma_{i=1}^{5} ip_i = \bar{x}$, $\Sigma_{i=1}^{5} i^2 p_i = s^2 + \bar{x}^2$, in conjunction with $0 \leq p_i \leq 1$, to restrict the possible values of the p_i).

Actually Burks's team found that, for three items, the maximum value of 5 did not seem adequate. They extended the scales to a sixth point, described as follows: Necessities. 6 = Conspicuously superior to the level receiving 5 points. Seldom given to any home. Denotes unusually luxurious living conditions. Parental Conditions. 6 = Conspicuously superior to the level receiving 5 points. Both parents superior on the mental test, and exceptionally harmonious in their relations. Parental Supervision. 6 = Care given the children and provision made for their welfare very exceptional. On this extended scale, the item means changed to 4.9, 4.5, 4.7, 4.9, 4.9, and thus the Whittier index mean increased to 23.9 on a 28-point scale. From this, it can be deduced that in 10-20% of the cases, a score of 5 was raised to a score of 6.

Material in this footnote was drawn from pages 231-233, 269 of Burks; see also Kamin (1974, Chapter 5).

[2] Burks herself dealt with selectivity on pp. 222-223, saying that

> home environment cannot be expected to have as large a proportional effect upon the mental differences of the children we studied as though they were being reared in families unselected as to race or geographical location throughout the world.

She felt that the problem was not too severe, contending that

> The distribution of homes of the children studied in this investigation was probably nearly as variable in essential features as homes of the general American white population (though somewhat skewed toward a superior level).

Her contention of course runs counter to the many indications of markedly superior environments noted in the text; the only evidence offered to support it is the fact that the variation of children's IQs was as large in the samples as it was in the standard population. This fact is indeed difficult to explain. The difficulty is apparent if we take the position that environment is an important determinant of IQ, but it also arises if we, like Herrnstein, take a contrary position. For, a main line of argument in his book is that parental intelligence determines both the children's intelligence (via heredity) and their environment (via parental success, earnings, achievement). This implies that in the population, parents' socioeconomic status must be correlated with children's IQs, which in turn implies that if we sample only families with high socioeconomic status we should find reduced variation in children's IQs. But such a reduction does not appear in Burks's control families, although they were chosen to match the foster families, and thus had similarly superior environments: see Burks, pages 263-277.

[3]For readers familiar with simple, but not with multiple, correlation: The multiple correlation of a variable y with a set of variables $x_1,\ldots,x_K$ can be interpreted as the simple correlation of y with a certain variable z. This variable z is constructed as a linear combination of the x's, namely $z = b_1x_1 + \ldots + b_Kx_K$, where the b's are chosen to maximize the correlation with y. These b's are regression coefficients; when all variables are measured in standard-deviation units, the b's are called standardized regression coefficients.

Burks tried some other explanatory variables but did not include them in her final multiple regression. She explains on p. 287:

> To have gone through the operation of computing multiple correlations that utilized all nine of the variables would have been enormously time-consuming. To save labor, certain variables were eliminated, after first demonstrating, through multiples using three or four variables, that they contributed practically nothing to an estimate of the child's IQ not already contributed by variables retained for the final multiple. For example, in the foster multiple, income was retained, but Whittier and Culture indices were dropped out, because the multiple of IQ with all three together (.34) was only .01 higher than the correlation (.33) between

> IQ and income alone; again, mother's vocabulary was retained, but mother's mental age and mother's education were dropped out because the multiple of IQ with all three together (.254) was only .005 higher than the correlation (.249) between IQ and mother's vocabulary alone.

She was convinced that including all nine of the variables would not have raised the R by more than .02. But because she did not publish a full set of correlations, we cannot verify that.

[4]This "Culture Index" is the sum of scores on five 5-point items referring to the parents' speech, education, interests, home library, and artistic taste. The respective mean scores in the foster group were 3.5, 3.7, 3.2, 3.1, 3.2 for a total of 16.9; in the control group they were 3.4, 3.8, 3.1, 3.0, 3.0 for a total of 16.3. To suggest the meaning of these figures without going into the verbatim detail of footnote 1, I note the following: Speech is based on a vocabulary test; Education measures the average number of grades completed by the parents, with 1-3 grades scored 1, 4-6 grades scored 2, 7-9 grades scored 3, 10-12 grades scored 4, and more than 12 grades scored 5; Interests measures the quality of parent's hobbies and activities; Home Library measures the number of books in the home, with less than 10 books scored 1,..., more than 500 books scored 5. The content of the Artistic Taste component is perhaps best captured by some direct quotation:

Artistic Taste

2 = ... trashy ornaments, such as kewpies and gaudy bric-a-brac scattered about.

3 = ... the Victrola records are not of a high type... Photographs of the family are usually abundant.

4 = ... no trashy ornaments about, and family 'photos' are absent or present in very moderate numbers.

5 = ... musical selections for piano or Victrola are from standard composers (though a little popular music or jazz may be included as well)...

For Wright's purposes, the Culture Index was preferable to the Whittier Index as a single measure of environment because it gave larger correlations. He felt unable to use a combination of several variables to represent environment because the correlations among them were not published by Burks. (Note indeed that for the foster group, even the correlation of the Culture Index with midparental IQ is lacking).

The correlations involving "midparent" P were constructed by Wright from the separate correlations involving father and mother, by a standard procedure.

Material in this footnote was drawn from pages 234-235 and 269 of Burks, and from correspondence with Wright.

[5]The set of variables used in the control group was: father's IQ, father's vocabulary, mother's IQ, Whittier Index; see Burks (p. 287). This is not quite the same as the list for the foster group; see footnote 3.

[6]Perhaps the two values of heritability are not very different? Heritability of .8 is sometimes interpreted to say that heredity is four times (.8/.2 = 4) as important as environment. On that interpretation, heritability of .68 would say that heredity is only twice (.68/.32 = 2.1) as important as environment. Actually Jensen (1972a, p. 428) suggests that such comparisons be made in terms of square roots, and thus interprets heritability of .8 to say that heredity contributes twice ($2 = \sqrt{4}$) as much as environment to the actual differences in IQ. Following that line, heritability of .67 would say that heredity contributes one-and-two-fifths ($1.4 = \sqrt{2.1}$) as much as environment. One wonders whether the great IQ debate would have developed as it did had it begun with the assertion that heredity is somewhat less than 1 1/2 times as important as environment.

I believe that this arithmetic says something about the meaningless of "relative importance" rather than about the deceptiveness of statistics.

[7]Wright (1974, Chapter 19) has recently refined his analyses of Burks's data. Without purporting to do justice to this new set of computations, we cite the following conclusions: "The maximum amount due purely to heredity is thus a little less than 80%... The minimum estimate of heritability (that if heredity is wholly additive) comes out about 45%."

[8]Having already invested some time in Burks, the reader might wish to proceed by checking out the reports of her study that have been given by Jensen (1972b, pp. 128-130; 1973, pp. 196-197, 203-204, 240) and by Eysenck (1971, pp. 63-65).

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