Never a Dull Moment

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The preceding comments on "Intelligence: Knowns and Unknowns" (Neisser et al., February 1996) reflect a wide range of viewpoints, and each deserves a reply. Because it is hardly practical to reconvene the whole task force for this purpose, the buck stops with me. I consider the comments under three headings: (a) Naglieri, Yee, and Mahlberg deplore certain omissions; (b) Spitz, Velden, Melnick, and Ernhart and Hebben present alternative interpretations; and (c) Reed, Frumkin, Lynn, and Rushton comment on the difference between Black and White IQ means. Readers should be aware that this response reflects only my views, not necessarily those of other members of the task force.

Omissions

Naglieri (1997, this issue) regrets that Neisser et al.'s (1996) survey of alternatives to traditional intelligence testing failed to mention three recently developed tests: the Kaufman Assessment Battery for Children, the Woodcock–Johnson Test of Cognitive Abilities, and the Das–Naglieri Cognitive Assessment System. He is right; the report certainly should have mentioned at least the first two of these tests, which have been in the literature for some years. I am glad that Naglieri has taken this opportunity to call them to the attention of readers of the *American Psychologist.*

Yee (1997, this issue) thinks Neisser et al.'s (1996) report should have responded more directly to *The Bell Curve* (Herrnstein & Murray, 1994) and focused more strongly on race. But although our charge was indeed rather vague, it certainly was not to review a specific book—doing so merely would have added to the ongoing cacophony. From the beginning, we were determined to deal exclusively with scientific issues. Yee mocks this goal: "As if... [science and policy] are easily separated" (p. 70), but it was part of our purpose to show that such a separation is not only possible but fruitful. Readers can judge for themselves whether we succeeded. As for race, Yee has argued for some time (Yee, Fairchild, Weizmann, & Wyatt, 1993) that APA should set up a blue-ribbon commission to settle the race question once and for all, and naturally is disappointed that our task force chose not to play this role. I think that such an undertaking would be a disaster. Scientific task forces can succeed only when there is substantial consensus on at least a large subset of the issues. Because this is not the case where race is concerned, the formation of such a commission would probably just exacerbate existing tensions.

Neisser et al.'s (1996) report described the worldwide rise in IQ scores as "striking" but, contrary to Mahlberg (1997, this issue), by no means as "inexplicable." Several plausible explanations for the rise were explicitly discussed, and of course there may be others as well. The situation is hardly desperate enough to force us (or anyone) to turn to mystical–Platonic–Jungian concepts like "collective memory" or "morphic resonance." Although some may be intrigued by the bold sweep of these notions, which explain "memory of past lives" (Sheldrake, 1988, p. 221) as easily as the Flynn effect, I find it impossible to take them seriously.

Interpretations

Spitz (1997, this issue) questions Neisser et al.'s (1996) reliance on the Abecedarian results as evidence that early intervention can raise IQ scores, on the ground that the experimental and control groups in that study were already six points apart at six months of age. Interested readers may consult the extended exchange between Spitz and Ramey on this subject (Ramey, 1992, 1993; Spitz, 1992, 1993a, 1993b). For my part, I find nothing implausible in the notion that even a brief environmental enrichment could have effects in the first year of life—effects that would dissipate if they were not maintained by further intervention throughout the preschool years. In any case, it is probably a mistake to take the six-month data too seriously; "intelligence" tests given at that age have little or no predictive validity.

Velden (1997, this issue) argues that the heritability coefficient, h^2, is of little value because its denominator includes V_E, the variability in IQ scores due to the environment, and the value of V_E may vary widely from one population to another. Although Velden's premises are correct, he pushes his conclusions too far. The task force was aware of the limitations of h^2; Neisser et al. (1996) noted that "heritability does not imply immutability" (p. 86) and stressed that the factors that create individual differences within a population may be unrelated to those that establish differences between populations (p. 95). But the fact that h^2 is of little use in comparing populations surely does not imply that one should "doubt the usefulness of such coefficients altogether" (Velden, 1997, p. 72). New insights into the causes of individual differences should always be welcome, even if they imply a larger genetic contribution to those differences than some of us would have expected.

Melnick (1997, this issue) thinks Neisser et al. (1996) should have challenged certain "repeated methodological errors" that allegedly support the "hereditarian position" (p. 74). I deplore this polarization of hereditarian and environmentalist positions in a domain where both genes and environment are clearly important. In any case, the two "errors" described by Melnick do not seem like errors to me. The first concerns the high IQ correlations exhibited by monozygotic twins reared apart: Melnick says these could arise because the twins look alike and hence attract similar behaviors, which might help to shape their intelligence in similar ways. But, on the one hand, there is no evidence that behaviors elicited by a person's appearance can create substantial changes in that person's intelligence, and, on the other hand, there is much evidence independent of twin studies (data from siblings, adopted children, etc.) for a genetic contribution. Melnick's other claim—that Scholastic Aptitude Test scores may underpredict African Americans' college performance despite our conclusion to the contrary—is based entirely on studies from the 1960s and 1970s. More recent work (e.g., Linn, 1982) does not support Melnick's view; on the contrary, modest overprediction is the usual finding.

Ernhart and Hebben (1997, this issue) think Neisser et al. (1996) should not have described the negative effects of exposure to lead as "well-established." They have a point. Although the existence of such effects is now generally conceded, their magnitude fluctuates oddly from one study to the next and is still the subject of dispute. Two recent reviews (Bellinger, 1995; Pocock, Smith, & Baghurst, 1994) agree that even very substantial body "lead burdens" are often associated with only small IQ decrements (1–3 points). Whether this finding means that the public funds committed to lead abatement would be better spent in other ways, as Ernhart and Hebben suggest, is beyond the scope of our report.

Black–White Differences

Reed (1997, this issue) is critical of the studies by Loehlin, Vandenberg, and Osborne (1973) and Scarr, Pakstis, Katz, and Barker (1977), which Neisser et al. (1996) cited as providing evidence against genetic explanations of Black–White IQ differences. These studies found no significant correlations between the IQs of African Americans and their estimated degree of African ancestry. According to Reed, these authors should have used a different and more powerful genetic analysis, one that "*if* the genetic hypotheis is true, probably would have confirmed the genetic hypothesis" (p. 77). I am not qualified to evaluate Reed's comment, which might more appropriately have been submitted to a specialty journal in genetics. In any case, the task force did not claim that the studies by Loehlin et al. and Scarr et al. were decisive, only that they provided a certain amount of relevant evidence.

Frumkin (1997, this issue) mentions several older studies of regional differences and migration gains that showed that sociocultural factors were at least partly responsible for the lower IQ scores of African Americans. The fact that Neisser et al. (1996) cited only the last of those studies (Lee, 1951) does not mean that they undervalued this research tradition. Inadequate education and other environmental constraints certainly can have negative effects on test scores; the question is whether the present environment of Blacks is sufficiently worse than that of Whites to explain the difference between their IQ means. As Neisser et al. pointed out, this question has no scientific answer at present.

Both Lynn (1997, this issue) and Rushton (1997, this issue) dispute the task force's conclusion that there is no direct evidence for a genetic interpretation of the Black–White IQ difference. Lynn's succinct comment cites two lines of evidence that he finds particularly persuasive: (a) the Minnesota adoption study and its 10-year follow-up and (b) studies relating head or brain size to intelligence test scores. I respond to these two points in some detail and then comment briefly on other issues raised by Rushton.

The original Minnesota study (Scarr & Weinberg, 1976) included both the adopted and the biological children of 101 middle-class families (each with two White parents), tested at an average age of about 7 years. The mean IQ of the adopted Black children was 106.3, well below the 111.5 of the adopted White children and the 116.7 of the biological children but a full standard deviation above the expected IQ mean of Blacks in Minnesota. Adoptees with one Black and one White birth parent scored higher than those with two Black birth parents, but even the latter averaged 96.8. These and other findings led Scarr and Weinberg to conclude that "the social environment plays a dominant role in determining the average IQ level of Black children" (p. 739). But follow-up testing when the children were about 17 years of age had quite a different result: The mean IQ of the retested Black adoptees was only 96.8, and those with two Black birth parents averaged 89.4 (Weinberg, Scarr, & Waldman, 1992). That is why Lynn (1997) says, "Black babies adopted by White parents registered no IQ gains" (p. 73), a point he has elaborated elsewhere (Lynn, 1994).

As Waldman, Weinberg, and Scarr (1994) made clear in their response to Lynn (1994), this conclusion is misleading. Everyone involved in this debate is well-aware that such comparisons must be corrected for the Flynn effect: Mean scores on all standard IQ tests seem to rise steadily at about 0.3 points per year. In the Minnesota study, where the tests used in the follow-up were generally not the same as those that had been given the first time, these corrections are complex and must be made on an individual basis. Until they have been made—Waldman et al. reported that they are in progress—raw figures like those above are relatively meaningless.

A further complication is that race and preadoptive experience were strongly confounded in the Minnesota study (Scarr & Weinberg, 1976). At the time they joined their new families, for example, the Black adoptees had had more prior placements, rated of poorer quality, than their White counterparts. This was especially the case for the children with two Black birth parents, who were not adopted until they were, on average, about 32 months old. Because any later IQ differences between these groups may have resulted from differences in preadoptive experience, the Minnesota data provide no clear evidence for the genetic hypothesis. But it is only fair to say that they do argue against certain versions of the environmental hypothesis (pending the necessary Flynn effect corrections): The mere fact of growing up in a middle-class home apparently does not, by itself, raise one's score on intelligence tests given at adolescence.

Both Lynn (1997) and Rushton (1997) insist that racial differences in the mean measured sizes of skulls and brains (with East Asians having the largest, followed by Whites and then Blacks) support their genetic hypothesis. They rely on the averaged results of the many anthropometric studies reviewed by Rushton (1995) in his book *Race, Evolution, and Behavior*. Although those studies exhibit many internal inconsistencies (and the within-groups variabilities are always much larger than the between-groups differences), there is indeed a small overall trend in the direction they describe. Even taken at face value, however, such a trend hardly constitutes evidence for a genetic interpretation. It is already known that body size is strongly affected by environmental variables (most obviously by nutrition); Lynn himself (1990) has documented the remarkable increases in adult height that have occurred in a great many countries. Why shouldn't one expect those same environmental variables to affect head size too? Given the fact that even rats show gains in brain weight when they are exposed to enriched environments (Bennett, Rosenzweig, Diamond, & Hebert, 1974), it is hard to see how data on the sizes of heads or brains can have any strong implications for a genetic hypothesis.

I do not have the space or the stomach to reply to all the points raised by Rushton (1997). His claim that the task force (Neisser et al., 1996) "sidestepped" many important pieces of evidence reflects his own evaluation of that evidence, which is by no means the only possible assessment. (In the case of Asian and Asian American IQs, for example, Flynn's [1991] analysis seems more convincing than that of Lynn [1993]). It also reflects his own very peculiar perspective: For some reason, Rushton believes that ranking different racial groups on various criteria is a matter of the utmost importance. Happily, most of us do not share that priority. (For a general critique of Rushton's theory of racial differences, see Zuckerman & Brody, 1988.)

Overall, working on this project has been a rewarding and educational experience. This is primarily because of the constructive collaboration of my colleagues on the task force, but I am also encouraged by the positive tone of many of the present critiques. As of now, the study of intelligence looks promising; perhaps some other task force in the not-too-distant future will be able to report more definite answers and leave fewer unresolved questions than we did.

REFERENCES

Bellinger, D. C. (1995). Interpreting the literature on lead and child development: The neglected role of the "experimental system." *Neurotoxicology and Teratology, 17,* 201–212.

Bennett, E. L., Rosenzweig, M. R., Diamond, M. C., & Hebert, M. (1974). Effects of successive environments on brain measures. *Physiology and Behavior, 12,* 621–631.

Ernhart, C. B., & Hebben, N. (1997). Intelligence and lead: The "known" is not known. *American Psychologist, 52,* 74.

Flynn, J. R. (1991). *Asian-Americans: Achievement beyond IQ.* Hillsdale, NJ: Erlbaum.

Frumkin, R. M. (1997). Significant neglected sociocultural and physical factors affecting intelligence. *American Psychologist, 52,* 76–77.

Herrnstein, R. J., & Murray, C. (1994). *The bell curve: Intelligence and class structure in American life.* New York: Free Press.

Lee, E. S. (1951). Negro intelligence and selective migration: A Philadelphia test of the Klineberg hypothesis. *American Sociological Review, 16,* 227–232.

Linn, R. L. (1982). Ability testing: Individual differences, prediction, and differential prediction. In A. K. Wigdor & W. R. Garner (Eds.), *Ability testing: Uses, consequences,* *and controversies* (Part II, pp. 335–388). Washington, DC: National Academy Press.

Loehlin, J. C., Vandenberg, S. G., & Osborne, R. T. (1973). Blood group genes and Negro–White ability differences. *Behavior Genetics, 3,* 263–270.

Lynn, R. (1990). The role of nutrition in secular increases in intelligence. *Personality and Individual Differences, 11,* 273–285.

Lynn, R. (1993). Oriental Americans: Their IQ, educational attainment, and socioeconomic status. *Personality and Individual Differences, 15,* 237–242.

Lynn, R. (1994). Some reinterpretations of the Minnesota Transracial Adoption Study. *Intelligence, 19,* 21–27.

Lynn, R. (1997). Direct evidence for a genetic basis for Black–White differences in IQ. *American Psychologist, 52,* 73–74.

Mahlberg, A. (1997). The rise in IQ scores. *American Psychologist, 52,* 71.

Melnick, M. (1997). Methodological errors in the prediction of ability. *American Psychologist, 52,* 74–75.

Naglieri, J. A. (1997). IQ: Knowns and unknowns, hits and misses. *American Psychologist, 52,* 75–76.

Neisser, U., Boodoo, G., Bouchard, T. J., Jr., Boykin, A. W., Brody, N., Ceci, S. J., Halpern, D. F., Loehlin, J. C., Perloff, R., Sternberg, R. J., & Urbina, S. (1996). Intelligence: Knowns and unknowns. *American Psychologist, 51,* 77–101.

Pocock, S. J., Smith, M., & Baghurst, P. (1994). Environmental lead and children's intelligence. *British Medical Journal, 309,* 1189–1197.

Ramey, C. T. (1992). High-risk children and IQ: Altering intergenerational patterns. *Intelligence, 16,* 239–256.

Ramey, C. T. (1993). A rejoinder to Spitz's critique of the Abecedarian experiment. *Intelligence, 17,* 25–30.

Reed, T. E. (1997). "The genetic hypothesis": It was not tested but it could have been. *American Psychologist, 52,* 77–78.

Rushton, J. P. (1995). *Race, evolution, and behavior.* New Brunswick, NJ: Transaction.

Rushton, J. P. (1997). Race, IQ, and the APA report on *The Bell Curve. American Psychologist, 52,* 69–70.

Scarr, S., Pakstis, A. J., Katz, S. H., & Barker, W. B. (1977). Absence of a relationship between degree of White ancestry and intellectual skills within a Black population. *Human Genetics, 39,* 69–86.

Scarr, S., & Weinberg, R. A. (1976). IQ test performance of Black children adopted by White families. *American Psychologist, 31,* 726–739.

Sheldrake, R. (1988). *The presence of the past.* Rochester, VT: Park Street Press.

Spitz, H. H. (1992). Does the Carolina Abecedarian Early Intervention Project prevent sociocultural mental retardation? *Intelligence, 16,* 225–237.

Spitz, H. H. (1993a). Spitz's reply to Ramey's response to Spitz's first reply to Ramey's first response to Spitz's critique of the Abecedarian project. *Intelligence, 17,* 31–35.

Spitz, H. H. (1993b). When prophecy fails: On Ramey's response to Spitz's critique of the Abecedarian project. *Intelligence, 17,* 17–23.

Spitz, H. H. (1997). Some questions about the results of the Abecedarian Early Intervention Project cited by the APA Task Force on Intelligence. *American Psychologist, 52,* 72.

Velden, M. (1997). The heritability of intelligence: Neither known nor unknown. *American Psychologist, 52,* 72–73.

Waldman, I. D., Weinberg, R. A., & Scarr, S. (1994). Racial-group differences in IQ in the Minnesota Transracial Adoption Study: A reply to Levin and Lynn. *Intelligence, 19,* 29–44.

Weinberg, R. A., Scarr, S., & Waldman, I. D. (1992). The Minnesota Transracial Adoption Study: A follow-up of IQ test performance at adolescence. *Intelligence, 16,* 117–135.

Yee, A. H. (1997). Evading the controversy. *American Psychologist, 52,* 70–71.

Yee, A. H., Fairchild, H. H., Weizmann, F., & Wyatt, G. E. (1993). Addressing psychology's problem with race. *American Psychologist, 48,* 1132–1140.

Zuckerman, M., & Brody, N. (1988). Oysters, rabbits and people: A critique of "Race Differences in Behavior" by J. P. Rushton. *Personality and Individual Differences, 9,* 1025–1033.

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