



Money Does Matter Somewhere: A Reply to Hanushek

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Money Does Matter Somewhere: A Reply to Hanushek

LARRY V. HEDGES RICHARD D. LAINE ROB GREENWALD

Educational Researcher, Vol. 23, No. 4, pp. 9–10.

Hanushek's (1994) comment on our reanalysis of his work is useful in that it clarifies certain aspects of his argument and our article. Our reply includes three general points and several specific ones.

General Points

First, Hanushek seems to question the validity of meta-analysis. We, and many other scientists and statisticians, disagree. For example, a recent report of a committee of the Mathematical Sciences Board of the National Research Council (1992), studying problems of combining information in the empirical sciences, concluded that "quantitative research synthesis—**meta-analysis**—has gained increasing use in recent years and rightly so. Meta-analysis offers a powerful set of tools for extracting information from a body of related research" (p. 2).

Second, whether one admits it or not, taking evidence from studies (even if only a significance decision, a p value, or the sign of a coefficient) and drawing conclusions about the likely values of the parameters that the coefficients estimate is *an inferential procedure*. Inference procedures must have performance properties that are reasonably well understood if their results are to be credible. The fact that an inference procedure (such as that used by Hanushek) is vague does not usually exempt it from scrutiny, quite the contrary.

Hanushek's statement that strong and consistent evidence meant to him that the preponderance of coefficients would have the same sign and be statistically significant, is a specification of his inference procedure. We admit that this inference procedure (vote counting) sounds sensible. However statisticians evaluate statistical procedures by deriving their properties. Hedges and Olkin (1980) showed that one would rarely obtain what Hanushek calls strong and consistent evidence *even if the true values of the coefficients (the parameters) were positive and exactly the same in every study*. Contrary to Hanushek's claim, Hedges and Olkin's results apply to a wide range of significance tests including t tests used for production function coefficients.

Hanushek misunderstands the inference problem in research synthesis in a subtle but important way. His interpretation is that we want to know whether the "estimated relationship in *any* study has the expected sign and is statistically significant." Statistical inference is used to draw conclusions about the *true values (parameters)* that characterize relations, not the (random) pattern of estimates themselves. The failure to distinguish the pattern of observed results (estimates) from the parameter structure that generates them

is a mistake that has led to a great deal of confusion in research synthesis (e.g., to the appeal of vote-counting), and Hanushek seems to make this mistake.

Third, Hanushek attributes to us the statement that none of our samples of "estimates [is] appropriate for the statistical methodology" because none are completely independent. We did not say this and do not believe it. Hanushek is aware of dependence in his data that could compromise his inferences, yet he apparently did not consider its consequences for the validity of his own conclusions. We did—by constructing a subsample of estimates for each resource variable that *was* independent.

Hanushek may wish that, unlike him, we had used full multivariate procedures in our meta-analysis to deal more efficiently with dependence (see Becker, 1992; Becker & Schram, 1994). However, they require even better reporting of data than the methods we used (only six of Hanushek's publications provided enough information to use these methods; see Becker & Kamata, 1994).

Specific Points

Hanushek criticizes us for failing to use information about results that are so poorly reported we do not know the direction of the effect, its magnitude, or the p value. Many of these nonsignificant results are reported as anecdotes in the text of articles, saying in essence, "We also ran several other models using [a particular input variable], but the coefficient was not significant." We felt that inclusion of such anecdotes would have been difficult to defend, since even the specification of the model was often unclear. However, even if we had included those results (using the least favorable p value), the evidence from studies that did report was so overwhelming that our combined significance tests would still have confirmed our overall findings for most variables (Hedges, Laine, & Greenwald, 1994, p. 12).

Hanushek complains about the lack of independence among the studies that he examined, but then criticizes us for creating subsets of the data that are independent. One

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cannot have it both ways, although we show that one would generally reach the same conclusions.

Hanushek notes that the heterogeneity in the p values could be a consequence of heterogeneity in the parameters or of failures to meet distributional assumptions in the primary analyses. Our two sets of combined significance tests (which Hanushek finds puzzling, but which are relatively conventional; see Oosterhoff, 1969, p. 2) were designed to examine the possibility of heterogeneity. The results are clear: There is strong evidence of positive effects and little evidence of negative effects. There may be heterogeneity, but it is in the realm of non-negative true values of the coefficients. Of course, if the underlying studies are seriously flawed, neither Hanushek's (1989) analysis nor our reanalysis would be valid.

We are surprised that Hanushek has so little to say about the generally positive values of the magnitudes of the relations that reinforce the conclusions of the combined significance tests.

Finally, we are surprised at Hanushek's suggestion that we, who had not published previously in the area of education production functions, would come to this area with a bias. We initially had a *methodological* interest: whether the data that Hanushek had been citing for over a decade actually supported his conclusions about the lack of relation between resource inputs and educational outcomes. Our analyses clearly suggest that it does not.

Conclusion

This interchange has moved the discussion forward. It has evolved from (a) the position that Hanushek's sample of studies show definitively that there is no relation between resources and outputs to (b) a discussion of how large the positive relations might be and of the characteristics of studies that best reveal this relation. This strikes us as progress.

Perhaps the data *are* flawed as Hanushek now admits. If so, policy recommendations based on them should be held in abeyance. But if we are to make policy recommendations based on Hanushek's data, they must be consistent with positive relations of resources to school outputs, which moves us to the question of *how* money matters.

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