This rejoinder responds to Sarah Turner’s recent, highly critical “comment” on our August–September 1999 Educational Researcher article entitled “Poor School Funding, Child Poverty, and Mathematics Achievement.” Turner’s contribution also appeared in ER, within the June–July 2000 issue, and we hasten to respond to it now.

Our initial reaction to Dr. Turner’s “comment” was one of surprise. Most critical reviews are written because the reviewer disagrees with the conclusions stated by authors, but Turner appears to agree in large part with our major claims. In our article we noted that a lot of confusion had appeared in prior research concerning the impact of poor school funding and child poverty on achievement in American schools, and that we hoped to resolve some of that confusion by reviewing problems in prior research and offering evidence from a new study of the issue. Having done these things, we then wrote of our findings:

Our results suggest that level of school funding and child poverty have substantial and statistically significant net effects on average student achievement among the school districts of America and that these effects stand up even when juxtaposed with those of two crucial, district-level control variables. (p. 11)

One would think from the critical tone of Dr. Turner’s review that she would disagree with these conclusions. Instead, she writes: “Starting with a point of agreement, students attending schools in districts with more resources should have achievement levels at least as large as students attending schools in districts with fewer resources, if all other characteristics of districts were the same” (p. 15). Further, “I suspect that most agree with the proposition that family income is positively related to student achievement” (p. 15). Given this level of agreement, why did Turner write her hostile review?

Alas, given the number of factual errors, unjust criticisms, and instances of poor scholarship and confusion in Dr. Turner’s text, it is difficult to answer this question. We begin with factual errors. Unfortunately Turner makes many claims about what we wrote that are simply not true. To illustrate, she writes that “Payne and Biddle interpret descriptive evidence from [their study] as proof of [their conclusions]” (p. 15.), but we didn’t. Again, “the estimates from Payne and Biddle do not provide an answer to the question of whether increases in school funding would change measured achievement” (p. 15), but we never claimed that they did. Again, “Payne and Biddle . . . assume that schools are operating efficiently and that the only objective of school administrators is to maximize student achievement” (p. 16), but we made no such assumptions, nor would we ever do so. Again, “Mr. Payne and Dr. Biddle present their research as a resolution of the debate concerning the effects of poverty and poor school funding” (p. 17), but we did not. And again, “Payne and Biddle seem to begin with the strong presumption that the covariance between school resources and poverty is expected to be large and negative” (p. 17), but we made no such presumption in our text or reasoning. And so it goes.

Some years ago Edwin G. Boring, then editor of Contemporary Psychology, used to distribute a list of basic principles that should be honored when preparing a fair and informative review. We have not retained a verbatim copy of this list, but we certainly remember some of its key tenets. One of them was: “Make certain that what you say about the work is justified, and back up your criticisms with appropriate quotes from the work.” Unfortunately, Dr. Turner’s “comment” fails this standard dreadfully.

But what about unjust criticisms? Dr. Turner leads off her text by claiming that our article “distorts the interpretation of prior research and presents a flawed interpretation of new empirical work” (p. 15). Let us examine how she justifies these two charges.

Regarding the first, Dr. Turner writes that our review of prior research on the effects of school funding “fail[s] to provide any synthesis of the methodological innovations and the theoretical insights [appearing in this literature] in the three decades since the release of the Coleman report” (p. 15), but this was not our intention. As our text made clear, our review was focused on one question: Why had so many studies of this problem failed to find significant net effects for differences in school funding? We answered this question by explaining that many of those studies had employed seriously flawed research designs—flaws that had been largely ignored in prior reviews of the literature—and that these flaws had depressed estimates those studies reported for the effects of school funding. We discussed these flaws in detail and cited example studies that exhibited some of them. We also cited a few exemplary studies that had not exhibited these flaws and pointed out that the latter studies had found significant net
effects for school funding. Turner would have every right to criticize possible shortcomings in the logic or conclusions of the review we actually wrote. She has no right to condemn us for failure to take on a different reviewing task.

Thus Dr. Turner’s “comment” also fails to honor another of Edwin Boring’s rules for fair reviewing (as we remember them): “Portray accurately what the authors intended to accomplish in their work, and judge whether they did or did not reach their goals.”

Regarding her second charge, Dr. Turner complains that our conclusion about the effects of childhood poverty and school resources in school districts “fails to rule out other plausible alternative explanations” (p. 16). To place this complaint in context, one must understand that our conclusion was based on regression analyses of survey data for a small sample (n = 67) of school districts representing the American population, and because of that small sample we could include only a few variables in our analyses. Although it is always true that one can criticize regression analyses for failure to examine other variables, the critic should explain why the latter are crucial and might “explain away” the effects being claimed. Turner argues that several variables such as district size, average level of parental education, and numbers of students with special needs should have been included in our analyses. She makes the case that these variables might generate significant effects for school achievement, but she fails to explain why they might “explain away” the impact of poverty or school funding.

So in this case, Dr. Turner’s “comment” fails to honor another principle for fair reviewing which might be phrased: “State clearly the logic and reasoning standing behind your criticisms.”

Nevertheless, had we had access to a larger data set and good information concerning variables such as the ones mentioned by Dr. Turner, we would certainly have considered them for our analyses. But we didn’t, so we focused instead on two district-level control variables that seemed, from prior research, to have the best potential for “explaining away” the effects of school funding and child poverty—level of curricular instruction and race—and found that each had an impact but did not “explain away” the effects of poverty and funding. We also discussed our reasons for this decision in our text, but in her “comment” Turner did not see fit to discuss either our reasoning or the findings we generated. Thus, again her “comment” failed to honor the intent of our effort and failed to assess whether that intent was realized.

In addition, Dr. Turner seems to consider our study inadequate because it involved only holistic indicators (such as measures of district-level poverty and school funding) and was based on cross-sectional data. Thus, it failed to examine the complex, underlying processes that link these indicators to school achievement. She writes: “The biggest challenge that is left untouched by this research is the role of differences in family circumstances and opportunities that are not observed by the researcher but correlated with district expenditures” (p. 17). We agree that this might make an interesting next step for research but note that to conduct such a study would have required a completely different data set and research design. So, in framing this statement as a criticism of our effort, Dr. Turner is asserting that we should have done a different empirical study altogether.

If we remember correctly, another of Dr. Boring’s admonitions was: “Never criticize others’ research because they failed to conduct a study you would have preferred, particularly if that study would have required a different data set or research design.” But Dr. Turner saves her strangest criticism until the penultimate point of her “comment.” Towards the end of her text she questions our statement that “America is a racist and bigoted country” and asserts that “the sign, magnitude, and significance of the race variable in this context are collectively only a red herring . . . Before launching general accusations of bigotry it is important to put forward a theoretical model of how and why race affects student outcomes” (p. 17). Good heavens, why on earth would anyone living in this country make such a statement? We cannot imagine that any serious American academic would be ignorant of the long history of scholarly monographs commenting on American racism. Nor is American racism dead in the contemporary world (despite claims from right-wing ideologues)—witness the works of Alphonso Pinkney (1984), Jennifer Eberhardt and Susan Fiske (1998), and any of the recent writings of Joe Feagin (currently President of the American Sociological Association). We included race as a control because of this history and the huge amount of evidence linking race with both poverty and student achievement (see Natriello, McDill, & Pallas, 1990, for a review), and—as we explained—we reasoned that some Americans would assume that our apparent findings for the effects of poverty were actually generated by race. Our findings suggested not only that race did not “explain away” the effects of poverty and poor school funding but that the latter accounted for some but not all of the apparent, first-order impact of race—but Dr. Turner also chose to ignore our reasoning and findings about this issue. So Payne and Biddle are now to be criticized for having written that “America is a racist and bigoted country” and including an indicator of race in their analysis? Give us a break.

Regarding poor scholarship, Dr. Turner also mischaracterizes several aspects of current “debates” concerning school funding and American education. She asserts that “the central disagreement among researchers from Coleman through Hanushek is whether available empirical evidence does a satisfactory job of measuring the relationship between school outcomes and the level of funding” (p. 15), but this is not the case. Disagreements about measurement and evidence have played only a minor role in debates about this issue; rather, most authors (and advocates) have focused on substantive concerns—that is, on findings and their policy implications, which have real consequences for millions of young Americans and their families. Turner also contends that “the debate has moved beyond the widely cited controversy between Hanushek . . . and Hedges, Laine, and Greenland” (p. 17), but again this is not the case. Right-wing ideologues (citing Hanushek) are still claiming that there is no research support for the notion that level of school funding is tied to student achievement, and scholarly works on the topic are still describing the debate between Hanushek and Hedges et al. in detail (see Elliott, 1998, for example).

To her credit, Dr. Turner recognizes that relations between resource and outcome variables differ depending on whether one conducts analyses for individuals, classrooms, schools, districts, or larger aggregate units, but she seems to believe that this is a recent discovery
of economists working in the field (see p. 16). However, this is not a new issue. Economists themselves have known about the aggregation problem for decades (see Marschak, 1939), and they have not been alone in discussing this issue. Psychologists (e.g., Thorndike, 1939), sociologists (Robinson, 1950), and statisticians (Simpson, 1951) have all written about the problem, and a long tradition of concern for this issue has appeared in prior studies of education (see Van den Eedan & Hüttner, 1982; Hüttner & Van den Eedan, 1995).

Dr. Turner also stresses the advantages of using longitudinal data for research on poverty and school funding (see p. 16), but she overstates the case. While longitudinal data offer advantages over cross-sectional data in the abstract, they also involve many problems in practice—among them their high cost and attenuation (as well as mortality) within their samples. In our study, we deliberately chose to use a cross-sectional data set from the Second International Study of Mathematics because we wanted to examine how the uniquely severe problems of childhood poverty and differences in school funding in our country had affected America’s achievement scores when they were compared with those of other advanced nations—an intention which, again, seems to have escaped Turner.

Dr. Turner also suggests that researchers should recognize that schools and school districts will differ in whether they use resources effectively, that short-term and long-term poverty may have quite different effects, and that additional processes (such as those associated with parental education) may also have significant effects on educational outcomes. These are not new points, of course, and research has already appeared on all of them, but it is useful to be reminded that they should be stressed in future studies.

In summary, although the text of Sarah Turner’s “comment” suggests agreement with the major thrust of our conclusions and makes modest but useful suggestions for future research, these contributions are enfolded within a barrage of statements involving misportrayals of what we wrote, unjust criticisms, poor scholarship, and confusion. In effect, Dr. Turner sets up a fictitious image of what we wrote and then spends much of her effort attacking that image.

On balance, Dr. Turner’s “comment” makes but small contributions, and we doubt it would have earned publication in ER had it not been framed as a review. If ER is to publish such reviews then surely it should set minimal standards for them. May we suggest that ER editors track down a copy of Edwin Boring’s list of tenets for fair reviews and adopt some version of them as a standard against which to evaluate such submissions in the future?

And while they are at it, ER editors should set up procedures so that review articles and rejoinders can be published in the same issue of the journal. Other leading journals have long since established such policies, because concurrent publication helps to mute the effects of reviewers’ false charges and facilitates reader comprehension of the issues being debated. Such procedures may require slight publication delays, but this is a small price to pay for greater veridicality and understanding.

Notes

Kevin J. Payne is an instructor in sociology and a Ph.D. candidate at the University of Missouri–Columbia, Department of Sociology, 12 Sociology Building, Columbia, MO 65211-1100.

Bruce J. Biddle is a professor of psychology and of sociology at the University of Missouri–Columbia, Department of Psychology, 210 McAlister Hall, Columbia, MO 65211.

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References


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