

Rejoinder

Isaac Ehrlich†

I appreciate Professor Peck's undertaking to comment on my reply, published in the last issue of this *Journal*, to critics of my work on murder and capital punishment. I wish to address briefly only a few of his remarks.

Peck suggests that the results of my regression analysis concerning the ranking of estimated elasticities of the murder rate with respect to measures of apprehension, conviction, and execution risk could be consistent with alternative theories of crime. In principle, such a possibility never can be denied. I believe his criticism would have been more constructive had he identified what specific theories he has in mind. Unfortunately, he errs in stating that I have not examined this possibility myself. In my own study, I have addressed the issue of whether a theory of crime based upon incapacitating effects of punishment could produce the same ranking of elasticities observed in the study.¹

I believe that Peck mistakenly ascribes to me an argument that I have not advanced. I have not argued that "errors of measurement" can *only* work against the hypothesized deterrent effect. My reference to the classical case of "errors of measurement" has been quite specific. Curiously, contrary to his own argument, Peck's example in which errors of measurement in apprehension risk are *positively* correlated with the murder rate² does, of course, further illustrate why "errors of measurement" are likely to work against the hypothesized deterrent effect of apprehension. My basic point has been that if the variables used in the empirical analysis grossly misrepresent the theoretical variables that they purport to measure, then there is no reason to expect findings consistent with the detailed theoretical predictions.

I am glad Peck agrees with my criticism of statistical errors committed by my critics in their inferences from regression analyses of

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1. Ehrlich, *The Deterrent Effect of Capital Punishment: A Question of Life or Death*, 65 *AM. ECON. REV.* 397, 413-14 (1975).

2. Peck, *The Deterrent Effect of Capital Punishment: Ehrlich and His Critics*, 85 *YALE L.J.* 359, 366.

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subperiods of the full sample.³ I do not agree with him, however, that my critics' work has addressed the issue of the stability of the regression equation, since they report no tests of stability. Nor do any of the works cited by Peck provide valid tests of stability. While I could not address this technical issue directly in my reply, I did indicate what I believe are the basic reasons for the apparent weak results obtained from analysis of certain specific subperiods.

Many difficulties in research on criminal behavior should rightfully occupy the thoughts of interested scholars, and skepticism is the essence of scholarly work. I am grateful to the editors of the *Yale Law Journal* for allowing me to express my skepticism on some aspects of the work that has been conducted in this area and to elaborate upon my own research.

3. *Id.* at 367.

It may also be of some interest to note that the test of optimal transformations in connection with the form of the regression equation—the Box and Cox procedure—that Peck suggests as the way of determining the efficiency of the logarithmic-linear form is the same one to which I have already referred in my reply. Compare *id.* at 361 & n.6 with Ehrlich, *Deterrence: Evidence and Inference*, 85 *YALE L.J.* 209, 218 (1975). Peck does not recognize, however, that the problem of treating zero values of variables can be overcome through an appropriate transformation.

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