state, "I do not believe the obligation can be shown." Surely he must believe that some obligation can be shown, or it would seem that the moral legitimacy of punishment is in deep difficulty.

Van den Haag also now denies that he believes society has an obligation to punish criminals in virtue of its promise to them. I am pleased that he no longer wishes to defend this view. But *Punishing Criminals* does:

Debts must be paid in the first place because they are owed, because one has promised to pay them. Retribution must be paid because it is owed, because it has been threatened, and a threat is a (negative) promise. The payment of debts (or of retribution) fulfills an obligation undertaken in the past (p. 14).

The book goes on to say: "Society has obligated itself by threatening. It owes the carrying out of its threats" (p. 15). Now it is true that van den Haag correctly argues against the view that the criminal exonerates himself by being punished, by "paying his debt." But the whole point of his discussion is to say that it is society's debt (not the criminal's debt) which is at issue. Indeed, he argues that society has an obligation to the criminal and "law-abiding citizens as well" (p. 16, emphasis added). Thus, I restate my point: society cannot create an obligation to punish the criminal simply by threatening (promising) to do so, unless it has a pre-existing right to make the promise. Perhaps it does. But van den Haag has not shown that it does.

As my review suggests, van den Haag adopts a popular role-utilitarian position in holding that "utility must prevail" at the legislative phase and that "justice must prevail in the judicial phase..." (p. 25). I suggested that he does not show why legislative retributivism would be wrong. He now says it would not be wrong—"I merely explain why it should not prevail." Let me rephrase the point. Why must utility prevail at the legislative phase? Why would a thoroughgoing retributivism be false? *Punishing Criminals* contains no answers to these questions. Moreover, if utility is the proper underlying objective of punishment, one needs a complex argument (not mere assertions) which shows why punishment should not be distributed on a (sophisticated) act-utilitarian basis. Assertions to the effect that "justice must prevail in the judicial phase" are not sufficient. The incompatibility of retributivism and utilitarianism cannot be so easily defused.

*Punishing Criminals* states: "Children surely should not be held responsible for their conduct to the extent adults are" (p. 173). The book goes on to ask: "But should we regard sixteen-year-olds or even fourteen-year-olds as children?" (p. 173) and then points out that the "need for social defense or protection [against juveniles] is the same" (p. 174). Now if there are good reasons for treating children as less responsible than adults, the fact that the need for social protection from juveniles is the same (it may be greater) as the need for social protection from adults does not and cannot establish the appropriate degree of juvenile responsibility. Nowhere did I suggest that van den Haag is acquainted with the notion of mens rea. It simply has not been adequately analyzed nor invoked in this context.

ALAN WERTHEIMER
University of Vermont

Comment on Rosenthal's Review
(Vol. 73, March 1979, pp. 321–23)

To the Editor:

In a recent review of my book, *The French Popular Front: A Legislative Analysis*, Howard Rosenthal raises a very important criticism that merits a response. He questions the statistical methodology that I employ in analyzing roll-call votes, and suggests that roll call analysts such as myself must "shoulder the burden of proof" that their techniques are appropriate to the underlying reality.

The "proof" that I have developed was presented in two articles, "A Reevaluation of Alternative Methodologies in Legislative Voting Analysis" and "The Definition and Measurement of Similarity among Roll-Call Votes" which were published in *Social Science Research* (1975) two years before my book appeared. This material was not repeated fully in the book because it seemed to me that a brief presentation of the technique and a greater concentration on the subject of the study itself was more appropriate in that case. Incidentally, Rosenthal will be interested to find that in those articles are developed: (a) the underlying assumptions about voting behavior, including a treatment of abstentions which is different from the assumption he made in criticizing me, and (b) a precise analysis of the relationship of Guttman scaling to principal components and an indication of when each is to be preferred. Needless to say, on this basis I reject the criticisms that Rosenthal makes of my methodology, which were based on his reading of one article which he admits does "not pertain directly to roll call-by-roll call analyses." Whether one accepts my approach to
legislative analysis or not, I should not be accused of not having thought through the issue of technique.

PAUL WARWICK
Simon Fraser University

Reply
TO THE EDITOR:
I am delighted that Paul Warwick has introduced his 1975 Social Science Research articles as additional evidence. These articles beautifully illustrate the central point of my review—that mindless application of a statistical black box (e.g., factor analysis, Guttman scaling) will generally fail to recover the underlying behavioral space of a legislature.

Warwick (1975, pp. 363–65) establishes a hypothetical two-dimensional legislature where legislators are points and roll calls cutting lines. “Yeas” and “Nays” lie on opposite sides of a line. Those exactly on the roll call’s line abstain.

What do his computations show?
(1) Principal components factor analysis can readily give the wrong dimensionality. Although the true dimensionality is two, Warwick generally finds three or four dimensions! (Similarly, unidimensional data [p. 255] needs three components.) The excess dimensionality immediately implies that the behavioral space is not recovered correctly.

(2) Even when the dimensionality is correct, the space may not be recovered. For example, two distinct roll calls, with legislators in all four quadrants formed by their intersection have identical (to the two decimal places reported) factor loadings. (See roll calls 2 and 13 in the last columns of Table 3 [p. 370].) Also, even within the same “scale,” roll calls that are relatively distant may have more similar factor scores than roll calls that are relatively close. (Compare roll calls 11, 13, and 15 in the D2 + D3 columns of Table 3 [p. 370].)

(3) If a number of roll calls have small angles of intersections, certainly likely in applications to real data, Warwick acknowledges the inappropriateness of principal components (p. 371) and as well, both the Q and gamma Guttman methods (p. 378).

Warwick does suggest (p. 263) that the unwarranted dimensions of principal components are useful in identifying blocs of legislators. But, in the case of one dimension, blocs fall out just as readily from a simple scalogram, such as Table 4 (p. 255). As to higher dimensions, Warwick presents no direct evidence that factor scores usefully cluster legislators as they appear in the original two-dimensional space.

Finally, as to my “reading of one article,” that seminal piece by Richard J. Morrison was enough to suggest the impoverished foundations of most roll-call analyses, including Warwick’s. Not surprisingly, Warwick’s own computations for roll call-by-roll call matrices confirm the lessons of Morrison’s analytical work on legislator-by-legislator arrays. If statistical techniques are not grounded in explicit models of political behavior, the black box is likely to be a garbage truck.

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References

Comment on Hoole’s Review
(Vol. 72, December 1978, pp. 1509–10)

TO THE EDITOR:
In his recent review of Stuart A. Bremer’s Simulated Worlds (1977), Francis Hoole issues a number of criticisms which are pertinent not only to the book, but also to the enterprise within which the volume must be placed: socio-political global modeling. Most specific points in the review are well taken. Yet the thrust of Hoole’s conclusions is, if misinterpreted, at cross purposes with Hoole’s avowed support of the “goals and approach” involved in developing computerized models of global social, political, and economic relations.

Hoole is concerned about the adequacy of the various verbal, mathematical, and flow-diagram expositions of the model in the volume. The difficulty of reconciling these presentations was an impediment to developing an “understanding” of the various meta-components of the model, including: (1) Inter-Nation Simulation (INS) theory (Guetzkow, 1963) and (2) the adaptive and self-monitoring decision-making algorithms Bremer developed, such as the international trade mechanism, diplomatic conflict, budgetary activity, and the formulation and monitoring of national goals vis-à-vis strategic military positions, domestic political sta-