that these are important issues that cannot be resolved by his pronouncements. I invite Mr. Germino to a public discussion of them.

Finally, perhaps because of Mr. Germino's special epistemological insights, he seems driven to the judgment that when I argue that Fascism, Leninism and Maoism share genus and special traits I fail to make any distinctions between them. Certainly, whatever special epistemology Mr. Germino embraces, he recognizes that many things share genus traits and are not identical; horses are not zebras although both belong to the same genus. Moreover, creatures can even share species traits and yet not be identical. Lapdogs are not Great Danes. For heuristic and classificatory purposes one may dwell on similarities—but for hunting badgers one does well to dwell on the distinction between Dachshunds and Saint Bernards. One must distinguish between cognitive and pragmatic purposes—between classification and evaluation. Where one doesn't one runs the risk of lapsing into silly judgments. Other reviewers have avoided Mr. Germino's mistake. The reviewer for the Journal of Politics simply states that I am "sensitive to the differences" but provide a convincing argument for subtending genus and perhaps species similarities.

In some obscure way, I suppose, Mr. Germino knows some of these things. That his knowledge is not evident in his review is, I conjecture, a consequence of his commitment to a new political style (which I have baptized with my own neologism): neo-visceralism.

A. James Gregor
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To the Editor:

The widespread use of statistics in political science (and the other social sciences) undoubtedly is contributing and will continue to contribute to its maturation as a science. This does not mean, however, that their indiscriminate use is wise, for the misapplication of such manipulations can produce results that are highly misleading. The recent article (June 1970) by Brian R. Fry and Richard F. Winters on "The Politics of Redistribution" may be taken as a case in point. Following the earlier work of Richard Dawson and James Robinson, Thomas Dye, Ira Sharkansky, and others, their study tries to answer the question, "Does politics make a difference in the policy formation process?" While it is obvious that in a broad sense politics is very germane to the process, statistical results that confirm the obvious among the American states have not been as readily produced as one might expect. Fry and Winters, using an ingenious measure of "redistribution" (in terms of expenditure as opposed to tax burdens) toward the lower income classes as their "policy" issue, argue that they have proof that "political variables" (including extent of political participation, interparty competition, aspects of elite behavior, etc.) are more important than the socio-economic variables (median income, industrialization, urbanization, education).

To prove their argument they test eighteen hypotheses (involving six socio-economic variables and twelve political variables), first by running zero-order correlations between the "redistribution ratio" and each of the eighteen variables, and second by running partials in which they control for the 17 other variables. If one looks just at the zero-order relationships, it is apparent that they have achieved some success (not complete, but some), although some of the correlations are of a low magnitude. While their successes are noted, if only in passing, they pass on to the partial relationships. They state: "Shifting our attention to partial coefficients of correlation, a measure of independent impact of each independent variable on redistribution, we find somewhat longer lists of contrary correlations (my italics)." Indeed the contrary correlations include negative relationships between the redistribution ratio and (1) median income (originally .18, when partialled it became -.27), and (2) industrialization (originally .29, it became -.02). To at least some of us this seems most surprising. But if one looks for an explanation of such results, Fry and Winters fail to provide one. A reason for this, it may be suggested, is that such findings are spurious, hence attempting to explain them could lead to strained mental contortions.

The reason that such findings are probably spurious (as well as some of the other findings that are a result of partial relationships, although in a less obvious way; the probability that the "independent" variables are more highly correlated to one another than to the "dependent" variable does not bode well) is that the independent variables in the set of socio-economic variables suffer from multicollinearity (highly correlated independent variables), so that when one "controls" for variation caused by, say, industrialization, urbanization and education to see if "median income" has any "independent" effect on "redistribution ratios," one actually controls the valid variation of, in this case, "median income." (Several methodologists have written about this (including Robert A. Gordon, "Issues in Multiple Regression," American Journal of Sociology, 73 (1968), 592-616, and Hubert M. Blalock, Jr., "Correlated Inde-
dependent Variables: The Problem of Multicollinearity," *Social Forces*, 42 (1963), 233-237, Robert Gordon's article being particularly useful.) For example, using Dye's results for the 50 states (Fry and Winter use 48; data are from *Politics, Economics, and The Public*, Chicago: Rand McNally, 1966), there is a zero-order correlation between median income and urbanization of .67, indicating that they are nearly equivalent for the purposes at hand. And, as Gordon notes, "When two variables are equivalent, they will both be equally valid to some degree, and controlling for one of them amounts to controlling for valid covariation. This makes as much sense as controlling for a parallel form of the same instrument." For reasons that cannot be gone into here for lack of space, but which Gordon discusses at length, such partialling can produce weird results. Such, it may be suggested, as those Fry and Winters report.

Probably at the heart of the problem of the use of statistics in such a problem (are political or socio-economic variables more important?) is the belief (see italicized part of quote from Fry and Winters above) that partialling provides "a measure of (the) independent impact of each independent variable. . . ." As Gordon notes, this view is "generally accepted," but incorrect insofar as multicollinearity exists. This letter, then, only attempts to point out for the unwary some of the pitfalls that Gordon tries to indicate and explain.

What implications does this analysis have for the other findings reported by Fry and Winters, including "The most interesting and significant finding in this study (concerning) the relative importance of political and socio-economic variables in determining redistributive fiscal policies in the states," namely that "politics plays a dominant role in the allocation of the burdens and benefits of public policies"? One can probably conclude, it may be suggested, that the other relationships are spurious too. In this section of their paper one may note that Fry and Winters' multiple regression methodology has far outdistanced their theory. They provide, that is, no explanation of, have no theory covering, the overall results of using, first, all eighteen, then "only" ten, variables in a multiple regression analysis, as opposed to their original hypotheses. More controlled investigation may have asked, upon finding a correlation between a certain political variable and the redistribution ratio, whether such a relationship was spurious, "reflecting" genuinely the effect of the socio-economic environment (assuming a correlation between the political variables and the socio-economic environment), and controlled just for socio-economic environment to find out if the political variables did have an independent effect. It may be pertinent to note here that their re-analysis using "only" ten variables in a multiple regression equation, where they find also that political variables are much more important than socio-economic variables, could have been expected if the socio-economic variables are more highly intercorrelated, as one suspects is the case, although Fry and Winters do not provide such data. (Gordon's article is very useful in this connection also.)

While Fry and Winters have introduced a useful policy consideration by way of their "redistribution ratio" for future analysts to consider, because of, apparently, multicollinearity among (some of) their independent variables, as well as allowing methodological possibilities to overshoot their theory, their method of analysis and of course their results (past the zero-order correlations) are most questionable. It is perhaps only by making and then becoming aware of such mistakes, however, that understanding increases.

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To the Editor:

Mr. Noell’s major point is well taken, namely, that multicollinearity is a serious problem in multivariate analysis where high inter-correlations among variables can lead to great difficulties in interpretation of, and misleading inferences can be drawn from, the data. He is also correct in his assertion that the presence of multicollinearity among the independent variables used in our analysis renders the partial correlations difficult to interpret and susceptible to misleading inferences. However, the misleading inferences to which Mr. Noell refers are his, not ours. Briefly, what Noell has done is to use a statistic (the partial correlation) to assess the relative explanatory value of political and socio-economic variables, a purpose for which it is neither particularly well-suited nor intended, while ignoring the statistic (the multiple-partial coefficient of correlation) which we had intended for use in such an assessment. The partial coefficients of correlation were reported simply to indicate the "marginal" relationship between each independent variable and the dependent variable controlling for all other variables in the set with the specific acknowledgement that the complex interrelationships among the individual independent variables would require further investigation (an endeavor in which we are currently engaged). The multiple-partial, on the other hand, *were* meant to be an indicator of the relative explanatory power of the political and socio-economic variables. In em-