Comment on Abramson and Aldrich (Vol. 76, September 1982, pp. 502-521)

TO THE EDITOR:

An article recently published in the Review (Abramson & Aldrich, 1982) purports to have solved Brody’s (1978) much discussed “puzzle” of declining electoral participation. The authors conclude that two factors, declining strength of partisanship and declining perceptions of external efficacy, can account for almost 70% of the decline in voter turnout between 1960 and 1980.

Ordinarily one would be pleased to learn that the solution to what was heretofore considered a complex puzzle has been found. And ordinarily one would be gratified to find that a simple, two-variable model could achieve this goal which has eluded researchers employing more complex, multivariate models. In this particular instance, however, the goal of identifying a simple and elegant model of declining participation has been perverted by statistical and ceteris paribus gymnastics which seek to maximize the explanatory power of this simple model at the expense of completeness and necessary complexity. Put simply, the authors of this article have fundamentally mis-specified the model by carefully selecting two variables that admittedly contributed to the decline, while at once ignoring variables that either: a) also contributed to the decline; or, b) operated contrary to the decline, and hence, constitute the puzzle of declining participation.

An example of the first category of missing variables is residential mobility. Scholars consistently have demonstrated that mobility depresses voter turnout; and during the time period considered by Abramson and Aldrich, residential mobility increased. It figures, therefore, that the increasing mobility of voters made a significant contribution to declining turnout, something we (Cassel & Hill, 1981) have demonstrated. Without a variable for voter mobility in the Abramson and Aldrich analysis, one does not know whether the apparent contribution of their two variables to the decline is undermined by spurious correlations. Thus, for example, we cannot be certain from the model that weakened partisanship made the claimed contribution to declining participation in that partisanship has weakened significantly among the growing cohort of young persons who are also among the most mobile.

In the second instance, Abramson and Aldrich ignore variables such as education that offset some of the decline in voting, variables intrinsically fundamental to Brody’s notion of a “puzzle.” Why did turnout decline when factors such as the increased education of the electorate should have engendered an increase in voting? This reader of the Abramson and Aldrich article cannot help but wonder how much of the explanatory power of the authors’ two-variable model would evaporate if the offsetting power of education were introduced into the model.

Of course, the authors are not unmindful of these problems and seek to rationalize their strategy by pointing out that their model and methodology assumes “everything else being equal” (pp. 507 and 512). Furthermore, the authors argue that they are concerned only with variables that caused the decline, not those which may have merely retarded the decline (p. 504). This is fancy footwork, indeed, but knowing that all other things were not equal and that the retarding power of some variables was substantial makes the authors’ assumptions and strategies somewhat puzzling.

We and other electoral students would be pleased if the puzzle of participation could be untangled with such a simple model as that employed by Abramson and Aldrich. But plainly it is not. A proper model will take into account all variables that are theoretically relevant. Only by developing such a model will we be able to understand fully and interpret future fluctuations in voter turnout. In our rush to explain political phenomena such as this decline, let us not as political scientists devolve to employing models that rest on questionable assumptions and achieve statistical victories at the expense of theoretical rigor and good common sense. Otherwise, we will be adopting some of the very methods that occasionally undermine confidence in the science of economics. This is particularly important when political scientists like Abramson and Aldrich make tacit recommendations for political strategies, as they do in suggesting that leaders can increase turnout by engendering more positive attitudes toward the political system (p. 520). A ceteris paribus experiment like that of Abramson and Aldrich hardly seems the proper foundation for such a suggestion to leaders or anyone else.

References


Brody, R. A. The puzzle of participation in America. In A. King (Ed.), The new American political sys-

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Three issues have emerged during this past year which require some editorial comment and some clarification of editorial policy. First, the authors of a small number of articles have complained that their ideas had been “stolen” by authors of other articles. In some cases the individuals in question were reviewing a manuscript for the Review when they discovered this similarity of ideas; in other cases the potentially offending manuscript had been published in a current issue of the Review. Given the great diversity of problems being studied in political science and the equally wide range of methodologies, in the broadest sense of that term, that are being used, I found these reactions both startling and encouraging. The overlap of ideas and methodologies (even if stolen) has to say something about convergence and cumulation in the field. This situation has certainly been true in the natural sciences, where the rush to get new ideas into print and the fear of being scooped is intense, as is the recognition of key problems and basic approaches. But although this development may be a positive one for the discipline as a whole, it does signal possible problems for authors, particularly for young, untenured ones.

Consequently, beginning with this issue we will henceforth publish with each article the dates on which the manuscript was first received in our editorial offices and if revised, the date when the revised manuscript was received. Although this measure will not resolve all disputes of timing, it may help to clarify time sequence.

We will also publish the date when each article was accepted, which will provide readers with two additional facts: it will indicate how long it took to process the manuscript, i.e., the time elapsed between receipt and decision, and also some idea of cue waiting time, i.e., the time between decision date and date of publication.

Two caveats should be borne in mind to interpret this information properly. Journal editors in political science have in recent years attempted to provide authors with a turnaround time of less than three months. Although this is certainly a goal of the Review, it is only possible with complete referee cooperation. Sometimes this happens, but sometimes it does not. Especially in the more technical and very specialized areas as many as five or six individuals may decline to review a manuscript, and this combined with the fact that many referees take more than the standard four weeks to respond, often causes uncontrollable delays that extend the three-month processing period.

The second caveat concerns “waiting times.” It must be remembered that there is a four-month lag between the time we send an issue of the Review to the printer and the actual date of publication. One must then add approximately two months for editorial work, which means that manuscripts accepted for publication in March of a given year can at best appear in a December issue, depending upon the existing backlog of publishable manuscripts.

A second problem of concern involves our policy regarding communications. As editors before me have discovered, the volume of communications can at times be excessive, so much so that space for articles may be threatened. I think most readers would agree that the emphasis of the Review should be on articles, not communications. Chuck Jones initiated a policy that I have followed; all communications, like manuscripts, are refereed, and only those that have received referee endorsement are published, which helps to regulate communication flows. Communications, however, typically question or attack previously published articles, and when these letters are accepted for publication, authors feel they have a right to respond and indeed that their responses should appear in the same or the subsequent issue. To attempt to do so this would, however, open a Pandora’s box. We will therefore adopt the following policy. A communication involving a previously published article will be sent to the authors of the article in question. The authors will be asked whether they wish to respond and given two weeks to do so. The package, communication plus author response, will then be submitted to the regular referee process. Referees will be asked to determine whether both items, only the communication, or neither item should be published. If authors do not respond within two weeks, the communication will be submitted to the normal referee process.

Every so often we receive articles that raise