First, Lipset's effort to understand the social bases of stable democracy encompassed one of the most powerful and enduring themes in comparative politics, and built on the work of many of the great political and social theorists, from Aristotle to Tocqueville and Weber. Writing in 1958, with democracy still largely a "Western" phenomenon and with most of Africa still under European colonial rule, Lipset did not anticipate that well over half of the independent states of the world would become democracies. Yet clearly, the expansive scope of global democratic change and aspirations has stimulated scholarly interest in its facilitating conditions. Today, much of comparative politics revolves around this issue.

Second, although his key thesis was remarkably simple and concise—"the more well-to-do a nation, the greater the chances it will sustain democracy" (p. 75)—his essay was theoretically rich in identifying a nexus of causal factors leading from level of economic development to prospects for stable democracy. Only two of his modernization variables (national income and education) have stood well the tests of time and more complex methods as drivers of democratization. But the key intervening variables that he adduced—changes in political culture, class structure, civil society, and state-society relations—have endured exceptionally well as explanations, spawning in themselves vast subliteratures. To this day, these remain the key social determinants of the democratic prospect (Diamond 1992; Lipset 1994).

Third, Lipset's thesis was embedded in the larger body of modernization theory, which would mobilize considerable evidence demonstrating that rising levels of income and education have diffuse impacts on attitudes and values, and through them on political systems. The effects are far from neatly linear, but higher levels of economic development do tend to generate the trust, tolerance, autonomous participation, and valuing of freedom that facilitate democracy (Inglehart and Welzel 2005; Inkeles and Smith 1974).

Fourth, Lipset's hypothesis about economic development and democracy—advanced at the time with crude and merely suggestive statistical measures—has since been supported by a vast array of statistical studies. Although there is debate about whether economic development actually causes democracy, clearly economic development *sustains* democracy (Przeworski et al. 2000), and virtually every multivariate analysis of the determinants of democracy identifies economic development as a powerful factor. Moreover, considerable case study evidence supports Lipset's (1994) (frequently overlooked) assertion that "a 'premature' democracy that survives will do so by (among other things) facilitating the growth" (p. 72) of the critical intervening variables, such as broader literacy, a vigorous civil society, limited inequality, and a democratic culture.

Finally, Lipset was also right in asserting that the stability of democracy (uniquely among political systems) depends on legitimacy, and that this belief heavily depends on effective performance, especially early in a regime. Here again Lipset gave us a rich set of propositions that have shaped political science theorizing about the need for democracies to moderate conflict, the value in doing so of cross-cutting cleavages, and the importance for new democracies to avoid threatening "the status of major conservative groups and symbols" (p. 87), a theme that resonates powerfully in the transitions literature (e.g., O'Donnell and Schmitter 1986).

Lipset was not the first to make these various arguments, but he was the first to state them clearly and systematically to a new generation of empirical social scientists, at a time when dozens of new nations were gaining independence and when transitions to and from democracy would become one of the dominant aspects of national development demanding explanation.

At the high end of the spectrum of development, Lipset's theory has held up remarkably: all but one (Singapore) of the 25 most developed states are democracies, and democracy has never broken down once established in a relatively rich country (Przeworski et al. 2000). However, contrary to Lipset's expectation, about two in every five poor countries (with low "human development" on the UNDP scale) are democracies today. It remains to be seen whether these countries can consolidate democracy, but to the extent they do so it will be by accumulating legitimacy through effective performance and by building up the supporting social and cultural requisites that Lipset identified in his seminal 1959 essay.

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Much as we would like to believe that the high citation count for this article is due to the brilliance and clarity of our argument, it is more likely that the count is due to our being in the right place (that is, the right part of the discipline) at the right time. In the 1960s and 1970s, serious quantitative analysis was used primarily in the study of American politics. But since the 1980s it has spread to the study of both comparative politics and international relations. In comparative politics we see in the 20 most cited *Review* articles Hibbs's (1977) and Cameron's (1978) quantitative analyses of the political economy of advanced industrial societies; in international relations we see Maoz and Russett's (1993) analysis of the democratic peace; and these studies have been followed by myriad others. Our article contributed to the methodology for analyzing what has become the principal type of data

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used in the study of comparative politics; a related article (Beck, Katz, and Tucker 1998), which has also had a good citation history, dealt with analyzing this type of data with a binary dependent variable, data heavily used in conflict studies similar to that of Maoz and Russett's. Thus the citations to our methodological discussions reflect the huge amount of work now being done in the quantitative analysis of both comparative politics and international relations.

Our methodology deals with the analysis of time-series–cross-section (TSCS) data. Quantitative comparative politics and international relations have leaned heavily on this type of data since the early 1990s. The early quantitative research in comparative politics, such as Cameron's (1978), used simple cross-sectional regression on typically fewer than 20 countries. With so few observations, it was impossible to tease out complicated relationships. The late 1980s saw arguments over whether results were stable if a single country was excluded. The alternative was to use more standard time-series analysis, but to examine each nation's time-series separately, as was done by Hibbs (1977). TSCS analysis pools these time series, allowing for rigorous quantitative analysis of both the temporal and the spatial dimensions of the data. TSCS data are complicated, and users of such data have been very interested in the appropriate methods for analyzing these complicated data.

Our article is also heavily cited because it deals with important issues relevant to political science research. Twenty years ago the small coterie of political methodologists taught out of standard econometrics texts. But the issues relevant to political scientists are not necessarily those relevant to economists. In our case, econometricians have developed sophisticated methods for studying panel data, which appear to look like TSCS data. Given econometricians' interests, they focused on data sets in which the cross-sectional units consisted of a huge number of survey respondents observed over a very small number of survey waves. Methods suitable for the analysis of panel data are not appropriate for TSCS data, and vice-versa. One reason our work is cited by political scientists is that as political scientists we worked on methodological issues relevant to the study of political questions.

Our article also reflects a growing interest in the finite sample properties of the estimators we all use. Econometricians tend to live and die by asymptotic properties; but we all have finite samples and sometimes what is good asymptotically is not good for finite samples. We started with a commonly used estimator that was fine asymptotically but displayed rather poor finite sample performance (at least for typical TSCS data). We showed that in typical data sets least squares, combined with a correction for standard errors, could be a huge improvement over the "asymptotically superior" estimator.

In thinking about our article, we cannot help but remember that we submitted it to the *Review* after it was rejected by the only other political science journal that at the time published serious methodological articles. It is good to remember that a rejection letter is not the same thing as a death sentence.

What do we wish we had done differently? The study of TSCS data has become vibrant with many ongoing debates. One point that almost everyone understands, or should, is that there is no magical method (or statistical package command) for analyzing TSCS data. The most critical issues are about specification, not about the choice (and surely not the mechanical choice) of an estimation method. Because of the context in which we were writing, we focused on estimation methods, though we always tried to move researchers away from mechanistic thinking about statistical analysis. The ongoing debates have focused our current work on more critical issues of specification. But because we now live in a world where methodological debates are commonplace, we do not worry that we missed many important issues in our article.

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The article started as a long paper on distributional inequality in the advanced capitalist societies that I presented at the 1976 annual meeting of the APSA. Using OECD data on the post-tax distribution of income in 12 countries, I found that the extent of inequality varied inversely with the extent to which Social Democratic and other leftist parties had governed in recent years. Control of government by left-of-center parties was closely associated with the extent of and growth in the extractive capacity of the state, defined in terms of the ratio of all public revenues to GDP, and the extent of and growth in extractive capacity were closely associated with the degree of distributional equality. But there were perplexing anomalies. For example, the Netherlands, where the Left had seldom governed, experienced the largest increase in the ratio of revenues to GDP since 1960 and was the most egalitarian in terms of the post-tax distribution of income. Belgium and Ireland also experienced unusually large increases in the ratio of revenues to GDP despite infrequent control of government by left-of-center parties.

Mindful of those countries' high dependence on trade and drawing upon Robert Dahl and Edward Tufte's (1973) discussion of the relationship between small size and trade dependence, the work of Odd Aukrust (1977) and Assar Lindbeck (1975) on "open" economies, that of Gerhard Lehmbruch (1977) on incomes policies, and that of Robert Gilpin (1975) on neomercantilism, I incorporated a measure of openness—the ratio of exports and imports to GDP—in

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