Econometrics and Presidential Elections

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The relationship between economic conditions and U.S. presidential election outcomes is one of the most thoroughly studied empirical relationships in the whole field of political science, and Ray Fair is one of the most prominent and persistent contributors to the scholarly literature on the topic. Fair's original (1978) analysis of "The Effect of Economic Events on Votes for President," along with the related work of Kramer (1971) and Tufte (1978), helped to spawn a host of imitations and extensions. Despite this crowding of the market, Fair's own subsequent work has continued to attract significant attention from scholars, and has also been (as Fair himself modestly puts it) "of interest to the news media, which every fourth year becomes fixated on the presidential election."

In "Econometrics and Presidential Elections," Fair (1996) summarizes and reflects upon his twenty years' work on as many data points. In addition, he describes the various significant

revisions he made in response to his model's rather dramatic failure to forecast Clinton's election in 1992.[1] My aim here is to report one political scientist's reactions to Fair's work, and in the process to introduce economists to some unresolved issues in the broader political science literature on economics and elections.

One of the most interesting aspects of Fair's essay is the unusually frank and detailed description it provides of the enormous amount of exploratory research underlying published analyses of aggregate election outcomes. What is the relevant sample period? Which economic variables matter? Measured over what time span? What does one do with third party votes, war years, or an unelected incumbent? In fewer than a dozen pages, Fair raises and resolves many such questions, as any data analyst must. In the process, he makes clear how much of what Leamer (1978) has referred to as "specification uncertainty" plagues this (or any other) statistical analysis of presidential election outcomes.

One practical implication of this specification uncertainty is neatly illustrated in Fair's Table 4, which provides seven different forecasts of the 1996 election outcome based on different data sets and model specifications, ranging from a narrow Dole victory to a Clinton landslide of near-historic proportions. To his credit, Fair forthrightly settles on one of these seven forecasts as his "'final' choice." Alas, as it happens, this one turns out to be the only one of the seven to get the election outcome wrong. Moreover, it is not a narrow miss in a close election, but a substantial error of more than 5 percentage points, well out in the tail of the forecast error distribution implied by Fair's reported standard error of 1.9 percentage points, or by his reported mean absolute error of 2.1 percentage points for "out-of-sample" forecasts based

1 While the models of Erikson (1989), Campbell (1992), Gelman and King (1993), and several others successfully forecast Clinton's victory, Fair's model had Bush garnering 56 percent of the two-party vote.

on applying his 1916-1960 parameter estimates to post-1960 elections.[2]

How is it that Fair managed to go so far wrong once again -- in a relatively tranquil election that most other analysts got right?

What is most striking in Fair's account is that, in almost every case, his choice of model specification seems to have been guided by goodness-of-fit considerations rather than by *a priori* political or economic considerations. His data set begins in 1916 because "some experimentation . . . using observations prior to 1916" produced results that "were not as good." Gerald Ford is sometimes counted as an incumbent and sometimes not, depending upon which treatment "improves the fit of the equation." Revised economic data produced significant changes in several key coefficients, prompting renewed searching "to see which set of economic variables led to the best fit," and so on.

Of course, *a priori* political and economic considerations cannot neatly resolve all the specific questions facing data analysts in this (or any other) field. But they can suggest sensible directions and constraints. Certainly no sane political scientist would ever have estimated a constant partisan trend from 1916 through 1976, whether or not the resulting coefficient had a "significant" t-statistic. Now the partisan trend is gone, but Bush's surprising defeat in 1992 has inspired a new economic variable with a similarly dubious theoretical rationale: the number of quarters in which the growth rate is greater than 2.9 percent. (Why 2.9 percent? Of course, because that value "gave the best fit.")

Fair acknowledges, with charming understatement, that "Data mining is a potentially serious problem in the present context, given the small number of observations." Indeed it is!

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2 Fair himself refers to the accuracy of the latter "out-of-sample" forecasts as "the strongest evidence in the paper in favor of the new voting equation." I put "out-of-sample" in cautionary quotation marks to reflect the fact that Fair's model is clearly designed in part to fit the post-1960 data, so that his "out-of-sample" forecasts capitalize on chance in the selection of variables, though not in the selection of parameter values.

Given his apparent willingness to follow every jot and wiggle in the meager historical record, it is hardly surprising that Fair's model has been subject to so many nasty surprises and significant revisions -- or if his account of the process of data analysis brings to mind the title and moral of Leamer's (1983) plea, "Let's Take the Con out of Econometrics."

It may be worth noting in passing that a simple average of Fair's seven forecasts nicely matches the actual 1996 election outcome. Of course, a simple average is *too* simple; but it is not hard to imagine more sophisticated ways, including some at least approximately true to the spirit of Leamer's critique, of averaging the forecasts implied by these (and many other) alternative models of the presidential vote -- and of allowing for the uncertainty implied by the differences among them.[3] In the meantime, it is certainly worth bearing in mind the lesson Beck (1994) drew from his own review of Fair's and other election forecasting models: "We Should Be Modest."

At the same time, we should be clearer about what election forecasting models are for. If the point is literally to predict the outcome of an election in advance, one can certainly do better using current survey data in combination with economic variables rather than either one alone. (All of the models that did well in 1992 and 1996 included political variables omitted from Fair's models, and those that did best in the sense of forecast accuracy included political variables closely related to the actual vote, such as presidential approval ratings or election trial heats, as measured in contemporary surveys.)

Moreover, if what we really care about is who will be president, we should be forecasting not just the national popular vote but the state-by-state votes that determine the real election outcome in the Electoral College. Working with state-level data vastly increases the size of our

3 Bartels (1997) provides a discussion and application of techniques for model averaging inspired, in part, by Leamer's (1978) analysis of "specification searches."

data sets, but also raises complex issues regarding the structure of correlations among stochastic factors across space and time. Rosenstone (1983), Campbell (1992), and Gelman and King (1993) provide examples of state-level models employing a variety of political and economic variables.

In truth, however, most political scientists -- even those interested primarily in American electoral politics -- don't really care all that much about forecasting presidential elections. (I write as one who spent the most recent election night watching the New York Knicks, confident that news of the election outcome would reach me well before the winner was sworn in some ten weeks later.) What most electoral scholars really care about is what the relationship between economic conditions and election outcomes tells us about voting behavior and democratic accountability.

On that score, what have we learned, and what have we yet to learn? The clearest and most significant implication of aggregate election analyses is that objective economic conditions -- not clever television ads, debate performances, or the other ephemera of day-to-day campaigning -- are the single most important influence upon an incumbent president's prospects for reelection.[4] Despite a good deal of uncertainty regarding the exact form of the relationship, the relevant time horizon, and the relative importance of specific economic indicators, there can be no doubt that presidential elections are, in significant part, referenda on the state of the economy.

Most of the available evidence suggests that voters weigh recent changes in economic conditions more than temporally distant changes -- and more than absolute levels of economic well-being. It also suggests, though rather less clearly, that changes in disposable income matter

4 The implications of this fact for our understanding of campaigns and elections remain relatively unexplored, though Bartels (1992) provides some preliminary debunking of the more popular view that candidates, their consultants, and their campaign tactics are the most important factors.

more than changes in GDP (which are presumably less tangible), which in turn matter more than changes in unemployment (which produce relatively few direct losers) and inflation (which produce many losers but also a good many winners). However, we know much less about the policy implications of these facts, including the extent to which elected politicians actually succumb to the temptation to adopt what Tufte (1978) referred to as “myopic policies for myopic voters.”

Do economic conditions matter because people vote their own pocketbooks, or because they respond to changes in the whole nation's economic condition? The work of Markus (1988) and others has demonstrated that personal and national economic fortunes are both important. However, this demonstration does almost nothing to resolve the related question of whether voters' underlying motivations are selfish or altruistic. (Selfish voters could rationally base forecasts of their own future incomes on recent changes in the national economy, while altruistic voters could rationally base expectations regarding their fellow citizens' future economic fortunes on their own recent economic experience.)

Can voters untangle the complex contributions of the president and other actors to the making of government policy? Can they untangle the even more complex contributions of government policy, exogenous economic forces, and dumb luck to observed levels of economic growth, wage changes, unemployment, or inflation? Alesina, Londregan, and Rosenthal (1993) attempted to distinguish between “rational” and “naive” economic voting by estimating separate electoral effects for economic “shocks” and economic growth that was “predictable” on the basis of previous growth, partisan effects, and military mobilization. They found no significant difference between these potentially distinct effects, a result they interpreted (1993, 23) as “consistent with the hypothesis of naive retrospective voting.” Nevertheless, much work remains

to be done in investigating the average voter's grasp of how the political economy actually operates.

Finally, and perhaps most importantly, we know remarkably little about *why* voters reward the incumbent president for prosperity or punish him for economic distress. Do they have any rational basis for supposing that economic conditions in the election year are indicative of future conditions if the incumbent is reelected? (As far as I know, nobody has demonstrated such a connection.) Do they know or care what, if anything, the out party would do differently? Or, as Ferejohn (1986) and others would have it, are they simply holding up their end of a simple-minded implicit contract intended to extract whatever effort a self-interested incumbent may be able to exert on their behalf -- the only sort of accountability feasible in a situation marked by massive uncertainty and asymmetric information?

Fair's own evaluation of his latest model is that "It may be . . . better at explaining the past than the future. Time will tell. If the equation predicts the next two or three elections within two or three percentage points, there may be something to it. Otherwise, I will have to keep searching or do something else in my updating week every four years." By that standard, it is indeed time for a change of course. However, my own hope is that Fair will apply his considerable expertise not to additional searching for elusive forecasting magic, or to a different hobby altogether, but to joining in a broader assault on the many significant questions that remain unanswered in the scholarly literature on economics and elections.

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