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Review of *Unequal Democracy*

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Larry M. Bartels, *Unequal Democracy: The Political Economy of the New Gilded Age* (Princeton: Princeton University Press, 2008).

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A common refrain among political scientists is that we need to more forcefully engage the popular conversation about politics and policy. There is, in other words, a sense that we are not especially relevant. The reasons for this are numerous, but they can be boiled down to two. First, political scientists tend to be obsessed with technical and arcane issues and problems. Second, political consultants and self-anointed pundits have cozied up to the news media and cognoscenti and whispered sweet nothings into their ears. Put even more simply, news providers have found experts who do a better job than political scientists at responding to their needs.

A few political scientists have managed to become media darlings. Even fewer have done so while representing the best that political science has to offer. Larry Bartels' most recent effort, *Unequal Democracy*, is the kind of book, by the kind of political scientist, that quite a few people in our discipline have been waiting for. It is thoughtful, provocative, and analytically rich. Statistical models are judiciously employed; they serve the story rather than distracting the reader. The prose is steady, casual, and easy to follow. And while I have some quibbles with a few of his arguments, Bartels' thesis provokes the kind of big picture questions with which we need to be engaged. Indeed, the fact that Bartels has been debating Thomas Frank, author of *What's the Matter with Kansas?*, and has become an intellectual partner with famed economist Paul Krugman buttresses this interpretation.

So what is it that Bartels does in *Unequal Democracy*? Bartels begins with the simple premise that "politics matters." For those of us who study campaigns and elections, there is some irony here in that Bartels is often associated with the view that campaigns have "minimal effects" on presidential election outcomes. But contending that campaigns are overrated is not the same as saying politics doesn't matter, and here Bartels defends politics—especially partisan differences—as critical to macroeconomic outcomes. More specifically, he presents data showing that real income growth rates are higher under Democratic administrations than Republican administrations. He then goes on to demonstrate that income inequality—again, as measured by differences in income rates—grows much more rapidly under Republican administrations. These twin “facts” dominate the first two chapters of the book, and lead Bartels to spend the remaining chapters (3 through 9 anyway) exploring why Republicans do pretty well in presidential elections despite the consequences of their policies.

Although those on the left may find it obvious that Democratic presidencies produce robust growth, particularly for the middle and lower classes, while only the affluent benefit under Republicans, I found the magnitude and consistency of the relationship stunning. Bartels clearly understands the potential contentiousness of this finding as well, and so he spends much of chapter 2 exploring how the relationship is affected by a range of alternative explanations. The bottom-line is that not much seems to diminish the fundamental finding that Democratic presidents are better stewards of the economy than Republicans. As a consequence, I think Bartels shifts the burden to the Republicans, and to conservatives more generally, to disprove (or at least reconsider) the relationship.

It is beyond the scope of this review to attempt an alternative analysis, but I do think it important (and fair) to question this finding at three levels. First, the nature of the relationship between presidents and economic performance demands a theoretically and methodologically compelling estimate of the lag term. That is, how long into an administration before we hold the president accountable for economic conditions? Bartels argues for one year, saying that this makes sense practically and that this lag offers the best fit of the data. I don’t have a strong sense of when a president’s policies begin to drive economic indicators. Perhaps the one year marker makes sense; perhaps not. Maybe it works this way sometimes, but not at other times. In addition, I’m not entirely sure what the best fit refers to in this context; presumably, this means a one-year lag explains the most variance and produces the strongest coefficient measuring the relationship between partisan control of the White House and economic performance. But I’m not sure we should be picking the lag term based on the extent to which it proves the partisan relationship we are supposedly testing.

The nature of this lag term was discussed in an email exchange between James Campbell and Alan Abramowitz last spring. Campbell posted the following:

> *I looked at both the mean and the median quarterly percentage change in the real GDP according to the most recent Bureau of Economic Analysis data. Without a lag for when the president really has responsibility, as Kinsley and Martinez found, there is the reported illusion of a better Democratic record: Democratic mean of 4.3 GDP growth vs. Republican mean of 2.8 GDP growth (medians are closer 3.7 v*

> *2.8). With a one year lag, there is still the appearance of a better Democratic record: Democratic mean of 3.9 to Republican mean of 3.1 (medians are again closer 3.4 v 3.0). Even if you use just the one year lag, with or without controls for lagged GDP growth, the party of the president does NOT have a statistically significant effect at .05 (one-tailed and adjusted R-square of the simple bivariate equation of .004 (N=242))…. With an 18 month (6 quarter) lag, the parties have essentially equivalent records: Democratic mean of 3.47 to Republican mean of 3.43 (by the median performance, Republicans have a slightly stronger record—3.1 median for Democrats vs. 3.5 median for Republicans).*
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> *Since 1947, there have been four transitions from a Democratic president to a Republican president: Truman to Eisenhower in 1953, Johnson to Nixon in 1969, Carter to Reagan in 1981, and Clinton to Bush in 2001. In those transition years, 7 of the 16 quarters (44 percent) exhibited shrinking real GDPs (the percentage of all post-1947 quarters with GDP declines was only 15 percent). The median GDP growth was an anemic 1.2 percentage points. In contrast, none of the twelve GOP-to-Dem transition year (1961, 1977, and 1993) quarters had shrinking GDPs (one quarter was zero) and the median growth was a robust 5.2 percentage points.*

Without choosing sides, the point here is simply that the appropriate lag term could be critical to Bartels' main findings.

This discussion leads to a second issue: Bartels relies almost exclusively on real income growth as a measure of economic performance. There is, to my mind, nothing wrong with this measure. In fact, given that Bartels is interested in inequality, it seems both obvious and appropriate. But might other measures show a more subtle or even different story? Two small points should be made in this vein. Initially, I would contend that income mobility is at least as important to the collective American psyche as income inequality. That is, most Americans aren't bothered by the fact that some people have so much while others do not, so long as they believe they can rise to the top by their own efforts. In this way, the key isn't the amount of wealth in the *Forbes 500*; rather, it is the permeability of the top echelons of the income distribution. Second, inequality may not be as important as the "floor." That is, most Americans probably don't care much about purchasing power of the very rich so long as the working and middle class can afford big-screen TVs, SUVs, vacations, and can send their children to a subsidized state university. This diminishes the sense of grievance among those not at the top of the earnings pyramid. The real issue could thus be that the "floor" is getting lower for many Americans, but that is not what is measured here. I don't know if looking at these aspects of the economy hurt or reinforce Bartels' argument, but I am interested in them. (To be fair, Bartels discusses average income, unemployment and GDP rates and they are consistent with this bottom-line. He also briefly considers income mobility.)

A third issue involves proper controls for alternative explanations. Bartels carefully considers oil prices, labor force participation, and over-time trend terms in his models. It is entirely possible that Bartels also tested other potential variables and found them lacking, but I would have liked to see the impact of (for example) military engagements or stock markets shocks. It would also be instructive to test additional political variables: how does unified versus divided control of the federal government affect things (i.e., does a Democrat with a Democratic Congress do especially well?).

In chapter 3, Bartels turns to electoral politics and takes up the claim that lower class voters have become more Republican since the 1980s (and, really, since the 1960s) because of so-called "values" issues. In doing this, Bartels directly confronts Thomas Frank's best-selling book, *What's the Matter with Kansas?* Bartels argues that lower income voters are still economic voters, and that cultural issue effects are over-stated. But if cultural issues do not explain why lower status whites have become more Republican, what does? In chapter 4, Bartels contends that voters, especially those at the lower rungs of the income distribution, are disproportionately sensitive to the economic success of those at the very top end of the income distribution. They are, conversely, relatively insensitive to income inequalities and more appropriate indicators of economic performance. This "false consciousness" is exacerbated by the spending advantages of incumbents, in general, and Republican incumbents, in particular.

As someone who is concerned with the minutia of politics and elections, these two chapters are near and dear to my heart. More to the point, they are thought-provoking and, in many ways, compelling. My main concern is that Bartels does not really engage Frank's argument on its own terms. The point of *What's the Matter with Kansas?* is not simply that lower-class voters care about values issues: it is that voters are influenced by context, and that context depends greatly on time and location. In this sense, I read Frank's book as being about people who live in rural and "micropolitan" settings, and not simply about lower income people.[1] In focusing on lower income voters, Bartels smartly adds to what we know about the role of class and economic voting in contemporary politics. But I am less sure that he directly engages the question of values voting. In fact, he cannot address these issues with the data at hand given the design of the National Election Study (NES). The NES does not include adequate sampling

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[1] Interestingly, in a recent exchange with Bartels, Frank indicates that his argument is really about education, not income. Based on this, maybe I read more subtlety into Frank's thesis than he intends.

units from rural and micropolitan areas (the cost of doing NES's face-to-face interviews in these areas is astronomical). This observation isn't so much about *Unequal Democracy* as it is about the need for the discipline to more seriously consider our knowledge of some of these under-represented and difficult to survey populations.

Chapter 5 offers a straight-forward analysis of whether or not Americans care about inequality. The evidence is mixed, with ideology and awareness conditioning these attitudes (i.e., highly aware conservatives are much less concerned, highly aware liberals are much more concerned). This chapter is quite important in that Bartels' argument has often been mischaracterized: he does not say that people support policies they should not because they are ignorant or uninformed. What he explicitly says in chapter 5 is that those who are more informed are more likely to be sensitive to cues coming from ideologically compatible elites (there is a heavy nod to Philip Converse and John Zaller here). In the case of taxes and inequality, he suggests that cues from conservative elites are misleading or misinformed, and produce opinions that are at odds with broader attitudes towards inequality. The data Bartels presents are detailed and largely persuasive, but I am still a bit dubious that Americans care all that much about inequality. In this sense, I am not sure how disturbed we should be about the susceptibility of voters to some of the persuasive effects charted here.

Chapters 6-8 offer case studies on the relationship between public policies and public opinion. Bartels examines the 2001 and 2003 Bush tax cuts (chapter 6), the campaign to repeal the estate tax (chapter 7), and the erosion of the federal minimum wage (chapter 8). Chapter 9 then offers a broader analysis of policy-making and politics across a range of issues. Of these chapters, I find chapter 6 ("Homer gets a tax cut") the most interesting. It is, I think, the best analysis and articulation of Bartels' more general thesis that people can be misinformed in a way that leads them to support policies at odds with their underlying interests and attitudes.

In explicating the case studies, Bartels spend a great deal of time pointing out how each of the policies at hand—tax cuts, repealing the estate tax, and raising the minimum wage—is good or bad for voters. He is careful to define "good" and "bad" in terms of income inequality, but it is impossible not to draw wider normative conclusions about the efficacy of Democratic proposals compared to those of the Republicans. By the end of chapter 9, a Republican reader is likely to feel him or herself pummeled into submission—support for tax cuts or opposition to increasing the minimum wage will have a horrendous impact on you and the U.S. economy, and the only reason you support these positions is that you have been duped by political elites who are in the pockets of the special interests (the true beneficiaries of these policies). Voting for Republican presidential candidates is thus a terrible thing that will ultimately doom America to oligarchy (and perhaps oblivion), but voters are not really to blame.

The relentless thrust of the analysis is bound to produce skepticism (even Krugman, in a blog entry, expressed shock at the nature of the macroeconomic-political relationship estimated in chapter 2). I, for one, am skeptical about attributing economic interests to specific survey respondents based on a reading of aggregate economic trends. I am also a little skeptical about glossing over variance in the different economic policies of different administrations, except to note how Republicans who briefly pursue more liberal policies see a concurrent rise in economic indicators (see the account of Nixon and wage and price controls in 1970-71). Finally, I am skeptical about the universal efficacy of Democratic economic policies over time when the economy is constantly changing. Richard Florida and others have chronicled the rise of the “creative class,” but this economic revolution and the concurrent rise of the high-tech and dot.com industries supposedly doesn’t come close to affecting income compared to whether George H.W. Bush or Bill Clinton is in the White House? Surely politics matters. But it seems equally likely that relationships—especially partisan relationships—are more complex than Bartels’ work would have us believe.

This skepticism should be read as testimony to the provocative nature of the book.[2] All told, Bartels is to be applauded for his careful scholarship (given his previous work, we would expect nothing less from him). In particular, Bartels succeeds by defining a good to be maximized (income growth and the reduction of inequality), measuring it, and estimating the impact of politics on delivery of this good. And his constant attention to broader issues of accountability and democratic functioning are commendable. In short, there are few people in the discipline who are as good as Larry Bartels, and fewer still who are better. This book challenges, prods, and pokes the reader to consider the power of politics. *Unequal Democracy* is a thus force to be reckoned with; students of partisan politics and public policy, as well as casual consumers of politics should read it carefully.

[2] As Reggie Jackson said when asked about Yankee fans razzing him, “They don’t boo nobodies.”