PARTY PLATFORMS, MANDATES, AND GOVERNMENT SPENDING

*I*n their 1990 Review article, Ian Budge and Richard Hofferbert analyzed the relationship between party platform emphases, control of the White House, and national government spending priorities, reporting strong evidence of a "party mandate" connection between them. Gary King and Michael Laver successfully replicate the original analysis, critique the interpretation of the causal effects, and present a reanalysis showing that platforms have small or nonexistent effects on spending. In response, Budge, Hofferbert, and Michael McDonald agree that their language was somewhat inconsistent on both interactions and causality but defend their conceptualization of "mandates" as involving only an association, not necessarily a causal connection, between party commitments and government policy. Hence, while the causes of government policy are of interest, noncausal associations are sufficient as evidence of party mandates in American politics.

In a recent article in this journal, Ian Budge and Richard Hofferbert (1990) analyze the relationship between U.S. party programs and federal government expenditures. Their purpose is to assess what they describe as "the doctrine of the party mandate, according to which the competing parties offer to the voters different government programs between which they can choose. The party which attracts the most votes on this basis then forms the government, but it is bound (both morally and by fears of retribution at the next election) to carry through the program on which it has been elected" (p. 111).

The authors operationalize party policy in particular issue areas using a content analysis of policy "emphases" in party electoral programs, described extensively by Budge, Robertson, and Hearl (1987). Their basic intention is to show that government spending in particular issue areas is related to party policy thus measured. They do this by specifying and estimating a series of linear regression models.

Budge and Hofferbert state their conclusions very clearly. They talk of the "remarkable success" of one of their models and a "stratospheric" R-squared associated with another (1990, 122). This leads them to conclude that "party government in the United States works largely as mandate theories say it should" (p. 130). This is a remarkable claim, especially given that decades of research on political parties has demonstrated again and again that American parties are more diffuse, porous, and less programmatic than those in most other countries (see Epstein 1986 and the citations therein). Strong evidence that the mandate theory applies to the United States would mean that hundreds of observers of American politics were wrong. It would not be the first time, but it would be very surprising.

In what follows, we do not evaluate the reliability or validity of Budge and Hofferbert's data, the general justification of their models, or the statistical techniques used to estimate them. Instead, we make three more-specific points. First, with a few trivial exceptions, we find that Budge and Hofferbert's results can be replicated very easily. This is no minor achievement for the authors, inasmuch as it is often difficult or impossible to replicate empirical research

published in the social sciences (see Dewald, Thursby, and Anderson 1986). Second, we show that the form in which Budge and Hofferbert present their analyses makes it impossible to evaluate their key hypothesis, because they do not provide enough information to calculate the appropriate standard errors or test statistics. We present their original results in an improved format, along with the corresponding correct standard error. Our re–presentation of their results indicates that fewer than half of their tests of their preferred specification support their hypothesis. Finally, we show that the few stratospheric coefficients that these authors report are to a large extent a product of their implicit implausible assumption that a new party achieves every budgetary desire immediately in its first budget cycle. We show that under more plausible and empirically verifiable assumptions, only a few of the many analyses support the authors' hypotheses. Moreover, even these rare positive results imply relatively modest causal effects, consistent with prior literature. This methodological improvement may also explain the anomalous signs associated with the coefficients in several of Budge and Hofferbert's published models.

METHODOLOGY

We focus on Budge and Hofferbert's most favored model for each of the 12 policy areas in their study. This is "Model 5, the general program model, positing dominance by the platform of the occupant of the White House" (1990, 129). Budge and Hofferbert specify this for each policy area as follows:

$$E(Y) = \beta_0 + \beta_r R + \beta_d D + \beta_{pr} RP + \beta_{pd} D(1 - P), \quad (1)$$

where Greek letters are parameters to be estimated; Y, the percentage of federal government spending in a policy area two years ahead (the first time a new president can influence the budget);[1] R, the percentage of the Republican platform devoted to a policy area matched to the spending area; and D, the same percentage for the Democratic party platform. P

FIGURE 1

Trends in Federal Spending
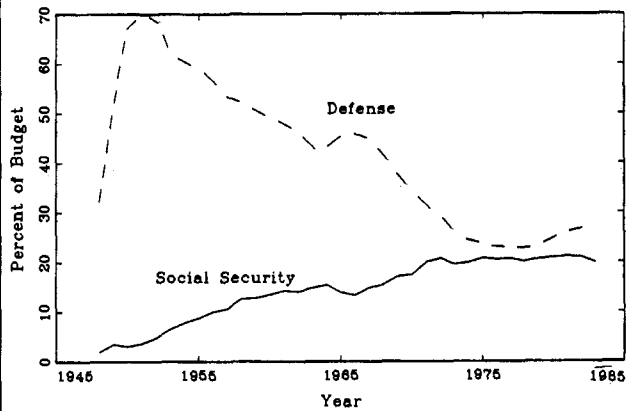
Source: Budge and Hofferbert 1990.

equals 1 for Republican presidents and 0 for Democratic presidents.[2]

In their tables 3–6, Budge and Hofferbert present estimates of the parameters of equation 1 in each policy area. The theoretical focus of their article is the effect of the party platform of the winning presidential candidate on the federal budget expenditures. Thus, the key parameters of interest are $(\beta_r + \beta_{pr})$ for Republican presidents and $(\beta_d + \beta_{pd})$ for Democratic presidents.[3] One can easily obtain these point estimates from their article by addition, but the standard errors necessary for evaluating these point estimates cannot be derived from their tables. Indeed, Budge and Hofferbert's article provides no appropriate assessment of the uncertainty of their most important empirical evidence.[4]

We also address a serious problem in their analysis of these data—their explicit decision to ignore the clear secular trends in most of the budget figures they analyze. Thus, their figure 1 describes a "hypothesized relationship between governing party . . . policy emphasis and annual spending" that is essentially one of trendless fluctuation in party programs, correlated with lagged spending patterns (1990, 118). The authors claim that "this relationship would hold whichever way the lines rotated in space, though for simplicity no long term upward or downward trend in either expenditures or emphases is shown" (p. 117). In practice, of course, there are some very pronounced time trends in the real, as opposed to the hypothetical, data. Figure 1 shows this for two of the major federal budget policy areas, defense and social security. The problem is that Budge and Hofferbert's statistical model fits neither their hypothetical figure (rotated in space or otherwise) nor their data. Their decision simply to regress spending against party policy incorrectly assumes that each observation is conditionally independent of all others.[5] That this independence assumption is false is easy to verify by calculating the Durbin–Watson statistics (or other tests of autocorrelation) from their original regres-

sions; our reanalyses indicate a textbook case of strong autocorrelation. Indeed, regressions with trending variables, very high R-squared values, and bad Durbin–Watson statistics are exactly the situation that Granger and Newbold (1974) described when they coined the term "spurious regression."

In other words, the goal of their analysis is to study what would happen to the federal budget in a policy area that received more attention by a party (as measured in its platform) while everything else was held constant. The fundamental problem in evaluating Budge and Hofferbert's claim is finding instances of party platforms that differ in the amount of attention given to a policy area for which "everything else" is constant. Indeed, because federal budget figures trend so strongly, these instances do not exist, so that the problem, in these and most other time series data, must be solved with statistical controls (given that it is not possible to replay four years of a presidency while changing only party emphases). From a substantive perspective, this can be interpreted as failure to control for a key explanatory variable—the history of their dependent variable the year prior to the party convention. This variable easily meets the requirements for producing omitted-variable bias: (1) it is correlated with the included variables (party platforms), (2) it influences the dependent variable, and (3) it is causally prior to the key explanatory variable. Thus, all the coefficients estimated in their paper and replicated in columns 1 and 3 of our Table 1, are biased.

Substantively, autocorrelation is equivalent to tricking yourself into believing that you have more information than you really do. This is the case when knowing last year's budget figures account for a large part of this year's. Ignoring this problem causes them significantly to underestimate their standard errors. To illustrate, if the unit of analysis in their original specification is changed to the presidential administration, the regression based on these nine quadrennial observations produces identical coefficients to their results but much larger standard errors (since there is no variation in platforms within administrations). (Indeed, if their unit of analysis were changed to monthly or hourly budget figures, their coefficients would also be identical, but the standard errors would drop considerably.) They openly worry about part of this problem, which they call the "double barrelled data extender," but the problem is not with how the data are organized. The problem is entirely with their specification, which picks up no more information from their 36 annual observations than do the corresponding 9 observation quadrennial data set. Only a specification with some sort of time series process will find additional information in the "extended" data set.

Budge and Hofferbert discuss and then dismiss the idea of detrending their variables as a possible solution to this problem. They are probably correct in deciding not to detrend, but the lack of any solution leaves the problem they recognized unresolved. Fortunately, it is easy to correct for both nonstationarity

and autocorrelation in these data. We do so now by adding only *one* extremely plausible assumption to their model, namely, that winning parties may not get everything they want all at once.

Budge and Hofferbert's idea is that political parties have a *preferred* level of budget spending in a policy area in year t, which we label as Y_t^P. This is determined by (and thus equal to) the right side of equation 1. Our additional assumption is that parties may only be able to adjust budget spending by a fraction α $(0 < \alpha < 1)$ of what they want toward their preferred level on average in each year:

$$E(Y_t) = Y_{t-1} + \alpha(Y_t^P - Y_{t-1}).$$

This "partial adjustment model" shows that on average the actual level of spending in the first year a new president can influence the budget is equal to last year's spending plus an adjustment toward the winning party's preferred spending level. For clarification, this equation can be written as

$$E(Y_t) = (1 - \alpha)Y_{t-1} + \alpha Y_t^P,$$

which shows that expected spending is a weighted average of last year's actual spending and this year's preferred level. The situation where $\alpha = 1$ is exactly Budge and Hofferbert's assumption that a winning party definitely gets everything it wanted all at once in the first year. Our alternative assumption that winning parties may not get all they want in the first year is correct if α is less than 1, an assumption that we can test for. Finally, we can rewrite the equation once more by substituting for Budge and Hofferbert's definition of what constitutes the preferred level of spending

$$E(Y_t) = \beta_0^* + \beta_r^* R + \beta_d^* D + \beta_{pr}^* RP$$
$$+ \beta_{pd}^* D(1 - P) + \beta_y Y_{t-1},$$

where $\beta_y = 1 - \alpha$ (see equation 1) and where the asterisks are included to indicate that these coefficients may differ from .those in equation 1. Thus, by adding only the assumption that newly elected parties may not get all that they want in spending immediately upon taking office, we derive a model that requires adding lagged spending to Budge and Hofferbert's list of explanatory variables. This assumption is correct if $\alpha < 1$, which we can evaluate by testing whether $\beta_y > 0$. This model also eliminates the omitted-variable bias, most of the problems with autocorrelation and nonstationarity, and the "double barrelled data extender" problem with defining the unit of analysis.[6]

RESULTS AND CONCLUSIONS

We provide revised coefficients and standard errors in Table 1. The first two columns of numbers provide estimates and standard errors of the effect of Republican platform emphases when the Republicans win the presidency $(\beta_r + \beta_{pr})$ and Democratic platform

emphases when the Democrats win the presidency $(\beta_d + \beta_{pd})$ for each policy area. Each of these numbers is interpreted in the same manner: for every one-percentage-point increase in the amount of platform content spent on a policy area, the coefficient is an estimate of how much the federal budget in this area will increase. Budge and Hofferbert say clearly that under their preferred model, there is "an assumed positive relationship between program emphases and expenditures when a party holds the presidency" (1990, 120). They hypothesize, more specifically, that $(\beta_r + \beta_{pr})$ and $(\beta_d + \beta_{pd})$ should be positive.

As is plainly clear from this table, only 10 of the 24 coefficients in the first two columns are two-or-more standard errors greater than zero. Thus, without any modification at all in the data, methods, or models from their original article, we find that Budge and Hofferbert's mandate theory fails in a majority of the policy areas they examined. As an example of how to interpret these results, a 10 percentage point increase in Republican platform emphasis on education should lead to about a 3.8% increase in the proportion of the federal budget devoted to education when the Republican wins (plus or minus the standard error of .09); the effect for Democratic presidents is about three times this figure.

The right side of Table 1 also presents the results of a reanalysis allowing for the possibility that newly elected parties do not get all that they wish immediately, and the differences between this and their uncorrected analysis are quite striking. First, note the rightmost column in this table, which presents the coefficients on the lagged value of the dependent variable for our revised model and hence a test of whether our additional assumption is correct. In all but possibly one case, the coefficients are clearly greater than zero, indicating that indeed, presidents do not achieve their preferred level of spending immediately. Columns 3 and 4 report the coefficients that are directly comparable in their specification to the ones in columns 1 and 2, respectively. Whereas 10 of the 24 coefficients in their original specification were at least two standard errors greater than zero, only 4 of 24 meet this same criterion in our corrected analysis. In addition, about 1.2 of these four "significant" coefficients would be greater than zero as a result of chance alone. In addition, the coefficient for Democratic platform defense emphases (one of these four) seem extremely counterintuitive. It indicates that an increased Democratic emphasis on defense raises the proportion of the budget devoted to defense in Democratic administrations about 15 times more than increased Republican platform emphases do when the generally more hawkish Republicans take the White House. Moreover, virtually all of the estimates from Budge and Hofferbert's specification are reduced substantially in ours, most commonly by about half.

We have shown here that the evidence that Ian Budge and Richard Hofferbert present in their article usually does not support the mandate theory that they favor. Moreover, a respecification of their model

TABLE 1

Effect of Winning Party Platforms on Federal Expenditures

ISSUE AREA	ORIGINAL SPECIFICATION		REVISED SPECIFICATION		
	REPUBLICAN PRESIDENT	DEMOCRATIC PRESIDENT	REPUBLICAN PRESIDENT	DEMOCRATIC PRESIDENT	COEFFICIENT ON LAGGED BUDGET
Social security	1.923 (1.669)	−1.318 (1.194)	−.064 (.379)	−.142 (.273)	.893* (.037)
Health & medicare	3.426* (1.229)	−3.285 (.879)	.207 (.170)	−.159 (.132)	.965* (.022)
Income security	−.047 (1.348)	−1.768 (.964)	−.494 (.481)	−.392 (.358)	.874* (.060)
Education & Employment	.381* (.090)	1.213* (.086)	.197* (.050)	.442* (.101)	.657* (.073)
Defense	−2.450 (1.083)	6.872* (1.828)	.213 (.289)	3.322* (.537)	.907* (.044)
International affairs	−.094 (.126)	1.015* (.213)	−.020 (.057)	−.284 (.165)	.741* (.099)
Natural resources	.104* (.040)	.072 (.068)	.090* (.020)	.041 (.035)	.689* (.096)
Energy	.064 (.038)	−.073 (.037)	.025 (.015)	−.051 (.014)	.844* (.065)
Transportation	.294* (.064)	.162* (.061)	.006 (.052)	−.037 (.043)	.911* (.115)
Community rehabilitation	.225* (.042)	−.064 (.041)	.056 (.032)	−.019 (.024)	.762* (.097)
Commerce & housing	.103 (.071)	−.003 (.068)	.084 (.075)	−.011 (.069)	.200 (.185)
General government performance	.110* (.050)	−.196 (.032)	.075 (.045)	−.083 (.044)	.441* (.152)

Note: Entries in the first four columns of numbers are estimated effects of platform emphases, when the specified party wins, on federal government spending in the given policy area. The first two columns are replicated results from Budge and Hofferbert 1990, calculated as $\beta_r + \beta_{pr}$ for Republican presidents and $\beta_d + \beta_{pd}$ for Democrats. Columns 3 and 4 are calculated the same way from a regression that also includes a lag of spending as an additional explanatory variable. When the coefficient on this lagged variable, which appears in the final column, is greater than zero, it confirms our alternative assumption that newly elected parties may not have all their influence on the budget in the first year. Throughout, standard errors appear in parentheses.
*denotes a coefficient that is at least two standard errors greater than zero.

to account for the possibility that presidents do not immediately get all that they want obliterates most of the remaining apparent effects. In short, the stratospheric coefficients and strong relationships reported by Budge and Hofferbert are to a large extent methodological artifacts of a failure to correct for the effect of secular trends in the data. Budgets almost always change incrementally, and any analysis of the relationship between political variables and budgetary allocations must be sensitive to the substantive and methodological effects of this pattern on the results.

Although this reanalysis is consistent with considerable prior research about the diffuse nature of American political parties, we do not wish to imply that there is no connection between party positions and federal priorities. Instead, we agree that the connection probably does exist: it is just far weaker

and more subtle than could be perceived with these data and methods.

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We are grateful for the care brought by Gary King and Michael Laver to their critique of our article (Budge and Hofferbert 1990). That research has something very important to say about party democracy. The data open a new and important avenue of comparative research. At the same time, King and

Laver have performed a service in pointing to certain key methodological questions that, if left unattended, could quite possibly result in doubt about the findings. We conclude, however, that a consideration of their critique leaves largely undisturbed the original claim to having demonstrated the operation of a party mandate in the American national policy process.

Although King and Laver refer to several considerations, two are key to their critique: (1) the use and interpretation of interactive terms and (2) the need to incorporate a lagged dependent variable on the right side of the equation.

INTERACTIVE TERMS

Interactive terms can be confusing. Generally the confusion arises (as it did in this case) because the coefficients indicate the *relative* effect of a variable. For example, the equation for our primary model takes the form

$$Y = a - b_1 R - b_2 D + b_3(R \times P_r) + b_4(D \times P_d) + e,$$

where all variables are as defined in the original article. The b_1 and b_2 coefficients tell the reader the policy priority effects (perhaps we should say "translation rates") of the Republican and Democratic platforms when that party loses the presidency. The b_3 and b_4 tell the reader the *difference* in the translation rate of the Republican and Democratic platform, respectively, when that particular party wins *relative* to when it loses. Thus, the b_3 and b_4 terms allow us to address (with associated reliability tests via the respective standard errors) what difference winning makes in a party's ability to translate its platform. To wit, we find, in 13 of the 24 instances reanalyzed by King and Laver, winning matters for positive translations of a party's platform emphases into expenditure priorities. This, we think, is important.

At the same time, the original article did not consistently phrase the argument in a manner so as to sustain attention to the relative nature of these elements in the equations. Thus, one can compare the following two statements:

> The contrast in parameter estimates for the untransformed term (R or D) and the matching interactive term (R × P) or (D × P) shows the *relative* gain in programmatic impact from holding the presidency. (p. 120, emphasis added)
>
> Thus, when they are *out* of the White House, for example, for every percentage change in the proportion of the Republican platforms devoted to social justice themes, we could have expected a 7.3% shift in social security's share of federal spending in the opposite direction. But when the Republicans are *in* the White House, each 1% up or down on social justice is directly reflected in a 9.7% shift in social security's share of outlays. (p. 123, emphasis original)

The first statement is a reasonably clear and correct summary of how to interpret the interactive terms in the total equation. The second statement is misleading, in that it does not clearly reflect the *relativity* of

the translation process. We should have been much more consistent in our rhetoric.

King and Laver chastise us for not reporting the standard errors for the combination of the (b_1 and b_3) and (b_2 and b_4) terms. These combined terms are estimates of the translation rates for the Republican (b_1 and b_3) and Democratic (b_2 and b_4) platform emphases into spending priorities, given that the particular party has won. Their results show that 10 out of 24 times, a party's platform emphases have a statistically significant and positive translation into spending emphases, given that it has won. The three instances of positive effects in our report and null effects in theirs are a consequence of our analysis' taking into account that losing would have a statistically significant and negative effect, which is erased but not turned positive when a party wins.

We must agree that King and Laver raise important questions. Still, two points are worth the readers' attention. First, while accepting their point of qualification, we do not see it at all as disconfirmation. In 13 instances, winning matters for the translation of a party's platforms within the model. And in 10 of those instances the party's platform has a net positive effect, given that it has won. Second, and more important, one must recognize that a positive coefficient for the winning party's favored policy does not necessarily mean that the net spending share *increases* when the party takes office. For that to be the expected outcome, the effect of a platform when a party wins must be greater than any negative effect associated with the losing party's platform. If the apparent policy *rejection* by the incumbents of their opposition party's platform emphases is sharper than the *acceptance* of their own, the sum may indeed be a net reduction. Often, the mandate appears to operate in both directions (i.e., a positive translation of the winning and a negative translation of the losing party's platform), with neither being more or less advantageous for mandate theory. The logic of the positive and negative facets of the mandate is fully explained and explored by Klingemann, Hofferbert, Budge, et al., in our forthcoming 10-country study.

LAGGED DEPENDENT VARIABLES

Contrary to King and Laver's caution, our research did not include a lagged dependent variable on the right side of the equation. Whether it should have done so depends (as do all answers to questions of research design and specification) on the purpose of the analysis. If the purpose had been to test whether one could identify the platforms as a causal agent of spending priorities, then attempts to control for the recent level of spending priorities would have been in order. Such was not the purpose, despite the causal language into which the text occasionally slipped (e.g., in the discussion of Figure 1 and the decision not to detrend). On the question of cause, the article is explicit: "It is enough for mandate theory that expenditures should *correspond* to emphases rather

than there should *necessarily* be a cause and effect relationship between them" (p. 118, emphasis original).

King and Laver disagree and therefore offer a causal specification for testing the relationship between platforms and spending priorities. A byproduct of their specification is that the congruence of trends or cycles of policy and platform emphases is irrelevant to mandate theory. This is not true, however; the similarity in cycles and trends are quite important. Imagine the logical extreme of their argument. Under their specification it is possible for platform emphases and expenditure priorities to follow opposite trends while still yielding the statistical impression that the platforms have a positive impact. This could happen when the platform and policy *deviations* from such contrasting trend lines are in a similar direction for each interelection period. In order for this to be a version of the mandate thesis, a party would have to say to itself, "We're saying less about this policy than we said a couple of elections back, but a bit more than the decline in our trend; therefore, we should spend relatively more." This reflects no notion of the mandate thesis of which we are aware. Therefore, while suitable for a causal analysis of spending, King and Laver's specification is not acceptable as a specification for examining the mandate thesis.

Their specification also raises the possibility of a false negative for the substantive theory of party. If we control through the lagged dependent variable, we in effect provide a proxy for all past influences on policy. One of those past influences, in years 2–4, is the platform itself. Since any policy associated with the platform is expected to hold through a four-year period, this proxy includes the effects of the platform for every year but the one transition year in each quadrennium.

So where does the conjunction of our and their findings put our understanding? In several policy categories, the winning party's platform emphases correspond to spending priorities (our finding), and in almost all of those categories, the correspondence is the apparent consequence of similar trends and cycles (their finding). Because the similarity in trends must be retained as part of any statement of the mandate thesis, the mandate thesis stands. Thanks to King and Laver, we know that it stands as a consequence of the trends, not just in part, but almost in its entirety.

The key question for the research is, Does government policy move in the direction indicated by the platforms? This is the key question of mandate theory. The research finds that the administrations give higher spending priority to the things emphasized in the incumbent president's party platform. It might indeed be that policy would have been the same regardless of the platform. But then the losers' and winners' platform emphases both ought to predict policy positively in the same way, which they do not.

How can that be explained? We agree with King and Laver that there are many causal forces driving

policy. But the winners' platforms predict policy positively, the losers', often negatively; the opposite statements are generally not true. This returns us to our first point. We assert that the parties *in* government have a mandate and that the ones *out* do not. That is all we are saying. But we think that is quite a lot.

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Notes

1. Budge and Hofferbert describe this variable as being "lagged 2 years"; but from their more detailed description, it is apparent that they are using the term *lagged* in a nonstandard way. For an explanatory variable at year t, the same row in the data matrix has Y at time $t + 2$.

2. When comparing equation 1 with the equation originally published by Budge and Hofferbert, note that we have added conditional-expected-value notation and subscripts to the regression coefficients. We also use P and $(1 - P)$, instead of Budge and Hofferbert's separate notation for each.

3. To see this, note that we can rewrite equation 1 as $E(Y) = \beta_0 + \theta_r R + \theta_d D$, where $\theta_r = \beta_r + \beta_{pr} P$ is the effect of R and $\theta_d = \beta_d + \beta_{pd}(1 - P)$ is the effect of D. Budge and Hofferbert are interested in θ_r when the Republicans win ($P = 1$) and θ_d when the Democrats win ($P = 0$).

4. Using b with the appropriate subscript to refer to the least squares estimate of the corresponding β parameter, the variance of the sum $(b_r + b_{pr})$ is $V(b_r + b_{pr}) = V(b_r) + V(b_{pr}) + 2C(b_r, b_{pr})$. The first two terms on the right side of this equation are the squares of the standard errors implied by the asterisks in their tables, but the covariance in the last term cannot be derived from information in their article. Alternatively, one could regress Y on $R(1 - P)$, DP, RP, and $D(1 - P)$. The coefficients on these variables are then the effects of the Republican platform when the Republican's lose, the Democratic platform when they lose, the Republican platform when they win, and the Democratic platform when they win, respectively.

5. More specifically, they assume, incorrectly, that the disturbance term is composed of independent random variables with mean zero. In fact, both the independence and the mean-zero assumptions are probably false. Given the obvious time trends in both dependent and independent variables, the technique the authors use is unable to distinguish between the possibilities that the pattern described in their figure 1 is true (but sloping upward) and that it is systematically false (but sloping upward).

6. We present revised estimates using these lagged values of the dependent variable but note that many other time series specifications may also be plausible. If more observations become available, it might be possible to do more detailed analyses in order to be more confident of the specific time

series process in these data. For the present, we know that there is some time series process and the numerous studies of budget figures usually indicate low-order autoregressive processes, so that merely including a lagged value of the dependent variable will control for a good portion of this phenomenon.

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