Estimating the Electoral Effects of Voter Turnout

Forthcoming at *American Political Science Review* (May, 2010)

Thomas G. Hansford
Associate Professor of Political Science
School of Social Sciences, Humanities, and Arts
University of California, Merced
5200 North Lake Rd.
Merced, CA 95343
thansford@ucmerced.edu
http://faculty.ucmerced/edu/thansford/

Brad T. Gomez
Assistant Professor of Political Science
Department of Political Science
536 Bellamy Building
Florida State University
Tallahassee, FL 32306
bgomez@fsu.edu
http://myweb.fsu.edu/bgomez/

An earlier version of this paper was presented at the Annual Meeting of the American Political Science Association, Boston, MA, August 28-31, 2008. We wish to thank Robert Jackson, Carl Klarner, Michael Martinez, Steve Nicholson, Alex Whalley, and the reviewers and editors of the *APSR* for their constructive comments during the writing of this paper. We also wish to thank Scott Edwards of EarthInfo, Inc. for his assistance with extracting the historical weather data and Dave Cowen, Courtney Russell, and, especially, Lynn Shirley from the Department of Geography at the University of South Carolina for their work and expertise in producing GIS interpolations of the weather data. Lastly, we thank the College of Arts and Science at the University of South Carolina, where this work commenced, for its generous financial support for this project. All errors should be attributed to the authors alone.

Abstract

This paper examines the electoral consequences of variation in voter turnout in the U.S. Existing scholarship focuses on the claim that high turnout benefits Democrats, but evidence supporting this conjecture is variable and controversial. Previous work, however, does not account for endogeneity between turnout and electoral choice, and thus causal claims are questionable. Using election day rainfall as an instrumental variable for voter turnout, we are able to estimate the effect of variation in turnout due to across-the-board changes in the utility of voting. We reexamine the Partisan Effects and Two-Effects Hypotheses, provide an empirical test of an Anti-Incumbent Hypothesis, and propose a Volatility Hypothesis, which posits that high turnout produces less predictable electoral outcomes. Using county-level data from the 1948-2000 presidential elections, we find support for each of these hypotheses. Failing to address the endogeneity problem would lead researchers to incorrectly reject all but the Anti-Incumbent Hypothesis. The effect of variation in turnout on electoral outcomes appears quite meaningful. While election-specific factors other than turnout have the greatest influence on who wins an election, variation in turnout significantly affects vote shares at the county, national, and Electoral College levels.

There are two undercurrents to much of the literature on voter turnout: a normative belief that high rates of voter participation are desirable and an empirical expectation that variation in voter turnout will have electoral consequences. To some extent, these undercurrents flow together. While one might argue that high turnout is preferable for purely expressive reasons, the typical normative claim asserts that increased levels of voter participation improve the quality of representation by reducing any bias that might result from dissimilarities between voters and nonvoters. Assumed differences between those who vote and those who do not are also at the heart of the commonly hypothesized empirical relationship between aggregate turnout and partisan vote share. As argued originally by the authors of *The American Voter* (Campbell et al. 1960, 96-115), if nonvoters and occasional voters hold preferences that differ from those of habitual (or core) voters, then variation in turnout is likely to have meaningful electoral implications. Higher turnout might advantage one party over another, might advantage incumbents or perhaps their challengers, or might lead to greater volatility in the electorate. In turn, each of these electoral implications from higher turnout is likely to result in significant policy consequences.

Given the potential importance of the link between voter turnout and electoral outcomes, the literature is replete with studies testing the existence and nature of this relationship. Yet, for all the effort, scholarly work has not produced an agreement about whether an empirical association between turnout and electoral outcomes actually exists, nor is there universal agreement regarding the causal mechanism that connects the two phenomena.

Almost all existing studies, for instance, have a partisan component—the most common conjecture being that Democratic candidates typically do better when turnout is high. Some, however, hypothesize that this partisan effect will be conditioned by the partisan composition of the electorate (DeNardo 1980, 1986), while others find instead that this partisan effect may depend on the presence of class cleavages (Martinez and Gill 2005). This debate over the causal nature of the relationship between turnout and partisan outcomes is fueled by mixed empirical evidence. Tests of the partisan conjecture vary remarkably in their findings, with Radcliff (1994) and Erikson (1995) anchoring the polar extremes of large partisan (i.e., pro-Democratic) turnout effects and no turnout effects, respectively.

An alternative to the partisan hypotheses is identified by Grofman, Owen, and Collet (1999), who suggest that higher turnout rates could be bad news for incumbents. They note that high turnout "can arise if growing unpopularity of an incumbent leads to an increase in voters who seek to unseat him/her turning out at the polls and/or if potential vulnerability of an incumbent to a successful challenge leads to a campaign by a well-financed challenger whose campaign succeeds in attracting more voters to the polls" (Grofman, Owen, and Collet 1999, 359). In this view, high turnout is likely to correspond with decreased vote share for incumbent candidates or the incumbent party. This hypothesized connection between turnout and incumbency is a reasonable alternative (or perhaps complement) to the partisan conjecture, but unfortunately it remains untested. Thus, there exists little scholarly agreement about either the partisan consequences of high turnout or its effect on incumbents in general.

One possible reason for the lingering questions about the effect of turnout on electoral outcomes is that the empirical approaches typically used to assess the relationship allow for claims of correlation but likely do not allow for claims of causation. From classic treatments of the subject (e.g., Burnham 1965; DeNardo 1980) to more recent studies (e.g., Citrin, Schickler, and Sides 2003; Nagel and McNulty 2000), existing scholarship generally investigates the effect of turnout on electoral outcomes using one of two approaches. One set of studies utilizes survey data on voters and nonvoters to assess the degree to which the latter hold different preferences from the former and thus would have altered the election in question if they had voted (e.g., Citrin, Schickler, and Sides 2003; Highton and Wolfinger 2001; Martinez and Gill 2005). Another set typically regresses aggregate vote share data on aggregate turnout to assess the effect of turnout on partisan outcomes (e.g., Erikson 1995; Nagel and McNulty 1996, 2000; Radcliff 1994).[1] Establishing a causal relationship between turnout and electoral outcomes—using either approach—is difficult because individual decisions to vote, as well as aggregate turnout data, are likely endogenous to dependent variables involving vote choice. To the extent that the endogeneity problem exists, current analyses suffer from coefficient bias and cannot lead to causal claims. In the end, of course, it is the causal claim that most interests scholars.

In an effort to assess the actual *causal* effect of variation in voter turnout on electoral outcomes, we employ an instrumental variable approach that uses election day weather, rainfall to be specific, as an instrument for voter turnout. Election day rainfall is a very attractive instrument because it predicts turnout and is exogenous to electoral outcomes. Moreover, we can expect the causal effect of weather-induced variation in turnout to be very similar to the general effect of variation in turnout (endogenous or exogenous) resulting from uniformly distributed changes in the costs or benefits of voting. While our instrument is unlikely to illuminate the causal effect of variation in voter turnout due to targeted mobilization (or suppression) efforts or idiosyncratic candidate characteristics, variation of the sort captured by our instrument is likely to compose a substantial portion of the total variation in voter turnout.

By using county-level data that extend longitudinally from the 1948 to 2000 U.S. presidential elections, we gain substantial leverage for our weather-based instrumental variable design and, consequently, substantial leverage in uncovering the electoral consequences of turnout variation in recent U.S. elections. Utilizing this approach, we test two previously examined partisan hypotheses: 1) increases in turnout enlarge the vote share of Democratic candidates, and 2) this partisan effect is conditioned by the partisan composition of the electorate. We find substantial support for both of these hypotheses. Additionally, we use the instrumental variable approach to test Grofman, Owen, and Collet's previously untested hypothesis: 3) increases in turnout decrease the vote share of incumbent candidates/parties. We also propose and test a Volatility Hypothesis: 4) increases in turnout lead to greater electoral volatility. We find substantial support for these latter hypotheses as well, revealing that by narrowly focusing on partisan outcomes previous scholarship has overlooked two important implications of higher turnout: anti-incumbent voting and general electoral volatility. When we compare the instrumental variable estimates with OLS estimates it becomes apparent that a failure to address the endogeneity in our model would lead researchers to incorrectly reject all but the Anti-Incumbent Hypothesis.

We conclude by reporting the results of a simulation that varies turnout two percentage points above and below its observed levels. We find that this four point swing in turnout causes an average change of approximately 20 Electoral College votes per election. Thus, while election-specific factors other than turnout have the greatest influence on who wins an election, variation in turnout appears to exert a significant effect on vote shares at the county, national, and Electoral College levels.

The Theoretical Connection between Turnout and Electoral Outcomes

If voters and nonvoters have the same propensities to cast ballots for particular candidates or parties, then turnout levels should not affect who wins elections. This strict condition, however, is unlikely to be met in practice, meaning that variation in turnout will likely result in potentially meaningful changes in electoral outcomes. Exactly how voters and nonvoters differ becomes the crucial question (e.g., Highton and Wolfinger 2001; Wolfinger and Rosenstone 1980), since the nature of these differences will define the political implications of turnout.

Below, we outline the theory behind two well-established hypotheses regarding the effect of turnout on electoral outcomes, one previously proposed but untested hypothesis, and one original hypothesis derived from extant theory. While our goal here is not to introduce a new, overarching theory of turnout effects from which each of these hypotheses can be derived, it is theoretically plausible for all four of the hypotheses to work in consort, instead of in competition. This prospect suggests that the effects of turnout may be more multifaceted than previous scholarship has surmised.

Partisan Effects and the "Two-Effects" Hypothesis

The hypotheses most often discussed in the literature involve the partisan implications of voter turnout. The theoretical rationale positing a partisan bias to turnout begins from an assumption that the same socioeconomic factors that influence whether people vote also correspond with partisan preferences. It is well established in the behavioral literature that U.S. voters tend to be better educated, wealthier, and older than nonvoters, creating a socioeconomic bias in who turns out to vote (e.g., Leighley and Nagler 1992). These same social factors also have been fairly stable predictors of support for the Republican Party and its candidates (Gelman et al. 2008). So it is reasonable to assume that those who are likely to vote are also more likely to sympathize with Republican candidates than those who are not likely to vote. Elections in which only high probability voters turn out at the polls should exhibit the greatest pro-

Republican bias. As turnout rates increase—resulting from lower probability voters casting ballots—this partisan bias should decrease.[2] Thus, the logic underlying the Partisan Effect Hypothesis corresponds with the conventional wisdom that Democratic candidates do better when turnout is high.[3]

Partisan Effect Hypothesis: Increases in turnout will lead to increases in the Democratic candidate's vote share.

In what is arguably the best known work on the effect of turnout on vote share, DeNardo (1980, 1986) concedes that Democrats will benefit from higher turnout *on average*, but argues that this effect is conditional. DeNardo contends that the electorate is composed of two types of voters, those who regularly vote ("core voters") and those who occasionally vote ("peripheral voters").[4] While core voters have strong partisan attachments, peripheral voters have much weaker partisan leanings and tend to be more vulnerable to short-term electoral forces. Consequently, peripheral voters are more likely to defect from their weakly-held partisan allegiances. As a result, the partisan composition of an electorate will influence the partisan effect of higher turnout.[5]

As an extreme example, consider a county in which every registered voter identifies as a Democrat. In a low turnout election, core voters, who are likely to be dedicated Democrats, are most likely to turn out and the vote share for a Democratic candidate should be close to 100%. In a high turnout election, where peripheral voters with weaker commitments to the Democratic Party are voting, a meaningful proportion of these added voters are likely to defect and vote for the Republican. Thus, higher turnout in this county will increase the vote share for the Republican candidate. Of course, the underlying mechanism works in exactly the same way if we alternate the Republican and Democratic labels in the given example. The point to be made here is that high turnout does not necessarily benefit the Democratic Party as the Partisan Effect Hypothesis (and conventional wisdom) suggests. As our extreme example illustrates, the partisan composition of an electorate will condition the effect of the level of voter turnout if peripheral voters are more likely to defect than core voters.

Thus, DeNardo contends that turnout has "two effects." Higher turnout will generally help Democratic candidates, but higher turnout also helps the minority party within an electorate. More precisely, the positive marginal effect of turnout on Democratic vote share should decrease in size as the proportion of Democrats in the electorate increases. In Republican-dominated electorates, Democratic candidates will be particularly helped by higher turnout as both the introduction of more Democratic voters and weak Republican identifiers will push Democratic vote share upwards. The more Democratic the electorate, the less helpful higher turnout will be for a Democratic candidate. In fact, in particularly Democratic electorates higher turnout could increase the Republican candidate's vote share.

> *Two-Effects Hypothesis: The more Republican an electorate, the more that increases in turnout will increase Democratic vote share. In particularly Democratic electorates, increases in turnout may decrease Democratic vote share.*

The Anti-Incumbent Effect

Grofman, Owen, and Collet (1999) offer a third hypothesis involving the political implications of turnout. While careful not to make a causal claim, they argue that higher turnout will be associated with lower vote share for the incumbent party.[6] In our view, there are two plausible justifications for this expectation. First, the conditions that cause voters to reject the incumbent party may also cause more voters to turn out at the polls. A troubled economy or unpopular war effort, for example, may stimulate both higher turnout and a collective vote against the incumbent candidate or party. In this situation, however, turnout and an anti-incumbent vote are associated, but the former does not cause the latter.

A second potential justification for this hypothesis builds on the assumption that core voters differ from peripheral voters. Here, the operative difference between these groups is not partisan, ideological, or socioeconomic. Instead, the assumption is that core voters are on average more supportive of the governmental status quo than peripheral votes because they played a more active role in establishing the status quo in previous elections.[7] By definition, occasional voters or perpetual nonvoters had a lesser hand in the formation of the status quo. The more that these voters are involved in an election, the worse the incumbent party's candidate will do. This justification for Grofman, Owen, and Collet's hypothesis clearly draws a causal arrow from turnout to the vote share of incumbent party's candidate. As we are interested here in causal claims about turnout, we rely on this second rationale:

Anti-Incumbent Hypothesis: Higher turnout will decrease the vote share of the incumbent party's candidate.

The Volatility Effect

Because peripheral voters tend to possess weaker partisan attachments and weaker affinity toward incumbents, it is likely to be more difficult to predict exactly how they will vote if and when they do come to the polls. Consider DeNardo's defection argument. He contends that peripheral voters are more likely to defect from whatever partisan leanings they may possess. If this is the case, one wonders what criteria peripheral voters do rely on when making an electoral choice. It is unlikely that peripheral voters vote based on policy preferences or ideology. In fact, Highton and Wolfinger (2001) find—relying on ANES data—that those who did not vote in the 1992 and 1996 elections were less likely to view themselves ideologically, had weaker policy opinions, and were more likely to view themselves as independents than those who did vote. Thus, not only are peripheral voters more likely to defect from their weakly-held partisan leanings, they are also less likely to vote based on policy preferences or ideology. This suggests that DeNardo's defection argument can be expanded beyond its original specification: peripheral voters not only have weaker levels of partisan attachment, they generally have weakly-held political attitudes that play a minimal role in determining their vote. Consequently, we suspect that if these typical nonvoters actually do vote, their votes will be less predictable than those cast by ideological partisans with strong policy preferences.[8]

High levels of turnout result from occasional voters and some perpetual nonvoters heading to the polls. This, in turn, means that a larger proportion of the electorate is nonideological, independent, and holds weak policy preferences. If these additional voters are less predictable in terms of their vote choice, then high turnout elections ought to be less predictable in the aggregate than low turnout elections.

Volatility Hypothesis: As turnout increases, vote share will become less predictable.

Current Approaches to Estimating Turnout Effects and the Problem of Endogeneity

Scholars have employed two different strategies when analyzing the political implications of voter turnout. The first approach uses aggregate data and regresses partisan vote share on levels of voter turnout. The results of these studies are quite mixed, with some finding strong support for what we label partisan effects (e.g., Radcliff 1994), some finding no support for partisan effects (e.g., Erikson 1995), and others providing evidence for the Two-Effects Hypothesis (e.g., Nagel and McNulty 1996, 2000).

The problem with this approach is that it assumes that levels of voter turnout are exogenous to partisan vote share. Yet it is very likely that voter turnout is actually endogenous. Downs's (1957) seminal work on the logic of voter turnout implies as much. First, Downs's calculus of voting suggests that eligible voters decide whether to vote based in part on the extent to which one candidate is preferable (i.e., provides greater expected benefits) to the other. If some segment of voters sees no (or little, if there is a cost to voting) difference in expected utility between the candidates, Downs's model predicts that it is rational for these voters to abstain (1957, 39). Of course, the relative utility of the candidates also determines for whom a voter chooses to vote. In other words, a voter's relative preferences regarding the candidates, and thus the likely direction of the vote, affect his or her decision to vote in the first place.

A second way in which Downs's logic suggests endogeneity is via the perceived closeness of the election. As the perceived probability of a close election increases, the instrumental value of voting also increases and turnout levels should rise (Nicholson and Miller 1997). Moreover, in close elections, candidates and parties also have a greater incentive to mobilize voters, another possible source of higher turnout (Cox 1988). Consequently, this could cause high turnout to appear to cause close elections when the reverse relationship is more likely.

While mobilization efforts may intensify during close elections, parties and candidates are likely to engage in some mobilization efforts no matter the expected closeness of the campaign (Rosenstone and Hansen 1993). These mobilization efforts alone could also contribute to the endogeneity problem. If, by example, the Republican Party's mobilization efforts target Republicans in heavily Republican areas, while the Democratic Party targets Democrats in heavily Democratic areas, then high turnout might appear to favor the Republican candidate in Republican areas and the Democratic candidate in Democratic areas, even though turnout was boosted in these areas as a function of anticipated vote share.

The second approach scholars have taken when analyzing the political implications of voter turnout is to use individual-level survey data to simulate what would have happened if nonvoters had decided to vote in the election (e.g., Citrin, Schickler, and Sides 2003; Highton and Wolfinger 2001; Martinez and Gill 2005). The appeal of this approach is that researchers can utilize data on the preferences of nonvoters to project how they might have voted. The problem, of course, is that these projections are not testable, and it is not necessarily clear how to forecast what nonvoters would do at the polls. Moreover, there is still the issue of endogeneity. For example, it is possible that nonvoters chose not to vote because there was not a candidate for whom they wanted to vote. Assigning preferences to these individuals may be presumptuous.

The main point here is that the decision to turn out cannot be simply assumed to be exogenous to vote choice. At both the individual and aggregate levels, turnout is likely endogenous to vote choice. If endogeneity exists, then the coefficient estimates of existing analyses are biased, and any attempt to infer a causal relationship between turnout and the vote will be invalid. In sum, prior work has not been able to provide a clear, causal connection between level of turnout and electoral outcomes.

An Instrumental Variable Approach to Estimating Turnout Effects

Instrumental variables regression analysis (IV) was developed explicitly for situations in which an independent variable is potentially endogenous.[9] This approach begins with the identification of a variable (or set of variables) that will serve as an instrument for the endogenous variable. The endogenous independent variable is then regressed on the instrument(s), and the results of this regression are used to predict values of the endogenous variable. These predicted values are then included as an independent variable in the main model of interest. Proper identification of the IV model requires that the model of the endogenous "independent" variable must include a subset of instruments that are excluded from the main model (consequently, we shall refer to such an instrument as an excluded instrument). Model identification also requires that there must be at least as many excluded instruments as there are endogenous independent variables in the main model. So long as the excluded instruments are 1) uncorrelated with the error term in the main equation (i.e., are exogenous to the dependent variable in the main model) and 2) correlated with the endogenous independent variable, then IV produces consistent coefficient estimates of the effect of the endogenous independent variable on the dependent variable.

For our model, the primary task is to identify an appropriate instrument for aggregate voter turnout. No such instrument has been recognized in the literature. One might assume that registration laws, such as closing dates, Motor Voter laws, and the like, might serve as suitable instruments for turnout, but none of these variables is clearly exogenous to electoral outcomes.[10] We believe we have found a useful instrument for pursuing an IV approach to estimating the effect of turnout on electoral outcomes—election day weather. Recent work by Gomez, Hansford, and Krause (2007) validates the long-held belief that bad weather on election day is associated with lower levels of voter turnout, and bad weather is clearly exogenous to electoral outcomes. These properties suggest that election day rainfall—our operationalization of weather—should serve as a suitable excluded instrument for turnout. To determine whether this instrument has sufficient explanatory power, the econometric literature indicates that an F-test statistic of at least 10 for an excluded instrument or set of excluded instruments should be obtained for the main equation estimates to be consistent (see Staiger and Stock 1997). For each of our analyses, we provide the relevant F-test statistic. Each of these F statistics clearly exceeds the threshold of 10, further validating our choice of election day rainfall as an instrument for voter turnout.[11]

Before proceeding, however, we must confront an important issue at the nexus of theory and method. While our weather-based instrument leverages *exogenous* variation in voter turnout to identify the causal effect of turnout on electoral outcomes, we need to consider the extent to which our IV approach will illuminate the electoral implications of *endogenous* sources of variation in voter participation. After all, one might ask 1) whether "variation in the endogenous regressor related to [our] instrumental variable [has] the same causal effect as variation unrelated to [our] instrument" (Dunning 2008, 291), and 2) whether the effect we capture is politically relevant.

The first question presumes that turnout can be divided into two components, TO_1 and TO_2, where the former includes all variation in turnout (exogenous and endogenous) that has the same effect on vote shares as the variation exogenously-determined by election day weather, and the latter includes variation in turnout that has an alternative effect. The question is: how much variation is contained by each component? Without additional instruments to allow us to empirically test whether different components of the variation in voter turnout have similar or different effects on vote shares, we must rely on theory to answer this question (Dunning 2008, 301).

At one extreme, TO_1 could consist only of weather-determined turnout. At the other, TO_1 could contain turnout in its entirety, meaning that our IV estimates represent the effect of any and all variation in turnout on electoral outcomes. Neither extreme is realistic. Theoretically, the causal effect of weather-induced variation in turnout should be similar to the effect of all variation in turnout (endogenous or exogenous) resulting from uniformly distributed changes in the costs or benefits of voting that particularly alter the turnout decisions of peripheral voters, who "sit the fence" between voting and staying home. This would include variation in turnout that results from any general change to the costs and benefits of voting that is not politically targeted (i.e., that does not target potential voters based on their likely vote choice) or determined by the appeal of a specific candidate. Importantly, variation of this type (i.e., non-targeted and non-candidate generated variation) likely represents a significant portion of the overall variation in voter turnout. For instance, alterations in registration or voter identification requirements represent changes in costs that are meted out equally across all eligible voters, and thus should be considered within TO_1. Research shows that changes in electoral laws are powerful predictors of aggregate voter turnout (see Geys 2006; Rosenstone and Wolfinger 1978). Another important source of variation in turnout that should fall into TO_1 is the closeness of the electoral contest (see Geys 2006). Because a close race increases the probability that any one vote will be influential, all voters will experience a change in their calculus of participation. These changes may be determinative for peripheral voters for whom a slight change in the benefits (or costs) of voting determines whether they turn out. Thus, a close race increases their turnout just as a sunny election day does. Expansive changes in the information environment are likely to uniformly reduce the cognitive costs (or increase the perceived benefits) of becoming engaged with and informed about the political world (Berinsky 2005). Changes of this sort will affect the utility calculations of all voters and may be pivotal in activating peripheral voters. Finally, mobilization efforts that are not politically targeted, such as general GOTV appeals, can increase turnout (e.g., Gerber and Green 2000). The increase in turnout resulting from these efforts is a function of adding marginal voters (see Arceneaux and Nickerson 2009), who should behave in much the same way as additional voters spurred on by a pleasant election day. In sum, our IV estimates should capture the causal effect of much of the politically interesting variation in turnout, exogenous and endogenous.

Variation in voter turnout attributable to candidate characteristics or mobilization (or suppression) efforts that *target* particular types of voters will likely have different electoral effects, however. For example, if the presence of a particularly charismatic candidate on the ballot causes an increase in turnout, this increase may not have the same consequences as variation in turnout generated by non-candidate-driven changes to the benefits or costs of voting. Similarly, if a candidate or party can effectively target and mobilize core supporters, any resulting increase in turnout will probably benefit that candidate or party, producing a potentially asymmetric effect on vote share. Of course, it is possible, if not likely, that the mobilization efforts of one party may be counteracted by similar activities by the opposition, indicating that increases in turnout due to this mechanism may have little or no net effect on electoral outcomes. Both of these types of influence on turnout should fall into TO_2, meaning that we cannot infer that our IV results apply to this type of variation in turnout.

Which type of determinant of turnout is of greater importance? Put differently, which is larger, TO_1 or TO_2? Again, we have no way to empirically answer this question since the absence of an exogenous instrument for TO_2 makes a direct comparison impossible. We do think it important, though, that two of the best systematic predictors of changes in aggregate-level turnout, changes to registration requirements and the anticipated closeness of the election, contribute to variation in turnout should have the electoral effects revealed by our IV model. In short, our IV results should apply well beyond the electoral consequences of truly exogenously determined variation in turnout, but may not encompass all possible sources of variation in turnout.

Data

Our dataset consists of observations from the nearly 2,000 non-Southern counties in the continental U.S. for each presidential election from 1948 to 2000.[12] Our unit of analysis is thus the county-election dyad. This low level of aggregation is particularly useful given that the excluded instrument we use for turnout is rain on election day. A larger unit of aggregation, such as the state, would prevent an accurate measure of rainfall, potentially muting the correlation between weather and turnout, and would thus hurt the power of this critical instrument. The inclusion of a substantial number of elections also guarantees a good deal of variation in election day weather.[13]

Studies examining turnout effects usually exclude the South. In our case, we exclude Southern counties for three reasons.[14] First, both voter turnout and presidential vote share are problematic in Southern states for the first half of our time span. Formal and informal barriers to voting depressed turnout until federal actions largely removed them. At the same time that these barriers were being overcome, the South experienced a major partisan realignment, which may lead to a spurious relationship between turnout and electoral outcomes (see Erikson 1995). This is quite important. Erikson (1995) argues that previous studies demonstrating a correlation between turnout and the vote are primarily driven by political change in the South. He argues that when analyses are restricted to non-Southern states there is "no evidence whatsoever of a turnout effect on the vote" (Erikson 1995, 387). A second justification for restricting our data is that regional third-party candidates garnered substantial electoral support in the South during a few of our earlier elections, making it difficult to analyze two-party vote share.[15] Third, F-tests reveal that rainfall does not have the same explanatory power when it comes to voter turnout in Southern counties as it does for non-Southern counties. In fact, for Southern counties rainfall does not produce F-statistics that meet the minimum threshold for properly estimating an IV model.

The dependent variable in our initial analyses is the percentage of the two-party vote received by the Democratic presidential candidate.[16] To measure our excluded instrument, rainfall on election day, we use the National Climatic Data Center's "Summary of the Day" data (made available by EarthInfo, Inc.). The Summary of the Day data report various measures of daily weather for over 20,000 weather stations located in the U.S. Despite the sizeable number of meteorological observations, not all U.S. counties have weather stations situated within their borders, while many counties have multiple weather stations. We therefore used a GIS method known as Kriging to estimate election day rainfall, in inches, for each county (see Childs 2004).[17] To provide a sense of what these rainfall data "look like," Figure 1 presents rainfall maps for the 1996 and 2000 elections, the two most recent elections in our analysis.

[Figure 1 Here]

It is likely that the behavioral effect of election day rain depends upon the typical weather conditions experienced in a county on that day. For this reason, we also calculated the normal (average) rainfall for each election date (ranging November 2-9) for each county using data from the entire 1948-2000 time span. We then subtracted the county's normal rainfall from the rainfall estimated to have occurred on each of the respective election days under analysis. By measuring *Rain* as a deviation from its normal, we account for typical regional variations in weather.[18] As we discussed the previous section, this measure will serve as our excluded instrument for voter turnout.[19]

Two of the hypotheses we seek to test require additional independent variables: partisan composition of the county and party of the incumbent president. *Partisan Composition* is measured as the moving average of the two-party Democratic vote share (i.e., percentage of the two-party vote cast for the Democratic presidential candidate) in the county in the three most recent elections. For county *C* in presidential election *E* this variable equals the average of *C*'s Democratic vote share in *E*-1, *E*-2, and *E*-3. The Two-Effects Hypothesis indicates that partisan composition conditions the effect of turnout on Democratic vote share. We therefore include the interaction of this variable with turnout and expect a negative estimate for this multiplicative term (indicating that the more Democratic a county, the less that increases in turnout add to Democratic vote share). To include an interaction term involving an endogenous variable, we need to create a second excluded instrument.[20] We thus include in our IV model the product of our rain variable and the partisan composition measure as a second excluded instrument predicting the product of turnout and partisan composition.[21] This strategy provides us with consistent estimates of the conditioning influence of partisan composition on the effect of turnout on vote shares.

Our incumbency variable is a dummy variable that equals one if the incumbent president is a Republican. The Anti-Incumbent Hypothesis suggests that the candidate of the same party as the incumbent president will be hurt by higher turnout. To test this hypothesis we need to interact the *Republican Incumbent* variable with turnout. We expect the coefficient estimate associated with the interaction term to be positive (indicating that turnout increases Democratic vote share when the incumbent is a Republican). Again, for our IV model, we must create an instrument for this interaction term, so we use the product of rainfall and the dummy variable for Republican incumbent as an additional excluded instrument predicting the product of turnout and Republican incumbent. Note that the dummy variable indicating a Republican incumbent is not included in the model on its own (as a constitutive term) because this effect is already accounted for by our inclusion of election-specific fixed effects, which we discuss below. For a presentation of the three first stage models (predicting turnout, turnout × partisan composition, and turnout × Republican incumbent), as well as an alternative IV approach to incorporating interaction terms including an endogenous variable, see the Appendix.

There are obviously many other factors that may influence a county's Democratic vote share in any given election. Our strategy for controlling these other potential influences is to include fixed effects for both counties and elections. These fixed effects should account for potential heterogeneity in the data and the accompanying correlation of residuals, while providing us with conservative coefficient estimates for our variables of interest.[22] Given that the Volatility Hypothesis implies heteroskedastic residuals, we estimate robust standard errors. As we discuss later in some detail, we also consider the potential problem posed by spatial autocorrelation.

For the purposes of comparison, we also use OLS to estimate our model with actual turnout instead of instrumented turnout. Our measure of voter turnout in each county is calculated by dividing the number of votes cast at the presidential level by the estimated voting age population.[23] We provide these parallel model estimations in order to assess the consequences of treating turnout as exogenous.

Results

The results of the OLS and IV models explaining the two-party vote share of Democratic presidential candidates are presented in Table 1. The first column in Table 1 presents the OLS estimates for the model when actual, *not instrumented*, turnout is included as the key independent variable. Based on these estimates, we would conclude that increases in turnout lead to increases in Democratic vote share if there is a Republican incumbent. Thus, the OLS results support the Anti-Incumbent Hypothesis but not the others. Of course, the OLS evidence in support of the Anti-Incumbent Hypothesis is itself suspect. If we had only estimated the OLS model, a skeptic would rightfully question the causal connection between incumbency and turnout. After all, it is quite possible that high turnout occurs because voters are dissatisfied with the incumbent president's performance. Davidson and MacKinnon's (1993) endogeneity test reveals, as suspected, that the OLS estimates using actual turnout suffer from endogeneity.[24] The OLS estimates are thus not consistent, and the IV estimates we discuss below are preferred.[25]

[Table 1 Here]

The second column of results in Table 1 presents the estimates for our fixed effects IV model.[26] An initial examination of these IV coefficient estimates and their standard errors reveals three tentative conclusions.[27] First, when the partisan composition of a county is zero percent Democratic—a purely hypothetical condition—and there is a Democratic president in office, the estimated effect of voter turnout is positive and (barely) statistically significant. Thus, in the most Republican county imaginable increases in turnout lead to increases in Democratic vote share. This conclusion is solely based on the estimate for the constitutive turnout variable, which captures the effect of turnout when the two conditioning variables equal zero. Of course, not much should be made of this particular result, given that counties are never zero percent Democratic.

Second, the coefficient estimates from this IV model also indicate that the positive effect of turnout on a Democratic candidate's vote share decreases as a county becomes more Democratic in composition. This conditional relationship is indicated by the negative and statistically significant estimate for the interaction term involving partisan composition. According to DeNardo (1980, 1986), high levels of turnout mean that more peripheral voters are voting. These voters are, by nature, more likely to defect from their loosely held partisan ties, a tendency that benefits the minority party in the county. The IV results support both of DeNardo's "two effects"—increases in turnout simultaneously help both Democrats *and* the minority party in the electorate in question. To be sure, prior work has provided evidence compatible with DeNardo's hypothesis (Nagel and McNulty 1996, 2000), but this is the first evidence that variation in turnout exerts this conditional *causal* effect on vote share. It is important to note that the OLS estimates presented in Column 1 "miss" this effect.

A third inference to be drawn from these IV estimates is that turnout exerts a larger positive effect on Democratic vote share when the incumbent president is a Republican. The positive and statistically significant estimate for the interaction term including the Republican incumbent dummy variable reveals this effect. This finding supports the Anti-Incumbent Hypothesis, which is based on the assumption that marginal voters are less supportive of the incumbent party (i.e., party of the president in office) than dedicated voters. According to this hypothesis, the positive effect of turnout on Democratic vote share should be conditioned by whether the incumbent president is a Republican or a Democrat. Our evidence shows this to be the case. While this particular result is also obtained with the OLS model, these IV results provide greater confidence in the causal claim that higher turnout lowers vote share for the candidate of the incumbent's party.

To gain a full picture of these IV results and a clear understanding of their implications for the hypotheses tested, we computed the full range of conditional coefficients and the accompanying conditional standard errors in the IV model. Given the presence of the two interaction terms, the effect of voter turnout on a Democratic presidential candidate's vote share is a combination of the coefficient estimates for the constitutive term and the two interaction terms. Specifically, the estimated effect (the conditional coefficient) of voter turnout is (.405 – (.011 × partisan composition) + (.469 × Republican incumbent)). This can be further simplified by considering separately situations in which the incumbent president is a Republican and those in which the incumbent is a Democrat. With a Republican incumbent, the conditional coefficient for turnout is ((.405 + .469) – (.011 × partisan composition)). With a Democratic incumbent, the coefficient is (.405 – (.011 × partisan composition)).

Figures 2 and 3 graphically illustrate the marginal effect (i.e., coefficient) of turnout on Democratic vote share, conditional on the partisan composition of the county. In Figure 2, the coefficients correspond to the presence of a Republican incumbent, while the coefficients in Figure 3 are calculated with a Democratic incumbent. For both figures, partisan composition ranges from 10% to 90% Democratic, a range of values closely approximating the actual range found in our data.

[Figures 2 and 3 Here]

When there is a Republican incumbent (a condition that is true in 7 of our 14 elections), turnout has a positive effect on Democratic vote share (i.e., the point estimate for the coefficient is greater than zero) as long as the county in question is under 80% Democratic. This effect is statistically significant for all values of partisan composition under 60%.[28] Put differently, increases in turnout generally help the Democratic candidate when there is a Republican in office. The magnitude of the effect is attenuated, though, by the partisan composition of the county. The more Democratic the county, the less that turnout helps the Democratic candidate. This declining conditional marginal effect (exhibited in Figure 2) is entirely consistent with DeNardo's Two-Effects logic and demonstrates that changes in turnout do simultaneously help both Democrats and the minority party in the electorate.

Interestingly, when there is a Democratic incumbent, as shown in Figure 3, turnout *never* has a positive and statistically significant effect on Democratic vote share. Turnout has a negative coefficient for all values of partisan composition above 36%, and these negative coefficients are statistically significant when partisan composition exceeds 62% Democratic. The declining conditional marginal effect of turnout on Democratic vote share is, again, consistent with DeNardo's thesis, but that turnout never enhances Democratic vote share under these conditions is surprising. When viewed in conjunction with the evidence in Figure 2, these results suggests that Democratic gains from turnout are likely to occur only when the party does not control the presidency. Indeed, when a Democratic president is in office, his party is likely to experience only the harmful effects of increased turnout. This finding suggests that DeNardo's "two effects" may not tell the whole story; a "third effect," incumbency, also conditions the relationship between turnout and vote share.

As a further illustration of the results of the IV model, we plot predicted Democratic vote share by turnout. In both Figures 4 and 5, three sets of predicted vote shares are presented, one for "toss-up" counties (partisan composition is 50% Democratic), one for Republican counties (partisan composition is 30% Democratic), and one for Democratic counties (partisan composition is 70% Democratic). The predicted vote shares in Figure 4 are generated with Republican incumbent set at one, while in Figure 5 the predictions are based on a Democratic incumbent. [29]

[Figures 4 and 5 Here]

These figures again reveal the conditional relationship between turnout and partisan vote share. When the incumbent president is a Republican, increases in turnout uniformly lead to a higher vote share for the Democratic candidate. The increase is steepest in Republican counties, and the slope becomes less steep as the size of the Democratic electorate increases. Given a Democratic incumbent, the direction of the effect of turnout depends upon the partisan composition of the county. Increases in turnout in Democratic counties help the Republican candidate, while increases in Republican counties assist the Democratic candidate, although as noted above this latter effect is not statistically different from zero. In toss-up counties, the model shows that high levels of turnout work *against* the Democrats.

Our IV results provide considerable support for all three of the hypotheses tested thus far. When we look at values of partisan composition and the partisanship of the incumbent president that lead to statistically significant turnout effects, we find a significant pro-Democratic effect in 45% of the observations (county-election dyads) in our data and a significant pro-Republican effect in only 2% of the observations. We can therefore state that when there is a significant turnout effect, it is generally pro-Democratic. This result is compatible with the Partisan Effect Hypothesis.[30] Our analysis also supports the Two-Effects Hypothesis. While increases in turnout more often help the Democratic candidate than the Republican candidate, the effect of turnout is clearly conditioned by the partisan composition of an electorate (a county, in our data). There is also clear evidence of an anti-incumbent effect at work. This is demonstrated by the fact that turnout exerts a statistically significant and positive effect on Democratic vote share for 95% of the observations in our data in which there is a Republican incumbent. When there is a Democratic incumbent, turnout never exerts a positive, significant pro-Democratic effect, regardless of the partisan composition of a county.[31] The only significant effect that turnout has in this scenario is a pro-Republican effect in very Democratic counties. In sum, increases in turnout captured by our instrument exert three effects: pro-Democratic, anti-majority party in the electorate, and anti-incumbent.[32]

Spatial Autocorrelation

We have been careful to account for election-specific and county-specific effects with our inclusion of fixed effects for each. In fact, the county fixed effects also account for any state effects that are constant over time, since the county effects are simply disaggregated state effects. Yet it is possible that there are state-election dyadic-specific effects that are unaccounted for in our model and thus lead to spatial autocorrelation, meaning that the residuals for the counties in a given state in a given election year might not be independent from each other. Furthermore, as illustrated in Figure 1, our instrument, election day rainfall, is not randomly distributed across counties in a given election. Neighboring counties are likely to experience similar weather on any given day, and thus our instrument naturally exhibits spatial correlation. Consequently, despite the fact that we have 27,401 county-elections in our data, there is a danger that a few regionally-concentrated storms are influencing our results. This concern is heightened by the fact that if we exclude from our analysis the two rainiest elections (1992 and 2000) our instrument loses power, our model identification suffers, and the two key coefficient estimates are no longer statistically significant (p = .089 and .093 with one-tailed tests, for turnout × partisan composition and turnout × Republican incumbent, respectively), although each is in the hypothesized direction and actually increase in magnitude.[33] It is important to note, however, that if we exclude either 1992 *or* 2000 the IV model is sufficiently identified and the estimates for these two interaction terms are statistically significant and in the same direction as in the results reported in Table 1.

An analysis of the residuals from our IV model reveals that they are not fully independent for the counties in a given state in a given election—in other words, the residuals are spatially correlated.[34] In general, there are a few possible ways to address the issue of spatial autocorrelation, although the IV context makes dealing with this problem a bit more complicated due to identification concerns. One straightforward solution to any potential correlation of residuals for counties within a given state for a given election is to include state-election fixed effects (e.g., a fixed effect for California in 1948, California in 1952, etc.) in our IV model. Unfortunately, diagnostics indicate that our IV model is no longer sufficiently identified when these additional fixed effects are included. These state-election effects reduce the explanatory power of our instrument in the first stage models since rainfall (or its absence) on any election day will be geographically clustered.[35] Even with the additional state-election fixed effects, the second stage model results are somewhat consistent with the results presented in Table 1, as turnout × Republican incumbent has a positive and significant coefficient (1.29, p = .011), while turnout × partisan composition has a negative and insignificant coefficient (-.005, p = .163). These particular results must be treated with caution, however, due to the lack of sufficient identification. It is possible that the identification problems we experience due to the inclusion of these additional fixed effects is further evidence that spatial autocorrelation is a concern here.

A very common way to address the effect that spatial autocorrelation can have on the estimates of standard errors is to estimate robust standard errors that "cluster" on the grouping variable in question. However, clustering is not straightforward when the grouping variable (state-election dyad) is different from the panel variable (county), which is our situation.[36] Nonetheless, when these clustered standard errors are estimated they are substantially larger than the robust standard errors that do not cluster on state-elections and these larger standard errors would cause us to fail to reject the null hypotheses for all of the key independent variables in the model (and all but two of the election dummy variables). We are skeptical about the size of these standard error estimates, though. These clustered standard errors are just as large, if not larger, when they are clustered on states, instead of state-elections, despite the fact there is no correlation of residuals within states (as compared to state-elections).

To attempt a blunt test of the consequences of spatial autocorrelation in our data, we aggregate our data to the state level (reducing our *N* from 27,401 to 517). By definition, these data cannot suffer from state-election spatial autocorrelation, since there is only one observation for each state in each election. This higher level of aggregation leads to insufficient identification for the two interaction terms in our model, but the constituent turnout variable is sufficiently identified to estimate an IV model of state-level Democratic vote share. In this model estimation, the effect of turnout is positive and statistically significant, which is reassuringly consistent with the average effect of turnout in the IV models presented in the second column of Table 1.

For our final and we believe most compelling effort to address the non-independence of residuals for counties in a given state and election year, we attempt to directly control for various potential sources of spatial autocorrelation by including a wide variety of independent variables that should soak-up much of the state-election effects. We identify and include four such types of variable: state laws regarding registration/voting, state-wide elections occurring at the same time as the presidential election, expected closeness of the state in the presidential election, and candidate connections to the state.[37] These variables might affect turnout and/or vote shares within a state in a particular election.

With all of these state-election specific variables in place, we re-estimate our IV model (still including county fixed effects and election fixed effects) and the results for the variables of interest are presented in the final column of Table 1. An analysis of the residuals for this model reveals that the inclusion of these controls reduces, but does not eliminate, the spatial autocorrelation.[38] After this reduction in spatial autocorrelation, the IV results remain very robust, which gives us confidence that spatial autocorrelation is not driving our results. For the rest of the article, we will utilize the IV results obtained without the inclusion these additional state-election variables, since there may be endogeneity concerns with some of the variables in the saturated model (e.g., registration requirements might be endogenous to vote shares). Furthermore, the results for the more parsimonious model are somewhat more conservative in the sense that the conditional effects of turnout are smaller in magnitude.

Implications for the Aggregate Behavior of Marginal Voters

To gain further information from our IV results, we use the estimates presented in the second column of Table 1 (i.e., those without the state-election controls) to simulate the voting patterns of the marginal voters who may or may not vote in a given election. For purposes of illustration, we base our simulation on a hypothetical county in which the baseline turnout is 50%. While varying both the partisan identification of the incumbent president and the partisan composition of the county, we use our IV estimates to predict the change in Democratic vote share based on a one percentage point increase in turnout (i.e., increasing turnout from 50% to 51%). Using this predicted change and some basic algebra we then determine how this additional one percentage point worth of voters (i.e., marginal voters) voted. Table 2 presents these predicted Democratic vote shares for these marginal voters.

[Table 2 Here]

The second column of Table 2 contains the simulated percentage of the additional, marginal voters voting for the Democratic candidate. The third column contains the difference between this vote share for the marginal voters and the vote share for the "core" 50% who turned out (which, for the sake of simplicity, is assumed to be equal to the partisan composition of the county – i.e., percentage Democratic). This simulation reveals that when the partisan, anti-majority, and anti-incumbent effects work together, there is a marked difference between the voting patterns of the marginal voters and the core voters, as we have defined them here. Specifically, when the hypothetical county is highly Republican (only 30% Democratic) and there is a Republican incumbent, the vote share for the marginal voters is nearly 28 percentage points more Democratic than that of the core voters. Typically, however, these three effects will not work together to this extent and thus the differences in the aggregate voting behavior of core and peripheral voters are usually smaller, although often still quite impressive.

Several survey-based studies compare the vote choice of actual voters with predicted vote choices of non-voters. While these studies typically conclude that changes in voter turnout would only occasionally affect election outcomes, they do report substantial differences in the vote choices of voters and the predicted or self-reported likely votes of non-voters. For example, from Highton and Wolfinger's (2001) results it can be derived that in 1996 Clinton's two-party vote share would have been 22.3 percentage points greater with non-voters than voters. Teixeira (1992, 96) examines several earlier presidential elections and finds that non-voters range from reporting likely vote shares that are somewhat disproportionately Republican in the 1980s to reporting likely vote shares that are over 12 percentage points more Democratic than actual voters in 1964. In their survey-based examination of the likely votes of non-voters in Senate elections, Citrin, Schickler, and Sides (2003) find that the difference in the vote shares of voters and non-voters ranged from approximately 10 percentage points more for the Democratic candidate to 5 percentage points more for the Republican. As these studies reveal, the vote share swings we report in Table 2 are not wildly out of line with those obtained by surveys, even though some of the swings are quite dramatic. The advantage to our simulations over the survey-based studies is that the surveys take place after the election and, as Campbell et al. (1960, 112) speculate, non-voters with weak partisan ties are likely to simply report that they would have voted for the winner of the election. Furthermore, we utilize a large number of elections while controlling for election-specific effects. The survey-based studies tend to rely on data from a small number of elections, which makes it more difficult to separate out general turnout effects from election specific influences.

How Meaningful are Turnout Effects at the Aggregate-Level?

Do our county-level results translate into important turnout effects at higher levels of aggregation? To address this question, we use the results of our IV model (without state-election controls) to predict for each election how Democratic vote share would have changed in each non-

Southern county based on changes from the actual, observed level of turnout in the county. Note that the effect of a change in turnout in each county depends upon the partisan composition of the county and the party of the incumbent president. Weighting each county by its number of voters, we then aggregate all of these predicted changes in vote share to determine the effect of uniform changes in turnout. These effects are aggregated at both the national level and at the state level. The former reveals the estimated effect of turnout on national vote share (excluding the South), while the latter is used to estimate the effect of turnout on Electoral College outcomes.

Through this aggregation process, we find that our county-level effects have implications at the national level. We simulate the change in national Democratic vote share based on a four-percentage point swing in turnout (i.e., ranging turnout from two percentage points below the actual turnout for the election to two percentage points above). Note that a four percentage point variation in turnout is not unreasonable, and may be viewed as conservative, given that national-level turnout has a much greater range than this over our 14 elections (ranging from 49.1% to 63.3%). Furthermore, prior research suggests that simple changes to voter registration requirements, for instance, could lead to changes in national-level turnout that exceed four percentage points (e.g., Rosenstone and Wolfinger 1978). In his meta-analysis of aggregate-level turnout studies, Geys (2006) finds that a one standard deviation increase in the closeness of an election leads to more than half of a standard deviation increase in turnout. Again, our hypothetical variation in turnout appears to be on the conservative side, given that Gey's result implies that moving from one standard deviation below the mean closeness of an election to one standard deviation above mean closeness would lead to more than a four percentage point swing in turnout (which has a standard deviation of 4.9% at the national-level during our time span). Based on our simulation, we find that a four-percent swing in turnout leads to an average change in Democratic vote share at the national level of just under one percentage point. [39]

When we aggregate our results to the state level, we find that varying turnout from two points above and below observed values causes an average change of approximately 20 Electoral College votes per election. In fact, in the 1948, 1960, and 1976 elections, at least 50 Electoral College votes change. These are not trivial effects, especially considering that they exclude the Electoral College votes of Southern states. While election-specific factors other than turnout have the greatest effect on who wins an election (Kaufman, Petrocik, and Shaw 2008, 147), it appears that variation in turnout, at least the sort captured by our IV approach, can exert an effect on vote shares at the county, national, and Electoral College level.

Testing the Volatility Hypothesis

The IV results presented in Table 1 suggest that high turnout elections are likely to have negative electoral consequences for the majority party in an electorate and for the incumbent party in the White House. As such, high turnout elections may be quite disruptive. Our Volatility Hypothesis builds upon this underlying logic and asserts that increases in turnout should lead to less predictable (i.e., more volatile) vote shares. By bringing nontraditional voters into the electorate, the electoral outcomes of high turnout elections should be more variable. To test this proposition, we use the predictability of Democratic vote share as the dependent variable in our final analysis. To measure this, we calculate the residuals from the IV model without state-election controls (presented in the second column of results in Table 1) and then square them. A large squared residual indicates a county-election dyad for which our model in Table 1 does a poor job of predicting Democratic vote share. Small squared residuals are associated with more predictable data points.[40]

We again use the IV approach and regress these squared residuals on instrumented turnout, as well as the county and election-specific fixed effects.[41] Because our dependent variable and key independent variable are predicted values, traditional standard errors cannot be used (Pagan 1984). We therefore bootstrap the standard errors associated with the coefficient estimates (Mooney and Duval 1993). These results are presented in Table 3.

[Table 3 Here]

As suggested by the Volatility Hypothesis, the coefficient estimate for instrumented turnout is positive and statistically significant. Increases in turnout cause larger residuals in our full vote share model. In other words, increases in turnout make the two party vote share less predictable at the county level. As turnout increases in a county, the fixed effect for the county (the county-specific constant), the partisan composition of the county, the fixed effect for the election (the national election-specific constant), and, ironically, the turnout variable itself lose some power to explain Democratic vote share. For a given presidential election, counties with higher turnout will deviate more from the national trend (captured by the election-specific fixed effect) in that election, all else equal. For a given county, high turnout elections are associated with a greater deviation from the mean Democratic vote share exhibited by that county over the latter half of the Twentieth Century.

In sum, these results imply that the irregular voters who contribute to high turnout event are less predictable than regular voters. When these peripheral voters do turn out, aggregate vote shares become less predictable. Importantly, the electoral volatility caused by increases in turnout is not detected by the standard approaches to this problem. The OLS estimate obtained with actual turnout is not statistically distinguishable from zero, suggesting that models that do not account for the endogenous relationship between turnout and vote share are unlikely to reveal the electoral volatility caused by higher turnout.

Conclusion

Political scientists have long pondered the electoral consequences of voter turnout. Most have presumed that in the United States turnout serves as a lever for the electoral fortunes of the Democratic Party. The conventional wisdom is that Democratic vote share rises with turnout, owing to an infusion of atypical voters—who tend to share a demographic profile with traditional Democratic voters—into the electorate. Yet the empirical evidence regarding the partisan consequences of turnout has been far from incontrovertible. Moreover, few scholars have looked beyond the partisan implications to examine the effect of high turnout on incumbency or electoral volatility in general. Our novel approach to testing the electoral implications of variation in voter turnout yields two central contributions.

First, we make a theoretical case for the endogenous relationship between turnout and vote choice. We then demonstrate empirically that aggregate turnout and aggregate electoral outcomes are endogenous. The fact that previous research has not accounted methodologically for this endogeneity strikes us as surprising. After all, most observers recognize that voters sometimes choose for whom they wish to vote before they actually decide whether to turn out to vote. This is true, for instance, whenever it is asserted that an emergent, perhaps charismatic, candidate is likely to bring new voters to the polls. It is also the case when a nonvoter decides to abstain because he or she perceives the choice as being a regrettable selection between Tweedledum and Tweedledee.[42] In cases such as these, or more axiomatically in Downs's calculus of voting, the vote choice is conceived as preceding the decision to go to the polls. By not accounting for the potential endogeneity in the relationship between turnout and electoral outcomes, the causal claims made in previous studies must be treated with some skepticism.

Second, by leveraging a unique instrument for voter turnout we are able to pin down the causal effect of much of the variation in voter turnout on electoral outcomes. As such, we believe we provide the strongest and clearest evidence to date of the political importance of variation in turnout, at least variation of the sort captured by our instrument. Our empirical results present a more complex, nuanced view of the effect of turnout on presidential elections. We show "four effects" to be at work. Democratic candidates, on average, are helped by higher turnout. The partisan composition of the electorate and the party of the incumbent president condition this relationship, however. Furthermore, we show that higher turnout results in greater levels of electoral volatility more generally.

In low turnout elections, where the electorate is comprised primarily of core voters, who possess relatively strong and unwavering partisan attachments, electoral outcomes reflect this underlying stability. Low turnout elections tend to benefit Republican candidates on average, a byproduct of the GOP's relative advantage with core voters, who tend to be of a higher socioeconomic status than peripheral voters. Our results also indicate that low turnout tends to validate the status quo, as elections of this type significantly advantage the party of the incumbent president. Combined, these results suggest that the voters in low turnout elections are likely to exert little "change" when they go to the polls. Put differently, lower levels of turnout may yield representational bias in the electoral connection. Interestingly, this bias is not simply partisan in nature, as low turnout also produces a pro-incumbent bias.

High turnout elections, on the other hand, bring both core and peripheral voters to the polls. Importantly, the infusion of this latter group into the electorate may contribute to outcomes that are both potentially disruptive to the existing political order and less systematically predictable. Peripheral voters are likely to possess weak partisan or ideological attachments at best and thus are more influenced by transient or idiosyncratic considerations. It appears the infusion of these atypical or peripheral voters into the electorate has consequences for America's political parties. Our findings conform to those hypothesized by DeNardo's Two-Effects thesis. That is, while higher turnout benefits Democrats on average, the magnitude of those benefits is conditioned by the composition of the electorate being brought to the polls. Indeed, in highly Democratic electorates, high turnout may actually help the Republicans, who benefit from weak partisans defecting from their Democratic attachments. In addition to these partisan effects, we find that high turnout also has a significant anti-incumbency effect. Peripheral voters not only have weak partisan ties, they also may be less likely to support the electoral status quo. Consequently, when the electorate expands with these voters, the incumbent party is fighting an uphill battle. Compared to their more participatory counterparts, infrequent voters bring both change and noise when they go to the polls. High turnout elections portend partisan change, anti-incumbency tendencies, and generally less predictable consequences.

Appendix

The IV models presented in Table 1 treat three independent variables as endogenous: turnout, turnout × partisan composition, and turnout × Republican incumbent. There are thus three first stage models, one for each endogenous variable. Following IV convention, each of these first stage models includes all three excluded instruments: rain, rain × partisan composition, and rain × Republican incumbent. The first stage estimates for the model without state-election controls are presented in the first three columns of Table A1.[43]

These estimates imply that the effect of rain on turnout is conditioned by Republican incumbent and partisan composition. Note that despite the positive "main effect" for rain in the turnout model, the average conditional effect of rain on turnout is negative (-.85, based on the average values of conditioning variables). The key considerations when determining whether rainfall is a valid instrument for turnout are 1) exogeneity and 2) explanatory power. We believe it is safe to assume that rainfall is exogenous to vote choice, so we will not belabor the point. Does it have enough explanatory power? The econometric rule of thumb is that an F-statistic of 10 or greater indicates that an instrument (or set of instruments) has sufficient explanatory power for IV estimation (Staiger and Stock 1997). As reported in Tables 1 and A1, our rainfall variable and associated interaction terms far exceed this threshold.

To ensure that the conditional effect of rain on turnout in the first stage of the IV model is not problematic for the estimation of the main model, we re-estimate the IV model while employing a somewhat different strategy. We predict just turnout in the first stage while including rain as instrument and excluding the interaction terms involving rain. The results of this alternative first stage are reported in the last column of Table A1. Then, we multiply predicted turnout with Republican incumbent and partisan composition and include these interactions in the main model. This approach forces rain to have a constant, non-conditional effect on turnout in the first stage. One disadvantage to this approach is that the estimated standard errors may not be correct in the second stage, but this can be resolved by bootstrapping these second stage standard errors. The results obtained by this alternative IV strategy are presented in Table A2. These results are quite similar to those presented in Table 1 and the fundamental inferences are the same. This indicates that allowing the effect of rain on turnout in the first stage to be conditional is not determinative of the ultimate conclusions drawn based on the second stage model of vote shares.

We then square the residuals from the model presented in Table A2 and regress them on the alternative version of predicted turnout to re-test our Volatility Hypothesis. These results are presented in Table A3. The result is fundamentally similar to that presented in Table 2. Both IV strategies lead to the conclusion that increases in voter turnout cause greater volatility in presidential vote shares.

TABLE A1. First Stage Results: Predicting County-Level Turnout, 1948-2000.

Independent Variable	Turnout Model	Turnout × Partisan Comp. Model	Turnout × Rep. Incumb. Model	Turnout Model (No Instrumented Interactions)
Rain	2.35* (.854)	346* (46.3)	-.360 (.947)	-1.02* (.207)
Rain × Partisan Composition	-.050* (.017)	-7.32* (.977)	-.047* (.019)	---
Rain × Republican Incumbent	-1.96* (.401)	-110* (20.3)	1.05* (.424)	---
Partisan Composition	-.008 (.009)	65.1* (.480)	-.023* (.008)	-.006 (.009)
N	27,401	27,401	27,401	27,401
F-test (all covariates)	1,399*	3,805*	63,849*	1,584*
F-test (Rain)	---	---	---	24.2*
F-test (Rain, Rain × Partisan Comp., Rain × Rep. Incumb.)	24.0*	34.2*	50.7*	---
R^2	.488	.748	.977	.487

* $p \leq .05$ (two-tailed test). Cell entries are coefficient estimates (robust standard errors). Fixed effects for county and for election year are included in all models. Southern counties are excluded from the analysis.

TABLE A2. Alternative IV Approach to Modeling County-Level Democratic Vote Share in Presidential Elections, 1948-2000.

Independent Variable	Coefficient Estimate (Bootstrap Standard Error)
Turnout	.502* (.140)
Turnout × Partisan Composition	-.002* (.001)
Turnout × Republican Incumbent	.208* (.012)
Partisan Composition	.821* (.034)
N	27,401
F-Statistic (all covariates)	3,352*

* $p \leq .05$ (one-tailed test). The model includes fixed effects for county and for election year. Southern counties are excluded from the analysis. The standard errors are bootstrapped (500 repetitions).

TABLE A3. Alternative IV Approach to Modeling Volatility in County-Level Democratic Vote Share in Presidential Elections, 1948-2000.

Independent Variable	Coefficient Estimate (Bootstrap Standard Error)
Turnout	6.13* (1.28)
N	27,401
Wald test for all covariates	753*

Notes: * $p \leq .05$ (one-tailed test). The dependent variable is the squared residuals from the model presented in Table A2. Fixed effects for counties and for election years are included in the model. Southern counties are excluded from the analysis.

Notes

[1] Other important studies not neatly fitting into our classification include those that examine the effect of voter turnout on racial or ethnic representation in local governments (Hajnal and Trounstine 2005) and the allocation of federal money (Martin 2003).

[2] Pacek and Radcliff (1995, 138) note that "the strength of the relationship between turnout and the (partisan) vote…depends upon the extent to which party competition is structured along socioeconomic cleavages—in other words, upon the extent to which parties represent the interests of specific social classes." In such cases, "the lower-status citizens who are the natural constituency of left parties tend to vote at lower and more variable rates than the higher-status supporters of center or right parties."

[3] Burnham (1965) was one of the first to demonstrate empirically that support for the Democratic Party increases as one moves from core voters to perpetual nonvoters.

[4] The distinction between core and peripheral voters was originated by Campbell (1966), building off Burnham's (1965) earlier delineation of the "American political universe" into three groups: core voters, occasional voters, and perpetual nonvoters.

[5] For a critique of DeNardo's Two-Effects Hypothesis, see Tucker and Vedlitz (1986).

[6] In fairness, Jacobson (2004) alludes to a relationship between turnout and incumbency advantage. Yet we choose to attribute the hypothesis to Grofman Owen, and Collet, who refer to it as the "competition hypothesis," because Jacobson does not expressly articulate the relationship as a hypothesis. Other existing studies on the effect of turnout control for incumbency effects on vote shares, but do not actually test whether increases in turnout benefit or hurt incumbents (Nagel and McNulty 1996; Radcliff 1994).

[7] It is also quite plausible that peripheral votes are generally less trustworthy of the political system, which would also cause them to exhibit anti-incumbent tendencies on those few occasions when they vote. Political trust predicts incumbent vote share (Hetherington 1999), but it is not clear whether individual-level trust increases voter turnout (see Levi and Stoker 2000).

[8] While not providing an explicit test of the Volatility Hypothesis, Petrocik (1981) argues that a "dramatic increase in the size of the electorate…will increase the proportion of psychologically uninvolved and uncommitted participants" (Petrocik 1981, 163-64). "An electorate composed of voters who rarely missed an election," Petrocik (1981, 166) continues, "would be quite stable. In contrast, an electorate composed of a large proportion of peripheral participants would be more volatile."

[9] See Wooldridge (2002, chapter 5) for a useful overview of IV.

[10] Changes in election law are unlikely to be randomly assigned. Poll taxes were adopted in the solidly Democratic South as a way of sustaining the party's electoral advantage. Motor Voter laws also appear to have been adopted for partisan reasons. Before Congress adopted the National Voter Registration Act of 1993, 19 states had already adopted such laws. Of these, 13 had Democratic majority state legislatures at the time of adoption. Given that it was widely assumed that Motor Voter laws would benefit Democrats, we find it hard to claim that these laws—and others that might affect turnout—are sufficiently exogenous. See Footnote 26 for details of an IV model utilizing variation in the closing date for registration.

[11] Kleibergen-Paap's (2006) rk Lagrange multiplier statistic also indicates that election day rainfall is a sufficiently powerful instrument to properly identify the IV model. We utilize Schaffer's (2007) Stata module to calculate this statistic and estimate the IV models presented in Table 1.

[12] Because Alaska and Hawaii did not enter the union until 1959, and because Alaska records election data by Election District rather than county, we excluded these states from the analysis. We also excluded Oregon from our 2000 data because the state implemented an early voting program that resulted in nearly all votes being cast before election day.

[13] See Gomez, Hansford, and Krause (2007) for details regarding the distribution of the rainfall variable.

[14] We define a Southern county as a county located in one of the eleven states of the Confederacy.

[15] Erikson (1995) notes the additional problem that a few Southern states did not have pledged Democratic slates of electors in the 1960 and 1964 elections.

[16] County level vote returns were gathered primarily from Congressional Quarterly's *America Votes* series and Congressional Quarterly's Voting and Elections on-line module.

[17] Comparative diagnostics (against an Inverse Distance Weighting function) indicated that "Universal Kriging with linear drift" provides superior model fit for our data and so was used to interpolate our data. Kriging assumes that the data follow a Gaussian distribution and determines the extent of spatial dependence between observations via the construction of a semivariogram. The Kriging method is regarded as a best linear unbiased estimator for spatial data (Stein 1999). Thus, spatial predictions based disproportionately on distant observations may exhibit a loss in efficiency, but they remain unbiased. With over 20,000 weather station observations per election day, this loss in efficiency is not a concern. Our county-level estimates of rainfall represent an average of the rainfall each of the estimated surface units (one unit = 4,000 square meters) in the county. In counties that experienced substantial intra-county variation in election day rainfall, our county-level estimate may result in a less efficient instrument in our IV model, but it is nonetheless unbiased. More details on these data and the GIS interpolation methodology can be found in Gomez, Hansford, and Krause (2007, 653-55).

[18] The mean value for election day deviation from normal rainfall is .005. This variable ranges from a minimum of -.419 to a maximum of 2.63 (with a standard deviation of .208).

[19] We use only rainfall as our instrument for turnout since adding snowfall (deviation from normal snowfall, specifically) generally *decreases* the F-statistics for the set of excluded instruments. The IV approach requires that we include at least one exogenous, excluded instrument for every endogenous variable in the main model. Additional excluded instruments are useful if they increase explanatory power, but this is not the case with snowfall. Snowfall may be weaker than rain as an instrument because snow is much rarer than rain in any given county on election day. Snow cannot, for example, explain temporal variation in turnout in most California counties because it has not snowed on election day in these counties during our time span (it has rained, however). If we include deviation from normal snowfall as a separate excluded instrument, our results are very similar to those presented here. If we

combine rainfall and snowfall into a single measure of precipitation that represents the total water content of the precipitation, our results are again very similar to what we present here. If we simply add inches of rain to inches of snowfall, this measure of precipitation no longer successfully identifies the IV model.

[20] Recent studies utilizing interaction terms in an IV model in the same way we do include Gabel and Scheve (2007) and Miguel, Satyanath, and Sergenti (2004).

[21] More precisely, we are utilizing rain × partisan composition as an excluded instrument for turnout × partisan composition.

[22] A Hausman test shows fixed effects to be more appropriate than random effects. Fixed effects were also used in the first-stage models predicting turnout and the interaction terms involving turnout. By including election-specific fixed effects, we control for short-term, election-specific effects, such as campaign effects, the state of the economy, etc. To ease presentation, we do not report any of the fixed effects. The election-specific effects reveal, however, that the 1956 and 1984 elections were particularly good for the Republican candidate (all else equal), while the 1964 and 1996 elections were especially good for the Democratic candidate. An examination of the county-specific fixed effects indicates that on average counties with small populations have fixed effects of a larger magnitude (positive or negative), while counties with large populations (typically urban counties) have small, often positive, fixed effects.

[23] County-level voting age population was gathered from two primary sources: the U.S. Census Bureau's *City and County Data Book* and the Census Bureau's website.

[24] Davidson and MacKinnon's (1993) test is based on a regression of the dependent variable in the main equation on the residuals generated by the first-stage equations. The test is similar to the more traditional Hausman test (sometimes referred to as the Wu-Hausman test in the context of choosing between IV and OLS), with the important exception that it is appropriate for panel data models with fixed effects.

[25] If we regress national Democratic vote share on raw national turnout for the 1832-2008 presidential elections, we find a positive but statistically insignificant estimate for turnout. The potential problem of endogeneity is apparent, however, when the vote share spread between the winning and losing candidate

is regressed on turnout. In this model, the estimate for turnout is negative and significant, indicating that as turnout increases the vote share gap between the candidates decreases. It is very likely that the causal arrow runs the other direction as both voters and elites seeking to mobilize voters have more at stake when elections are likely to be close. This form of endogeneity is likely muting any partisan effect.

If we regress national two-party vote share for the candidate of the incumbent president's party on raw national turnout, we find that turnout has a negative but statistically insignificant coefficient. The 1920 election appears to be a substantial outlier (perhaps due to the preceding ratification of the Nineteenth Amendment) and if it is excluded from the analysis the estimate for turnout is negative and significant. These aggregate results regarding the Anti-Incumbent Effect are thus at least weakly consistent with those of our county-level IV model.

[26] Election day rainfall is the best available instrument for turnout as it 1) has sufficient explanatory power to identify the IV model, and 2) is clearly exogenous to electoral outcomes. Yet to further explore the robustness of our results we replicated our IV results using the length of time between the statewide closing date for registration and election day as an instrument for turnout. This variable has sufficient explanatory power when predicting turnout but is NOT clearly exogenous to electoral outcomes. It should thus be viewed as an inferior instrument to election day rain. Identification tests reveal that (turnout × partisan composition) is not sufficiently well explained by the excluded instruments to identify the IV model. We therefore only include turnout and (turnout × Republican incumbent) in this model. The IV estimate for turnout with this specification is .095 (SE = .144) and the estimate for (turnout × Republican incumbent) is .421 (SE = .024). These results are quite consistent with the IV results obtained using rainfall. As a somewhat rough test of whether the closing date might be endogenous to electoral outcomes, we tested whether changes to the closing date are predicted by which party controlled the legislature that adopted the change. Not surprisingly this test suggests that Democratic legislatures are more likely to push the closing date closer to election day.

[27] The standard errors in the IV model are considerably larger than those in the OLS model, but this is quite typical (Wooldridge 2002, 86).

[28] To perform these tests of statistical significance, we calculate the conditional coefficients and conditional standard errors (Brambor, Clark, and Golder 2006).

[29] For Figure 4, the election-specific fixed effect is set at the average effect for elections in which there was a Republican incumbent. For Figure 5, we use the average effect for the elections in which there was a Democratic incumbent.

[30] Given our strategy of pooling data from 14 presidential elections, we implicitly assume that any turnout effects are, on average, constant over the 1948-2000 time span. To get a sense of how our results might change over time or are otherwise sensitive to particular elections, we employed a jackknife approach and estimated our IV model 14 times while each time excluding one election. After each estimation, we calculated the average effect of turnout, meaning the effect of turnout on Democratic vote share while holding Republican incumbent and partisan composition at their means. We then subtract this average effect from the average effect found when all elections are included. The resulting quantity tells us how much the inclusion of the election in question changes the average effect of turnout. This examination reveals that the vast majority of the individual elections have little influence over our results. The noteworthy exception is the 1992 election. When this election is excluded, the average effect of turnout increases considerably. This result suggests that, for whatever reason (perhaps the presence of a significant third-party candidate), the average effect of turnout in 1992 was considerably smaller than in other elections. Interestingly, the exclusion of either the 1996 or 2000 election leads to a slightly smaller average effect for turnout, meaning that the two most recent elections in our data actually exhibit somewhat larger than average turnout effects. There is little evidence that, 1992 aside, there has been a change in the effect of turnout over our time period.

[31] Since there are only 14 presidential elections in our data, our results regarding the Anti-Incumbency Hypothesis could be sensitive to an outlier election. Yet this is not the case. We again used a jackknife

procedure to see if any one election drives our results. No matter which election we exclude from our analysis, the estimate for turnout × Republican incumbent remains positive and statistically significant.

[32] One might be concerned that particularly rainy days in our analysis might be exerting an undue influence. We assessed this possibility by estimating our model while excluding the very rainiest observations (the 40 county-election dyads that experienced greater than 1.5 inches of rain above the average for that day). The inferences for the two interaction terms remain the same. We then estimated our model again while excluding the 231 observations that experienced greater than one inch of rain about the daily average. Again, the inferences remain the same. The only implication of excluding these rainiest observations it that the estimate for turnout is no longer statistically significant (it is only just barely significant in the IV model presented in Table 1). This has little meaning, though, as this coefficient only reveals the effect of turnout when there is a Democratic president and a county with a partisan composition of zero percent—no such case exists in reality.

[33] The Kleibergen-Paap (2006) rk Lagrange multiplier statistic indicates that when we exclude these two elections our IV model is insufficiently identified.

[34] We calculated the global Moran's *I* for the residuals for all four components of our IV model (the three first stage models and the second stage model). For a given county *c* for election *e*, we use a weight of 1 for every other county in *c*'s state in election *e*. Every other county in the data is then weighted at 0. With this weighting matrix, Moran's *I* ranges from a low of .310 (for the turnout × partisan composition first stage model) to a high of .414 (for the second stage, vote share model).

[35] If a state is rainy on a particular election day, then the inclusion of the relevant state-election effect (which appears, as it must, in all the first stage models as well as the second stage model) wipes out all of our instrument's leverage in terms of explaining turnout in the counties of this state compared to drier states. If a state is completely dry on election day, then the inclusion of the relevant state-election effect wipes out all of our instrument's leverage for explaining turnout in the counties of this state compared to

those in wetter states. All that is left to identify the model are the state-elections in which a meaningful number of counties experienced rain and a meaningful number did not.

[36] The specific routine we are using to estimate our model, xtivreg2 (Schaffer 2007, implemented in Stata) does not allow panels to span multiple clusters (nor were we able to find any other existing routine that allows this specification). This may be due to a lack of consensus regarding the proper degrees of freedom weighting when panels are not encompassed by clusters (personal correspondence with Schaffer, 9/24/2009). In pursuing this line of investigation, Jeff Lewis very kindly created R code that estimates fixed effects IV models while allowing for clustering on any grouping variable, regardless of panel variable. The results discussed in this paragraph were obtained with this code.

[37] Specifically, we include dummy variables indicating whether the state had a literacy test, or a poll tax, or motor voter law for the election in question. We also include the minimum number of days in advance of the election by which a voter must register. To control for the presence of other statewide contests, we include dummy variables indicating whether a gubernatorial election or Senate election was also taking place. To control for both the expected closeness of the presidential race in a given state for a given election and the State's recent voting history/ideological orientation, we include a one-election lag of state-level Democratic vote share and its square (a linear relationship between prior and contemporary vote shares can thus be accommodated by this specification, as can a non-linear relationship in the first-stage models). We also include the mean Common Space score for the state's U.S. House delegation as an additional measure of the state's ideological orientation at that point in time (see Holbrook 1991). Finally, we also include dummies indicating whether either of the parties' presidential or vice-presidential candidates were from the state in question. We then also interact these four dummies with the voting age population of the state since there is evidence that home state advantages are conditioned by the size of the state's population (see Dudley and Rapoport 1989; Lewis-Beck and Rice 1983).

[38] The Moran's *I* statistics for the residuals decrease in size for all three of the first stage models and the second stage. For example, *I* for the second-stage model decreases by 12%.

[39] The magnitude and direction of the change in Democratic vote share varies from election-to-election, depending upon the party of the incumbent president and over-time variation in our measure of partisan composition of the counties.

[40] The IV approach assumes that the excluded instrument (rainfall) is uncorrelated with the error term in the main model. Our Volatility Hypothesis does not suggest that such a correlation is present. Instead, we hypothesize that increases in turnout (instrumented) causes increases in the *variance* of the error term.

[41] We cannot employ a conventional heteroskedastic regression model because of the inclusion of an endogenous variable in both the mean and variance equations.

[42] In 1968, third-party presidential candidate George Wallace famously asserted that the two major political parties were as close as Tweedledum and Tweedledee.

[43] The instrument (i.e., predicted values) for turnout has a mean of 65.6 and a standard deviation of 9.2. The instrument for turnout × partisan composition has a mean of 2,887 and a standard deviation of 751. The instrument for turnout × Republican incumbent has a mean of 33.3 and a standard deviation of 33.7.

References

Arceneaux, Kevin, and David W. Nickerson. 2009. "Who is Mobilized to Vote? A Re-Analysis of 11 Field Experiments." *American Journal of Political Science* 53 (January): 1-16.

Berinsky, Adam J. 2005. "The Perverse Consequences of Electoral Reform in the United States." *American Politics Research* 33 (July): 471-491.

Brambor, Thomas, William Roberts Clark, and Matt Golder. 2006. "Understanding Interaction Models: Improving Empirical Analyses." *Political Analysis* 14 (Winter): 63-82.

Burnham, Walter Dean. 1965. "The Changing Shape of the American Political Universe." *American Political Science Review* 59 (March): 7-28.

Campbell, Angus. 1966. "Surge and Decline: A Study of Electoral Change." *Public Opinion Quarterly* 24 (Autumn): 397-418.

Campbell, Angus, Philip E. Converse, Warren E. Miller, and Donald E. Stokes. 1960. *The American Voter*. New York: John Wiley & Sons, Inc.

Childs, Colin. 2004. "Interpolating Surfaces in ArcGIS Spatial Analyst." *ArcUser*. Redlands, CA: ESRI Press.

Citrin, Jack, Eric Schickler, and John Sides. 2003. "What if Everyone Voted? Simulating the Impact of Increased Turnout in Senate Elections." *American Journal of Political Science* 47 (January): 75-90.

Cox, Gary W. 1988. "Closeness and Turnout: A Methodological Note." *Journal of Politics* 50 (August): 768-775.

Davidson, Russell, and James G. MacKinnon. 1993. *Estimation and Inference in Econometrics*. New York: Oxford University Press.

DeNardo, James. 1980. "Turnout and the Vote: The Joke's on the Democrats." *American Political Science Review* 74 (June): 406-20.

DeNardo, James. 1986. "Does Heavy Turnout Help Democrats in Presidential Elections?" *American Political Science Review* 80 (December): 1291-1304.

Downs, Anthony. 1957. *An Economic Theory of Democracy*. New York: Harper and Row.

Dudley, Robert L., and Ronald B. Rapoport. 1989. “Vice-Presidential Candidates and the Home State Advantage: Playing Second Banana at Home and on the Road.” *American Journal of Political Science* 33 (May): 537-540.

Dunning, Thad. 2008. “Model Specification in Instrumental-Variables Regression.” *Political Analysis* 16 (Summer): 290-302.

Erikson, Robert S. 1995. “State Turnout and Presidential Voting: A Closer Look.” *American Politics Quarterly* 23 (October): 387-396.

Gabel, Matthew, and Kenneth Scheve. 2007. “Estimating the Effect of Elite Communications on Public Opinion Using Instrumental Variables.” *American Journal of Political Science* 51 (October): 1013-1028.

Gelman, Andrew, David Park, Boris Shor, Joseph Bafumi, and Jeronimo Cortina. 2008. *Red State, Blue State, Rich State, Poor State: Why Americans Vote the Way They Do.* Princeton: Princeton University Press.

Gerber, Alan S., and Donald P. Green. 2000. “The Effects of Personal Canvassing, Telephone Calls, and Direct Mail on Voter Turnout: A Field Experiment.” *American Political Science Review* 94 (September): 653-663.

Geys, Benny. 2006. “Explaining Voter Turnout: A Review of Aggregate-Level Research.” *Electoral Studies* 25 (December): 637-663.

Gomez, Brad T., Thomas G. Hansford, and George A. Krause. 2007. “The Republicans Should Pray for Rain: Weather, Turnout, and Voting in U.S. Presidential Elections.” *Journal of Politics* 69 (August): 649-663.

Grofman, Bernard, Guillermo Owen, and Christian Collet. 1999. “Rethinking the Partisan Effects of Higher Turnout: So What’s the Question?” *Public Choice* 99 (June): 357-376.

Hajnal, Zoltan, and Jessica Trounstine. 2005. “Where Turnout Matters: The Consequences of Uneven Turnout in City Politics.” *Journal of Politics* 67 (May): 515-535.

Hetherington, Marc J. 1999. “The Effect of Political Trust on the Presidential Vote, 1968-96.” *American Political Science Review* 93 (June): 311-326.

Highton, Benjamin, and Raymond E. Wolfinger. 2001. “The Political Implications of Higher Turnout.” *British Journal of Political Science* 31 (January): 179-223.

Holbrook, Thomas M. 1991. “Presidential Elections in Space and Time.” *American Journal of Political Science* 35 (February): 91-109.

Jacobson, Gary C. 2004. *The Politics of Congressional Elections*, 6th ed. New York: Longman.

Kaufman, Karen M., John R. Petrocik, and Daron R. Shaw. 2008. *Unconventional Wisdom: Facts and Myths about American Voters*. New York: Oxford University Press.

Kleibergen, Frank, and Richard Paap. 2006. “Generalized Reduced Rank Tests Using the Singular Value Decomposition.” *Journal of Econometrics* 133 (July): 97-126.

Leighley, Jan E., and Jonathan Nagler. 1992. “Socioeconomic Class Bias in Turnout, 1964-1988: The Voters Remain the Same.” *American Political Science Review* 86 (September): 725-736.

Levi, Margaret, and Laura Stoker. 2000. “Political Trust and Trustworthiness.” *Annual Review of Political Science* 3 (June): 475-507.

Lewis-Beck, Michael S., and Tom W. Rice. 1983. “Localism in Presidential Elections: The Home State Advantage.” *American Journal of Political Science* 27 (August): 548-56.

Martin, Paul S. 2003. “Voting’s Rewards: Voter Turnout, Attentive Publics, and Congressional Allocation of Federal Money.” *American Journal of Political Science* 47 (January): 110-127.

Martinez, Michael D., and Jeff Gill. 2005. “The Effects of Turnout on Partisan Outcomes in U.S. Presidential Elections 1960-2000.” *Journal of Politics* 67 (November): 1248-1274.

Miguel, Edward, Shanker Satyanath, and Ernest Sergenti. 2004. “Economic Shocks and Civil Conflict.” *Journal of Political Economy* 112 (August): 725-753.

Mooney, Christopher Z., and Robert D. Duval. 1993. *Bootstrapping*. Beverly Hills, CA: Sage Publications.

Nagel, Jack H., and John E. McNulty. 1996. “Partisan Effects of Voter Turnout in Senatorial and Gubernatorial Elections.” *American Political Science Review* 90 (December): 780-793.

Nagel, Jack H., and John E. McNulty. 2000. “Partisan Effects of Voter Turnout in Presidential Elections.” *American Politics Quarterly* 28 (July): 408-429.

Nicholson, Stephen P. and Ross A. Miller. 1997. “Prior Beliefs and Voter Turnout in the 1986 and 1988 Congressional Elections.” *Political Research Quarterly* 50 (March): 199-213.

Pacek, Alexander, and Benjamin Radcliff. 1995. “Turnout and the Vote for Left-of-Centre Parties: A Cross-National Analysis.” *British Journal of Political Science* 25 (January): 137-143.

Pagan, Adrian. 1984. “Econometric Issues in the Analysis of Regressions with Generated Regressors.” *International Economic Review* 25 (February): 221-247.

Petrocik, John R. 1981. “Voter Turnout and Electoral Oscillation.” *American Politics Quarterly* 9 (April): 161-180.

Radcliff, Benjamin. 1994. “Turnout and the Democratic Vote.” *American Politics Quarterly* 22 (July): 259-276.

Rosenstone, Steven J., and John Mark Hansen. 1993. *Mobilization, Participation, and Democracy in America*. New York: Macmillan Publishing Company.

Rosenstone, Steven J., and Raymond E. Wolfinger. 1978. “The Effect of Registration Laws on Voter Turnout.” *American Political Science Review* 72 (March): 22-45.

Schaffer, Mark E. 2007. “xtivreg2: Stata Module to Perform Extended IV/2SLS, GMM and AC/HAC, LIML and k-class Regression for Panel Data Models.” <http://ideas.repec.org/c/boc/bocode/s456501.html>

Staiger, Douglas, and James H. Stock. 1997. “Instrumental Variables Regression with Weak Instruments.” *Econometrica* 65 (May): 557-86.

Stein, Michael L. 1999. *Interpolation of Spatial Data: Some Theory for Kriging*. New York: Springer.

Teixeira, Ruy. 1992. *The Disappearing American Voter.* Brookings Institution Press.

Tucker, Harvey J., and Arnold Vedlitz. 1986. “Does Heavy Turnout Help Democrats in Presidential Elections?” *American Political Science Review* 80 (December): 1291-1304.

Wolfinger, Raymond E., and Stephen J. Rosenstone. 1980. *Who Votes?* New Haven, CT: Yale University Press.

Wooldridge, Jeffrey M. 2002. *Econometric Analysis of Cross Section and Panel Data*. Cambridge, MA: MIT Press.

TABLE 1. County-Level Democratic Vote Share in Presidential Elections, 1948-2000.

Independent Variable	*Actual Turnout* OLS Estimate (Robust Stand. Error)	*Instrumented Turnout* IV Estimate (Robust Stand. Error)	IV Estimate (Robust Stand. Error)
Turnout	-.066 (.020)	.405* (.242)	.397* (.231)
Turnout × Partisan Composition	.004 (.000)	-.011* (.003)	-.012* (.004)
Turnout × Republican Incumbent	.155* (.007)	.469* (.102)	.723* (.107)
Partisan Composition	.595* (.029)	1.43* (.231)	1.48* (.231)
State-Election Controls?	No	No	Yes
N	27,401	27,401	27,401
F-test, all covariates	3,371*	2,901*	1,341*
R^2	.798	.644	.588
F-test, excluded instruments for Turnout	---	22.1*	30.1*
F-test, excluded instruments for Turnout × Partisan Comp.	---	51.1*	34.1*
F-test, excluded instruments for Turnout × Republican Inc.	---	53.5*	67.0*

Notes: * $p \leq .05$ (one-tailed test). Fixed effects for counties and for election years are included in all the models. Southern counties are excluded from the analysis.

TABLE 2. Inferred Voting Patterns of Marginal Voters When a County's Turnout Increases from 50% to 51%.

Partisan Composition/ Democratic Vote Share at 50% Turnout	Percentage of Additional Voters Voting for Democratic Candidate	Difference from the Rest of the Voters
Republican Incumbent		
30%	57.7%	+ 27.7%
40%	62.1%	+ 22.1%
50%	66.5%	+ 16.5%
60%	70.9%	+ 10.9%
70%	75.3%	+ 5.3%
Democratic Incumbent		
30%	33.8%	+ 3.8%
40%	38.2%	- 1.8%
50%	42.6%	- 7.4%
60%	47.0%	- 13.0%
70%	51.4%	- 18.6%

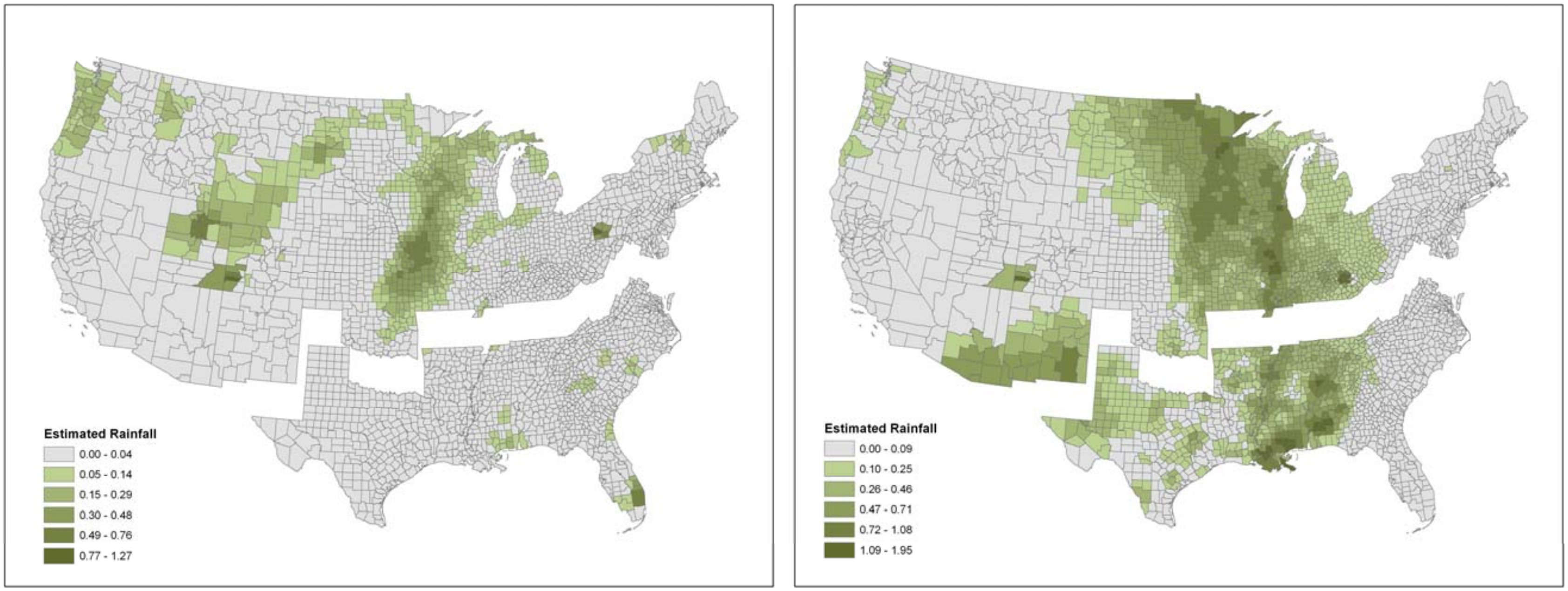
Notes: These vote shares are calculated based on the IV results presented in column 2 of Table 1. For all of these calculations the baseline turnout is set at 50% and the baseline Democratic vote share is set equal to partisan composition (e.g., a partisan composition of 60% Democratic equals a baseline vote share of 60% Democratic).

TABLE 3. Volatility in County-Level Democratic Vote Share in Presidential Elections, 1948-2000.

Independent Variable	*Actual Turnout* OLS Estimate (Bootstrap Standard Error)	*Instrumented Turnout* IV Estimate (Bootstrap Standard Error)
Turnout	-.072 (.051)	2.00* (.858)
N	27,401	27,401
Wald test for all covariates	832*	684*
R^2	.029	.026
F-test for excluded instruments for Turnout	---	22.1*
F-test for excluded instruments for Turnout × Partisan Composition	---	51.1*
F-test for excluded instruments for Turnout × Republican Incumbent	---	53.5*

Notes: * $p \leq .05$ (one-tailed test). The dependent variable for the OLS model is the squared residuals from the OLS model presented in Table 1. The dependent variable for the IV model is the squared residuals from the IV model without State-Election controls presented in Table 1. Fixed effects for counties and for election years are included in the model. Southern counties are excluded from the analysis.

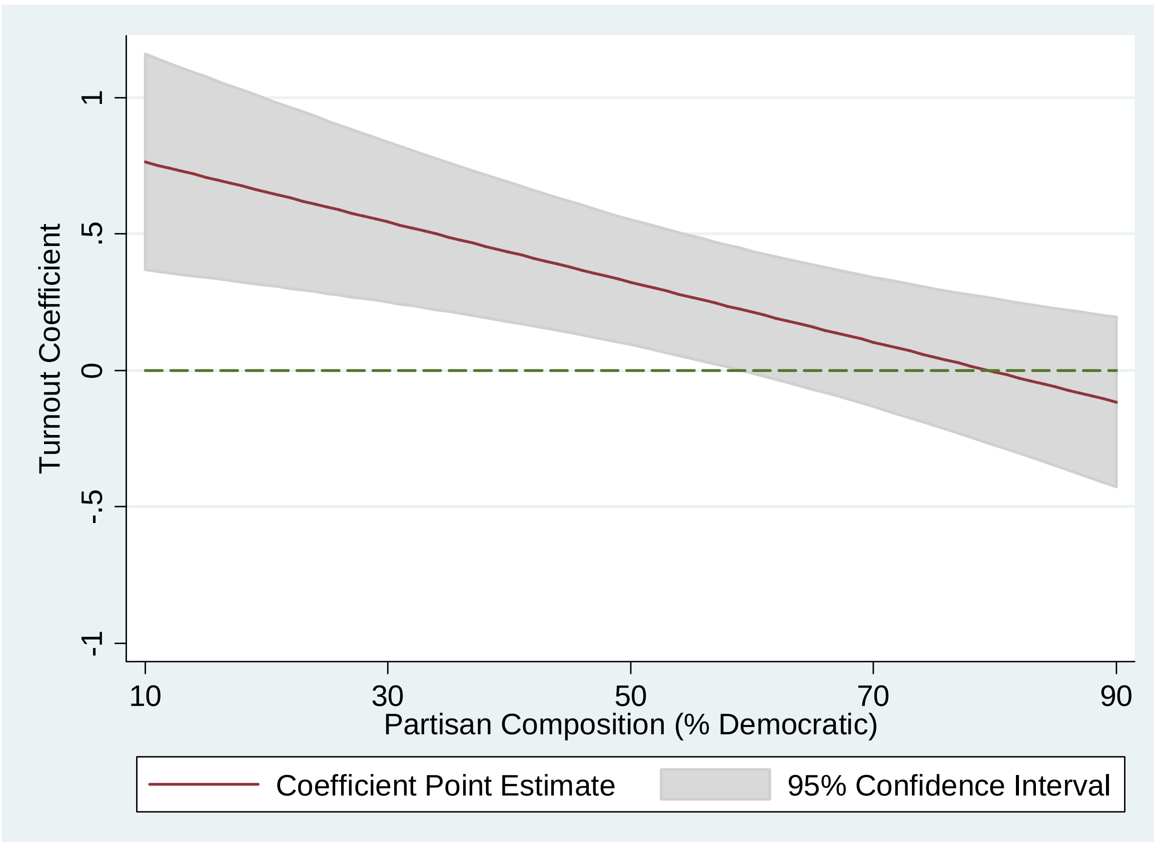
FIGURE 1. Estimated Rainfall During the 1996 and 2000 Elections.



November 5, 1996 November 7, 2000

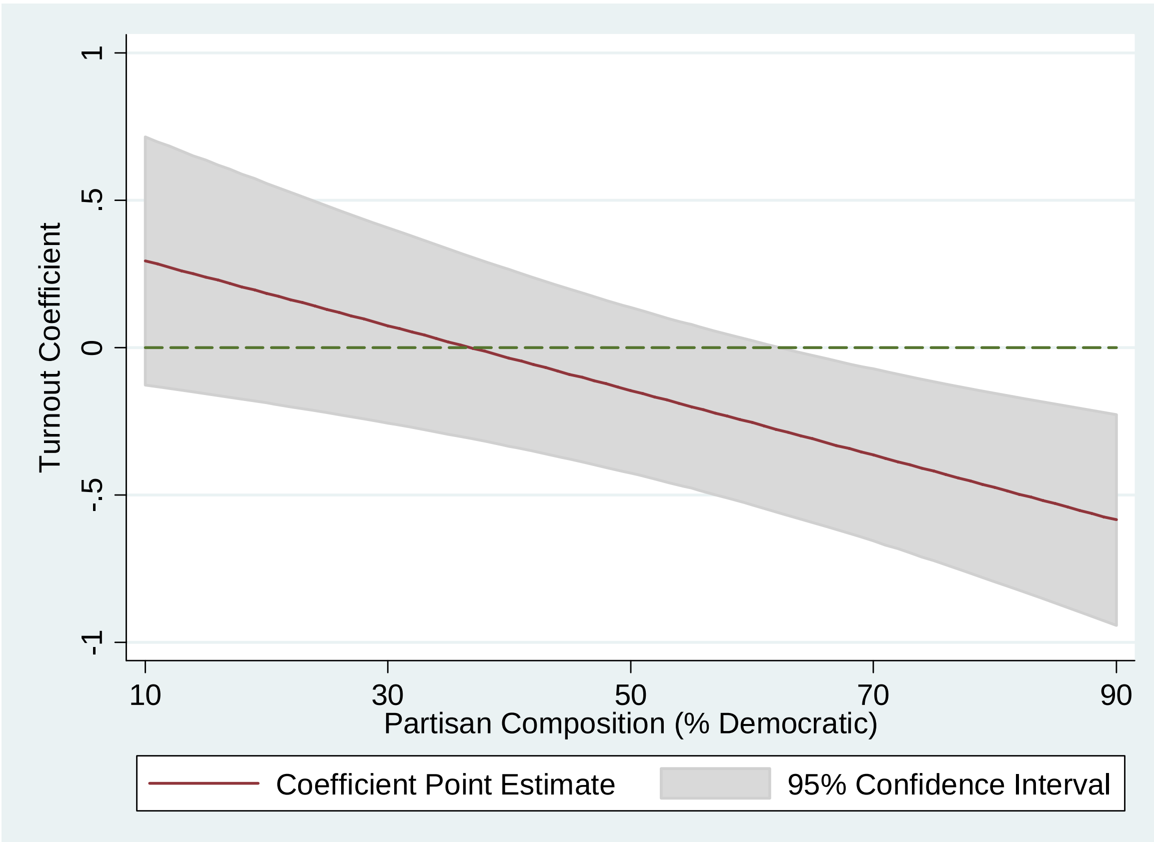
Note: Estimated rainfall is in inches. The eleven states of the Southern Confederacy were excluded from the analyses presented in this paper but were used in the GIS interpolation of our county-level rainfall estimates. Similarly, the state of Oregon, though shown here, was excluded from our 2000 data because the state utilized a vote-by-mail program that resulted in nearly all votes being cast before election day but was used for interpolation.

FIGURE 2. Coefficient for Turnout, Conditional on Partisan Composition (Republican Incumbent).



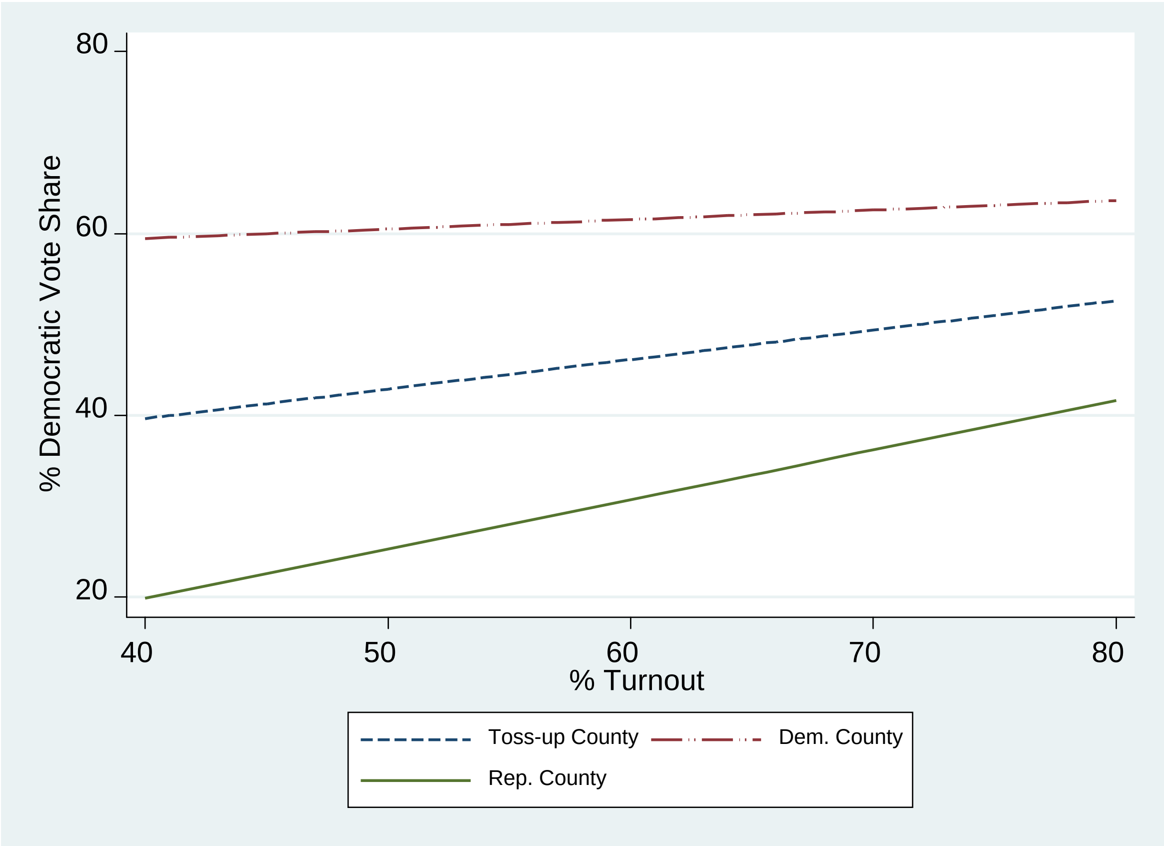
Note: Coefficients and confidence intervals are generated by the IV results in the second column of Table 1.

FIGURE 3. Coefficient for Turnout, Conditional on Partisan Composition (Democratic Incumbent).



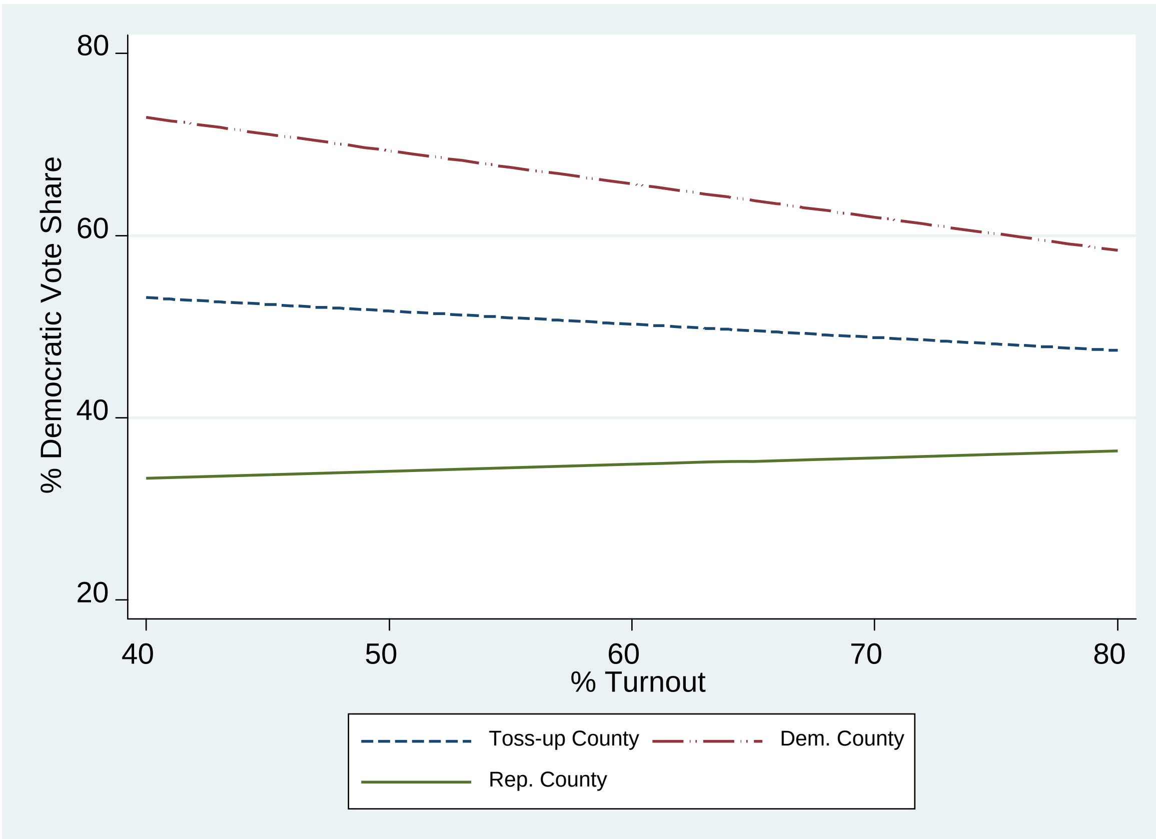
Note: Coefficients and confidence intervals are generated by the IV results in the second column of Table 1.

FIGURE 4. The Effect of Turnout and Partisan Composition on Predicted Democratic Vote Share (Republican Incumbent).



Note: Predicted values are generated by the IV model presented in the second column of Table 1. For the purposes of this figure, we define toss-up, Republican, and Democratic counties as ones in which (Democratic) partisan composition equals 50%, 30%, and 70%, respectively. The election-specific fixed effect is set at the average effect for elections in which there was a Republican incumbent.

FIGURE 5. The Effect of Turnout and Partisan Composition on Predicted Democratic Vote Share (Democratic Incumbent).



Note: Predicted values are generated by the IV model presented in the second column of Table 1. For the purposes of this figure, we define toss-up, Republican, and Democratic counties as ones in which (Democratic) partisan composition equals 50%, 30%, and 70%, respectively. The election-specific fixed effect is set at the average effect for elections in which there was a Democratic incumbent.