The Unswayed Voter: How a Polarized Electorate Responds to Economic Growth

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November 12, 2021

Abstract

It is well known that higher economic growth benefits incumbents in elections. However, in the last thirty years, US politics has been marked by substantial increases in political polarization and a decline in the number of swing voters. Accordingly, we would expect that the effect of economic growth on incumbent vote share has declined. Indeed, using a Bartik-type instrument, I present new evidence that this effect is smaller under conditions of polarization. Using separate state-level data and individual-level data sets, I find that the effect of state economic growth on incumbent vote share is smaller when state-level polarization, or individual partisanship, is stronger. Using a swing voting propensity score, I show that swing voting and economic voting are closely linked. Lastly, I find evidence that college-educated voters are a major driver of the decline in both swing voting and economic voting. (JEL D72, H70, N42)

*University of Toronto, **Email:** robert.embree@mail.utoronto.ca , **Website:** robertembree.ca . I'd like to thank my supervisor Shari Eli for her time and guidance during the writing of this paper. I am also grateful to my committee members Peter Loewen and Kory Kroft, who have been hugely supportive and insightful. I have benefited greatly from the support of the faculty at the University of Toronto, and would like to thank Loren Brandt, Gustabo Bubonis, Stephan Heblich, Rob McMillan, Jordi Mondria, David Price, Michael Smart, Michael Stepner, Colin Stewart, Nate Vellekoop, Ronald Wolthoff, and many seminar participants. I would also like to thank Michael Donnelly, Eric Merkley, and the rest of the Political Behaviour Group at the University of Toronto Political Science Department, and my fellow Ph.D. students John Cairncross, Dylan Gowans, Torsten Jaccard, Aly Somani, Tom Stringham, Alison Taylor, and Jamie Uguccioni. Dan Westlake, Vikram Rai, Sean McGarry, Larry Bartels, and Christopher Achen provided helpful advice. All errors are my own.

1 Introduction

The context of modern American politics is one of polarization. By almost any measure, politics is much more polarized than it was in the 1970s and 1980s. Voters are more partisan, there are fewer swing voters, and voters interpret facts through a more partisan lens. Many politicians have responded to these trends by becoming more partisan and have deliberately tried to polarize the electorate themselves. We would expect that these factors change the relationship between economic growth and voter behaviour.

There is certainly anecdotal evidence of a change in the effect of economic growth on incumbents. Between 1948 and 1992, there were many elections in which economic growth appeared to play a clear and decisive role. The landslide re-elections of Lyndon Johnson in 1964, Richard Nixon in 1972, and Ronald Reagan in 1984 were driven in part by substantial election-year economic growth, while Jimmy Carter's landslide loss in 1980 could be attributed to that year's recession. More recent elections, such as Al Gore's loss in 2000 in spite of a booming economy, might suggest a change in the way swing voters make decisions. As a motivating result, consider Figure 1, which shows the lines of best fit for tenure-adjusted national popular vote margins by the incumbent in the eras 1948-1992 and 1996-2016. (I have omitted 2020 due to the unusual gap between disposable income growth and earnings growth in that year). We can see the much shallower slope in the modern era, and we might suspect that partisanship has substituted for economic factors in voter decision-making. Furthermore, less voter responsiveness to economic outcomes may impact the usefulness of elections as accountability mechanisms under polarization.

However, despite the rise in voter partisanship and polarization, the political science and economics literature on presidential elections and the economy typically posits that the effect of economic growth is constant over time and does not vary with levels of partisanship. Furthermore, much of that literature uses OLS and is thus not robust to endogeneity concerns. In this paper, I question these approaches, and seek to understand the effect of economic growth on a polarized electorate with an instrumental variables approach. My research question is: how does political polarization change the effect of economic growth on incumbent vote share? The goal would be to deepen our causal understanding of the relationship between economic growth and incumbent vote share under these conditions, not simply to make a predictive statement.

I use two distinct data sets: state-level data with actual election results and US Census data, and individual-level data from the American National Election Survey (ANES), each of which have advantages and disadvantages. The state-level data uses actual results and thus has no issue with sampling error, but aggregate results make it more difficult to understand the mechanisms of individual voter behaviour. The individual-level data allows for a more detailed analysis of specific factors influencing individual voters, but contains some state-years with few or no observations. By using both data sets separately, I can compare results between state-level and individual-level regressions and confirm that they are consistent. The relationship between growth and incumbent vote share is confounded by many factors, such as state-specific policy choices, and so OLS estimates may not be reliable. Therefore, I use a Bartik-type shift-share instrument in order to identify the effect of state economic growth on incumbent vote share. A shift-share instrument is a type of instrument that weights the variable of interest (in this case state earnings per capita) by the sector-specific trend. This is, in other words, how the state's earnings per capita would be expected to have grown if each sector followed national trends. Using this as an instrument will omit idiosyncratic parts of state growth which are attributable to purely local factors and which may give rise to endogeneity concerns. For example, without an instrument, we might be concerned that if the president supports spending in one particular state, and that spending is popular, then the spending could cause both growth and incumbent popularity. In such a case, the growth would be correlated with the popularity but not causing it. Furthermore, the use of the Bartik instrument may reduce concerns about measurement error in state-level data. This is always a concern in state-level data, much more so than national-level. It may be the case that national-level sector specific growth rates have less measurement error than aggregate state-level growth rates.

I then consider a series of 2SLS models, which each include a different measure of polarization as an interaction term with growth. In the individual-level data, I examine four different variables which are interacted with growth: state legislative polarization, partisanship, swing voting propensity, and college education. For the state-level data, I am able to use two of those interaction terms (state legislative polarization, and college education levels) to look for comparable effects at the aggregate level. State legislative polarization is a useful measure because it is indicative of the state's electorate being more polarized.

The results all strongly suggest that the effect of economic growth on incumbent vote share declines under conditions of political polarization. Specifically, individual voters are less affected by economic growth in decision making when they are more partisan, more likely to be swing voters, college educated, or living in states with more polarized legislatures. For state legislative polarization and college education, the state-level results are consistent with the individual-level findings.

This paper extends four main strands of the literature in both political economy and economic history. It contributes to the literature on modelling presidential elections using economic growth at the national level; the literature on state and local effects of the economy on elections, including the literature that engages with issues of endogeneity; the literature on political polarization and its effects, and related work on swing voting and partisanship in US elections; and the economic history literature on the effect of growth in different time periods.

Economic conditions matter enormously for vote choice, a fact well established in the political science literature (Lewis-Beck and Stegmaier, 2000). A useful starting point is the model of Achen and Bartels (2017), which explains incumbent vote shares in US presidential elections using only 2 variables: incumbent party tenure in office, and real disposable personal income growth in the 2 quarters before an election. However, their model is less accurate for the last 5 US elections from 2000 through 2016. This may be related to increasing polarization in US politics. Achen and Bartels (2004, 2017) show that voters are myopic, and care about the short term more. Bartels and Zaller (2001) argue that disposable personal income may be a better measure of the economy for forecasting purposes than GDP growth. Duch (2007) summarizes major contributions in economic voting research, and Lewis-Beck and Steigmeier (2007) explain differing models of economic voting, with both agreeing that economic conditions typically play a major role and that incumbents benefit from economic growth independent of policy choices. Duch and Stevenson (2010) discuss how perceptions of competency are shaped by global and local economic conditions, and argue that voters are at least partly able to determine how unexpected shocks are the result of actions by incumbents. Lewis-Beck and Costa Lobo (2017) discuss the challenges of studying the effect of the economy on vote choice in the more economically volatile 21st century, and Kayser (2005) studies economic actions by incumbents that are motivated by electoral concerns, and finds that such manipulative actions decrease with both overall economic growth and economic volatility. Almost all of these models, such as Lewis-Beck and Tien (2014) or the work of Hibbs (2000, 2008), are assuming a constant effect of economic growth across time, a view challenged by this paper. In particular, I am contributing more to the literature that treats economic growth as a causal factor, such as the state-level work below, rather than those papers which are using economic growth simply as a predictor of outcomes or parsimonious forecasting models (such as Hibbs 2000, 2008; Achen and Bartels 2017; Bartels and Zaller 2001; Abrams and Butkiewicz 1995). As well, we should be aware that these approaches use OLS and are not, in general, robust to endogeneity concerns. Thus, I consider them mostly as a point of departure.

There is an extensive literature on state and local economic effects on voting (Park and Reeves 2018, Kramer 1983). The sociotropic voting literature, beginning primarily with Kinder and Kiewet (1979), argues that voters care about the nation, not just their own income, which explains why national growth rates seem to matter more than state or local ones. Consumption of national media leads to a higher salience of national than state economic conditions. The literature comparing national to state or local effects tends to be closer to an analysis of causal factors than simply engaging in forecasting, as these papers typically want to determine the mechanisms through which economic factors affect voter behaviour. In terms of state-level or more local effects, Abrams and Butkiewicz (1995), and Blackley and Shepard (1994), each show that economic conditions affected the 1992 presidential election. Brunk and Gough (1983) find that in the 1980 election, declines in income helped the challenger but higher unemployment actually helped the Democratic incumbent. Eisenberg and Ketcham (2004) study the different effects of national, state, and county economic conditions, and find useful estimates which are a good comparison point for this paper. Holbrook (1991) shows that state outcomes are responsive to both national-level and state-level factors. Healy and

Lenz (2017) use a rich data set to show that, when data is good enough to avoid measurement error, local factors may matter a great deal. They suggest that conventional findings about the small impact of local economic conditions may be wrong. This gives additional motivation to the use of an instrument, as national-level shocks and state earnings shares may be measured with greater precision than state-level growth, especially for just two quarters of growth. Past work on voting using either an instrument or exogenous variation includes exposure to Chinese imports (Autor et al. 2017) or the regionally-clustered winners of the lottery in Spain (Bagues and Esteve-Volart 2016). Wolfers (2002) uses an oil-price instrument for gubernatorial elections, and mentions that he attempts a Bartik-type instrument but that it lacks power to explain gubernatorial election results. Rudolph and Grant (2002) use a responsibility instrument in survey data but are not performing a traditional IV. This paper's use of a shift-share instrument in this context is novel.

The literature on the effects of polarization has several strands. Abramowitz (2018) describes the rise in polarization along the lines of identity and ideology in the US, and how these trends have intensified since the late 1990s. Abramowitz (2014) discussed how polarization affects presidential elections, and specifically makes outcomes more stable and margins of victory smaller. Bitecofer (2020a) suggests a model that includes no economic variables at all, and assumes that negative partisanship drive voting outcomes in the Trump era, suggesting that voting can be forecast more accurately by demographic factors such as race and education. Abramowitz (2012) finds that, with the polarization adjustment for the modern era, the coefficient on GDP growth actually goes up - but, he is including approval ratings, which are highly correlated with GDP growth. Small and Eisinger (2020) show that the link between presidential approval and consumer sentiment, typically positive, was severed when Obama became President, and that under both Obama and Trump there is little to no relationship at all. This paper extends the literature by showing a well-established reduction in the effect of economic growth under conditions of polarization.

This paper is also motivated further by the literatures on both swing voting and partisan attribution behaviour. There is a substantial reduction (Smidt 2017) in the number of swing voters across time. Note that this is not seen in a reduction in the number of registered independents, but as an increase in the partisanship of both registered party members and independents. Alt et al. (2016) argue that swing voters are more likely to consider economic information if they are more sophisticated voters, and discuss the role of information level in economic voting. This suggests the importance of controlling for information level, as very-low-information voters will not be responsive to economic variables - but can still be swing voters. Gelman et al. (2016) argue that the apparent presence of poll swing in contemporary elections, which suggest the existence of swing voters, may instead be due to response biases which are not properly accounted for by most pollsters. Hansford and Gomez (2015) find that when an incumbent is on the ballot, economic assessments by voters are affected by incumbent assessments and are not exogenous. Stanig (2013) finds substantial polarization in such assessments alongside the polarization in the electorate. Evans and Pickup (2010) find evidence that economic perceptions are due to levels of incumbent support rather than the reverse, and this is consistent with experimental evidence (Bisgaard 2019) and global findings from many countries (Becher and Donnelly 2013). We would expect that higher numbers of swing voters, and increase in partisan attribution behaviour, would be associated both polarization and with a lower effect of economic growth on incumbent vote share - an expectation confirmed by the results here.

In a historical context, Lin (1999) suggests that the sensitivity of US election outcomes to the economy was much higher in the mid-20th century than the late 19th century. A more recent paper is the work on the economy in historical US presidential elections by Guntermann et al. (2021). They find evidence for the existence of retrospective economic voting in all time periods. However, they do not consider the possibility that more recent (post-1996) elections have a lower coefficient on economic growth, and instead follow the general practice of assuming a single coefficient across longer time periods.

This paper has further ramifications for both democratic accountability and strategic decision-making by campaigns. If indeed the effect of economic growth on incumbent vote share has declined, that may imply that increases in polarization have weakened the accountability mechanisms in US democracy. Przeworski et al. (1999) examine the strength of accountability mechanisms in democracy, and discuss whether they are sufficient to ensure effective representation. They differentiate between the "mandate" view of democracy and the "accountability" view. Problems with the accountability view can arise if voters are not sufficiently informed, or if their voting considerations do not depend sufficiently on accountability considerations. Bitecofer (2020b) suggests that polarization may substantially reduce democratic accountability if job performance by incumbents cannot sway voters. This concern represents one of the major theoretical implications of this paper, that political polarization reduces the likelihood of holding incumbents responsible for economic policy and outcomes, and thus may affect incumbent behaviour in negative ways. Furthermore, Vavreck (2009) argues that messaging matters in presidential elections, and the strategic choice of message by a candidate should depend in part on whether they are favoured by the economic fundamentals. Thus, this work may have strategic implications for campaigns: using models that assume a constant effect, they may make incorrect assumptions about optimal strategies.

2 A Simple Model of Economic Voting

I construct a model that will take specific predictions about micro-level behaviour observed in survey data, such as a decline in swing voting, and understand how that might increase or decrease the effect of economic growth on voting for the incumbent. Define $B(g)$ as the utility that the voter gets from aggregate growth level g, which could combine personal financial benefit and the utility benefit of the entire community having higher growth ("sociotropic voting"). Assume $B(g) > 0$. As a simplifying assumption, in the first version of the model we assume that B is constant across individuals, and not correlated with threshold t.

Let n correspond to the non-economic events (candidate choices and actions, media coverage, leadership ability, scandals) that affect voter choices, and let $V(n)$ represent the net benefit to the incumbent, relative to the challenger, of those events.

Define a threshold of partisan preference $t \in [-T, T]$ which represents the strength of the voter's underlying partisan and ideological preference. This preference t varies across the population, with T as the boundary of maximum possible partisan preference. This preference is a threshold, because it determines the voter's threshold for switching from their preferred candidate due to the effects of growth and non-economic events. For voters with a very strong preference for the incumbent, t is very low. Let $P(t)$ be the CDF of the distribution of the thresholds, with $p(t)$ the PDF.

The voter votes for the incumbent if

$$B(g) + V(n) \geq t \tag{1}$$

And for the challenger if

$$B(g) + V(n) < t \tag{2}$$

We assume everyone votes. We want to find the median voter, the one who is indifferent, to pin down the vote share. We define m to be the opinion of the median voter, the one who is indifferent between the two parties. The value of m will depend on g and n.

Median voter position m will represent the solution to the equation

$$f(m, g, n) = B(g) + V(n) - m = 0 \tag{3}$$

Let $S(g)$ be the incumbent vote share with growth level g. Then

$$S(g) = \int_{-T}^{m} p(t)dt \tag{4}$$

Totally differentiating $f(m, g, n) = 0$ yields the following.

$$\frac{\partial f}{\partial m}dm + \frac{\partial f}{\partial g}dg + \frac{\partial f}{\partial n}dn = 0 \tag{5}$$

$$(-1)dm + B'(g)dg + V'(n)dn = 0 \tag{6}$$

Setting $dn = 0$ we have

$$\frac{dm}{dg} = B'(g) \tag{7}$$

And,

$$\frac{\partial S(g)}{\partial g} = p(m)\frac{dm}{dg} = p(m)B'(g) \tag{8}$$

Clearly, a decline in $\frac{\partial S(g)}{\partial g}$ can be explained either by a drop in $p(m)$, corresponding to fewer swing voters, or a drop in $B'(g)$, lower responsiveness to economic events. More precisely, this model makes the following prediction:

Proposition 1. *A decline in the density of swing voters $p(m)$ will reduce the effect of economic growth on incumbent vote share.*

A decline in $p(m)$ could correspond to a general shift of voters away from moderate and towards more extreme thresholds on either side. That could be an incremental shift by voters at all points on the distribution, or it could be a sharper move of voters from the middle towards a bimodal distribution clustered around the tails. Either type of shift would be consistent with increased ideological and partisan polarization, and a decline in the number of swing voters as in Smidt (2017). The model would also predict that a drop in $B'(g)$ would reduce $\frac{\partial S(g)}{\partial g}$, but that is a distinct research question outside the scope of this paper. By using measures of political polarization that would be associated with $p(m)$, we can test the prediction that it would reduce $\frac{\partial S(g)}{\partial g}$.

3 Data

I use two different data sets: state-level data on election outcomes, demographics, and real personal income per capita, and individual-level survey data from the American National Election Survey (ANES). I obtain state-level presidential election data for all presidential elections up to 2012, and for 2020, from Dave Leip's Atlas of Election Results. Election data for 2016 is from the MIT Data Centre. I take the years 1948-2020 as my baseline data set, with subsets used where some demographic controls are not available. This gives 19 years, 51 states, and 959 observations in the full state-level data set: there is no AK or HI for 1948, 1952, 1956 because they were not yet states, and no DC for those years or 1960 because it did not yet get to vote in presidential elections. I look at state-level vote totals in presidential elections, ignoring any differences in voter behaviour that may result from Maine and Nebraska's system of allocating electoral votes by Congressional district. I remove third party candidates and compute the share of the two-party vote received by the incumbent party in a presidential election.

From the BEA, I obtain real earnings and sector-specific earnings at the national and the state level, as well as state and national personal income and real disposable personal income for robustness checks. For state-level college attainment data, I use American Community Survey data, obtained through IPUMS and the Census Bureau. From the US Census bureau I obtain data on white and Black population shares (whites being non-Hispanic whites where that data is available), population density, and homeownership rate (which is detrended by year).

I use the four Shor-Mccarty indices of state legislative polarization. Shor and McCarty calculate the ideological position of all members of a legislative chamber (state house/assembly or state senate), then compute the distance between the median Democrat and median Republican (giving the two "difference" measures). In practice, the medians vary between -3 and 3, and the distances vary from 0 to 4. They also compute the average distance among all members (the two "distance" measures). They prefer the difference measure as a cleaner measure of polarization. The Shor-McCarty indices of state legislative polarization are obtained through the Harvard Dataverse.

For the individual-level data, I start with the ANES, with data covering 1952-2016. In DeBell (2010), there is a useful discussion of the best practices for working with the ANES data. There are probability weights for representativeness of each observation, which I use below. The ANES does not sample equally from every state-year, and their probability weights are designed for national, rather than state-year-level, demographic representativeness. Indeed, the ANES systematically undersamples state-years in smaller and heavily white states, which thus have different characteristics such as population density and the income share from farm earnings. There is a correlation between undersampling (relative to state population) and the relationship between the economy and incumbent vote share - voters in undersampled states tend to also have a lower relationship between the economy and incumbent vote share. Thus, to ensure that my individual-level and state-level results are comparable, I modify the ANES weights in the following way. I sum the ANES-provided probability weights in each state year, and then multiply them by a state-year-specific constant so that the sum of those weights exactly equals the state's population (in hundreds of thousands) in that year. Thus, each state's observations in that year are proportional to that state's share of the national population. This is precisely analogous to the weighting procedure I follow for state-level results above, where I have probability weights equal to state populations.

Additional information on data sources and methods can be found in Appendix A.

4 Identification Using a Bartik-Type Instrument

4.1 Motivation for the Instrument

The use of OLS to estimate the effect of economic growth on either incumbent vote share or individual vote choice raises concerns about omitted variable bias. For example, if presidential policies are causing growth in some sectors nationally, and if those changes also have political implications, this could cause spurious correlation between growth and political outcomes. State growth can be correlated with any number of state-specific policies, such as infrastructure spending. These state-specific policies enacted by the president may cause an endogeneity problem. For example, suppose defence spending affects particular states, and that those states have a higher population of veterans and military families. Then, military spending may be popular in those states because of the composition of the electorate, and military spending may also cause idiosyncratic state growth, but they may merely be correlated and not causally related.

A second concern could be measurement error, which is always a possibility when using state-level growth measures, as discussed in Wolfers (2002). Any growth rate calculated at the state level will typically be less accurate than a national growth rate, due to smaller bases of sampling or observation. In the context of economic voting, Healy and Lenz (2017) show that using extremely accurate local data will increase the estimated coefficient on local economic growth substantially, suggesting that attentuation bias from measurement error is an issue.

To overcome these two concerns, I use a Bartik-type shift-share instrument. This instrument will separate idiosyncratic state growth from growth due to differing exposure to national shocks. As well, Bartik-type instrument may actually be a more accurate measure of state growth than the state growth itself, so the IV approach will reduce attenuation bias due to measurement error. The Bartik-type instrument reslies on national sector growth rates, which are calculated from large national data sets, and thus include much less measurement error. State shares of each sector have less volatility from quarter to quarter than overall state growth rates, and thus likely have less measurement error as well.

The Bartik-type instrument lets us identify those parts of state growth which stem only from differential exposure to national shocks, and thus excludes both the purely national growth rate (removed with time FE) and any idiosyncratic state growth. The exclusion of national growth effects with time FE is important because the sociotropic voting literature establishes that national growth affects incumbent vote share more strongly than state growth does.

4.2 Construction of the Instrument

I construct a Bartik-type shift-share instrument: a shift-share instrument where national trends in the growth of each sector are weighted by earnings shares in each state. Specifically, index sectors by i, states by j, and elections by t. Let $n_{i,t}$ correspond to two quarters of national growth in sector i leading up to election t, and let $a_{i,j,t}$ represent the shares of each sector in the state at the start of the first quarter of the year of election t, with $\sum_i a_{i,j,t} = 1$. Then we can write the Bartik-type instrument as $Bartik_{j,t} = \sum_i a_{i,j,t} n_{i,t}$.

The weights are earnings shares, rather than employment shares as in the canonical Bartik instrument. There are 10 sectors, the details of which are discussed in Appendix A. The earnings instrument is preferable as it more closely matches the exposure of the state earnings to different sectors, and earnings is our main independent variable of interest. To the extent that there are differences in earnings per worker in different sectors, earnings shares may be slightly better than employment shares at capturing sectoral exposure. Furthermore, I have earnings-share data going back to 1948, while the employment share data starts in 1968, so for some results the earning share instrument provides additional observations. Broxterman and Larson (2020) discuss how Bartik-type instruments using wages or earnings and wage shares (rather than the traditional employment-share Bartik instrument) have been used in other work (Diamond 2016; Guerrieri et al. 2013; Partridge and Rickman 1995). In years where the shares may be negative, for example due to negative farm earnings, I treat the share as 0. Results are robust to the use of employment shares in the shift-share instrument, as the two instruments (earnings share or employment share) have a correlation of 0.91 and are highly similar.

I can use either current earnings shares (meaning that it is the actual share of earnings from that industry in that year), or use the constant shares taken from 1980, which corresponds to the rough midpoint of my data set. Current shares are sometimes also called updating shares in the literature on shift-share instruments. The advantage of using constant shares is that it may avoid endogeniety problems where shares go up due to extra growth in that sector, but the disadvantage is a weaker instrument. The use of current shares may be less of a problem in this case because we are only using data once every four years, so most of the changes between observations will have occurred in between, not in the quarters we are directly observing. In the results here, I am using current shares, but results are robust to the use of constant shares as discussed in the Appendix.

Achen and Bartels (2017) show that using the two quarters prior to the election gives more predictive power than using 1 year of growth, and much more than using 4 years. Following this, I take growth from Q1 to Q3 of the election year – growth in the two quarters before the election. However, one possibility is that this two-quarter finding is only true for national growth, rather than for state growth. A useful robustness check is to try 3 or 4 prior quarters of state income growth, or election calendar year growth. The results are very similar.

This instrument can be interpreted in different ways, but essentially it represents the contribution to state income growth that is due to exposure to national-level trends in each sector. Any remaining growth will be idiosyncratic growth in that particular state.

4.3 Exclusion Restriction and Validity of a Bartik-Type Instrument

For Bartik-type instruments, there is a recent literature discussing the key concerns related to establishing identification. Contemporary research on the use of Bartik-type shift-share instruments focuses on the need for either the shares to be exogenous (Goldsmith-Pinkham et al. 2018) or the shocks (growth rates) to be exogenous (Borusyak et al. 2018; Adao et al. 2019).[1] Goldsmith-Pinkham et al. (2018) say "[a] researcher is likely using research design based on the shares assumption if they [...] describe their research design as reflecting differential exogenous exposure to common shocks."

Thus, for my IV approach to be valid, the following related assumptions must hold. The exclusion restriction must hold: that the instrument will only affect the outcome variable through exposure to the variable of interest. This in turn requires that the shares be uncorrelated with changes in the levels of the outcome, either through correlation with the controls or through correlation with some omitted variable.

For example, there is the possibility of an alternative transmission mechanism related to an omitted variable rather than some included control. We could imagine that workers in some sector, for example farmers, tend to grow dissatisfied with the incumbent over time due to a lack of support. If workers in different sectors have different political attitudes towards the incumbent, that is another potential transmission mechanism where the shares are affecting the changes in the level of the outcome through a mechanisms other than differential exposure to shocks. In general, the literature does not provide strong support for this idea.

Goldsmith-Pinkham et al. note that "the empirical strategy can be valid even if the shares are correlated with the levels of the outcomes." Indeed, in this case the shares, notably farm earnings, are in fact correlated with the outcome variable, incumbent party vote share. However, the exclusion restriction still holds because I can establish that the shares are not correlated with changes in incumbent vote share, even though they might affect the levels. Goldsmith-Pinkham et al. give us a series of checks to test for this issue, which are covered in Appendices B and C.

There are two possible threats to identification which cannot be addressed by a Bartik-type instrument. Firstly, any changes in federal policy that are endogenous to state-specific expected growth based on sector shares could still be a potential issue. For example, suppose that the incumbent president seeks to target states which are heavily based in manufacturing by supporting pro-manufacturing policies. In such a

[1] Adao et al. (2019) focus on the possibility that correlations among the residuals from states with similar share profiles might lead to overly low estimates of SEs. They provide two correction procedures, which are discussed as robustness checks in Appendix B.2.

case, there could be additional growth in those states, and the incumbent is more popular in those states, but the popularity is coming from support for the policies rather than from the growth. Secondly, we would expect some selection bias in the states in which incumbents campaign. In slower-growing states, the incumbent will be less popular, and their campaigns may react strategically by devoting more campaign resources to those states, thus mitigating the drop in support (Wolfers 2002). There is not a concern about reverse causality, because while low growth might cause incumbent campaign effort and boost incumbent vote share, that would have the opposite sign of the main effect, where low growth hurts the incumbent. Thus, this biases the estimated coefficients of growth downward, so our coefficients will likely be underestimates of the true effect.

For the individual-level results below, we have the same exclusion restriction: that the instrument will only affect the outcome variable through exposure to the variable of interest. This approach passes all of the relevant robustness checks, discussed in detail in Appendix C. In particular, there is no evidence that the sector shares are correlated with the controls or introduce any issues when dropped from the growth variables, when included as separate controls, or when used separately as instruments with alternative estimators.

In the results below, I find that OLS gives smaller coefficients for earnings growth than does an IV approach. The bias depends on the correlation between the omitted variable and growth, times the effect that the omitted variable has on incumbent vote share. One of these must be positive and one must be negative for us to have a biased-downward OLS estimate.

One possible mechanism for this is state-specific fiscal policy in states with governors of the opposite party from the presidential incumbent - such fiscal policy could be positively correlated with growth, and cause political shifts away from the presidential incumbent. Another possibility is state-specific federal program spending which is counter-cyclical, and in some way treats growth differences from national shocks differently than idiosyncratic state growth. However, the most likely candidate for the source of this bias is differences in measurement error, as discussed above.

4.4 Estimating the Effect of State Growth

To identify the effect of state growth on incumbent vote share, without any interaction terms, I use two stage least squares as follows. Here, δ_j^1 and δ_t^1 correspond to first stage state and time fixed effects respectively, with δ_j^2 and δ_t^2 representing the second stage fixed effects. First stage, giving fitted values for $State\widehat{Growth}_{j,t}$:

$$StateGrowth_{j,t} = \beta_1 Bartik_{j,t} + Controls + \delta_j^1 + \delta_t^1 + \epsilon_{j,t} \tag{9}$$

Second stage:

$$Voteshare_{j,t} = \beta_2 State\widehat{Growth}_{j,t} + Controls + \delta_j^2 + \delta_t^2 + \epsilon_{j,t} \tag{10}$$

I use demographic variables and state fixed effects to control for partisan lean and election-specific outcomes. Controls which have a differential partisan effect, such as race, are multiplied by -1 when Democrats are in power and +1 when Republicans are in power. This is because our dependent variable is incumbent vote share, rather than Democratic or Republican vote share. Note that the demographic controls all vary from year to year, capturing changes in the demographic composition of each state over time. I control for presidential and vice presidential candidates' home states. I include two types of state FE. The dependent variable is the incumbent party's vote share, so the use of state FE will capture how much each state swings for or against the incumbent party on average. I also have a fixed effect for each state's partisanship - implemented as the dummy times 1 if Republicans are in power and -1 if Democrats are.

For data starting in 1988, the ANES has information on strata and primary sampling units for clustering purpose. However, most of my regressions include pre-1988 data, so I do not use that information, and indeed, it is not included in the 1948-2016 ANES data. However, I cluster standard errors by state because there will be serial autocorrelation at the state level which will not be captured fully by state fixed effects (implemented using Guimaraes & Portugal 2010; Gaure 2010; Baum et al. 2010; Correia 2017). Many forecasting papers (e.g. Berry and Bickers 2012; Hummel and Rothschild 2014) do not consider clustering at all and simply report regular standard errors. Wolfers (2002) uses robust standard errors, and observes that a "natural question when assessing the effects of the national cycle is whether to estimate standard errors as though there are 636 independent experiments in the data, or whether there is effectively only one independent experiment each year. Fortunately theory resolves the issue: under the null of voter rationality there will be no cross-state correlation in anti-incumbent sentiment, and hence each observation is an independent experiment." This is related to the broader issue of whether state-year observations are truly independent. For examinations of state economic growth, it is reasonable to only cluster at the state level.

I present the result where states are weighted by population, which has the interpretation of finding the effect of economic growth for the average voter in a way that is directly comparable to the individual-level results, rather than for the average state. The general result is robust to the use of unweighted observations, but that is not true for all interaction term results below. Typically, estimates of growth for geographic units are calculated in an unweighted way, as in Eisenberg and Ketcham (2004). The effect is somewhat larger for the weighted result than it is when unweighted.

4.5 Estimating the Effect of Interaction Terms

To estimate the effect of growth under varying levels of political polarization, I will use an interaction term. I am interacting a variable with growth, and growth is the variable that we need to instrument for. Simply regressing growth on the instrument and the controls, taking the fitted values for growth, and then using those fitted values in a second regression with an interaction term, is incorrect. Doing so is a variant of what Wooldridge calls a "forbidden regression", in this case due to the fact that the Bartik-type instrument times the interaction term might be correlated with the residuals from regressing growth on the instrument alone. Using the fitted values for growth with an interaction term in an OLS regression will not only give incorrect standard errors because of the use of the OLS rather than the IV estimator, but will also lead to inconsistent estimates of the coefficients. (See Wooldridge 2002 pp. 236-237, Angrist and Pischke 2008 pp. 142-143, and Baum 2007. For examples of this issue specifically with an interaction term, see Atanasov and Black 2019 and Xie et al. 2019.)

Thus, I use two instruments: the Bartik-type instrument and the Bartik-type instrument times the interaction term. I use two-stage least squares, which implies two first stage regressions: regressing state growth on both instruments and the controls, and then regressing state growth times the dummy on both instruments and the controls. Then, in the second stage, I estimate the effects of the fitted values of both state growth and state growth times the polarization interaction term $P_{j,t}$. The δ terms represent the state and time fixed effects for the two first stage regressions and the second stage regression. So, the first stage will find fitted values $State\widehat{Growth}_{j,t}$ and $\overline{StateGrowth_{j,t} * P_{j,t}}$, where $P_{j,t}$ is the interaction term of interest:

$$StateGrowth_{j,t} = \beta_1 Bartik_{j,t} + \beta_2 Bartik_{j,t} * P_{j,t} + \beta_3 P_{j,t} + Controls + \delta_j^{1a} + \delta_t^{1a} + \epsilon_{j,t} \quad (11)$$

$$StateGrowth_{j,t} * P_t = \beta_4 Bartik_{j,t} + \beta_5 Bartik_{j,t} * P_{j,t} + \beta_6 P_{j,t} + Controls + \delta_j^{1b} + \delta_t^{1b} + \epsilon_{j,t} \quad (12)$$

Second stage:

$$Voteshare_{j,t} = \beta_7 State\widehat{Growth}_{j,t} + \beta_8 \overline{StateGrowth_{j,t} * P_{j,t}} + \beta_9 P_{j,t} + Controls + \delta_j^2 + \delta_t^2 + \epsilon_{j,t} \quad (13)$$

Here, β_8 is our main coefficient of interest. Below, I use four types of measures as the interaction term $P_{j,t}$: state legislative polarization, partisanship, a swing voting propensity score, and college education. Each of these specifications has the structure above, where I have two endogenous regressors and two instruments.

I include all state-level controls mentioned above, and I include the share of the population which is college educated (detrended with a regression on year fixed effects), times a dummy for Republican incumbency, to adjust for the differing partisan lean of college-educated voters.

5 Results

5.1 The Effect of State Growth

The results for the effect of state growth are seen in Table 1, which presents the coefficient on our variable of interest, state-level earnings growth in the two quarters before the election. For the state-level, incumbent vote share runs from 0 to 100, while the dependent variable at the individual level is either 100 or 0, to ensure easy comparability between the magnitudes of the state and individual results. To interpret the magnitudes, note that growth is given in percentage points (i.e. 1% growth is 1). I show the individual-level results in the right-hand columns of Table 1. At the state level, I find that a one percentage point increase in earnings growth boosts incumbent vote share by about 1.2 percentage points. At the individual level, I find that a one percentage point increase in earnings growth boosts the likelihood of voting for the incumbent vote share by about 1.8 percentage points. The estimated coefficient on state growth is significant at the 1% level in both the state-level and individual-level regressions. The individual-level result is higher, possibly because the ANES having some state-years that are missing completely, with no voters sampled. This affects the estimated coefficient because smaller states tend to be more partisan on average. As a reference point, Strumpf and Phillipe (1999) find a 1 point increase in state-level real per capita income growth in the election year translates to a 0.4 point increase in incumbent share of the two-party vote, and Eisenberg and Ketcham (2004) a 1.1 point increase in incumbent share of the two-party vote. These papers are both using election-year growth.

As discussed in Appendix B, the instrument passes all relevant robustness checks, and has a first-stage F-statistic of 118.4 for the state-level results and 42.2 for individual-level results. I use the earning-share instrument in all cases.

To test for the various types of sector-related issues, I drop each sector, and do the Bartik IV strategy using only the other 9 sectors. This is considered in Appendix B, after the main results, where I find that the point estimates when dropping each sector are remarkably consistent. Another test, also discussed in Appendix B, is to include each of the shares in turn as a control variable, which again has little to no effect on the point estimates. These tests suggest that the Bartik IV method is valid in this case.

As a another robustness check, I control for turnout in each state-year, and that has no noticeable effect on point estimates or standard errors. Lastly, one additional robustness check - which may also be separately of interest - is to explore differences between years where an incumbent president is on the ballot to those without an incumbent. When using incumbency as an interaction term, I find no statistically significant difference between the two. Running them as two completely separate regressions, I find that the coefficient does tend to be slightly higher for incumbents, and other estimated coefficients vary little. The presence of year fixed effects should eliminate any possible issues due to overall incumbent advantages.

Another potential concern is the perceived salience of different growth rates. As discussed in the literature review, there is a well-documented tendency for state or local growth to have a smaller marginal effect on incumbent vote shares than national growth does. Our Bartik-type instrument captures the component of state growth that is due to different exposures to national shocks, and discards idiosyncratic state growth. Thus, to make conclusions about the effect of state growth, we must assume that voters perceive both of those types of state growth (sector-shock-driven and idiosyncratic) in the same way when affecting incumbent performance. This may or may not be true, but it seems unlikely that many voters are sufficiently sophisticated to capture such distinctions in a way separate from the national growth (which is captured by year fixed effects).

The literature contains many papers which are engaging in forecasting or making a predictive statement, but this paper is attempting to make a causal statement. Thus, although the inclusion of presidential approval rating would make the predictive power of the model stronger, it would make it more difficult to interpret the results because approval rating and economic growth are highly correlated. If approval ratings are included, it would be unclear whether a changing coefficient on growth is genuinely due to a lower effect, or whether the channel by which growth matters is moving from a direct effect to an indirect effect working through approval ratings. Accordingly, approval ratings are not used as a control.

5.2 State-Level Legislative Polarization

As the first interaction term, I use a state legislative polarization index designed by Shor and McCarty (2011), described in the Data section above. Higher polarization in a state's electorate should increase all four measures. If polarization in a state's politics is going up, we would expect the economy to affect presidential elections less. The proposed mechanism here is that the state legislative polarization is simply an indicator. The voters in the state are more polarized, which is causing both the lower coefficient on growth, and the higher Shor-McCarty index. I am not proposing that the polarization in the state legislature itself is causing the different impact on growth, merely that it will be associated with states that have more polarized electorates. As discussed in Appendix D, there are four different versions of the index, and I take state senate difference as the main interaction term of interest. The result is robust to using state senate distance, and the two house measures give similar but not significant effects.

The state-level and individual-level results, using the state senate difference measure as an interaction term, can be seen in Table 2. The interaction terms are demeaned, so the growth column shows the effect for a state with an average level of polarization. The left-hand two columns show the state-level results for comparison. Again, we find a large and negative effect of the state senate difference, significant at the 1% level, for both states and individuals. A one percentage point increase in state growth increases incumbent vote share by 0.77 percentage points at the state level, and increases the likelihood of a voter in voting for the incumbent by 1.7 percentage points. These effects are reduced by half, or more, by increase in state senate distance by a distance measure of 1. The state-level growth coefficients are lower, which is plausible, as many individuals with a below-average effect of growth are people who are not responsive to the economy at all. I weight states by population using probability weights to ensure that the result reflects the behaviour of the typical voter, although the significance of the result is robust to weighting each state equally.

In terms of the strength of the instrument, in Table 2 I display the Kleibergen-Paap Wald F-Statistic, which is 35.7 for state-level results and 72.2 for individual-level results. This test is used because I have more than one instrument, and I have clustered standard errors. (The Cragg-Donald minimum eignevalue statistic is used for multiple instruments, but with i.i.d. errors, and is not appropriate in this case.) The Kleibergen-Paap statistic is what I report in all tables with multiple instruments. In general, the individual F-statistics for the two endogenous regressors separately are each higher than this joint F-statistic. The instrument is much stronger when weighting by state population. Lee et al. (2020) discuss the interaction of t-tests and F-tests for significance, and argue for a tF-test of significance that considers the way low F-statistics affect the required t-test thresholds. Using the critical values they provide for the tF-test, the results in this section are still significant at the 5% level because of the high F-statistics.

One possible concern is that state growth and state legislative polarization may be related, and indeed, there is a slight tendency for more polarized states to grow faster after controlling for demographics and fixed effects. Accordingly, I apply numerous detrending methods as robustness checks. I detrend state senate difference with a time trend, with time fixed effects, with state growth itself, and use fitted values from demographic, state, and time effects. I also detrend growth with state fixed effects or with the polarization index. The main result is robust to these checks, as shown in Appendix D. Indeed, the interaction term is significant at the 1% level for 5 of the 6 variations, and at the 5% level when using the fitted value of the polarization index, with coefficients that vary from -0.339 to -0.362. This suggests that association between growth and polarization is not an issue

The main result is also robust to the use of different numbers of quarters: 1, 2, 3, or 4 quarters, or growth in the election calendar year. Thus, I have no reason to believe that the effect of polarization is in some way associated with changes in voter myopia or time pressure. When I include the 2020 election year, these results are not as clear. The effect of legislative polarization is still clear when 4 quarters of growth are used, but less so when 1, 2, or 3 quarters are used. This makes sense given the extreme swings in quarter-by-quarter growth due to the COVID-19 pandemic in 2020. As well, I am using polarization in the state legislature as a proxy for polarization in the state electorate, for which we lack direct measures. It is plausible that the relationship between those things breaks down by 2020. With state gerrymandering, the relationship between fundraising and partisan extremes, and other factors, heterogeneity in state-level legislative polarization may no longer be a good proxy for state electorate polarization in 2020.

This result is also robust to the use of non-farm earnings rather than all earnings. Indeed, the use of non-farm earnings is slightly more statistically significant. I perform several other checks to ensure that the indices are not correlated themselves with either incumbent vote share or changes in incumbent vote share, and that they are not statistically significant predictors of such changes in a regression.

5.3 Individual Voter Partisanship

The individual-level data from the ANES has four levels of partisanship: true independent (coded as 1), leaning independent (2), weak partisan (3), and strong partisan (4). This is an identification by the survey respondent. A leaning independent refers to a registered independent who leans to one of the parties. There is an extensive recent literature on this measure, which mostly focuses on the point that leaning independents are not "true" independents and are closer to weak partisans. This literature argues that leaning independents and weak partisans should be considered the same in their level of partisanship (Petrocik 2009; Abrams and Fiorina 2011). There is an older strand in the literature, starting with Petrocik (1974), who finds that, for some measures, leaning independents are actually more partisan in their behaviour than weak partisans. Thus, we should check this measure for non-monotonic behaviour.

To more specifically identify the effect of economic growth on incumbent vote choice by individual voters, and how that is affected by partisanship, I pursue an IV strategy. I use a linear regression (2SLS) with incumbent vote choice (1 or 0) as the dependent variable. I include ANES probability weights as discussed above. I use the same earnings-share Bartik-type instrument used in the state-level regressions, together with year fixed effects. I have all of the usual demographic controls, as well as state partisan and state incumbent lean fixed effects.

Firstly, to investigate monotonicity, I take the four levels of partisanship as separate dummies and interact them with state economic growth in an IV. That corresponds to 4 endogenous regressors (baseline growth, and interacted with partisanship level 2,3,4) and 4 instruments. The baseline category is the voters who are true independents. I find a statistically significant difference in the interaction term, which appears to have a monotonically increasing pattern - although, the difference between leaning independents and weak partisans is small. This is shown in Table 3. I find that, for a true independent, a one percentage point increase in state growth boosts the likelihood of voting for the incumbent by 4.5 percentage points, and that this effect is falling in partisanship. It is lower by 2.8 percentage points for weak partisans, and by 3.3 percentage points for strong partisans. This suggests that, for a study of economic voting, an assumption of monotonicity in the effect of partisanship is reasonable.

Accordingly, in Table 4, we see the results of using partisanship as a continuous interaction term. This implicitly relies on an assumption of monotonicity, as we are using partisanship as a continuous variable. We see that when partisanship is higher, the effect of economic growth on voting for the incumbent is lower, and that this effect is significant at the 1% level. However, we might be concerned that partisanship is endogenous - for example, that when growth is higher, supporters of the incumbent become more partisan in their behaviour. Thus, I construct a fitted score for partisanship by regressing partisanship on fixed demographic and geographic variables, and using the fitted values as a partisanship score. Using this score as an interaction term also shows a negative coefficient that is significant at the 1% level. This is shown in column (3) of Table 4, which has remarkably large and significant coefficients. The results show that, for a voter predicted to be a true independent by their demographic characteristics, the effect of the economy on voting is immense - a one percentage point increase in state growth has a staggering 15 percentage point increase in the likelihood of voting for the incumbent. Furthermore, for a voter predicted to be a strong partisan (corresponding to a fitted value of their partisanship index of 4), the effect of state growth would be completely eliminated.

I cluster standard errors at the state level. As with the state-level results earlier in this paper, these results are robust to using other numbers of quarters of growth: 1, 2, 3, or 4 quarters, or election calendar year growth. As well, if I use a different interaction term (a dummy that is 1 if the voter is either a leaning independent or a strong partisan, and 0 if they are not), this result is even stronger. Such combining of categories is motivated by the above discussion of non-monotonicity, which strongly suggests that leaning independents more closely resemble strong partisans than weak partisans.

5.4 A Swing Voting Propensity Score

There are two key stylized facts in the data on swing voting in US politics over the last 50 years: a decline in swing voting over time, and an increase in the predictive power of party identification. In Figure 2, I show the share of voters who switched their votes between successive elections, excluding voters who voted for third parties. This share is clearly declining over time, and much lower by 1996. Data for 2012 is not included.

There are three sets of mechanisms that could explain the declining impact of economic growth under conditions of polarization. Firstly, it could be a mechanical consequence of having fewer swing voters. There is a large literature showing the decline in swing voting and how it is associated with political polarization. Secondly, it could be due to a change in the type of person who is a swing voter, with that composition change causing different behaviour. Thirdly, it could be that swing voters are the same types of people, but their behaviour is different. I find strong evidence that the decline in swing voting is associated with a lower effect of economic growth, and little evidence for changes in the type of voters who are swing voters or in the behaviour of swing voters.

If swing voting propensity is what drives economic voting, I should be able to show that swing voting and economic voting are closely associated. Being an economic voter requires changing one's vote, so a non-swing voter can't be an economic voter. However, it is less clear that the likelihood of being a swing voter is strongly correlated with likelihood of being an economic voter. Perhaps the most fickle voters are not economic voters at all. We can use the propensity score to test this relationship, and indeed, I find a strong relationship between propensity to be a swing voter and economic voting, identified using the shift-share instrument. My method is distinct from other methods. Bartels (2016) creates a swing-voting propensity score by looking at the probability of voting Republican or Democratic, regressing that on demographic characteristics, and then treating those voters close to a 0.5 probability as being swing voters. Weghorst and Lindberg (2013) calculate swing voting propensities using survey data from Ghana. Among other variables, they control for age cohort, partisanship, ethnicity, gender, education, and information level.

To use swing voting propensity as an interaction term, I proceed in three steps: first, I run a logit regression of vote changing on voter characteristics. In this regression, the dependent variable is a binary dummy variable that is 1 if the voter voted for a different party than they did in the last election, and 0 if they voted for the same party. Secondly, I keep the fitted values of that regression, which becomes the swing voting propensity score, obtained from exogenous demographic characteristics of each survey respondent. This score measures how likely it would be, based on demographics, for the voter to change the party they vote for between elections. Thirdly, I use that swing voting propensity score as the interaction term in a 2SLS regression with state growth, following the same structure discussed at the start of Section 5.

I present in Table 5 the results for the first stage, the propensity to be a swing voter. We see that college educated voters are much less likely to be swing voters, as are older voters and non-white voters. There is a modest tendency for female voters to be more likely to be swing voters. In Appendix D, I use a post-1995 dummy as an interaction term and find no statistically significant coefficients on that interaction term. This suggests that there is little change in which sorts of groups are swing voters - rather, the reduction in overall swing voting propensity stems from composition changes, as the electorate becomes older, more college educated, and less white over time.

I use the fitted values from the demographics-only regression as the swing voting propensity score. As expected, the yearly average of that score is declining over time, as seen in Figure 3. Some year-to-year differences are interesting - for example, the slightly higher swing voting propensity in 2008 may be due to a surge in younger voters brought into the process by Barack Obama.

The results of the interaction term regression are seen in Table 6. I find an extremely strong and clear connection between swing voting propensity and economic voting: the effect of the economy on vote choice is much larger for voters with high swing propensities. For a voter with a theoretical swing propensity of 0.5 (meaning they are as likely to vote for incumbent as not), a one percentage point increase in growth would increase their likelihood of voting for the incumbent by 10 percentage points. As with the results above for a fitted partisanship score, this is both a large magnitude and a large degree of significance.

The result is significant at the 1% level when clustering at the state level. If I cluster on both state and year using two-way clustering, the interaction term coefficient is still significant at the 5% level. In a sense, the direction and magnitude of the result is not surprising, but the strength of the connection suggests a close relationship between swing voting and economic voting that has not been deeply explored in past work. The use of an instrument variable makes the relationship clear, and a useful contribution to the literature.

This result is robust to the use of the Bartels (2016) measure of swing voting. The Bartels measure determines propensity to vote for one party or the other based on demographic characteristics, and then treats swing voting propensity as being closeness to an even chance of voting for either party. I use a very similar version of his approach, and take the absolute value of the difference between that fitted value and 0.5, and then rescale to get a 0 to 1 propensity score that is comparable to mine. Thus, it is using the potential for vote-changing, rather than my approach which relies on actual vote-changing behaviour.

As well, these results are robust to using 1, 2, 3, or 4 quarters of growth in earnings, or using election calendar year growth. The proportional magnitudes of the interaction terms and main effects, as well as the statistical significance levels, are comparable. I also calculate the propensity score with linear regression and find a very similar result.

5.5 College Education

As seen in the examination of swing voting propensity above, college-educated voters are much less likely to be swing voters. College education also increases the likelihood that voters will be strongly partisan. Thus, we would expect that college education levels are associated with a reduction in economic voting.

I first examine the use of the (detrended) college educated share of the population term as an interaction term in the state-level data set, with the results seen in Table 7. I find a negative effect of college education on the relationship between state growth and incumbent vote share, significant at the 1% level. As I will explore more in the microdata section below, college-educated voters are less affected by the economy than non-college-educated voters. There is also a correlation of about 0.3 between the detrended college share and the polarization index.

Column (4) shows the use of average college share over the period 1992-2016 as the interaction term, as a robustness check. States that had a higher college share over the whole time period had a lower effect of economic growth on incumbent vote share. In other words, the effect is not driven by unusual year-to-year variation over time. This strongly suggests that college education levels have a major effect on the relationship between the economy and election outcomes. As well, there is no association between level of college education and state growth after adjusting for the demographic controls, when regressing college education on state growth or vice versa.

The results suggest that, for states with an average college share, a one percentage point increase in state growth will increase incumbent vote share by about 0.8 percentage points - but that, if college share is 10 points higher, state growth would have no effect at all.

The state-level results are consistent with the individual-level results shown in Figure 8. This is a 2SLS regression where the dependent variable is voting for the incumbent or not, the same methodology used in all the interaction term results above. For the 2SLS method, each percentage point of state growth boosts the likelihood of voting for the incumbent by 2.3 points for a non-college voter, and this effect is reduce by 2.3 points for a college educated voter. In other words, this suggests that college educated voters are completely unaffected by state economic growth, and the result is statistically significant at the 1% level. This suggests that the share of college educated voters is an important mechanism for reducing the effect of the economy over time. Higher levels of college education in a state may be associated with increases in both partisanship and polarization.

As a robustness check, I run that regression (seen in column 4) separately on voters in each income group. The concern would be that college education is simply driven by an income effect. There is a reduction in the effect of the economy of about half for voters in the highest income group (above the 95th percentile), and the statistically significant reduction of nearly all of the effect of the economy for voters in the 33rd to 67th and 67th to 95th income percentiles. For voters in the 17th to 33rd percentile, college education actually increases the effect of the economy (roughly doubling it), but isn't statistically significant, and the coefficient is again negative for voters in the 0th to 17th percentiles. Thus, the overall effect appears to be driven by the very large and statistically significant reductions in the 33rd through 95th income percentiles. Thus, this is not simply driven by moving between income percentile groups, but is an effect that exists within those groups. We could speculate that college-educated voters are in economic sectors, or full-time contract roles, in which they have less exposure to economic volatility than a non-college voter would at the same income.

6 Conclusion

All of the results found in this paper point in the same direction. Since the 1980s, political polarization in the US has grown, and this has reduced the number of swing voters. This should, as predicted by the model, reduce the effect of economic growth on incumbent vote share. I find that this is the case, with six sets of results showing variables which change the coefficient on state growth. I show that state legislative polarization, partisanship, and college education are each associated with a lower effect of economic growth, and that swing voting propensity is associated with a higher effect. In the survey data results, I find that swing voting behaviour can be predicted by demographic characteristics, and that there is a close association between swing voting and economic voting. I also find that swing voting is associated with very similar demographic groups before and after 1996, suggesting that the main driver of declining swing voting is a composition change in the electorate, rather than different types behaving differently.

These findings have several consequences. Firstly, this affects the field of presidential election modelling and forecasting. Models which assume a constant coefficient on economic growth are likely to be biased in attempting to make future predictions. Furthermore, both voters and candidates should be aware of the factors which determine election outcomes. This work can help us understand why politicians receive as many votes as they do, and may affect the strategic choices made by presidential campaigns.

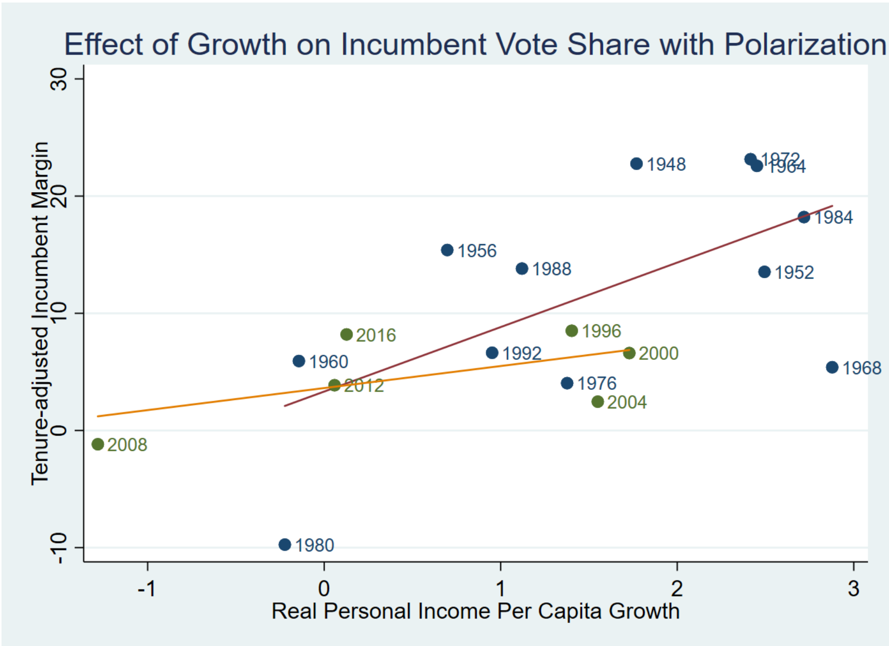
Secondly, this adds to our causal understanding of the mechanisms by which economic growth affects voter behaviour and election outcomes. The close relationship between the impact of the economy and levels of either partisanship or swing voting propensity illuminates the close relationship between swing voting and economic voting. This goes beyond simply improving our capacity to engage in accurate election forecasting from fundamentals, and improves our understanding of exactly why and how economic fundamentals are important to election outcomes.

Another implication of this work is that not only does coefficient on growth vary over time, it varies significantly across states with their college-educated share or other characteristics of their state electorate. This has implications for the political geography of election forecasting. In theory, one can estimate which states will be more or less affected by economic growth, which may inform both campaign strategy and future political science research.

Lastly, this has concerning ramifications for our view of democracy as providing an accountability mechanism. Polarization has reduced the extent to which vote choices are affected by the economy, and weakened the accountability mechanism for governing outcomes. Economic voting, while often myopic in nature, still provides a useful mechanism for voters to reward or punish incumbents for their performance in office. The large reductions in the magnitude of economic voting implied by this work may change incumbent behaviour if economic outcomes have a reduced effect on the the prospect of re-election. Indeed, incumbents who adopt deliberately polarizing strategies may be counting on precisely this effect. We should be concerned that weakening the power of this accountability mechanism reduces one of the benefits of a system of democratic elections.

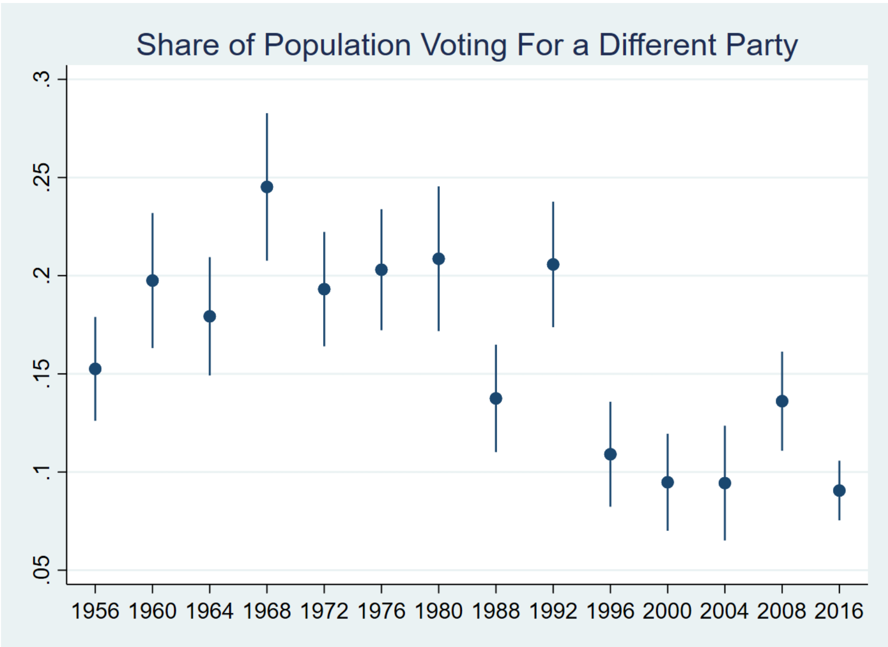
7 Main Figures and Tables

Figure 1: Tenure-adjusted Incumbent Margin vs. Growth



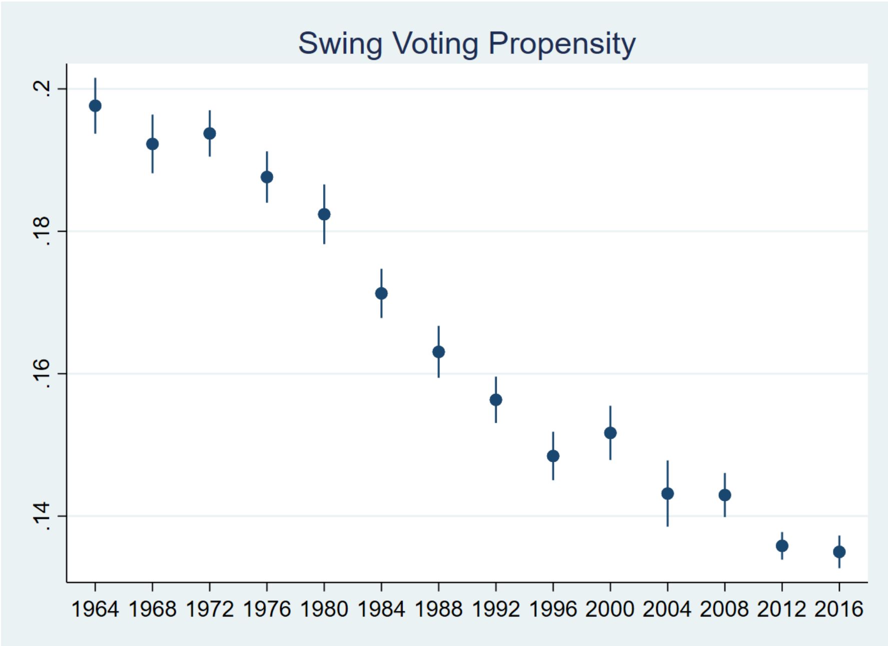
Data on national popular vote margin of incumbent party is from Leip Atlas, two-quarter change in real personal income per capita in the election year is from the BEA, and tenure is a count of how many consecutive terms beyond 1 the incumbent party has been in power. There are 18 observations, spanning 1948-2016. I first regress incumbent margin on growth and tenure, and then calculate the effect of tenure on each observation by multiplying tenure by the estimated coefficient on tenure. This is negative, so the adding to incumbent margin removes the negative effect of excess tenure (Achen and Bartels 2017). The observations are then plotted, with the Y-axis showing tenure-adjusted incumbent margin and the X-axis showing growth. Lines of best fit are for 1948-1992 (red) and 1996-2016 (yellow).

Figure 2: Share of Population Voting For a Different Party Than Prior Election



For each year, population-weighted share of ANES survey respondents who voted for a different party in the prior election than the current one. 2012 is omitted due to a data limitation.

Figure 3



For each year, this shows the population-weighted average of the fitted value from a logit regression where the dependent variable is switching the party voted for between elections, regressed on demographic controls and state FE. Uses ANES data.

Table 1: Effect of State Earnings Growth on Incumbent Vote Share

	State-Level		Individual-Level	
	(1)	(2)	(3)	(4)
	OLS	2SLS	OLS	2SLS
State Growth	0.424***	1.194***	0.822*	1.835***
	(0.137)	(0.277)	(0.440)	(0.675)
N	959	959	20360	20360
R^2	0.762	0.747	0.209	0.209
F		118.4		42.18
Method	OLS	2SLS	OLS	2SLS
Controls	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
State Partisan FEs	Yes	Yes	Yes	Yes
Clustering	State	State	State	State
Weights	Population	Population	Population	Population

Columns (1) and (2) are from the state-level data set, drawn from actual state-level election results. Columns (3) and (4) are from the individual-level data set, drawn from the ANES. Earnings growth is in percentage points, as is incumbent vote share, so the coefficient corresponds to the increase in points in incumbent vote share from a one percentage point increase in state earnings growth. For the individual level results, independent variable is a binary dummy variable for voting for the incumbent party, scaled by a factor of 100 so coefficient magnitudes can be compared easily to the state-level results. F-Statistic is first stage Kleibergen-Paap rank Wald F-Statistic. Weighted using population-matched probability weights. Regressions cover 1948-2020. Standard errors are in parentheses below coefficients and are clustered at the state level. *p<0.1; **p<0.05; ***p<0.01

Table 2: Effect of Growth and Polarization Indices on Incumbent Vote Share

	State-Level		Individual-Level	
	(1)	(2)	(3)	(4)
State Growth	0.309	0.773***	0.912	1.714
	(0.225)	(0.235)	(0.960)	(1.276)
Senate Difference	1.738	1.527	-1.412	-1.793
	(1.902)	(1.508)	(4.660)	(4.806)
Senate Difference*Growth	-0.315**	-0.336***	-1.520**	-1.460***
	(0.129)	(0.117)	(0.756)	(0.554)
N	294	294	10309	10309
R^2	0.940	0.938	0.236	0.236
Kleibergen-Paap rk Wald F-Stat		35.73		72.23
Method	OLS	2SLS	OLS	2SLS
Controls	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
State Partisan FEs	Yes	Yes	Yes	Yes
Weighting	Population	Population	State Population	State Population

Earnings growth is in percentage points, as is incumbent vote share, so the coefficient corresponds to the increase in points in incumbent vote share from a one percentage point increase in state earnings growth. For the individual level results, independent variable is a binary dummy variable for voting for the incumbent party, scaled by a factor of 100 so coefficient magnitudes can be compared easily to the state-level results. Regressions cover 1992-2016. State senate difference is demeaned, so the growth coefficient can be interpreted as the effect of growth for a state with average polarization. Standard errors are in parentheses below coefficients and are clustered at the state level. *p<0.1; **p<0.05; ***p<0.01

Table 3: Effect of State Growth on Voting for Incumbent, by Partisanship

	(1)	(2)	(3)
	OLS	2SLS	2SLS, No Partisanship
State Growth	1.952**	4.530***	1.835***
	(0.799)	(1.316)	(0.675)
True Independent	0	0	
	(.)	(.)	
Lean Independent	0.739	2.104	
	(2.021)	(2.318)	
Weak Partisan	3.714*	5.315**	
	(2.001)	(2.167)	
Strong Partisan	5.147***	6.845***	
	(1.549)	(1.805)	
True Independent*State Growth	0	0	
	(.)	(.)	
Lean Independent*State Growth	-0.619	-2.071*	
	(0.871)	(1.147)	
Weak Partisan*State Growth	-1.141	-2.825**	
	(0.974)	(1.200)	
Strong Partisan*State Growth	-1.562	-3.358***	
	(0.965)	(1.101)	
Observations	20314	20314	20360
R^2	0.210	0.209	0.209
Kleibergen-Paap rk Wald F-Stat		10.61	42.18

Regression where independent variable is a binary dummy variable for voting for the incumbent party, scaled by a factor of 100 so coefficient magnitudes can be compared easily to the state-level results. Column (3) shows only the effect of state growth, as a point of comparison. F-Statistic is first stage Kleibergen-Paap rank Wald F-Statistic. Weighted using population-matched probability weights. Standard errors are in parentheses below coefficients and are clustered at the state level. I include year and state FE. *p<0.1; **p<0.05; ***p<0.01

Table 4: Partisanship Reduces the Effect of State Growth on Voting For the Incumbent

	(1)	(2)	(3)
State Growth	0.0234***	0.0449***	0.152***
	(0.009)	(0.011)	(0.031)
Partisanship	0.0196***	0.0233***	0.0333
	(0.004)	(0.004)	(0.045)
Partisanship*State Growth	-0.00495	-0.00877***	-0.0465***
	(0.003)	(0.003)	(0.010)
Observations	20314	20314	20360
Method	OLS	2SLS	2SLS
Interaction Term	Partisanship	Partisanship	Fitted Partisanship
R^2	0.210	0.209	0.209
F		21.04	21.12

Regression where independent variable is a binary dummy variable for voting for the incumbent party, scaled by a factor of 100 so coefficient magnitudes can be compared easily to the state-level results. This all treats partisanship as a continuous, monotonically increasing variable from 1 to 4. Column (3) uses demographic-based fitted partisanship score as the interaction term, so that it is fully exogenous. F-Statistic is first stage Kleibergen-Paap rank Wald F-Statistic. Weighted using population-matched probability weights. Standard errors are in parentheses below coefficients and are clustered at the state level. I include year and state FE. *p<0.1; **p<0.05; ***p<0.01

Table 5: Effect of Demographics on Vote Changing

	(1)	
1. Grade school or less (0-8 grades)	0	(.)
2. High school (12 grades or fewer, incl. non-college	-0.218*	(0.120)
3. Some college (13 grades or more but no degree;	-0.395***	(0.143)
4. College or advanced degree (no cases 1948)	-0.802***	(0.144)
1. 17 - 24	0	(.)
2. 25 - 34	-0.365***	(0.133)
3. 35 - 44	-0.469***	(0.165)
4. 45 - 54	-0.464***	(0.162)
5. 55 - 64	-0.699***	(0.167)
6. 65 - 74	-0.952***	(0.169)
7. 75 - 99 and over (except 1954)	-1.203***	(0.185)
1. Male	0	(.)
2. Female	0.107*	(0.059)
3. Other (2016)	0	(.)
1. White non-Hispanic	0	(.)
2. Black non-Hispanic	-1.039***	(0.119)
3. Other	-0.190*	(0.113)
1. 0 to 16 percentile	0	(.)
2. 17 to 33 percentile	0.0868	(0.129)
3. 34 to 67 percentile	-0.201*	(0.110)
4. 68 to 95 percentile	-0.205*	(0.114)
5. 96 to 100 percentile	-0.338**	(0.153)
Observations	12028	

Logit regression where independent variable is a binary dummy variable for voting for opposite parties in consecutive elections. Standard errors are in parentheses to the right of coefficients and are clustered at the state level. *p<0.1; **p<0.05; ***p<0.01

Table 6: Effect of State Growth and Swing Voting on Voting For Incumbent

	(1)	(2)
	OLS	2SLS
State Growth	0.755	1.604*
	(0.454)	(0.840)
Swing Propensity	52.19	29.61
	(34.895)	(38.129)
Swing Propensity*State Growth	6.015	16.64***
	(3.832)	(5.400)
Observations	20227	20227
Kleibergen-Paap rk Wald F-Stat	.	23.47

Regression where independent variable is a binary dummy variable for voting for the incumbent party, scaled by a factor of 100 so coefficient magnitudes can be compared easily to the state-level results. Swing propensity is de-meaned, so the coefficient on growth corresponds to the growth effect for an average voter in that time period. F-Statistic is first stage Kleibergen-Paap rank Wald F-Statistic. Weighted by to match sample weights with state-year population. Includes demographic controls and state FE. Standard errors are in parentheses below coefficients and are clustered at the state level. *p<0.1; **p<0.05; ***p<0.01

Table 7: Effect of Growth and College Share on Incumbent Vote Share

	(1)	(2)	(3)	(4)
	OLS	2SLS	2SLS Not Detrended	2SLS Average All Years
State Growth	0.185	0.791***	0.833***	0.813***
	(0.186)	(0.243)	(0.235)	(0.239)
College Share	-1.095***	-0.947***	-0.921***	
	(0.296)	(0.250)	(0.251)	
College Share*Growth	-0.0779***	-0.0825***	-0.0824***	-0.0818***
	(0.028)	(0.027)	(0.028)	(0.028)
N	357	357	357	357
R^2	0.928	0.924	0.922	0.924
Kleibergen-Paap F-Stat		34.85	36.80	36.63
Method	OLS	2SLS	2SLS	2SLS
Controls	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes
State Partisan FEs	Yes	Yes	Yes	Yes
Weighting	Population	Population	Population	Population
College Detrended By	Year	Year	Not Detrended	Not Detrended

All interaction terms are demeaned, so the coefficient on growth reflects that coefficient for an average level of college. Column (4) uses the average college share from 1992-2016, but not detrended by year, as the interaction term, and thus the interaction term doesn't vary across time. Column (4) still controls for time-varying college share separately. Regressions cover 1992-2016. College share is share of population with a Bachelor's degree or higher, detrended as indicated. Standard errors are in parentheses below coefficients and are clustered at the state level. *p<0.1; **p<0.05; ***p<0.01

Table 8: Effect of State Growth on Voting for Incumbent, by College Education

	(1)	(2)	(3)
	OLS	2SLS College	2SLS No Interaction
State Growth	1.011**	2.306***	1.835***
	(0.435)	(0.739)	(0.675)
College Grad	-0.300	1.049	-1.325
	(1.938)	(1.922)	(1.751)
College Grad*State Growth	-0.988*	-2.333***	
	(0.518)	(0.561)	
Observations	20360	20360	20360
R^2	0.209	0.208	0.209
Kleibergen-Paap rk Wald F-Stat		21.70	42.18

Regression where independent variable is a binary dummy variable for voting for the incumbent party, scaled by a factor of 100 so coefficient magnitudes can be compared easily to the state-level results. Column (3) shows only the effect of state growth, as a point of comparison. F-Statistic is first stage Kleibergen-Paap rank Wald F-Statistic. Weighted using population-matched probability weights. Standard errors are in parentheses below coefficients and are clustered at the state level. I include year and state FE *p<0.1; **p<0.05; ***p<0.01

A Appendix A: Data Sources and Methods

BEA data on earnings is annual starting in 1929 and quarterly starting in 1947. All BEA income and earnings data, including the sector-specific data, is seasonally adjusted at annual rates. I use annual population data from the BEA by state and assume uniform quarterly growth over the year. Population growth is midyear, so I assume it is Q2. This enables calculation of quarterly real personal income per capita. Census Bureau data on state areas is used to calculate population density. For the 1968 employment shares I used 1969 data from the BEA. Earnings includes wages and salaries but does not include dividends or rents, and does include employer contributions to taxes. Earnings does include proprietor's income. Earnings can be decomposed into 10 different sectors: Farm Earnings; Agriculture, Forestry, and Fishing; Mining; Construction; Manufacturing; Transportation and Utilities; Wholesale and Retail Trade; Finance and Real Estate; Services; and Government. The data for the period 1948-57 uses the 10 SIC categories, which I follow in constructing the data set. Data from 1958-1998 uses SIC codes with wholesale and retail trade separated, so these are merged. Thus, I use the same methodology for all periods, with wholesale and retail trade always treated as a single sector. Starting in 2000, data uses NAICS codes, which are consolidated to match the old SIC. Four missing data observations for state-year specific earnings levels were changed to prior year data where appropriate. State and national growth rates are all expressed as totals over the time period, not in annualized terms. I follow this practice for both the earnings and employment instruments. All of the time periods are combined into one time series. There is also national-level data in addition to state-level, so I calculate the national growth rates of each of these types of earnings. Then, the growth rate of each sub-type of earnings is multiplied by earnings shares or employment shares. The sum of all these terms is the instrument for both types.

For state-level college shares, I interpolate 1990 and 2000 data for the years 1992 and 1996. For the Shor-McCarty indices of polarization, I use 2018 for 2020 observations where relevant. Nebraska has a unicameral and non-partisan legislature, and is omitted from the Shor-McCarty measures. I use the 1993 measure of polarization for the 1992 election year where available. The index is not available for some state-year pairs in the

1990s.

I generate the Presidential and Vice Presidential candidate home state data while making a few adjustments from state of registration. Eisenhower was raised in Kansas so this is coded as his home state in 1952 and 1956, although he was registered in New York in 1952 and Pennsylvania in 1956. Nixon had previously won statewide office in California, and grown up there, so that is coded as his home state for 1960, 1968, and 1972 despite New York registration as of 1968. In years where both candidates are from the same state (for example, in 1972 both Agnew and Shriver were from Maryland), the home state variable is coded as 0. For 2020, Donald Trump's home state is treated as New York despite registration in Florida. Alabama does not have the Democratic candidate on the ballot in 1948 or 1964, so I include a dummy variable for those years.

I obtain state-level historical data on homeownership from the Census Bureau (Census 2020). I then interpolate, and take the years corresponding to presidential election years. For Alaska and Hawaii in 1940, I used the 1950 rate.

Data on the racial composition of data from the 1940, 1950, 1960, and 1970 censuses was found in ICPSR 2896, which I interpolate similarly (see also Haines 2005). Additional census years were obtained directly from the Census Bureau website. For Alaska and Hawaii 1940 and 1950 race data, I used 1960 black share as prior years are not available. I use the 5% sample method for the 1970 Hispanic count in the census data. Additional census data is available for presidential election years starting with 1992, which obviates the need for interpolation over that time period. I used the 1992 and 1996 data. Starting in 1970, I have explicit counts for non-Hispanic whites which I can include. I subtract biracial individuals where relevant to avoid double-counting. The 1930-1960 censuses do not count Hispanics, so they may be included as white in those years for some individuals. This is a smaller effect size in that era. Utah's 1980 stated number of non-Hispanic whites leads to a excess-total error, which reflects a measurement error problem, so the total number of whites was used for that year instead.

For the south region dummy variables in robustness checks, I include the 11 states of Confederacy (Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia) plus Oklahoma and Kentucky. I do not include Washington, D.C., West Virginia, or Maryland. Another robustness check, when using lags of state-level incumbent party vote share, is to also control for third-party candidates following Hummel and Rothschild (2015). All past election of the 3rd party candidates are lagged by 1 cycle, to adjust for voters who may once again vote for one of the two main parties in the next cycle. I control for Strom Thurmond in 1948 because his bid had strength which was strongly concentrated in one region (the South); George Wallace in 1968; John Anderson in 1980; and Ross Perot in 1992 and 1996 (combined as one variable). Ralph Nader made 4 third-party bids from 1996 to 2008, but the vote totals won are comparatively smaller and so I did not include these.

B Appendix B: Robustness Checks for State-Level Results

B.1 Dropping Sectors or Controls

One useful robustness check is to drop each of the sectors from both the growth rate calculation and the instrument, and re-run the IV. I perform this robustness check in 5 different combinations of years: 1948-2016, 1996-2016, 1948-1992, 1964-1992, and 1948-1960. This gives 50 different specifications to examine for patterns, presented in Table A1. The point estimates are all broadly similar. As a first pass, I consider the use of current shares for all of these combinations. In general, these specifications return coefficients that are close to the all-year, all-sector value of 0.742. We find similar results with 1980 shares, although as expected the instruments are weaker.

For the full time period, dropping the Mining, Oil, and Gas sector leads to a lower coefficient estimate of 0.54, and a p-value of 0.11. This is consistent with the Rotemberg weights, which suggest that much of the identification comes from this sector. All other sectors, including the government earnings sector, lead to little change. For the 1948-1960 period, dropping farm earnings has a similar effect - now there is a negative coefficient, and the p-value is 0.59. For that era, we may be identifying almost entirely off of differences in the farm earnings share. However, there is no seeming issue with Mining, Oil, and Gas in this era. For the 1964-1992 period, almost all of the coefficients are negative and the p-values are always above 0.10, except when we drop the farm earnings sector. This occurs with 1980 shares as well. Another possibility is that the instrument may be weak in this era, due to the presence of years with substantial differences between farm earnings and other sectors. For the 1996-2016 period, we get a much clearer finding: Mining, Oil, and Gas gives a negative coefficient and 0.87 p-value when dropped. For none of the eras does dropping government, and only considering private earnings, have a big effect on the coefficient. Overall, the lack of change in the point estimate suggests that there is no sector-specific concern.

I also test dropping each demographic control in turn. For the main IV with no interaction term, the results for dropping each of the demographic variables are overwhelming similar with one exception. When excluding the Black share of the population from the sample, the estimated coefficient on growth over the whole time period goes up from 0.74 to 0.90. This control, interacted with the Republican incumbent dummy, is strongly predictive of incumbent vote share in a state and is important to include.

B.2 Other Shift-Share Instrument Robustness Checks

As a starting point, Goldsmith-Pinkham et al. (who I refer to sometimes as GPSS) suggest running regressions of each share on the controls. When I check whether the current shares or 1980 shares are affected by the controls, I find that some of the sector shares are affected by two of the controls: white population share, and homeownership rate. But, the R^2 is very low (less that 0.05) for each of these regressions. Furthermore, when I run the IV without either or both of these controls, the point estimates and SEs are very similar. I also check whether 1980 shares are correlated with the controls specifically in that year, and there is some evidence of this. One key test is whether the shares are correlated with any controls and whether those controls in turn are correlated with changes in the level of the outcome. It is acceptable if the controls are correlated with the level - the problem only arises when the shares are affecting changes in the level of the outcome through the controls rather than through the differences in shocks. The real question is whether the shares are correlated with some of the controls, and those controls are in turn correlated with changes in the levels of the outcome. The political controls (home states of candidates and the Alabama Democratic absence dummy) are correlated, but the other variables are not. The demographic controls do predict incumbent vote share, but crucially have no statistically significant effect on the changes in the level of incumbent vote share. Thus, we can safely treat this transmission mechanisms as not being a problem.

Goldsmith-Pinkham et al. suggest the following other robustness checks in addition to examining correlates of the shares: comparing pre-trends, overidentification tests using the shares as instruments, alternative estimators such as LIML, examining the contributions of positively and negatively Rotemberg-weighted sectors, and using the Rotemberg weights to understand the effect of different sectors.

An examining of pre-trends is particularly useful for a difference-in-difference study or similar methods where there is a clear treatment. In this case, we are examining data over a long time period, and there is no binary treatment. Goldsmith-Pinkham et al. say "[t]he analogy to difference-in-differences is most straightforward when the shares are fixed over time. In this case, the industry shares measure the exposure to the policy change, while the national growth rates proxy for the size of the policy change. In these settings, it is natural to test for pre-trends." In this paper, we are studying the effect of the national growth rates themselves, rather than using them as a proxy for something else. There is no pre-period in the conventional sense as we are using all data available. One potential test of pre-trends is to see whether growth in the election year appears to have an impact "before it happens". Accordingly, I use the next-period value of state growth and of the instrument, with all the same current period controls. I find no effect of forward growth either in OLS or an IV with the forward instrument.

One important robustness check discussed by Goldsmith-Pinkham et al. is the use of different estimators, seen in Table A2. In column (1), I use OLS with the national variables and no time FE. All other specifications include time FE. The other tests are TSLS with the Bartik estimator, and TSLS and LIML with the shares interacted with time FE. Using the shares as instruments gives very different effects, which I believe is due to the interaction of the shares with time FE in the presence of heterogeneous effects. We do not have homogeneous effects so we wouldn't necessarily expect those two estimators to give similar results.

The political science literature suggests that the salience of the economy changes, and is lower in recession years. Thus, we likely have heterogeneous effects by time. Goldsmith-Pinkham et al. explain that users of a shift-share instrument should do an overidentification test, where the instruments are each share are interacted with time fixed effects – in my case, 180 instruments. The failure of the Sargan overidentification test with these 180 instruments is expected because of the presence of heterogeneous effects.

The overall coefficient estimated by TSLS with the Bartik-type instrument is equal to the Rotemberg-weighted sum of the coefficients you would estimate with each of the share instruments (aggregating across years by sector). The Rotemberg weights can be interpreted as the "sensitivity-to-misspecification" of each instrument. Essentially, the Rotemberg weight tells you how much each sector matters to the final coefficient.

Sometimes these weights are negative, so we separate the contributions of the negative and positive weights. Goldsmith-Pinkham et al. say "[i]f the weighted sum of the instruments with the negative [Rotemberg weights] is relatively large, then it is more likely that there are negative weights on the [coefficients estimated from each share] that are important in the overall estimate." However, the below panel, following the format in Goldsmith-Pinkham et al. (2018), shows that this is not a concern here, as the negative weights make up less than 10% of the estimated coefficient.

For the period 1948-2016, there are some negative Rotemberg weights, as seen in Table A3. Note that this displays the sums of both positive and negative weights.[2] However, when 2020 is included, all Rotemberg weights are positive, so this is not a concern.

When examining the visual evidence, Goldsmith-Pinkham et al. explain what a more desirable pattern of dispersion would look like. We want "less dispersion in the point estimates among the high-powered industries." It is desirable if "the high-weight industries are clustered more closely to the overall point estimate" and that if "there are negative Rotemberg weights, these industries are a small share of the overall weight." So, we have few issues if among high powered (high F-statistic) sectors, they aren't dispersed in the betas, and that the high-alpha sectors are also close in their betas.

For the years 1948-2020, seen in Figures A1, A2, and A3, there are no negative Rotemberg weights, and the dispersion of Rotemberg weights is relatively small. Mining, oil, and gas is the most important sector, but not overwhelmingly dominant. Running

[2] LaTeX code originally by Goldsmith-Pinkham, Sorkin, and Smith, with the bartikweight Stata package

the IV regression without the mining sector finds very similar point estimates for all of the coefficients of the controls, but the impact of growth on vote share is somewhat smaller (0.50 instead of 0.61) and less significant. However, the mining sector does not have any significant issues in being correlated with or predictive of any of the controls. Although much of the identification is driven by the large Rotemberg weight on mining, oil, and gas, the best solution is just to use overall earnings, because much of the state-level variation comes from this sector. If we drop the 7 states where the mining sector is, on average, more that 5% of earnings, and use only the 44 other states, we find a very similar point estimate (0.52) overall for our coefficient of interest - and again, point estimates on all other controls do not change very much, suggesting that those states are not substantially different in other respects. Thus, I conclude that the Bartik-type instrument is an acceptable instrument for the period 1948-2020. As well, I drop the 2020 year and run all of these tests on the sub-period 1948-2016, finding very similar results.

Lastly, Adao et al. (2019) discuss two possible correction procedures for standard errors in a shift-share instrument situation. I perform for the main regression as robustness checks even though, conceptually, the exogeneity of the shocks is not the correct assumption in this application. For the main state-level results without clustering, and using a version of the instrument without farm earnings to avoid collinearity among the shares, I find that that the SEs are either similar or lower.

B.3 Alternate Inflation Measures

One concern from the above section is that, as mentioned in Wolfers (2002), the BEA data may be capturing excess growth in state price indices rather than actual real income growth. If there is more such state inflation during the high inflation era, that may explain the negative coefficient in the 1964-1992 era. Another possibility is that the GDP deflator, which I use above, is an inappropriate deflator and that we should be using CPI. Thus, I try using nominal data, and using data deflated with a region-specific CPI index.

This suggests that using the GDP deflator on earnings may be inappropriate in some eras. From the BLS, I obtain a series of regional price indices starting in 1966. The four regions are Northeast, Midwest, South, and West. Data is quarterly until 1977, bimonthly 1977-1987, and monthly thereafter. I make monthly data into quarterly data by using the 3rd month of the quarter, and I make bimonthly data into quarterly data by using the price growth from the 2nd to 8th month of the election year rather than the 3rd to 9th. I then make real amounts for each sector using this regional CPI index.

We can then make nominal growth rates and CPI regionally-adjusted growth rates for each state. For the instruments, we use the national sector growth rates, weighted by state-specific shares.

Using nominal data, we get positive coefficients as we would expect. The results are not meaningfully different for regional-CPI or national-GDP deflator. During the high-inflation era of 1968 through 1984, there is no statistically significant impact of real state growth on incumbent vote share, nor is there an effect of nominal state growth. Excluding Southern states does not impact this result.

C Appendix C: Robustness Checks for Individual-Level Results

C.1 Dropping Sectors or Controls

The estimated coefficient on economic growth in the IV is robust to dropping any one demographic control. The coefficient is still significant at the 5% or 1% level when each control is dropped, as seen in the table.

I drop each of the 10 sectors in turn from earnings growth and from the shift-share instrument, and re-run the calculation, seen in Tables A5 and A6. Note that the point estimates for the effect of growth do not change substantially. As with the state-level results, dropping the mining, oil, and gas sector makes the estimated coefficient less significant, but the point estimate is very similar overall.

C.2 Other Shift-Share Instrument Robustness Checks

As in Appendix B, I follow the suggested GPSS robustness checks: running regressions of each share on the controls, overidentification tests using the shares as instruments, alternative estimators such as LIML, examining the contributions of positively and negatively Rotemberg-weighted sectors, and using the Rotemberg weights to understand the effect of different sectors.

For controls, we check if the shares are correlated with the controls. For each share, there are some statistically significant correlations with some of the demographic controls, especially education, race, and age. Some sectors, notably the farm earnings sector, are associated with a lower education level, a less racially diverse population, and an older population. The controls are also not associated with changes in the level of the outcome variable, either in one-on-one correlations or when the shares themselves are included as controls in a regression. The exception is the manufacturing sector, but point estimates do not significantly change when that sector is dropped from both earnings growth and from the shift-share instrument.

I use five alternative estimators: OLS, 2SLS, 2SLS with shares as instruments, LIML, and GMM. The results are seen in Table A7, and again, the point estimates are remarkably similar. Using LIML or GMM with the shares as instruments is also very similar.

Goldsmith-Pinkham, Sorkin, and Smith (2018) suggest calculating Rotemberg weights for each sector. I do that calculation above for the state-level results. However, the method of calculating these weights is computationally intensive for larger samples, because it relies on calculating an $N \times N$ matrix, where N is the number of observations. This is straightforward for the state-level data set, with fewer than 1000 observations, but difficult for the 20 000+ observation individual-level data set. Accordingly, I do not calculate the Rotemberg weights here. The fact that dropping each sector does not significantly alter any of the point estimates, and that the alternative estimators also give similar point estimates, reduces concerns about sector-specific exposure differences that might exist.

As with the state-level results above, I use fixed 1980 shares rather than updating shares to construct an alternative shift-share instrument. The point estimates are very similar, although the F-statistic is of course lower. The use of updating shares is preferable because of the concern that the instrument is weakening as the distance from the fixed point grows, and that this weakening will be correlated with a change in the level of political polarization.

D Appendix D: Other Robustness Checks

There are four Shor-McCarty indices, and I use each of these measures in turn as the interaction term. I also include the interacted variable as a separate regressor. The results are seen in Table A8. Each of the polarization indices are demeaned - so the coefficient on state economic growth can be interpreted as the coefficient for a state with an average level of polarization. When interacting each of these indices with state growth, there is evidence that state senate polarization changes the effect: when polarization is higher, the impact of state economic growth is lower. I conclude that additional polarization in the state senate of a state is strongly associated with a lower effect of state economic growth on incumbent vote share, and that this is likely due to the higher polarization in that state's electorate.

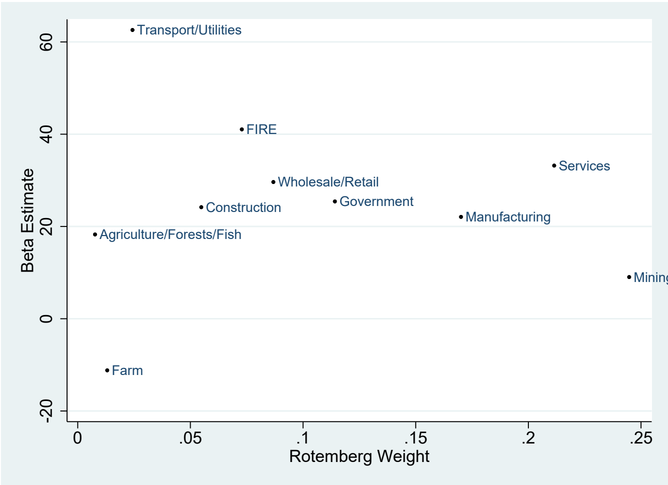
State senate average difference and average distance are more significant than the measures for state lower chambers (houses and assemblies). This may be due to lower chambers being more likely to be controlled by one party for long stretches of time, making those measures less responsive to changes in the state electorate's polarization.

I present additional robustness checks with types of detrending in Table A9. I use a detrended version of the state senate difference variable, using either time fixed effects or a linear time trend. I also regress the state senate difference variable on state growth, and keep the residuals and use them as the interaction term, thus eliminating any polarization that might be associated with differences in growth. Lastly, I regress the senate difference variable on all of the controls and the state and time fixed effects and then keep the fitted values as my interaction term, thus retaining only the polarization levels explained by demographic, state, and time effects. I also explore detrending the growth variable. I detrend state growth by regressing it on state fixed effects and keeping the residuals, and then use those residuals as the growth variable. This gives almost identical coefficients and standard errors when re-running the regression. Similarly, if I detrend state growth by regressing it on the Shor-McCarty index and keeping the residuals, it also has almost no effect on coefficients or standard errors. The robust results of all of these various tests suggests that there is not a concern about the association between state growth and state polarization.

Lastly, to test for differences in swing propensity in the periods 1964-1992 and 1996-2016, I run the same logit regression as in Table 5, but this time interact all controls with a dummy variable that is 1 starting in 1996. This is shown in Table A10. None of the coefficients on the interaction terms are statistically significant. This suggests that the determinants of swing voting propensity are relatively similar under conditions of polarization, post-1995.

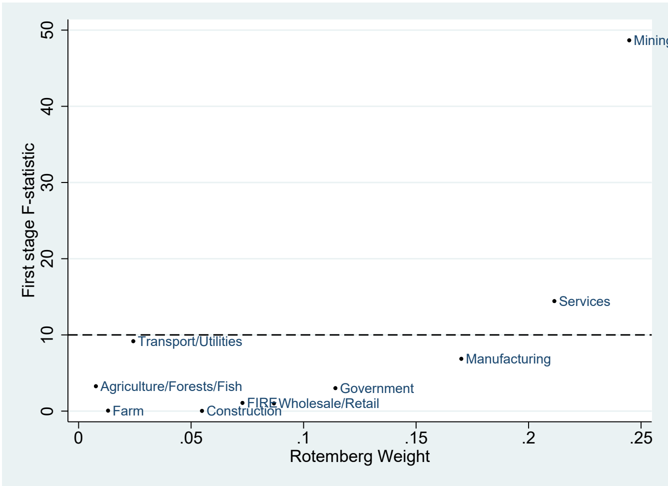
E Appendix Figures and Tables

Figure A1: Beta estimate vs. Rotemberg Weight, by Sector, 1948-2020



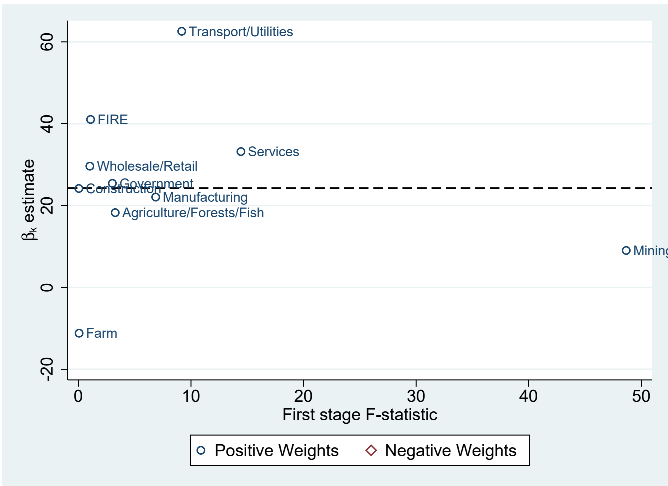
Beta estimate of growth implied by using that sector alone in 2SLS regression, plotted against Rotemberg weight of sector in the shift-share instrument.

Figure A2: F-Statistic vs. Rotemberg Weight, by Sector, 1948-2020



First stage F-Statistic found by using that sector alone in 2SLS regression, plotted against Rotemberg weight of sector in the shift-share instrument.

Figure A3: F-Statistic vs. Beta estimate, by Sector, 1948-2020



First stage F-Statistic found by using that sector alone in 2SLS regression, plotted against Beta estimate of growth implied by using that sector alone in 2SLS regression.

Table A1: Results, dropping different sectors

	Removed:	Farm Earnings	Agri/Forests/Fish	Mining	Construction	Manufacturing	Transport/Utilities	Wholesale/Retail	Finance	Services	Government
1948-2016	Coefficient	0.96899081	0.73570075	0.54375688	0.70143858	0.77350652	0.70900636	0.62578476	0.71973304	0.63697245	0.68908044
	SE	0.38180439	0.32317862	0.33426334	0.31967242	0.30708059	0.30291169	0.27797693	0.31521336	0.2656603	0.29340648
	P-value	0.0143	0.0271	0.1101	0.0329	0.015	0.0233	0.0288	0.0267	0.0203	0.0228
1948-1960	Coefficient	-0.64149326	0.81519498	0.91251975	0.75276234	0.97622118	0.76760719	0.67753869	0.78842974	0.72851013	0.71185791
	SE	1.1920847	0.46219587	0.5399901	0.44456226	0.59594609	0.43933244	0.38918248	0.44541996	0.40457932	0.39584234
	P-value	0.593	0.0843	0.0977	0.097	0.1081	0.0871	0.0882	0.0832	0.0782	0.0785
1964-1992	Coefficient	-1.3554896	-0.25790478	-0.0562101	-0.38600584	0.08441334	-0.2521762	-0.24245068	-0.2128941	-0.1302901	-0.1947312
	SE	0.76989223	0.36086434	0.46822057	0.33975062	0.38398816	0.35405277	0.33744793	0.38516961	0.26508693	0.34008256
	P-value	0.0844	0.4781	0.9049	0.2613	0.8269	0.4796	0.4758	0.5829	0.6252	0.5695
1996-2016	Coefficient	0.86135972	0.72029916	-18.61025	0.64734342	0.79892827	0.71619115	0.60241674	0.76371742	0.50785057	0.67488895
	SE	0.27061332	0.32048099	118.34601	0.32648554	0.29231335	0.30532618	0.29671267	0.30666193	0.20762769	0.2732607
	P-value	0.0025	0.029	0.8757	0.0529	0.0087	0.023	0.0477	0.0161	0.018	0.017

Table A2: Different Estimators of the Effect on Incumbent Vote Share

	(1) OLS National	(2) OLS Time FE	(3) 2SLS Bartik	(4) 2SLS Shares	(5) 2SLS LIML
State Growth	0.587***	0.354**	0.163	0.248*	-0.258
	(0.159)	(0.142)	(0.381)	(0.137)	(0.264)
N	959	959	959	959	959
R^2	0.665	0.752	0.307	0.309	0.275
Sargan Statistic				373.1	370.4
Sargan P-Value				1.95e-17	4.15e-17
Controls	Yes	Yes	Yes	Yes	Yes
Time FEs	No	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes
State Partisan FEs	Yes	Yes	Yes	Yes	Yes
Lags	No	No	No	No	No

Regressions cover 1948-2020. Standard errors are in parentheses below coefficients and are clustered at the state level. *p<0.1; **p<0.05; ***p<0.01

Table A3: Negative and positive weights

1948-2016			
	Sum	Mean	Share
Negative	-0.047	-0.023	0.043
Positive	1.047	0.131	0.957
Panel B: Estimates of β_k for positive and negative weights			
	α-weighted Sum	Share of overall β	Mean
Negative	0.083	0.074	-6.740
Positive	1.041	0.926	2.070

Table A4: First Stage Coefficients and Instrument Strength

Instrument	Start Date	End Date	First Stage Coefficient	SE	Partial F-Stat
Current Shares	1948	1992	1.513***	0.109	192.35***
Current Shares	1948	2016	1.227***	0.116	111.00***
Current Shares	1948	2020	1.226***	0.111	123.00***
Current Shares	1996	2016	0.751***	0.161	21.63***
Current Shares	1996	2020	0.815***	0.144	31.92***
1980 Shares	1948	1992	0.979***	0.286	11.74***
1980 Shares	1948	2016	0.722***	0.133	29.53***
1980 Shares	1948	2020	0.707***	0.113	38.94***
1980 Shares	1996	2016	0.682***	0.163	17.50***
1980 Shares	1996	2020	0.544***	0.144	27.40***

*p<0.1; **p<0.05; ***p<0.01

Table A5: Effect of Dropping Each Sector

	(1)	(2)	(3)	(4)	(5)
	Farm	Forests/Fish	Mining	Construction	Manufacturing
State Growth	0.0144***	0.0150***	0.0147	0.0157***	0.0144**
	(0.006)	(0.006)	(0.011)	(0.006)	(0.006)
Observations	20261	20261	20261	20261	20261
R^2	0.208	0.208	0.208	0.208	0.208
F	115.8	96.21	24.49	82.19	88.72

F-statistics are for the first stage. Regressions cover 1964-2016. Standard errors are in parentheses below coefficients and are clustered at the state level. *p<0.1; **p<0.05; ***p<0.01

Table A6: Effect of Dropping Each Sector

	(1)	(2)	(3)	(4)	(5)
	Utilities	Wholesale/Retail	Finance	Services	Government
State Growth	0.0153***	0.0151***	0.0157***	0.0156***	0.0147***
	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)
Observations	20261	20261	20261	20261	20261
R^2	0.208	0.208	0.208	0.208	0.208
F	104.9	96.97	77.46	42.84	103.5

F-statistics are for the first stage. Regressions cover 1964-2016. Standard errors are in parentheses below coefficients and are clustered at the state level. *p<0.1; **p<0.05; ***p<0.01

Table A7: Effect of Alternative Estimators

	(1)	(2)	(3)	(4)	(5)
	OLS	2SLS	2SLS Shares	LIML	GMM
State Growth	0.00822*	0.0184***	0.0201**	0.0184***	0.0184***
	(0.004)	(0.007)	(0.008)	(0.007)	(0.007)
Observations	20360	20360	20360	20360	20360
R^2	0.209	-0.000297	-0.000621	-0.000297	-0.000297
F	.	42.18	3.381	42.18	42.18

F-statistics are for the first stage. Regressions cover 1964-2016. Standard errors are in parentheses below coefficients and are clustered at the state level. *p<0.1; **p<0.05; ***p<0.01

Table A8: Effect of Growth and Polarization Indices on Incumbent Vote Share

	(1)	(2)	(3)	(4)	(5)
State Growth	0.309	0.773***	1.041***	0.773***	1.064***
	(0.225)	(0.235)	(0.294)	(0.225)	(0.311)
Senate Difference	1.738	1.527			
	(1.902)	(1.508)			
Senate Difference*Growth	-0.315**	-0.336***			
	(0.129)	(0.117)			
House Difference			1.976		
			(1.627)		
House Difference*Growth			-0.122		
			(0.129)		
Senate Distance				4.981*	
				(2.762)	
Senate Distance*Growth				-0.741***	
				(0.278)	
House Distance					2.458
					(3.925)
House Distance*Growth					-0.179
					(0.232)
N	294	294	291	294	291
R^2	0.940	0.938	0.928	0.940	0.927
Kleibergen-Paap rk Wald F-Stat		35.73	49.20	32.68	47.65
Method	OLS	2SLS	2SLS	2SLS	2SLS
Controls	Yes	Yes	Yes	Yes	Yes
Time FEs	Yes	Yes	Yes	Yes	Yes
State FEs	Yes	Yes	Yes	Yes	Yes
State Partisan FEs	Yes	Yes	Yes	Yes	Yes
Weighting	Population	Population	Population	Population	Population

Earnings growth and vote share in percentage points. Regressions cover 1992-2016. All interaction terms are demeaned, so the growth coefficient can be interpreted as the effect of growth for a state with average polarization. SEs clustered at the state level. *p<0.1; **p<0.05; ***p<0.01

Table A9: Effect of Growth and Polarization Indices on Incumbent Vote Share

	(1)	(2)	(3)	(4)	(5)	(6)
State Growth	0.780***	0.794***	0.764***	1.910***	0.773***	0.773***
	(0.235)	(0.234)	(0.248)	(0.371)	(0.236)	(0.235)
Senate Difference*Growth	-0.340***	-0.339***	-0.341***	-0.362**	-0.343***	-0.341***
	(0.113)	(0.113)	(0.115)	(0.159)	(0.119)	(0.118)
Senate Difference	1.543	1.537	1.271	-3.432*	1.368	1.166
	(1.497)	(1.498)	(1.541)	(2.037)	(1.528)	(1.483)
N	294	294	294	357	294	294
R^2	0.938	0.938	0.938	0.911	0.938	0.938
Kleibergen-Paap rk Wald F-Stat	35.41	35.90	37.24	66.33	35.67	35.58
Method	2SLS	2SLS	2SLS	2SLS	2SLS	2SLS
Detrended Variable	Polarization	Polarization	Polarization	Polarization	Growth	Growth
Detrended By	Time Trend	Time FE	Growth	Fitted	Polarization	State FE

Earnings growth and vote share in percentage points. Regressions cover 1992-2016. SEs clustered at the state level. *p<0.1; **p<0.05; ***p<0.01

Table A10: Effect of Post-1995 on Swing Propensity

	(1)	
	Swing Propensity	
1. Grade school or less (0-8 grades) × Post1995	0	(.)
2. High school (12 grades or fewer, incl. non-college × Post1995	-0.377	(0.455)
3. Some college (13 grades or more but no degree; × Post1995	-0.510	(0.477)
4. College or advanced degree (no cases 1948) × Post1995	-0.498	(0.482)
1. 17 - 24 × Post1995	0	(.)
2. 25 - 34 × Post1995	-0.247	(0.499)
3. 35 - 44 × Post1995	0.0409	(0.431)
4. 45 - 54 × Post1995	-0.133	(0.466)
5. 55 - 64 × Post1995	0.371	(0.436)
6. 65 - 74 × Post1995	0.387	(0.488)
7. 75 - 99 and over (except 1954) × Post1995	-0.125	(0.460)
1. Male × Post1995	0	(.)
2. Female × Post1995	0.102	(0.115)
3. Other (2016) × Post1995	0	(.)
1. White non-Hispanic × Post1995	0	(.)
2. Black non-Hispanic × Post1995	0.253	(0.252)
3. Other × Post1995	-0.151	(0.252)
1. 0 to 16 percentile × Post1995	0	(.)
2. 17 to 33 percentile × Post1995	0.281	(0.322)
3. 34 to 67 percentile × Post1995	0.0604	(0.217)
4. 68 to 95 percentile × Post1995	0.144	(0.278)
5. 96 to 100 percentile × Post1995	-0.393	(0.358)
Observations	12014	

Logit regression where independent variable is a binary dummy variable for voting for opposite parties in consecutive elections. Standard errors are in parentheses to the right of coefficients and are clustered at the state level. *p<0.1; **p<0.05; ***p<0.01

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