Voter Turnout and the Labor Market *

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Abstract

Using county level data from 1969-200 and various OLS and TSLS models, we find that increases in local per capita earnings and employment lowers voter turnout in gubernatorial and Senate elections but has not effect on Presidential turnout. We present a model in which risk-averse agents vote only if sufficiently informed about political candidates. When agents work more, as they do in periods of high local wages and employment, they devote less time to information acquisition, are less informed and thus vote less. This negative effect should be smaller in elections, such as that for the President, where information is so ubiquitous that reductions in political attentiveness have little effect on uncertainty. Consistent with the model's predictions, we find using individual data from several waves of the American National Election Study (ANES) we find that: less informed voters, and especially political moderates, are less likely to vote; uncertainty is smaller in Presidential as opposed to other elections; and that, voters' accessing of media, political knowledge and interest in politics vary negatively with changes in employment.

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1. Introduction

Does voter turnout rise or fall as the labor market improves? Why? Although economists and political scientists have long been interested in how the performance of the labor market affects voting behavior, virtually no work in the "economic voting" literature studies voter participation – the most fundamental dimension of voting behavior. Instead, most previous work, generally using state or national level data, has studied the incumbent's vote share – the fraction of votes in an election received by the incumbent or the incumbent's party. [1] But since the vote share summarizes how those *who have chosen to vote* cast their ballots, this focus presents an incomplete picture of economic voting if the propensity to vote in the first place is affected by the state of the labor market. Moreover, an exclusive focus on the vote share could lead to misleading theoretical conclusions about the reason for a relationship between the state of the labor market and agents' voting behavior.

This paper studies how voter participation is affected by the level of earnings and employment in the labor market. Using county-level data from for elections between 1969 and 2000, we find that, once county fixed effects and state-year fixed effects are accounted for, increases in county per capita earnings and per capita employment lead to lower voter turnout in Senate and gubernatorial elections, but have no effect on turnout in Presidential elections.

To address potential concerns about either the mis-measurement or the endogeneity of county-level changes in labor market conditions, we employ a Two Stage Least Squares (TSLS) strategy in which we instrument for county labor market performance with county-level exposure to exogenous shocks in coal and oil prices. Consistent with our expectation of the likely bias in the OLS estimates, the TSLS results for the Senate and gubernatorial elections are of the same sign but larger in absolute value than their OLS counterparts. Strikingly, the TSLS Presidential results are relatively precisely estimated zeroes, as is the case for the OLS estimates. We next estimate simple difference models on data aggregated to a higher level than the county: the State Economic Area, or SEA. These OLS regressions do not rely in any way on our instrument so are thus not affected by the concern that might be raised about the TSLS estimates that the instruments directly affect voting, possibly via transitory migration. And, because of the higher level of aggregation, labor market conditions are likely measured with less error at this level than at the

[1] Kramer (1971) is widely regarded as the first systematic empirical analysis in this literature. See Stigler (1973) and Arcelus and Meltzer (1975) for other early work. See Nannestad and Paldam (1994), Lewis-Beck and Stegmaier (2000), and Hibbs (2005) for careful reviews of the large recent empirical literature on economic voting. Owens and Olson (1980), who study district-level congressional voting, is one of the only studies on economic voting we have been able to locate in the United States which uses local data.

level of the county. These results are very similar to those from the other strategies: economically meaningful and negative relationships between local labor market performance and turnout for gubernatorial and Senate elections; no statistically significant relationship between these variables for Presidential elections.

To our knowledge, these results are new to the literature on economic voting. We spend the remainder of the paper exploring an explanation for them. The theoretical argument usually posited for why labor market conditions might affect voting behavior dates back to the seminal work of Downs (1957), and holds that macro-economic conditions are informative about the incumbent's competence as a steward of economic matters. Positive labor market realizations cause voters to think more highly of the incumbent, which in turn makes them more likely to support that person or his party at the following election. This argument is consistent with results from the previous literature that the incumbent's vote share is increasing rises when the labor market improves. [2] This traditional argument has long been controversial, however. [3] Skeptics have pointed out that since no single person can affect macro-economic variables such as aggregate wages, economic growth or unemployment, rational voters should neither credit nor punish an incumbent for something he could not possibly have affected. [4]

We take no position on whether part of people's support for the incumbent is how well the labor does under his stewardship, or the rationality of that position. For our purposes, the key issue is that this argument cannot explain our various results for voter turnout.[5] Following the recent literature on "expressive voting", we present a simple alternative model in which agents vote when they are sufficiently certain about the candidates' relative closeness to them in political space. Uncertainty is a function of the information the voter has been able to access about political matters, and this information depends in turn on the time spent accessing media or information sources and the availability of political information on those sources. When job market conditions are good or wages high, agents work more,

[2] Beginning with Kramer (1971), many authors have found using aggregate state or national data that the vote share is positively related to average income for different types of elections. Interestingly, as summarized by Paldam (1991), some studies do not find the usual estimated relationship between some measures of labor market performance and vote share .

[3] See Stigler (1973) and (1979) for sharp criticisms of both the theoretical justification for and empirical tests in the literature about macro economic performance and the vote share. Peltzman (1990) makes very similar arguments.

[4] If the only aim of voting is to affect the likelihood of who wins an election, the fact that an individual votes at all is a "paradox" (Downs (1957) and not consistent with rationality, since no individual ballot is pivotal in determining the outcome. Confronting the fact that many millions of people routinely vote, recent work has argued that voting is also partly "expressive", and also partly an act by which people discharge a basic civic or social responsibility.

[5] If, as the standard argument says, positive labor market performance leads citizens to think more highly for the incumbent and makes them more likely to vote for him. But negative labor market performance which presumably leads them to think less highly of the incumbent should *also* make them more likely to vote – this time to register their negative opinion of him and his party.

are less politically attentive, are less informed and are thus less likely to vote. This is consistent with our aggregate finding for gubernatorial and Senate elections.

Our model also implies that when political information is ubiquitous, as is the case in Presidential elections, lower political attentiveness arising from greater labor market specialization should have little effect on uncertainty and thus little effect on voting. Finally, although explaining them is not our focus, our model can also reconcile previous results about pro-cyclical vote share without relying on the controversial assumption that citizens credit politicians for the performance of the labor market.

The insight that an absence of political information can lead to lower voter turnout is not new. For example, Feddersen and Pesendorfer (1996) show that, in a game-theoretic model, uninformed voters may choose to abstain and allow the informed voters to decide the outcome of an election. Empirical evidence on the relationship between information and turnout has been found using observational data (e.g., Palfrey and Poole 1987; Wattenberg et al. 2000; Coupe and Noury 2004), in the lab (Battaglini, Morton, and Palfey, Forthcoming), and with natural experiments on the presence of media (e.g., Gentzkow 2005; Lassen 2005; Gentzkow, Shapiro, and Sinkinson 2009). Our contribution in this paper is the idea that labor market fluctuations not only affect labor supply decisions by altering the opportunity cost of time working but also affect the amount of time allocated to other activities, including time devoted to collecting political information. If political information is an important determinant of voter turnout as posited by the prior literature, then rational individual time allocation decisions should lead to a systematic relationship between labor market fluctuations and voter turnout.

An especially attractive feature of our model is that it yields a number of implications that can be directly tested in individual level data. Using individual level data from several waves of the American National Election Study (ANES), we find strong support for several of these in the data. First, we show that, as our model argues, voters of all types of partisanship are more likely to vote the better informed they are. Second, we show that, as the model predicts political moderates are most sensitive to information, in the sense that the gap in turnout between those of them who are informed and those who are not is larger than the corresponding gap for political extremes. Third, and least surprisingly, we show that voters are best informed about candidates in Presidential elections compared to other contests. Fourth, and most importantly, using panel models on multiple waves of individual ANES data, we find that changes in individual labor supply are negatively associated with time spent accessing media, and knowledge about and interest in politics.

In the next section we outline the basic empirical framework for our county level analysis. Section 3 discusses the county analysis and Section 4 discusses these results. In Section 5 we present a model that reconciles the various aggregate results. Section 6 tests implication of that model with individual level data. Section 7 concludes.

2. Empirical Specification using Aggregate Data

Suppose voter turnout, V_{cst}, in county c and state s for an election occurring at date t is given by

$$V_{cst} = \beta_0 + \beta_1 x_{cst} + \beta_2 E^*_{cst} + \delta_c + \pi_{st} + \eta_{cst}. \quad (1)$$

In (1), x_{cst} is a vector of observed county characteristics, and η_{cst} is an independent, mean-zero error. E^*_{cst} represents measured local labor market performance in the county in the time period immediately preceding the election; the variable is starred to denote the fact that we observe error-ridden versions of actual labor market outcomes. The variable δ_c is a vector of county-fixed effects that account for fixed, latent factors specific to a county that affect voter turnout. The variable π_{st} is a set of state×year fixed effects that captures the effect of factors specific to a state in a given election year that affect voting behavior, especially those like the popularity (or wealth) of the challenger that might vary systematically with economic conditions. To account for these state×year effects, one must use data at a finer level of aggregation than the state, as we use here with county-level data.

We seek to estimate the parameter β_2 -- the effect of labor market conditions on voter turnout. Consider a differenced version of (1), where Δ_c represent the county-level difference between consecutive election years in a given variable[6]:

$$\Delta_c V_{cst} = \beta_0 + \beta_1 \Delta_c x_{cst} + \beta_2 \Delta_c E^*_{cst} + \Delta_c \pi_{st} + \Delta_c \eta_{cst}. \quad (2)$$

Regression (2) is purged of the effects of any latent, fixed factors, and still controls for state-specific factors that change across elections. Unfortunately, there are two reasons to worry that the estimate of β_2 forthcoming from the estimation of (2) will not be an unbiased estimate of the parameter. One is the familiar concern about difference estimators that any attenuation bias associated with the fact that labor market conditions is measured with error is likely even worse in the difference model (2) than in the level

[6] If time were denoted in terms of election years only, then the term "difference" would be equivalent to a "first-difference." However, since we let the subscript t refer to calendar year, we use the term "difference" to avoid confusion between differences in consecutive calendar years and differences in consecutive election years.

equation (1) (Bound et al. 2001). A second potential problem is that even if local labor market performance is measured with no error at all, the estimate of β_2 forthcoming from (2) is unbiased only if $\operatorname{cov}\left(\Delta_c E^*_{cst}, \Delta_c \eta_{cst}\right) = 0.$ That is, the estimate is biased if within-county *changes* in economic performance are uncorrelated with *changes* in time varying county-specific factors that also affect voting behavior.

One way to deal with both attenuation bias caused by measurement error and any bias arising from the endogeneity of earnings growth is to instrument for changes in county labor market performance, $\Delta_c E^*_{cst}$. In much of the analysis to follow, we focus on counties in states especially affected by exogenous variation in coal and oil prices. [7] In these "coal" and "oil" states, counties will be differentially affected by price variation because of differential endowments of the relevant resources. If counties in these states are sorted into bins representing the importance of oil or coal production within them, a natural instrument for the change in county-level earnings is

$$\Delta P \times I\left(county\_size\right) \tag{3}$$

where ΔP is the change in the price oil or coal between election years, and $I\left(county\_size\right)$ is an indicator variable denoting the "bin" into which county can be sorted, based on the importance of oil or coal production in the county. The resulting Two Stage Least Squares (TSLS) model estimates (2), with the change in earning replaced by the predicted value from the regression

$$\Delta_c E^*_{cst} = a_0 + a_1 \Delta P \times I\left(county\_size\right) + a_2 \Delta_c x_{cst} + a_3 \Delta_c \pi_{st} + \Delta_c e_{cst} \tag{4}$$

Although we estimate some modified versions of these expressions, which we discuss below, equations (2)-(4) are the foundation of the analysis of aggregate data to follow.

3. Aggregate Data

County-Level Data

We study gubernatorial, senate and presidential elections occurring between 1969 and 2000. We focus throughout on two different measures of aggregate labor market performance: per capita earnings and

[7] Other authors have used shocks to energy prices to isolate variation in local labor market outcomes – most notably Black et al (2002) who use only coal shocks in their study of disability payments. Acemoglu et al (2009) use the impact of oil price shocks on the economies of areas in the South with large oil wells to study health spending. A paper by Wolfers (2002) in the economic literature also uses oil price shocks. That work very much differs from our analysis in that it focuses on incumbent vote share and uses aggregate state-level data. Also, following the logic of the previous literature, evidence of voting responses to exogenous changes in outcomes arising from oil shocks would be considered evidence against voter rationality, something we do not argue.

employment per population. The Bureau of Economic Analysis's Regional Economic Information System (REIS) provides annual county-level data on earnings and employment beginning in 1969. Earnings include wage and salary disbursements, other labor income, and proprietors' income. The primary source for REIS wage and salary disbursements and employment data is compiled by the Bureau of Labor Statistics (BLS) using ES-202 collected as part of the state unemployment insurance program.[8]

County-level wages and employment are known to be measured with error. Some employers with establishments in multiple counties may only report wages and employment ES-202 information at the state-level. These reports are allocated back to counties based on their industry level distribution by county among employers reporting at the county-level. Components of other labor income and proprietors income (e.g., pension plan contributions, health and life insurance contributions, and private worker's compensation contributions) are only collected at the state-level and also make use of an allocation rule to determine county-level totals.

In our empirical analysis, we control for both the level of county population and the within-county population age distribution. The REIS reports population data, but only county-level population totals are provided; there is no population information by demographic characteristics. The Census Bureau provides county-level population by age, sex, and race beginning in 1970.[9] However, prior to 1980, county-level population distributions by age are limited to five year age bands (0 to 4, 5 to 9, etc.). Therefore, our county-level estimates of the number of voting age individuals is calculated as the number of individuals aged 20 and over.

We compile voting information from two sources. County-level voting totals by party of the candidate are available from the Inter-University Consortium for Political and Social Research (ICPSR) as part of "General Election Data for the United States, 1950-1990" (ICPSR study no. 13). We collected data for the remaining years from the CQ Press Voting and Elections Collection. Voter turnout at the county-level is constructed by dividing the total number of votes cast in an election by the Census estimate of the number of individuals ages 20 and over residing in the county. We have verified, and corrected when necessary, outlier values for voter turnout as well as missing data from both data sources. These corrections are detailed in the Data Appendix. We exclude elections in particular state-years that might be affected by mechanisms different from those that prevail in the standard case. Thus, we focus on Senate elections

[8] Documentation for all of the REIS sources are available on-line with the REIS data at http://www.bea.gov/regional/docs/cd.cfm.
[9] These data are available on-line at http://www.census.gov/popest/archives/. We impute 1969 county population information by simply using the 1970 population data.

which are separated by the standard six year interval, and gubernatorial elections that are four years apart. These restrictions ensure that no special elections are part of our sample. We also drop a small number of unopposed elections.

An important portion of our empirical analysis focuses on the changes in a county's economic performance induced by changes in the exogenously determined prices of oil and coal. To identify these counties, we first define a set of "coal" and "oil" states. "Coal" states are the four states that span the country's large coal seam. Following Black et al. (2002), these are Kentucky, Ohio, Pennsylvania, and West Virginia. We define "oil" states to be those in which at least 1 percent of annual state wages in the 1974 County Business Patterns are from the oil and gas industry. These states are Colorado, Kansas, Louisiana, Mississippi, Montana, New Mexico, North Dakota, Oklahoma, Texas, Utah, and Wyoming.[10]

We construct a measure of the importance of the oil and gas industry and the coal industry in the various counties within these states using County Business Patterns (CBP). Since 1974, CBP data have been based on the Census Bureau's Standard Statistical Establishment List. Because of the risk of disclosing firm specific information, exact employment numbers for two-digit industries such as coal and oil and gas are not available at the county-level. However, the CBP provides county-level information on both the number of firms in each two-digit industry and the number of firms that fall into a specific firm-size category (e.g., 20 to 49 employees) for these industries. By weighting the number of firms in a firm-size category by the mid-point of the number of employees in that category, we create an estimate of the number of employees in each two-digit industry at the county-level. We then create county-level estimated employment shares by industry as the ratio of the estimated industry employment to the estimated total county employment where the total county employment is also estimated by using the firm-size methodology. By this method, we can create estimates of the oil and gas industry and the coal industry using two-digit information as early as 1974.[11]

Appendix Table 1 compares our "oil" and "coal" states to other states in which mining is important. The first column presents, in decreasing order, the 25 states with the highest share of total employment in mining according to the 1974 CBP. The next two pairs of columns show the share of mining establishments in these states in oil and gas and in coal using the 1974 CBP and the 1967 Census of

[10] This list excludes Alaska because changes in FIPS code definitions during the sample period create difficulties for matching across data sources.
[11] CBP data for 1970-1976 were obtained from UCLA's Institute for Social Science Research Data Archive through the kind help of Libbie Stephenson. CBP Data for 1977-1996 and 1998-1999 were obtained from ICPSR with the remaining years being obtained from the U.S. Census Bureau.

Mineral Industries (CMI). The numbers indicate that the states we call "oil" and "coal" by our definition line up very well with the importance of mining overall, and of the specific type of mining, across states. For example, our "oil" state of New Mexico had the third highest fraction of total employment in mining in 1974. And, more than 80% of the mining establishments in the state are in oil and gas. The pattern is very similar for "coal" states like Kentucky or West Virginia. Importantly, the table also shows that the same basic pattern from 1967 CMI Mining data, suggesting that the states and counties we label as important oil and coal producers would have been so described even before the large energy price shocks described below. The table also shows that various states labeled "oil" or "coal" have much of the other type of mining within them. [12] Consequently, although we characterize states as either "oil" or "coal", in the empirical work we measure the importance of *both* oil and coal in the counties in the states, regardless of whether the county falls into an oil state or a coal state.

Throughout our analysis, we define counties with less than 5% of their 1974 total employment in the production of oil and gas as "small" oil and gas producers. Counties with at least 5% but less than 20% of 1974 employment in the production of oil and gas are "medium" oil and gas producers and counties with 1974 employment shares of 20% or more are "large" oil and gas produces. We also divide counties into analogous coal producing categories based on their share of total 1974 employment in coal production. Figure 1a and 1b are maps of the oil and coal states in our analysis, showing the distribution of "large", "medium" and "small" oil and gas producing counties within oil states and coal producing counties within coal states. As summarized in the list in Appendix Table 1, our oil and coal states span a large swath of the country, with the oil states running essentially through the midsection of the country and the coal states a large part of the upper South and lower Mid-West. The figures also show that there is tremendous variation in the importance of oil and coal across counties *within* states. It is this variation that our TSLS analysis exploits.

Table 1 shows summary statistics in the Census years 1970, 1980 and 1990 for the age distribution, gender, education, and occupation in "oil" and "coal" states for all workers, and for workers in the particular sectors. Both oil and coal – and especially coal -- are heavily male industries: despite a modest decline in the fraction of oil workers who were male over this period, as late as 1990 the share remained above 80%; the coal industry was almost exclusively male in 1970 at 97% and remained so twenty years later. By 1990, about a quarter of oil and coal workers were college graduates, and nearly half had attended college – numbers similar to those for other workers in these states. By contrast, only 5% of

[12] For example, 40% of the mining in the "coal" state of West Virginia is devoted to oil and gas. Similarly, 15% of the mining in the "oil" state of North Dakota is in coal.

coal workers in 1990 were college graduates, and only 20% had attended college. These attainment rates were quite low not only when compared to oil workers, but also compared to other workers in coal states.

Corresponding to these educational differences are the differences in workers' occupations. One quarter of oil workers were managerial or professional workers; the comparable rate for coal workers was only 6% in 1990. Coal is heavily blue-collar, with about three-quarters of all coal workers employed as craftsmen, repairers, operator or laborers. Whereas these occupations were also heavily represented among oil workers, the rates in that industry are much lower than those for coal. An important consequence of these differences is that our TSLS results do not hinge on voting behavior among a particular set of citizens (such as those with very low levels of schooling), as would have been true if we had focused exclusively on either oil or coal price shocks.

Coal and Oil Shock Measures

For the TSLS analyses, we obtain data on energy prices from the Energy Information Administration's *Annual Energy Review*. Oil prices are the U.S. average first purchase price per barrel, gas prices are the wellhead price per thousand cubic feet, and coal prices are the total price per short ton. National oil and gas industry and coal industry employment is taken from CBP data. We restrict our attention to the Continental United States (i.e., we drop Alaska and Hawaii). We also exclude Washington, D.C. since its residents do not cast votes in either gubernatorial or senatorial elections.

Our Two Stage Least Squares strategy focuses on the years 1969 to 1990, a period during which there were two large exogenous shocks the world oil supply: the OPEC oil embargo following the Yom Kippur War; and the period from the end of 1979 to early 1981, following the overthrowing of the Shah of Iran and the start of the Iran-Iraq War. [13] These geo-political events affected the real prices of oil, coal and natural gas, as shown in Figure 2a. Oil prices doubled over the 1973-1974 period, were stable for several years, then increased fourfold over a two year period – reaching a then record high of $60 a barrel by 1981. Oil prices fell dramatically over the next four years to levels in the mid-1980s slightly lower prices in the mid-1970s. Coal prices were rising before 1973 but these increases were swamped by rises between 1973 and 1976, when coal prices essentially doubled. Coal prices fell consistently over the next two decades, by the late 1990s returning to levels not seen since the early 1970s. Natural gas real prices followed very much the same pattern as that for oil prices. Gas prices increased 5- fold between 1970

[13] In 1967, when OPEC had a very similar policy response after the Six Day War, the effect on the price of oil was small and fleeting. In the early 1970s, however, with so much of oil used in the West imported from the Middle East, matters were quite different.

and the early 1980s and then declined by more than a half over the next 6 years. Our information on employment does not distinguish between oil and gas production, so our analysis uses only oil prices to instrument for oil and gas employment. The close co-movement of oil and gas prices over the sample period shows that this assumption is reasonable.

Figure 2a shows that oil prices were relatively flat during certain periods in the 1970s – the result of the very heavy regulation that then characterized this industry.[14] One could argue that these policy-determined intervals of unchanging oil prices show that oil price variation was not totally exogenous. To deal with this concern we also conduct all of our TSLS analyses using total national employment in oil and gas as our instrument rather than the change in oil prices. Figure 2b shows that this employment variable matches the basic pattern in prices and, in particular, does not have the flat portions that are evident with the oil price series. When we use the change in national employment as our instrument, our results are almost identical to those using price changes.

Although our measures of the county-level importance of the coal and oil and gas industries are computed at the beginning of the boom in the resource prices, one possible concern is that our 1974 employment measures have been influenced by the price changes. Unfortunately, County Business Pattern data prior to 1974 does not include the same two-digit firm size detail available in subsequent years. Alternatively, we make use of the one-digit data from the 1967 County Business Patterns data.[15] We are able to create measures of the total mining share of total employment using the same firm-size methodology as with the 1974 CBP, but we cannot do so for each specific mining industry such as coal and oil and gas. One drawback to using this alternative 1967 measure of the importance of mining is that we must treat all counties on oil states as being affected oil prices and all counties in coal states as being affected by coal prices. This approximation is not too onerous since, as shown in Appendix Table 1, nearly all of the mining in oil states is in the oil and gas industry and a majority of mining in coal states is in the coal industry. Thus, while this alternative measure of importance pre-dates the early 1970s boom in prices, it sacrifices the ability to delineate between coal and oil and gas production at the county-level. As we

[14] Since the early 1900s, oil production in U.S. has been overseen by various state regulatory boards, such as the Oklahoma Corporations Commission, the Louisiana Conservation Commission and, most importantly, the Texas Railroad Commission. Although the specific language outlining each board's functions and objectives differ from state to state, these agencies set limits on level of extraction and exploration in their particular states so as to stabilize price, and prevent over-exploitation of oil reserves. In the immediate post-WWII years, the various commissions gradually allowed increased production activity, as U.S. energy demand increased. This careful pro-rationing, and the relatively low (by later standards) level of U.S. demand, allowed domestic supplier to receive a stable price for oil. And, the rationing also meant that oil production in these years was substantially less than capacity which meant that increases in production in early years was not accompanied by large price increases.

[15] CBP data for 1967 were obtained from the University of Wisconsin's Data and Information Services Center (DISC) through the kind help of Cynthia Severt.

show formally later, the oil and coal price shocks shown Figure 2 led to differential changes in the earnings and employment in counties based on how important oil or coal production was within the county. The portion of our analysis that utilizes TSLS methods exploits this variation.

Table 2 summarizes turnout and labor market outcomes for the counties in our sample. The table shows that across all elections from 1969 to 2000, county turnout in Presidential elections averaged around 55%, while gubernatorial and Senate turnout averaged 43% and 47% respectively. This basic pattern was very similar for counties in counties in the oil and coal states, both during the entire 1969-2000 period, and also during the interval the boom and bust period of 1969-1990. Oil and coal counties had slightly lower earnings than did the rest of the country during overall, and this difference grew very slightly during the boom and bust years. For employment, there is a modest difference in labor market outcomes between these counties and the rest of the country only during the period of the boom and bust.

4. Empirical Results using Aggregate Data

4.1 OLS and Difference Estimates

Table 3 presents estimated results for various versions of regressions (1) and (2). As will be the rule for virtually all of the results to follow, the table shows results for both labor market outcomes and for gubernatorial, Senate and Presidential elections. This table studies all counties in the U.S., over the period 1969-2000. For each election type, we present three sets of estimates. The estimates in column (a) are from pooled OLS regressions that control for state and year fixed effects. Column (b) presents difference estimates in which the change in county voter turnout is regressed on the change in the relevant labor market outcome. These regressions include year effects. The regressions in column (c) replace the year effects from column (b) with state×year fixed effects. The movement from columns (a) to (b) reflects the importance of controlling for fixed county characteristics;the movement from columns (b) to (c) the importance of factors specific to that particular state×year. In addition, we present results for each labor market outcome separately with log per capita earnings as the earnings measure shown in the first row and employment per adult shown in the second row.

The results are striking. In pooled OLS models that control separately for state and time effects, better local labor market performance is associated with higher voter turnout. The estimated effects are very strongly statistically significant for all election types and for both labor market measures. The results in column (b) in which we relate the *change* in county voter turnout to the *change* in labor market performance are very different. In particular, the previously positive point estimates become negative for the Senate and gubernatorial elections. For the governor, both of estimated negative effects are significant, but neither is for the senate. For Presidential elections, estimating the simple difference model in column (b) does not change the signs of the various point estimates, but the estimated effect of labor market performance on turnout is no longer significant. When we add state×year effects to the difference model, the negative gubernatorial estimates for earnings are cut in half but remain significant. The point estimate for employment remains negative but falls by an even larger amount than does the earnings results, and is no longer statistically significant. For both the senate results, the addition of state×year effects lowers the estimated labor market effects to zero. For the president, the effect of earnings remains zero, but that for employment is now positive and significant, albeit at less than half the magnitude of the pooled OLS results.

On the whole, these results show that fixed factors specific to the county, and considerations specific to a state in a given year matter importantly for turnout. In all that follows we therefore use as our benchmark the difference model (2), which includes state×year fixed effects. We have noted two reasons to be concerned about the consistency of the point estimates in column (c) are: measurement error in changes in county level labor market measures, and the possible endogeneity of those measures. Measurement error likely biases the estimates towards 0; endogeneity likely biases the estimates positively through 0.[16] Both of these would lead to estimated negative effects that are too small. We turn next to our instrumental variables strategy for dealing with these problems.

4.2 TSLS Estimates

We begin our instrumental variables analysis with results from the first-stage regressions (4), and of the reduced form equation in which we relate the change in voter turnout directly to the instrument $\Delta P \times I\left(county\_size\right)$. These county-level results are presented in Table 4. The various panels in the table are from a regression of the change in relevant variable between election years on the instrument set, over the years spanned by the different election types during the boom/bust period of 1969-1990. Because these regressions control for state×year fixed effects, the models in the Panels A and B assess whether deviations in the growth rate of a county's earnings (or employment) from the time series pattern

[16] Since better educated and richer people are more likely to vote, we would expect that counties in which income or unobserved dimensions of human capital were growing would also exhibit growth in the likelihood of voting for other reasons.

within the state are systematically related to the exogenous change in the change in oil/coal price and whether this effect differs across counties in which the resource was differentially important.

For log earnings shown in Panel A, the results show that for all three elections the price shock was systematically related to changes in county earnings, in the direction expected. The *F*-stat for the full set of instruments are above 10 for types of elections, and the estimated effects of the shocks, are largest for “large” counties, and next biggest for those that are “medium”. With some minor differences, we find much the same pattern for employment per adult in Panel B. The most noteworthy differences are that the *F*-stat for the shock variables is only 8.3 for the senate elections years; they are by contrast, very large for governor and presidential election years. On the whole, our price instrument appear to be exogenous drivers of county-specific employment and log earnings growth – even over the years specific to any type of election.

Panel C in the table presents reduced form results where the change in voter turnout is the dependent variable. If our empirical strategy is valid, it would be reassuring if county-specific changes in turnout are systematically related to measures of the energy price shock, with magnitudes consistent with the size of the shock. For the most part, this is what the results show – especially for “large” counties. The results suggest that a given shock to oil or coal prices *lowered* voter turnout in “large” counties relative to “small” or “medium” counties for both senate and gubernatorial elections. The reduced form results for presidential elections are quite different: oil price shocks seem to have had no effect on the change in voter turnout for any kind of oil county; and coal price shocks appear to have seem to have increased turnout, and done so more in “medium” than in “large” counties.

Table 5 presents OLS and TSLS estimates of the effect of *changes* in the two labor market measures on the *change* in voter turnout in oil and coal counties over the boom/bust years. The TSLS estimates of labor market performance on turnout are negative and strongly statistically significant for both gubernatorial and senate elections. The fact that these estimates are larger (in absolute value) than the negative OLS point estimates strongly suggests that measurement error and endogeneity bias affect the latter results in the manner we have described. The difference between the gubernatorial and senate elections on the one hand, and with the presidential results on the other, is very striking. For Presidential elections, both of the TSLS point estimate are positive but neither is statistically different from zero– just as was the case in the (albeit more precisely estimated) OLS results.

How large are these various estimated effects? The estimated coefficient of -0.168 for the log earnings variable in the gubernatorial regressions implies that a twenty percent increase in a county's per capita earnings – a 0.2 log point change – leads to about 3.4 percentage point reduction in county voter turnout. Relative to the mean of close to 43% across all gubernatorial elections in the U.S., this implied effect is economically significant but not overwhelming large. Similarly, the point estimate for Senate elections of -0.39 implies that an increase in the employment rate per adult by 20 percentage points leads to a reduction in county turnout of about 3.8 percentage points --- another effect that is economically significant but not implausibly large given levels of average turnout for Senate elections.

Rather than using the change in the coal and oil and gas prices to construct our instrument, we could have used instead the change in total national coal and oil and gas employment – a series which we earlier showed tracked the price changes very closely. In addition, instead of measuring a county's "size" by the 1974 CBP employment share in coal and oil and gas, we could have used the alternative 1967 CBP measure mining employment share as presented above. We have already discussed what we see as the limitations of each of these two options relative to the ones we chose, but we conducted a variety of robustness exercises using alternative specifications of the instrument set that correspond to these two possibilities. As summarized in Appendix Table 2, we find essentially the same results irrespective of the particular instrument specification used: for gubernatorial and Senate elections, the TSLS estimates of the effect of both labor market measures are negative, larger than OLS results, and strongly statistically significant; and, for Presidential elections, none of the TSLS estimates is statistically different from 0.

4.3 State Economic Area Estimates

We believe that the TSLS estimates for gubernatorial and Senate elections are larger than the OLS estimates because measurement error and endogeneity bias (arising from the fact that counties with high earnings growth rates might, for idiosyncratic reasons, exhibit higher vote rates) should both likely bias the OLS estimates towards 0. We are reassured in this conclusion by the fact that both sets of presidential results are 0. This suggests that the TSLS estimates do not instead reflect the some direct of resource price shocks on voting, such as that arising from temporary migration, as this should affect behavior across all types of elections. Nonetheless, it would be reassuring if we found qualitatively similar results with another estimation strategy which did not rely on our instrumental variables approach at all.

To that end, we estimate OLS models using data aggregated to the level of State Economic Areas (SEA).[17] SEAs are aggregate economic units originally developed for the 1950 Census which consist of either a single county or a set of contiguous counties which do not cross state lines (Bogue 1951). Thus, we can use the same difference specification including controls for the important state×year fixed effects. Because SEAs are much larger than counties, labor market conditions should be much better measured at this level. OLS estimates with SEA level data should be much larger in absolute value than corresponding county level results if attenuation bias in the latter estimates is a serious concern, as we have argued. And, because the SEA results do not depend on the oil and coal price shocks, there is no concern about the results being driven by some failure of the exclusion restriction as might be claimed about the preferred TSLS estimates shown earlier.

Table 6 shows that, indeed, the OLS difference estimates with SEA data are, for gubernatorial and senate elections, substantially larger than the corresponding OLS county-level estimates in Table 3. For example, while the estimated effect of the change in log per capita earnings on the change in gubernatorial voting was -0.014 in the county level regression without state×year fixed effects, the corresponding SEA estimate is -0.058. Adding state×year sharply decreases the SEA point estimates, as was the case with the county level OLS results, but again the estimates are much larger than the corresponding OLS results. So for example, whereas adding state×year effects lowers the OLS county estimate of the effect of employment to 0 for gubernatorial elections, with SEA data the point estimate falls from -0.16 to -0.047, and remains statistically significant. Interestingly, notice that unlike the other two types of elections, for the Presidential election none of the OLS results using SEA data level point estimates is statistically different from zero.

On the whole, these results combined with the other aggregate estimates, suggest that for gubernatorial and Senate elections, improvements in labor market conditions lead to reductions in voter turnout. Presidential turnout, by contrast, seems unaffected by changes in local labor market conditions.

5. Theoretical Framework

What theoretical account can explain the results in the previous section? We present a simple model of expressive voting in which risk-averse voters care about candidates' views on some purely political question -- say abortion or gun-control -- but have imperfect information about candidates' views. Information quality depends how much non-market time agents consume and thus devote to activities like

[17] Most states have between 6 to 11 times the number of SEAs as counties.

reading newspapers, listening to the radio, or watching television and the amount of political information on these media outlets. When wages and employment are high, voters devote less time to the collection of and are less knowledgeable about politics. We argue that the implication for this on voting behavior may explain our finding above, and may also explain findings from previous work about the incumbent's vote share.

Following the expressive voting literature, we suppose that voting is a costly expression of a voter's relative approval between two candidates, based on how close candidates' views are to the voter's. Formally, let voters j have political sentiment θ_j which can be positive or negative. Suppose θ_j is distributed Standard Normal to reflect the fact that people with extreme political views - those whose θ_j are large in absolute value - are relatively rare, compared to the majority of voters whose political sentiments place them just over one side or the other of the political divide. Elections are contested between two candidates representing two political parties, P. Voters do not directly observe candidates' political sentiments, but know that candidates from the ***L***(eft) party have preferences x_L, drawn from some density F_L over the range $(-\infty, 0]$, and that candidates from party ***R***(ight) have political sentiments x_R, drawn from some density F_R over the range $(0, \infty]$.[18]

The political distance between a voter and the candidate from P is $d_j^P = \left|x_P - \theta_j\right|$, and the binary variable $C_j^L = I\left[d_j^L \leq d_j^R\right]$ indicates that the voter j is closer to the L candidate than to the R candidate.[19] Because candidate sentiments are un-observed, voters do not know C_j^L, but given the densities F_L and F_R, there is for each voter j a known prior probability, π_j^L that a voter of given is closer to the candidate from party L than to the candidate from party R. That is,

$$\pi_j^L\left(\theta_j\right) = \Pr\left[C_j^L = 1\right]. \tag{5}$$

Since a voter is less likely to be closer to an L candidate the more intensely R partisan that voter's views, we assume that $\partial \pi_j^L \big/ \partial \theta_j < 0;$ and $\pi_j^L\left(\infty\right) = 0$ and $\pi^L\left(-\infty\right) = 1.$ Since π_j^L is a monotone function

[18] We allow the process generating political candidates to have different distribution of traits than is true for non-candidates in the party.

[19]Our decision to cast our analysis in terms of whether the voter is closer to the candidate from the L party is, of course, irrelevant. Given the binary nature of the problem, all of the reasoning goes through if we focused instead on the probability that the voter is closer to candidate from party R..

of θ_j, the fraction of people in the population with a prior probability of - say $\pi_j^L(a)$ - is $\varphi(a)$ where φ is the p.d.f. of the Standard Normal distribution.

Before an election, voters receive some information s_j^L about whether they are closer to the L candidate. This information depends on a voter's true relative distances from the two candidates in the following way:

$$\begin{aligned} C_j^L = 1 \Rightarrow &\begin{cases} s_j^L = 1 \ \text{ with probability } (1-e) \\ s_j^L = 0 \ \text{ with probability } e \end{cases} \\ C_j^L = 0 \Rightarrow &\begin{cases} s_j^L = 1 \ \text{ with probability } e \\ s_j^L = 0 \ \text{ with probability } (1-e) \end{cases} \end{aligned} \tag{6}$$

The "error" e is a summary measure of information quality, and we suppose $0 \le e \le 1/2$. We assume that

$$e = e\left(l_j, \gamma\right) \tag{7}$$

where l_j is the amount of leisure (or non-market time) that the voter consumes, and $\gamma \ge 0$ summarizes how much information about political matters is forthcoming from time spent accessing media sources during leisure time. We suppose that $\partial e / \partial l_j < 0$ and $\partial e / \partial \gamma < 0$. In labor markets with high aggregate wages and employment, l_j is, of course, smaller for the typical voter.[20]

Returning to the model, suppose that after voters receive information, they can take one of three actions: vote for the L candidate; vote for the R candidate; or abstain, denoted respectively by the binary indicators v_j^L, v_j^R, and v_j^a, with $v_j^L + v_j^R + v_j^a = 1$. A voter receives a utility flow of 1 if he votes for the candidate he believes himself closer to, and a utility flow of $-\Lambda$ if he votes for the other candidate. We assume that $\Lambda > 1$ to represent the fact that voters are risk averse and dislike uncertainty. Voters pay the small sum $0 < \kappa_P < 1$ when they cast a ballot for the candidate from party P. We allow these logistical costs of voting to differ across candidates to allow for the possibility that the parties might differ in the

[20]This assumption is true by definition for total employment. However, there is the theoretical possibility that higher wages could lead to a reduction in labor supply -- that is, higher *l*- because of the income effect. Our assumption that the substitution effect dominates is consistent with the evidence showing the positive correlation between aggregate wages and employment and is relatively innocuous for the purposes of the model.

assistance they provide to those likely to vote for them through such mechanisms as rides to polling stations or reminder phone calls. Voters who abstain receive no utility flow from expressing their opinions, but also incur no cost of voting.

Let $\lambda_j^L\left(s_j^L\right)=\Pr\left[C_j^L=1\,|\,s_j^L\right]$ be a voter's conditional belief that he is closer to the L candidate after receiving information, and let $\Omega(v)$ be the voter's net payoff from a given voting action. A voter's alternative net payoffs from voting for the L candidate, voting for the R candidate, and abstaining are, respectively:

$$
\begin{aligned}
\Omega\left(v_j^L\right)&=\lambda_j^L\left(s_j^L\right)-\Lambda\left[1-\lambda_j^L\left(s_j^L\right)\right]-\kappa_L\\
\Omega\left(v_j^R\right)&=\left[1-\lambda_j^L\left(s_j^L\right)\right]-\Lambda\lambda_j^L\left(s_j^L\right)-\kappa_R\\
\Omega\left(v_j^a\right)&=0
\end{aligned}
\tag{8}
$$

and the voter's optimal voting decision, v_j^*, is therefore

$$
\begin{aligned}
v_j^*&=v_j^L \ \text{ iff } \lambda_j^L\left(s_j^L\right)>\left(\kappa_L+\Lambda\right)/\left(1+\Lambda\right)\\
v_j^*&=v_j^R \ \text{ iff } \lambda_j^L\left(s_j^L\right)\le\left(1-\kappa_R+\Lambda\right)/\left(1+\Lambda\right)\\
v_j^*&=v_j^a \ \text{ iff } \left(1-\kappa_R+\Lambda\right)/\left(1+\Lambda\right)<\lambda_j^L\left(s_j^L\right)\le\left(\kappa_L+\Lambda\right)/\left(1+\Lambda\right)
\end{aligned}
\tag{9}
$$

The two quantities $T_L=\left(\kappa_L+\Lambda\right)/\left(1+\Lambda\right)$ and $T_R=\left(1-\kappa_R+\Lambda\right)/\left(1+\Lambda\right)$ in (9) both lie between 0 and 1, with $T_L>1/2>T_R$. Remembering that $1-\lambda_j^L\left(s_j^L\right)$ is a voter's conditional belief that he is closer to the R candidate after receiving a signal s_j^L, expression (9) says that people vote for a particular candidate only if their conditional belief that they are closer to that candidate is above a threshold specific to the candidate (T_L or T_R). Otherwise, the voter abstains. It follows immediately that, a widening of the distance between T_L and T_R increases the likelihood of abstention for every voter. Thus, an increase in the uncertainty cost Λ increases abstention, as does an increase in either of the logistical voting costs κ_L

or κ_R.[21] Notice that a general increase in logistical voting costs leads disproportionately to a reduction in voting for the candidate whose logistical costs κ_P rise by more.[22]

By Bayes' Rule, the conditional probability $\lambda_j^L\left(s_j^L\right)$ can be expressed in terms of e and the prior probability $\pi^L\left(\theta_j\right)$ - a monotonic function of θ_j. When the voter's information is $s_j^L=1$, ,

$$\begin{aligned}\lambda_j^L\left(s_j^L=1\right) &= \frac{\Pr\left(s_j^L=1 \mid C_j^L=1\right)\pi_j^L}{\Pr\left(s_j^L=1 \mid C_j^L=1\right)\pi_j^L+\Pr\left(s_j^L=1 \mid C_j^L=0\right)\left(1-\pi_j^L\right)} \\ &= \frac{(1-e)\pi_j^L}{(1-e)\pi_j^L+e\left(1-\pi_j^L\right)}\end{aligned} \tag{10}$$

and when $s_j^L=0$

$$\begin{aligned}\lambda_j^L\left(s_j^L=0\right) &= \frac{\Pr\left(s_j^L=0 \mid C_j^L=1\right)\pi_j^L}{\Pr\left(s_j^L=0 \mid C_j^L=1\right)\pi_j^L+\Pr\left(s_j^L=0 \mid C_j^L=0\right)\left(1-\pi_j^L\right)} \\ &= \frac{e\pi_j^L}{e\pi_j^L+(1-e)\left(1-\pi_j^L\right)}\end{aligned} \tag{11}$$

Differentiating the two expressions (10) and (11) shows that $\lambda_j^L\left(s_j^L=1\right)$ is strictly decreasing in e, from a maximum of 1 when $e=0$, to π_j^L when $e=1/2$; and $\lambda_j^L\left(s_j^L=0\right)$ is strictly rising in e, from a minimum of 0 when $e=0$, to a maximum of π_j^L when $e=1/2$.[23] Figure 3 depicts the conditional belief λ_j^L for the two different values of the information s_j^L, and at different levels of information quality, e. The two horizontal lines in the figure are the two voting thresholds T_L and T_R. When the conditional

[21] Our model therefore trivially reproduces the result from models on instrumental voting that increases in the cost of voting lead on increases in abstention. Notice, however, that those models do not stress the effect of uncertainty, which is our focus. Nor do those models posit, as we do here, that logistical voting costs might differ across candidates. The conventionally studied case where "voting costs" are the same, irrespective of the candidate for whom the agent votes, is a special case in our model.

[22] In general, if $\kappa_L \neq \kappa_R$ the voter who is just indifferent between abstaining and voting for the candidate with the higher logistical costs is more certain that he is closer to that candidate than the person who is just indifferent between abstaining and voting for the candidate with the lower logistical costs is sure about is closeness to that other candidate. In the special case where $\kappa_L=\kappa_R$, these voters have exactly the same level of confidence in their relative closeness to the candidate for whom they vote.

[23] The derivative of $\lambda\left(s_j^L=1\right)$ with respect to e is $\pi(\pi-1)\big/(e+\pi-2e\pi)^2<0$. The derivate of $\lambda\left(s_j^L=0\right)$ with respect to e is $\pi(1-\pi)\big/(e+\pi-2e\pi)^2>0$.

belief for a given signal lies in the shaded area between these thresholds, a voter abstains when he receives that particular bit of information.

To give some intuition for these expressions, consider the extreme case of perfect information when $e = 0$. In that case, $\lambda_j^L\left(s_j^L = 1\right) = 1 > T_L$ and $\lambda_j^L\left(s_j^L = 0\right) = 0 < T_R$, for every voter. That is, each voter is certain about whether he is closer to the L or to the R candidate and no person abstains. More realistically, information is imperfect. Consider the most extreme case, when information is of the worst possible quality, or $e = 1/2$. Expressions (10) and (11) imply that a voter's conditional belief that he is closer to the L candidate is π_j^L, whether he receives information $s_j^L = 0$ or $s_j^L = 1$. When information is of the lowest possible quality, a voter's conditional belief after getting information is simply what he would have believed had he received no information at all -- that is, his prior probability π_j^L. Whether the person votes or not in the case of worthless information, and for whom, depends on how π_j^L compares to T_L and T_R: if $T_R \le \pi_j^L \le T_L$ abstains, and he votes for the L candidate if it is above T_L and votes for the R candidate if it is smaller than T_R.

In general, the shape of expressions $\lambda_j^L\left(s_j^L = 1\right)$ and $\lambda_j^L\left(s_j^L = 0\right)$, plus the fact that every voter receives one or the other signal with positive probability, means that increases in e increase the likelihood of abstention, but do so differentially depending on the voter's partisanship, π_j^L.[24] For voters with $T_R \le \pi_j^L \le T_L$, increases in e unambiguously increase the likelihood of abstention; as e increases beyond the perfect information case, there is some level of e at which each of these persons switches from voting for the candidate recommended by his information to abstaining.

Persons for whom $\pi_j^L > T_L$, such as the voter in the figure, are strongly L partisan. These persons vote for the L candidate whenever they receive information $s_j^L = 1$, irrespective of the quality of information. The interesting issue for these voters is how they behave when they receive the signal $s_j^L = 0$ at different levels of e. When information is very good these persons vote for the R candidate when they receive

[24] For a voter with prior probability $\pi^L\left(\theta_j\right)$ the probability that the voter receives a signal of $s_j^L = 1$ is $\pi\left(1-e\right)+\left(1-\pi\right)e = \pi + e - 2e\pi.$

$s_j^L = 0$. However, as e increases $\lambda_j^L\left(s_j^L = 0\right)$ eventually rises above T_R, at which point the voter abstains when $s_j^L = 0$. For further increases in e, $\lambda_j^L\left(s_j^L = 0\right)$ eventually rises above even T_L for these voters. These very partisan voters eventually vote for the L candidate when getting information $s_j^L = 0$, provided that information quality is sufficiently low. This is in contrast to more moderate voters for whom $\lambda_j^L\left(s_j^L = 0\right)$ never rises so much that they are willing to vote for the L candidate when they get information $s_j^L = 0$. All the foregoing, is of course, symmetrically true for those strongly R partisan voters for whom $\pi_j^L < T_R$.[25]

If partisanship is Normally distributed as we have assumed, higher levels of e, such as that caused by greater labor force attachment and decreased political attentiveness, should lead to overall increases in abstention. As e increases some moderates to abstain for certain, while more strongly partisan voters either abstain more often *or* to switch from sometimes abstaining to voting for their party's candidate. But since, as the second panel of Figure 3 shows, the number of moderates thrust into abstention will tend to sharply exceed the number of extreme partisans who switch from occasionally abstaining to definitely voting for their party's candidate. Aggregate turnout should fall with better labor marker performance, as we find for Senate and gubernatorial elections.[26] Notice also that if, as casual observation suggests, the availability of information γ on media outlets is substantially larger for Presidential as compared to other types of elections, reduced political attentiveness from higher labor market activity leads to a smaller reduction in uncertainty and thus a smaller negative effect on abstention in Presidential contests, as we find above.

Our model also predicts that only the most strongly partisan persons should vote when information is especially bad, with distribution among titled towards supporters of the candidate with lower κ_P. That incumbents enjoy a electoral advantage is a well-established result in the literature, although the reasons for the advantage are not clear. One way of capturing that advantage in our model is to assume that the

[25] These persons vote for the R candidate whenever they receive information $s_j^L = 0$. When they receive the signal $s_j^L = 1$, they vote for the L candidate at very low levels of e; abstain at intermediate levels of e; and vote for the R candidate when information is of very low quality.

[26] In the empirical work in the previous section we stressed how instrumenting for local labor market conditions helps deal with the measurement error problems associated with the mis-measurement of local wages. Notice that another advantage of the TSLS approach in the context of our model is that those results isolate variation in employment prospect that derive from exogenous shifts in aggregate demand, thereby accounting for the fact that people of different levels of partisanship may be idiosyncratically inclined to collect political information.

logistical costs of voting for an incumbent are lower than for challenger. This would imply that the incumbent's vote share should rise as the labor market improves, as previous work finds. Our model can thus previous finding, without relying on the controversial assumption made in much of the previous economic voting literature about the vote share that voters credit politicians for outcomes like the equilibrium distribution of wages and employment that lie outside the politician's control.[27]

6. Micro Data Evidence on Political Attentiveness

In addition to theoretically reconciling the various aggregate level results, an attractive feature of the above simple model is that several of its assumptions and implications can be directly tested in micro-level data. In particular, if the mechanism outlined above accounts for the aggregate voting patterns we have documented then: (a) individual voters who are less informed should be less likely to vote; (b) any negative relationship between being informed and turnout should be steeper for moderates than for more strongly partisan voters, since the voting of moderates is predicted to be more sensitive to information; and (c) voters should be much better informed about the candidates in Presidential elections compared other elections. Most importantly, if the aggregate voting patterns we have documented arise from the mechanism we outline, then at the individual level we should observe that labor force attachment is associated with lower attentiveness to political matters, diminished use of media, and more uncertainty about political matters.

We use several years of individual level data from the 1948-2004 American National Election Studies (ANES) Cumulative Data File (Sapiro et al. 2004) to test for these implications. The ANES is primarily a repeated cross-sectional survey that has been carried out every other year since 1952 as well as in 1948. Occassionally, the ANES also includes a panel component which we will also use in our analysis below. Individuals are interviewed twice during presidential election years, once in the weeks prior to the election and once again in the weeks following the election. In non-presidential election years, individuals are only interviewed once following the election.

[27] Although commonly invoked in previous work on economic voting to account for the finding that the incumbent's vote share in pro-cyclical, this assumption has been controversial. Stigler (1973), perhaps its harshest critic, argued that it was inconsistent with rational behavior and questioned the basic finding that motivated it. Recent writers, such as Wolfers (2002), using different and better evidence, also find procyclical incumbent vote shares and suggest that voters may indeed (irrationally) credit politicians for the performance of the aggregate labor market. Whether voters credit incumbents for the performance of the labor market or not, or whether that belief is rational or not, our model suggests an alternative mechansim that can, in part, explain this finding.

In Presidential election years since 1968, the ANES includes a variable which reflects the ANES *interviewer's assessment* of the respondent's general level of information about politics and public affairs, based on the respondent's answers to various questions over the course of the survey interview. This measure is collected in both the pre-election and post-election interviews. In addition, the survey also reports an index constructed by the ANES which measures a respondent's exposure to campaign information from multiple media sources which is available in all Presidential elections years during the post-election interview from 1952 to 1996, except for 1988, and a handful of non-Presidential election years. This Media Exposure Index summarizes the number of four different types of media (t.v., radio, magazines, newspapers) from which the respondent reports having accessed information about the recently completed electoral campaign. The third bit of information is the respondent's statement about his level of interest in the current year's political campaigns. This measure is collected during the pre-election interview during Presidential election years since 1952 and during the post-election (only) interview in non-Presidential election years since 1958. All of these measures correlate roughly with the idea of "political attentiveness" or "political information".

We restrict our analysis to Presidential election years since two of the three political attentiveness measures are primarily collected in those years. In addition, we restrict our analysis of the interviewer assessment to the pre-election surveys although it is available in both the pre-election and post-election surveys. Since the labor supply measures that are available in the ANES are only collected in pre-election interview, we use the pre-election measure to eliminate discrepancies between the timing of the labor force measures and the timing of the attentiveness measure. However, since the media exposure index is only available in the post-election survey, our analysis of this measure may suffer slightly due to the differences in the timing of the labor force measure and the media exposure data.

In addition to these attentiveness measures, ANES respondents report their political partisanship in a series of two questions which are then translated to a seven point scale where "Strong Democrat"=1, "Independent"=4, and "Strong Republican"=7. In addition, respondents report whether they voted in the election preceding the post-election survey. The three graphs in Figure 4 show the share of respondents who voted in the election, by the respondent's self-reported partisanship and by their level of political information as measured by the three ANES measures. The top two lines in each figure are the average turnout rate in the election for informed and un-informed persons of the given partisanship type; the bottom line in each graph shows the difference in these two means, with 95% confidence interval bands. The figure shows that, consistent with the main assumption in our model, better informed persons of each partisanship type were more likely to vote by statistically significant amounts. The graphs also show that, as our model predicts should be the case, moderates are more sensitive to political information: the gaps in turnout between informed and un-informed moderates is statistically larger than the corresponding gap for voters at the extremes of the partisanship distribution.

A final piece of graphical evidence consistent with the model is found in Figure 5, where we plot the fraction of respondents who are knowledgeable of both candidates in an election based on whether they can rate each candidate on a "thermometer" scale which ranges from 0 to 100. We categorize respondents as not being "recall" a given candidate if when answering this question they either do not recognize the candidate's name or they state that they cannot judge the candidate. Valid numeric responses to this question are categorized as "recalling" the candidate.[28] This is the only available measure of information differences across elections, is admittedly quite coarse, and is not elicited for gubernatorial elections. It is nonetheless quite reassuring that, as we argued above, the levels of ignorance about candidates rise the "bigger" the election: not surprising everyone knows the names of the two candidates for the Presidency, but only 63% or so of voters know the names of the two candidates in a recent Senate election, and about only about a half know both persons who competed in a recent race for the House of Representatives.

While these different pieces of evidence are suggestive that there is to our uncertainty and attentiveness argument, none of them gets at the model's main micro-implication: the direct relationship between individual level labor supply and political information. The ANES provides information about respondents' employment status and state of residence, so we can directly relate the attentiveness and information measures described above to the individual's labor employment status, controlling for state×year effects.

Suppose that the political attentiveness of respondent i living in state s at time t, A_{ist}, is given by

$$A_{ist} = b_0 + b_1 x_{ist} + b_2 E_{ist} + \alpha_i + \pi_{st} + \omega_{ist}. \tag{12}$$

where E_{ist} measures whether the respondent is employed; x_{ist} is a vector of individual level observables; α_i is a fixed person effect; π_{st} is a vector of state×year fixed effects and ω_{ist} is an error term. We are interested in estimating the effect b_2. In a pooled crossed section model, there is the problem that particular types of people (represented by their α_i) who are more likely to be employed at a point in time

[28] Thermometer questions for candidates are beginning available in 1968 for Presidential elections. However, since these questions are only asked of Senate and House candidates beginning in 1978, we use non-Presidential years as well for Figure 5 in order to increase the sample size.

also are probably also more likely to be politically attentive. Fortunately, in various years including 1956-1960, 1972-1976, and 1992-1996, the ANES was a panel study, in that the same people were interviewed across successive presidential years. Using this panel sample, we can estimate individual fixed effects models which account for fixed latent factors α_i.

Table 7 present three sets of results for each of the three attentiveness measures. The results in column (1) show the effect of employment status on political attentiveness, information and interest from pooled cross section models. In column (2) we estimate the pooled OLS model but restricted the data to only include the ANES panel sample. Column (3) shows the results from the within model – estimated, of course, on the panel sample. We find that in the pooled cross-section, being employed is very strongly positively correlated with being thought by the interviewer to be a politically informed person, and with being rated highly on the Media Exposure index. There is no effect of employment on self-reported interest in politics. When we limit the much smaller set of people who constitute the panel sample, the pooled OLS regressions yield similar results for being thought to be highly informed about politics, but the estimated effect of employment status on the other two variables are not statistically different from zero. No causal interpretation should be attached to these estimates, since they are undoubtedly contaminated by the presence of unobserved person effects, α_i.

The within-estimator shown in column (3) relates *changes* in an individual's attentiveness across elections to *changes* in the person's employment status. These results are very different, and are consistent with prediction of the model above. Once fixed, person-specific effects are accounted for, we find that whether the person is informed about politics and their level of media exposure to political events are negatively related to their employment status. The evidence for the person's self-reported interest in politics moves in the same direction as the other two variables, but it is only marginally statistically significant. Note that the estimated effects for gaining employment on being informed and for having a high level of media exposure of -.09 and -.07 are both relatively large compared to the means of the two variables of 0.44 and 0.42.

We regard this micro-evidence as strongly supportive of the model presented above which rationalizes the aggregate county and SEA level results.

7. Conclusion and Discussion

Using county-level data and a variety of estimation methods, we find that improvements in local per capita earnings and per capita employment lead to reductions in voter turnout in Senate and gubernatorial elections. For Presidential elections, by contrast, labor market conditions have no effect on county level turnout in our data. Although the relationship between voting behavior and local labor market conditions has long been studied by political scientists and economists, virtually all of the previous literature in what is called the economic voting literature has focused on the incumbent vote share – how the fraction of votes cast received by the incumbent varies with changes in the labor market. Our focus on and results about the extensive margin of voting behavior thus represent an important extension of this literature. Most previous work has used state or national level data, so our use of county level data, and the ability we have as a consequence to control for state×year fixed effects, is another important way in which we extend previous work.

We present a model of expressive voting in which people vote only if they have some minimum level of information about candidates' positions. The extent of voter uncertainty in the model is assumed to depend the time that agents can devote to accessing information from media sources, and the information about politics on those outlets. The key insight is high wages and abundant employment opportunities lead to an increase in market work and to a reduction in political attentiveness. We show that with this very simple framework can reconcile all of our various county level results, and may also explain previous estimates by others that the incumbent's vote share is pro-cyclical. This latter finding has been accounted for by the argument that voters credit politicians for improvements in the macro-economy. Although we are agnostic about whether this argument explains voting behavior in part, this argument has been criticized in the literature on the grounds that it is inconsistent with rational behavior. Our model can explain that previous finding without appealing to the previous theoretical argument.

Our model yields a number of testable implications. Using several years of individual level data from the American National Election Study (ANES), we find strong evidence for most of these in the data – including, most importantly, the finding from individual panel data models that changes in labor supply are systematically negatively related with changes in knowledge about political affairs.

Our analysis suggests several directions for future work in this area. One important empirical challenge for the future will be the estimation of models of the effect of labor market outcomes on voting behavior that jointly study both the extensive (the decision to turnout) and intensive (whom to vote for, conditional on voting) dimensions. This issue is analogous to the problem in the labor supply literature of estimating the effect of wages on hours of work, accounting for the fact that wages affect selection into the labor market and many challenges confronted in that large literature are likely to attend the estimation of the joint models in the economic voting literature. Nonetheless, only from the specification and estimation of models of that form will it be possible to carefully assess the various dimensions along which labor market conditions affect voting behavior and why.

We believe that the various bits of evidence we have presented in this paper suggests strongly that one way that the performance of the labor market affects voting is by varying agents political attentiveness and thus their level of uncertainty about political affairs. We have proposed a particularly simple form of uncertainty:, in which agents do not know their relative closeness to candidates seeking office. It is, however, possible that the effect of uncertainty on voting is more nuanced. In particular, it might be the effect of uncertainty about the challenger has different effects on behavior than uncertainty about the incumbent. Indeed, although we do not show these results in the paper, we find in the ANES data that when most of voter uncertainty concerns uncertainty about the challenger rather than the incumbent for all election other than that for the President. Studying the effects of different types of uncertainty on various dimensions of voting behavior, phenomena like the incumbency advantage, is an interesting area for future work.

Finally, it is clear that from our results and the previous literature on economic voting that variation in local labor market conditions affect various aspects of voting behavior. But labor market performance measured at different levels of aggregation affects a voter's decision making through very different channels. In our paper, where we care about the local job and earnings opportunities affect voting behavior, it is important that we use data at a fine enough level of to allow us to control for state*year fixed effects. But for other questions in the economic voting area, such whether good labor market outcomes make people thinking better of the incumbent, it is not at all clear that very local labor markets are what voters refer to when forming these judgments. For that input into their voting decision they might care about how their state performs overall, how particular persons in their states fare, or even about the performance of *other* states in the case of national elections. Indeed, this last is precisely the argument made by advocates of the notion that voting is partly socio-tropic (see Kinder). An important task of future work in this area will be analyze what determinants of voting behavior are and are not affected by labor market variation at different levels of aggregation.

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Data Appendix

This Appendix discusses a number of issues surrounding the data used in our analysis.

Matching Across Data Sources

Matching the data from the various sources was facilitated by the use of county FIPS codes. Merging was based on the modified FIPS codes used by the REIS. These county fips codes are generally the same as the standard FIPS codes with the exception that many independent cities in Virginia are merged with neighboring counties to create new ``counties''. Since most data sources contain separate observations for these independent cities, these observations are first summarized according to the definitions of these new counties and then merged by the REIS FIPS codes. (Independent cities in Maryland, Missouri, and Nevada appear as separate observations in the REIS data and thus require no further adjustment.)

The voting data available from ICPSR uses the ICPSR county codes as opposed to the FIPS county codes. A bridge file between FIPS and ICPSR county codes is used to connect the ICPSR files with the remaining data sources.

Changes in FIPS codes during the sample period also needed to be addressed. Miami and Dade County merge in 1999 generating a new FIPS code (12086) which replaced the previous code (12025). LaPaz county, AZ (4012) is established in 1983 after splitting off from Yuma, AZ (4027).

Voting Data

The voting totals found in ``General Election Data for the United States, 1950-1990'' (ICPSR study no. 13) were adjusted for a variety of issues detailed here.

First, since the ICPSR data are available for even-numbered years only prior 1980, gubernatorial elections which occur in odd-numbered year are, for the most part, not found in the data. Kentucky, Louisiana, Mississippi, New Jersey, and Virginia have odd year governor elections. These data were obtained from the CQ Press Voting and Elections Collection. However, for some odd-year elections prior to 1972, the election results are incorrectly entered into an adjacent even year. These observations were moved to the appropriate years.

Second, while is nearly all cases Democratic or Republican candidates totals fall into these columns, in Minnesota these candidates typically are listed under a different party name. Democratic gubernatorial and senatorial candidates in Minnesota are typically found in the

Democrat Farmer Labor party category while their Republican counterparts are found in the Independent Republican category.

Third, in some instances all of the data for a single state for a given election was replaced with data from CQ either due to missing data or numerous suspicious values: 1972 Wisconsin Presidential election, 1978 Louisiana Senate election, 1980 Alabama Senate election, 1982 Florida Senate election, 1988 Minnesota Presidential election,

Finally, a small fraction of individual counties in each year have data that are arise most likely because of incorrect coding. In most instances, these errors appear to be due to a switch in values for observations which appear consecutively in the alphabetized county list. A list of these counties and elections which have individual election problems that were corrected are available from the authors.

Table 1. Demographic and Occupational Distribution of Workers in Oil and Coal States. Data from Multiple Years of Census IPUMS.

	Oil States						Coal States					
	Oil Workers			All Workers			Coal Workers			All Workers		
	1970	**1980**	**1990**	**1970**	**1980**	**1990**	**1970**	**1980**	**1990**	**1970**	**1980**	**1990**
Age												
Under 25	16.2	25.2	8.1	25.7	26.4	17.8	10.3	16.6	4.9	24	24.7	18.3
25-34	22.9	32.7	32.5	20.3	27.7	27.5	18.3	36.7	24.4	18.8	25.8	25.9
35-44	24	17	30	18.7	17.8	24.7	22	20.4	38.8	18.7	17.6	24.3
45-54	22.2	13.6	15.9	17.4	14.1	15.5	29.1	14.7	19.9	19.7	15.8	16.2
55-64	11.5	9.2	10	12.6	10	9.8	18	10.4	9.8	13.9	12.5	10.9
65+	3.2	2.3	3.6	5.3	4	4.8	2.3	1.3	2.3	4.9	3.7	4.5
Male	86.8	82.8	81.9	61.4	57.4	54.4	97.3	95.6	95.1	61.4	57.9	54.1
Education												
8 or less yrs	17.3	9	5.6	20.4	9.8	6.1	48.1	20.6	10	18.6	8.4	3.8
9-11 yrs	18.8	15.2	10.1	22.3	17.3	11.4	22.2	20.3	17.1	22.7	17.4	11.7
12 yrs	30	35.2	33.9	30.7	35.5	31.8	24.4	45.3	52.4	37.6	43.8	41.7
Some Coll	15.5	19.4	26	14.9	20.3	30.6	4	9.1	15.3	11	15.7	25
Coll Grad.	18.5	21.2	24.5	11.7	17.1	20.1	1.2	4.6	5.1	10.1	14.7	17.9
Occupation												
Managerial/Professional	22.3	21.1	26.2	19	20.1	23.4	3.3	5.2	6.3	17.1	19	21.9
Tech., Sales, Admin.	19.6	22.2	19.8	25.8	29.8	31	3.9	10	6.4	26.4	28.2	30
Service	1.5	1.4	1.4	14.3	13.2	14.4	1	1.5	2.2	12.7	13.6	13.9
Farming, Forestry, Fishing	0.1	0.1	0.1	6.3	3.9	4.2	0.1	0.1	0.1	2.8	2.3	2.5
Prec. Product., Craft, Repairers	47.8	38.3	34.1	12.5	13.9	11.7	68.5	55.5	55.7	13.1	13.4	11.9
Operatives, Laborers	8.8	17	18.4	18.8	17.1	15	23.2	27.7	29.3	27.1	23	19.7

Table 2. Means of Economic Measures, Voter Turnout and Incumbent Vote Share for All Counties and For Counties in Coal and Oil States. By Time Period and Across Presidential and Non-Presidential Years. (Stan. Dev. In Parentheses)

	All Counties	Counties in Oil and Coal States	
	Years: 1972-2000	Years: 1972-2000	Years: 1972-1990
A: Voter Turnout			
Gubernatorial			
Mean	.43	.41	.43
Std. Dev.	.11	.11	.11
# elections	22375	8377	5581
Senatorial			
Mean	.47	.46	.46
Std. Dev.	.11	.11	.11
# elections	30460	11702	8052
Presidential			
Mean	.55	.55	.56
Std. Dev.	.09	.08	.08
# elections	24513	9343	5837
B: Economic Measures			
log per capita earnings			
Mean	9.66	9.59	9.54
Std. Dev.	.44	.43	.41
Employment per adult			
Mean	.76	.75	.73
Std. Dev.	.22	.18	.17
# County/Years	47942	18624	12388

Data drawn from multiple sources on aggregate voting data. See text for details.
"Oil" are those in which 1% of 1974 from oil and gas workers.
"Coal" States are four states in the "coal seam": WV, KY, PA, OH.

Table 3. OLS Estimates of Effect of Local Economic Performance on Voter Participation: County-Level Regresssions for all Counties in U.S. for 1969-2000 Elections. Pooled Cross-Section and Difference (Changed Since Last Election) Models.

	Governor			Senate			President		
	Pooled OLS	Difference Models		Pooled OLS	Difference Models		Pooled OLS	Difference Models	
	(a)	(b)	(c)	(a)	(b)	(c)	(a)	(b)	(c)
Local Labor Market Measure									
(1) Log per capita Earnings	.031	-.031	-.014	.038	-.017	.001	.043	.004	.001
(county measure)	(.006)	(.012)	(.005)	(.006)	(.013)	(.005)	(.007)	(.008)	(.004)
Log per capita Earnings									
(state measure)									
(2) Employment per Adult	.030	-.062	-.004	.039	-.055	.003	.045	-.007	.017
(county measure)	(.007)	(.038)	(.010)	(.007)	(.033)	(.010)	(.007)	(.020)	(.008)
Employment per Adult									
(state measure)									
Year Effects	Yes	Yes	No	Yes	Yes	No	Yes	Yes	No
State Effects	Yes	No	No	Yes	No	No	Yes	No	No
State*Year Fixed Effects	No	No	Yes	No	No	Yes	No	No	Yes
Observations	22375	19878	19878	30460	23657	23657	24513	21427	21427

Each point estimate in table represents results from a different regression. Standard Errors Account for Arbitrary Clustering within States.
R-squared statistics for 9 regressions in row (1) are, in turn, 0.70, 0.29, 0.75, 0.70, 0.84, 0.94, 0.63, 0.57, and 0.79.
R-squared statistics for 9 regressions in row (2) are, in turn, 0.70, 0.29, 0.75, 0.70, 0.84, 0.94, 0.61, 0.57, and 0.79.
All regressions control for total Population; % Population Female; % Population: Black, Race "Other"; % Population aged 30s, 40s, 50s, 60s, 70+

Table 4. OLS Estimates of Effect Oil and Coal Price Shocks on *Change* Since last Election in County Labor Market Outcomes and Change in County Voter Participation for 1969-1990 Elections.

	A. Δ County per Capita Annual Earnings			B. Δ County Employment per Adult			C. Δ Voter Turnout		
	Gov.	Senate	Pres.	Gov.	Senate	Pres.	Gov.	Senate	Pres.
Oil/Coal Price Shock:									
(Δ Coal Price) X I(Medium_Coal_1974)	.098	.149	.127	.016	.026	.036	-.021	-.010	.016
	(.025)	(.029)	(.021)	(.006)	(.005)	(.005)	(.013)	(.012)	(.006)
(Δ Coal Price) X I(Large_Coal_1974)	.214	.225	.237	.044	.030	.062	-.045	-.001	.011
	(.058)	(.064)	(.044)	(.008)	(.014)	(.007)	(.024)	(.014)	(.017)
(Δ Oil Price) X I(Medium_Oil_1974)	.057	.032	.049	.018	.007	.013	-.002	-.006	-.003
	(.011)	(.007)	(.021)	(.005)	(.005)	(.012)	(.004)	(.007)	(.002)
(Δ Oil Price) X I(Large_Oil_1974)	.167	.110	.128	.063	.034	.047	-.030	-.014	.002
	(.018)	(.021)	(.036)	(.011)	(.011)	(.022)	(.003)	(.004)	(.003)
State*Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat on Oil/Coal Shock	25.3	22.3	12.6	16.6	8.3	26.6			
Observations	4731	5657	4666	4733	5659	4666	4731	5657	4666

Robust standard Errors Account for Arbitrary Clustering within States.
Counties are "medium" if share of employment in oil/coal at least 5% but less than 20%; "large" if share > 20%
All regressions control for **Change Since Last Election** in: Total Population; % Population Female; % Population: Black, Race "Other"; % Population Aged 30s, 40s, 50s, 60s, 70+

Table 5. OLS and TLS Estimates of Effect of *Change* in County-Level Labor Market Outcomes on *Change* in Voter Turnout. Gubernatorial, Senate and Presidential Elections from 1969-1990 in "Oil" and "Coal" States.

	Governor		**Senator**		**President**	
	OLS	TSLS	OLS	TSLS	OLS	TSLS
(1) Log per capita Earnings	-.029	-.168	-.018	-.068	-.002	.034
	(.008)	(.032)	(.006)	(.040)	(.003)	(.038)
(2) Employment per Adult	-.057	-.500	-.036	-.386	.004	.108
	(.019)	(.130)	(.016)	(.194)	(.019)	(.114)
State*Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Instruments for TSLS Models: Price of Oil/Coal * I(County "medium"/"large")						
Observations	4731	4731	5657	5657	4666	4666

Each point estimate in table represents results from a different regression.
Robust standard Errors Account for Arbitrary Clustering within States.
Counties are "medium" if share of employment in oil/coal at least 5% but less than 20%; "large" if share > 20%
All regressions control for **Change Since Last Election** in: Total Population; % Population Female; % Population: Black, Race "Other"; % Population Aged 30s, 40s, 50s, 60s, 70+

Table 6. OLS Estimates of Effect of State Economic Area (SEA)-Level Economic Performance on Voter Participation: SEA-Level Regresssions for all SEAs in U.S. for 1969-2000 Elections. Difference (Change Since Last Election) Models.

	Governor		Senator		President	
(1) Log per capita Earnings	-.058	-.035	-.029	.002	.014	.004
	(.028)	(.012)	(.028)	(.013)	(.019)	(.010)
(2) Employment per Adult	-.162	-.047	-.141	-.014	-.055	.012
	(.082)	(.024)	(.062)	(.029)	(.033)	(.017)
Year Effects	Yes	No	Yes	No	Yes	No
State*Year Fixed Effects	No	Yes	No	Yes	No	Yes
Observations	3047	3047	3677	3677	3251	3251

Each point estimate represents results from a different regression.

Robust standard Errors Account for Arbitrary Clustering within States.

R-Squared for all OLS regressions > .6

SEA defined as large if share of employment in oil and gas or in coal greater than 10; "small" if share less than 3%.

All regressions control for **Change Since Last Election** in: Total Population; % Population Female; % Population: Black, Race "Other"; % Population Aged 30s, 40s, 50s, 60s, 70+

Table 7. OLS and Within Individual-Level Estimates of Effect of Being Employed on Alternative Measures of Political Information, Media Exposure and Political Interest in the American National Election Study.

	(1)	(2)	(3)
	Pooled OLS		**Within Estimator**
	Full Sample	Panel Sample	Panel Sample
A:	**Interviewer reports R's general level of information about politics/public affairs seemed "very high" or "fairly high"?**		
	(Mean of Dep. Variable: 0.38 full sample; 0.44 panel sample)		
R. Employed?	.069	.050	-.091
	(.008)	(.026)	(.038)
# Observations	19227	2492	2492
# Unique R. Numbers			1246
B:	**ANES Media Exposure Index "high": R exposed to political campaign on 3 or more of tv, radio, magazine or newspaper?**		
	(Mean of Dep. Variable: 0.38 full sample; 0.42 panel sample)		
R. Employed?	.028	.017	-.072
	(.008)	(.019)	(.032)
# Observations	21783	3916	3916
# Unique R. Numbers			1958
C:	**R "very much" interested in policial campaigns this year?**		
	(Mean of Dep. Variable: 0.33 full sample; 0.37 panel sample)		
R. Employed?	-.002	-.014	-.043
	(.007)	(.019)	(.031)
# Observations	25468	3888	3888
# Unique R. Numbers			1944

All Regressions Include Constant Term, State*Year Fixed Effects, Age, Age-Squared. OLS Regressions also include dummy variables for whether Respondent Male.
All Regressions weighted using ANES sample weights.
Data from Multiple Years of American National Elections Study. See text for details.
R-Squared Statistics for all regressions in table all approximately 0.1 .

Appendix Table 1 -Top Twenty Five Mining States By 1974 CBP Employment Shares

	Percent of 1974 CBP Employment from Mining	1974 CBP Share of mining establishments found in:		1967 CMI Share of mining establishments found in:	
		Oil and Gas	Coal	Oil and Gas	Coal
Wyoming	**15.8%**	**77%**	**5%**	**81%**	**4%**
West Virginia	*13.3%*	*40%*	*53%*	*40%*	*57%*
New Mexico	**7.8%**	**82%**	**1%**	**82%**	**2%**
Louisiana	**5.9%**	**88%**	**0%**	**92%**	**0%**
Montana	**4.3%**	**66%**	**5%**	**66%**	**5%**
Kentucky	*4.3%*	*16%*	*74%*	*28%*	*60%*
Arizona	4.1%	21%	2%	22%	1%
Alaska	3.9%	70%	3%	59%	4%
Oklahoma	**3.9%**	**90%**	**1%**	**94%**	**1%**
Utah	**3.7%**	**47%**	**5%**	**36%**	**10%**
Texas	**3.2%**	**88%**	**0%**	**94%**	**0%**
Colorado	**2.2%**	**58%**	**6%**	**50%**	**11%**
Nevada	2.1%	14%	N/A	11%	0%
Kansas	**1.8%**	**85%**	**1%**	**86%**	**1%**
South Dakota	1.7%	21%	N/A	22%	4%
Idaho	1.6%	10%	N/A	1%	0%
Virginia	1.4%	4%	67%	1%	78%
Pennsylvania	*1.2%*	*25%*	*50%*	*26%*	*56%*
North Dakota	**1.2%**	**65%**	**15%**	**63%**	**19%**
Mississippi	**1.1%**	**81%**	**N/A**	**81%**	**0%**
Minnesota	1.1%	9%	1%	3%	0%
Alabama	1.0%	23%	34%	15%	44%
Arkansas	0.8%	66%	3%	67%	4%
Tennessee	0.7%	9%	41%	5%	39%
Ohio	*0.7%*	*39%*	*20%*	*40%*	*22%*

Notes: States in **bold** are "Oil States" in our analysis; States in *italics* are "Coal States"

Appendix Table 2. TSLS Estimates of *Change* in County Labor Market Outcomes on *Change* in Voter Turnout under Alternative Specifications of Oil and Coal Shock Instruments. (Robust Standard Error in Parentheses).

	Governor		Senate		President	
	Endogenous Regressor		*Endogenous Regressor*		*Endogenous Regressor*	
Instrument Specification:	Δ County per Capita Annual Earnings	Δ County Employment per Adult	Δ County per Capita Annual Earnings	Δ County Employment per Adult	Δ County per Capita Annual Earnings	Δ County Employment per Adult
1. (Δ Oil/Coal Price) X	-.155	-.364	-.043	-.211	.048	.133
I("medium", "large" Oil/Coal 19**67**)	(.035)	(.115)	(.032)	(.098)	(.033)	(.114)
F-Stat on Excluded Instuments	16.5	18.4	17.3	7.3	10.4	20.4
3. (Δ National Coal/Oil Employment) X	-.133	-.589	-.064	-.303	.021	.053
I("medium", "large" Oil/Coal 1974)	(.047)	(.183)	(.022)	(.136)	(.024)	(.090)
F-Stat on Excluded Instuments	14.0	4.5	11.7	7.7	10.0	10.2
3. (Δ National Coal/Oil Employment) X	-.061	-.188	-.047	-.143	.033	.085
I("medium", "large" Oil/Coal 1967)	(.026)	(.077)	(.022)	(.120)	(.026)	(.118)
F-Stat on Excluded Instuments	16.5	8.8	16.9	29.4	5.5	14.2
State*Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4731	4733	5657	5659	4666	4668

Each point estimate in table represents results from a different reg

Counties are "medium" if share of employment in oil/coal at least 5% but less than 20%; "large" if share > 20%

All regressions control for **Change Since Last Election** in: Total Population; % Population Female; % Population: Black, Race "Other"; % Population Aged 30s, 40s, 50s, 60s, 70+

Figure 1a - 1974 County Oil and Gas Employment Share in Oil States

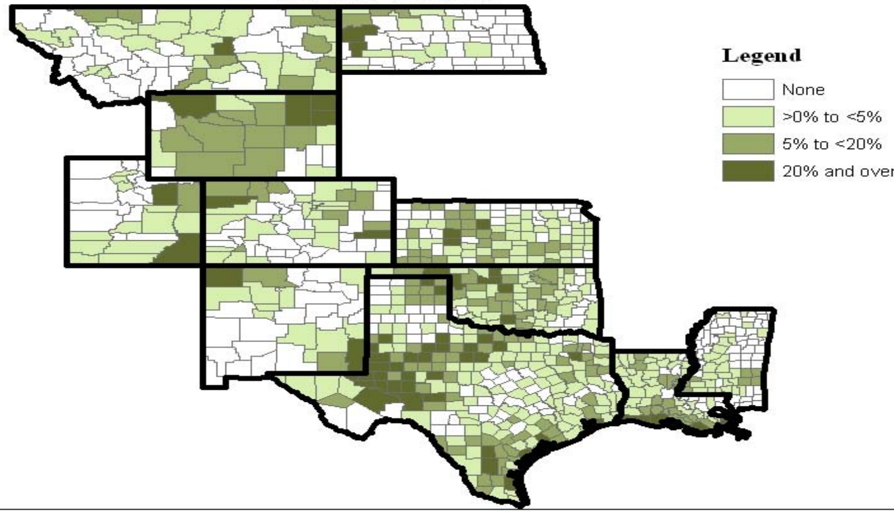
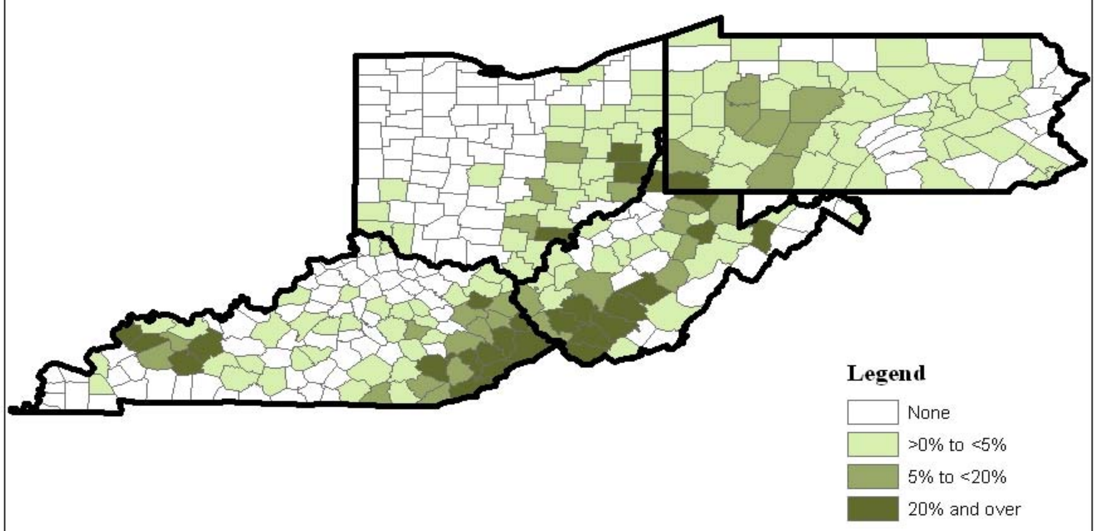


Figure 1b - 1974 County Coal Employment Share in Coal States



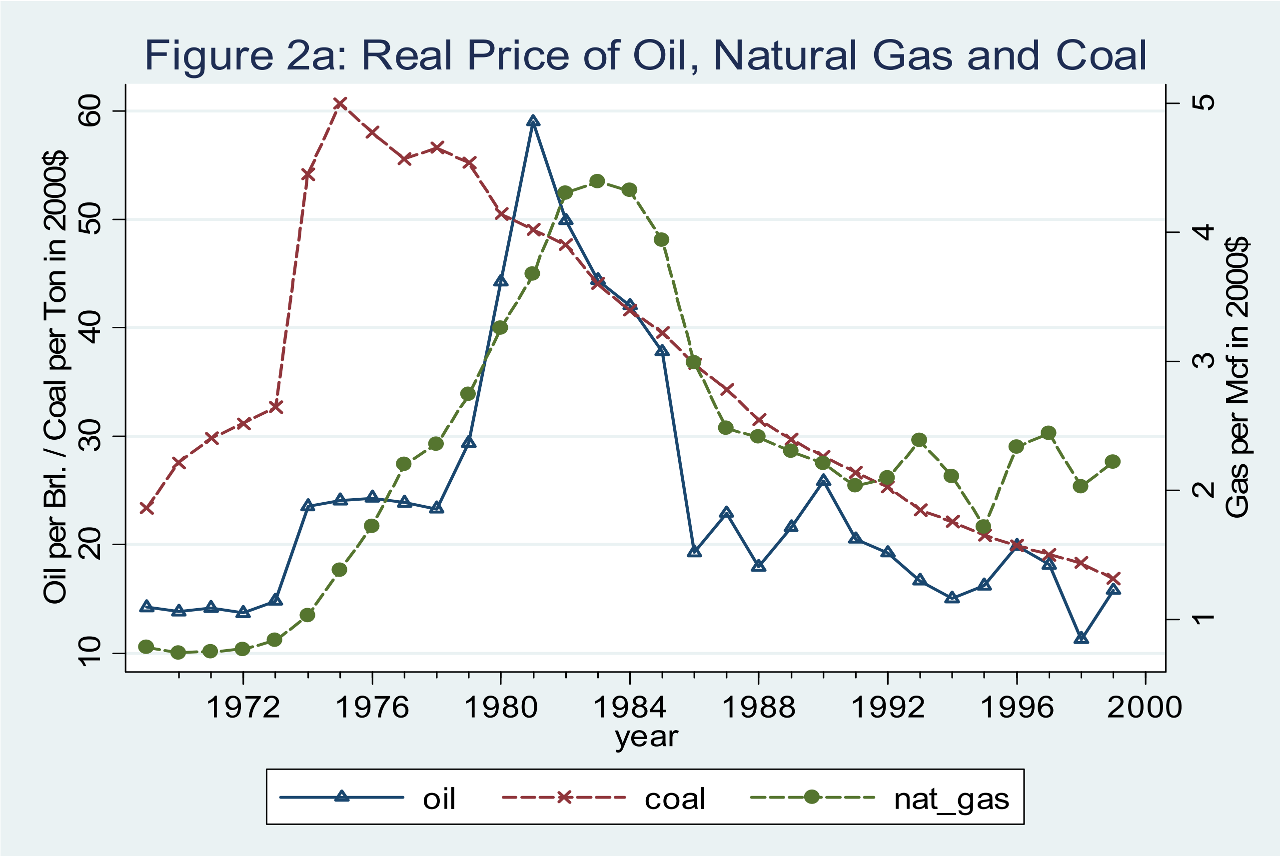


Figure 2a: Real Price of Oil, Natural Gas and Coal
Oil per Brl. / Coal per Ton in 2000$
Gas per Mcf in 2000$
year
oil
coal
nat_gas

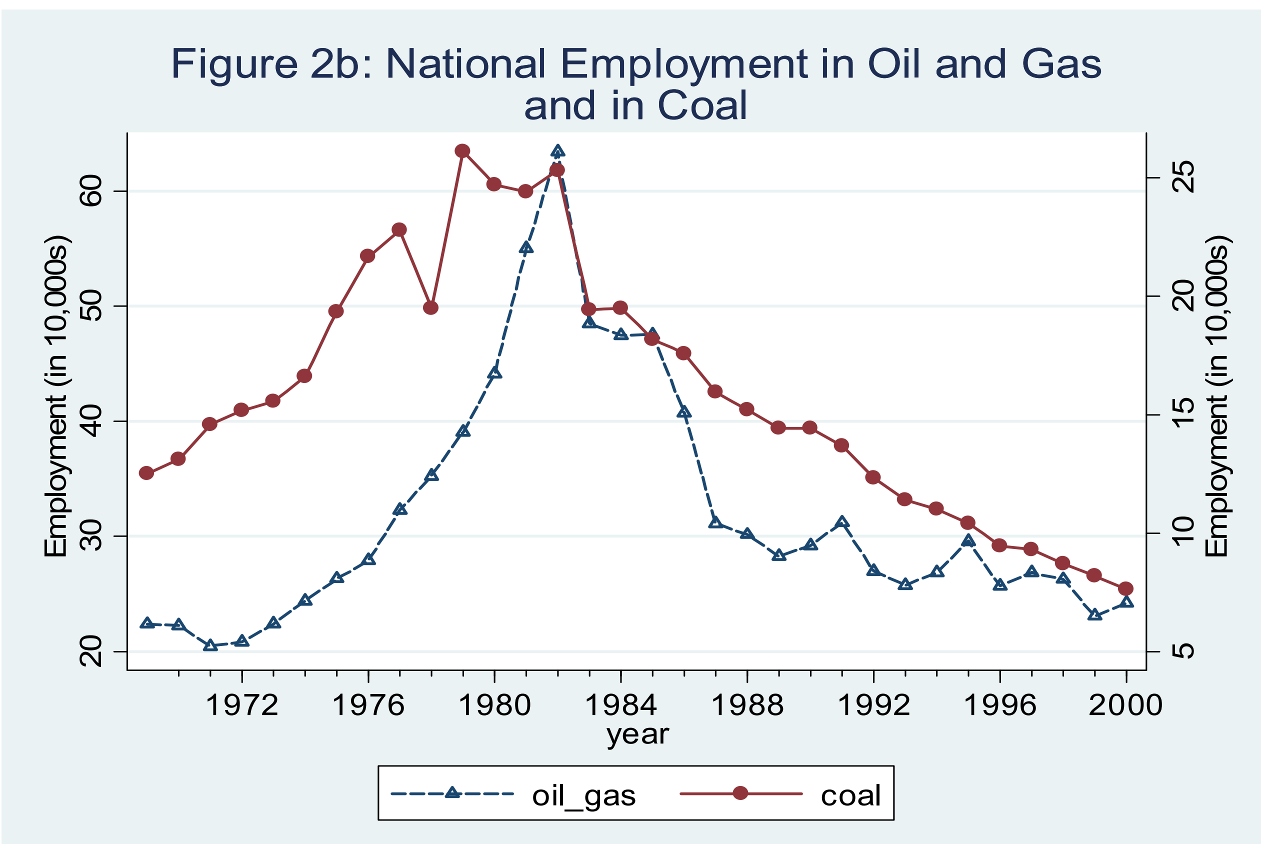


Figure 2b: National Employment in Oil and Gas and in Coal
Employment (in 10,000s)
Employment (in 10,000s)
year
oil_gas
coal

Figure 3. Voter's Conditional Beliefs about Relative Distance From Two Candidates, and Voting Thresholds.

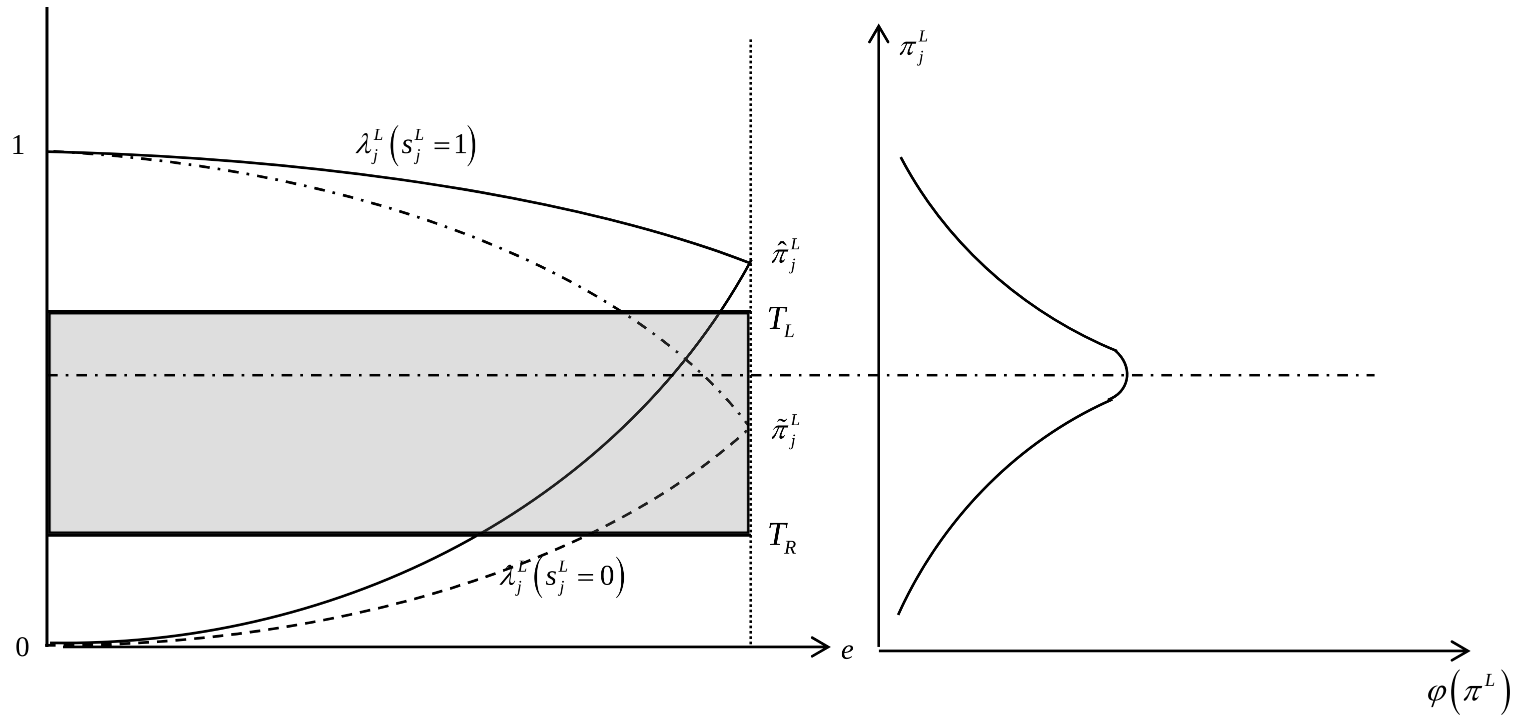


Figure 4. Probability of Voting by Respondent Partisanship and Level Of Political Information for Three Alternative Measures of Information.

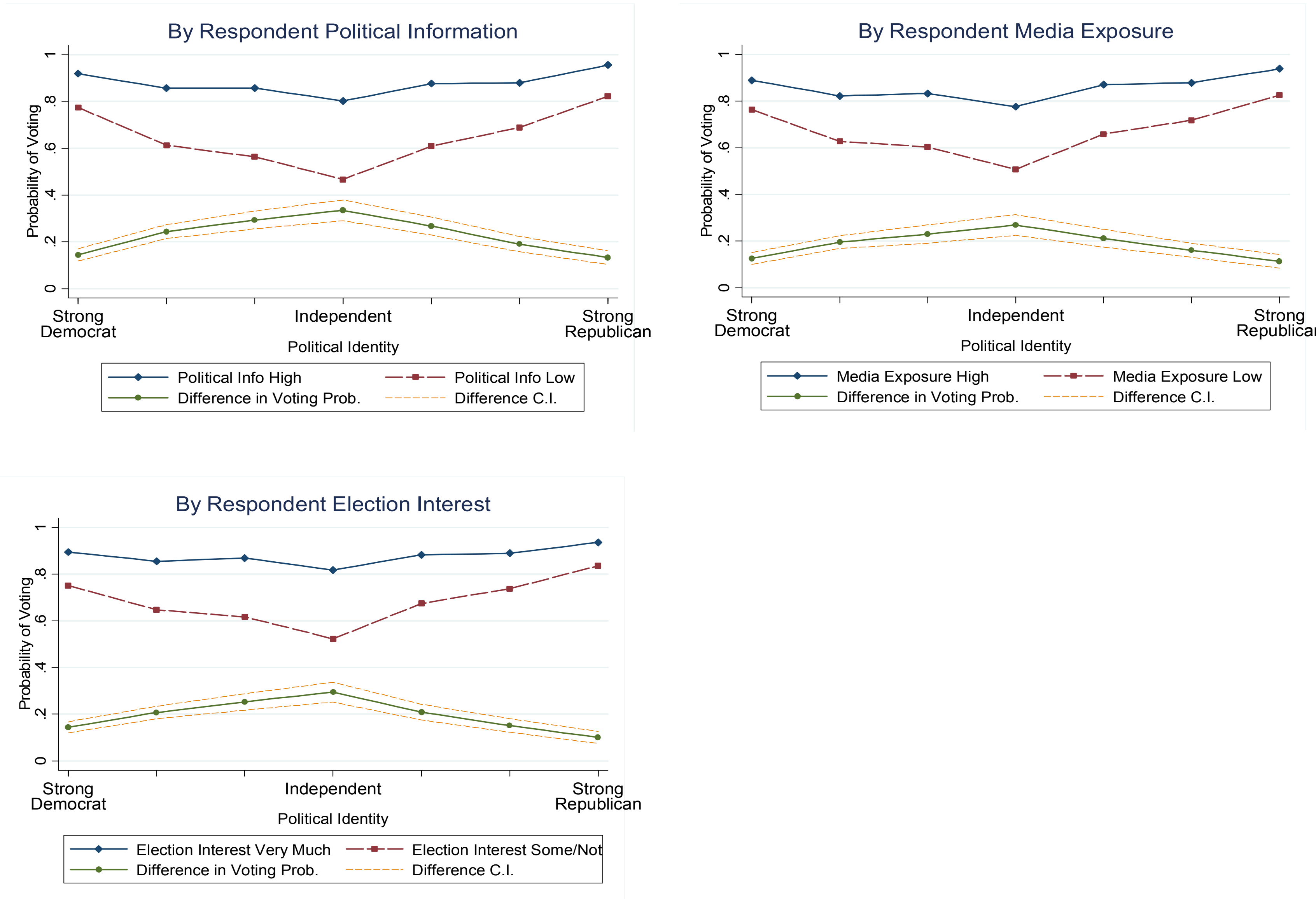


Figure 5. Share of ANES Respondents Who Recall Names of Both Candidates in Election, by Partisanship, for Three Types of Elections.