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ABSTRACT

This paper argues that, since activities that provide political information are complementary with leisure, increased labor market activity should lower turnout, but should do so least in prominent elections where information is ubiquitous. Using official county-level voting data and a variety of OLS and TSLS models, we find that increases in wages and employment: reduce voter turnout in gubernatorial elections by a significant amount; have no effect on Presidential turnout; and raise the share of persons voting in a Presidential election who do not vote on a House of Representative election on the same ballot. We argue that this pattern (which contradicts some previous findings in the literature) can be fully accounted for by an information argument, and is either inconsistent with or not fully explicable by arguments based on citizens' psychological motivations to vote in good or bad times; changes in logistical voting costs; or transitory migration. Using individual-level panel data methods and multiple years' data from the American National Election Study (ANES) we confirm that increases in employment lead to less use of the media and reduced political knowledge, and present associational individual evidence that corroborates our main argument.

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1. Introduction

Given the importance of voting as an input into policy-making and as the source of the legitimacy of democratic government, understanding variation in the decision to vote has been the subject of substantial research in economics and political science.[1] Recent theoretical models of voting emphasize the effect of information limitations on turnout, arguing that poorly informed voters may abstain for *strategic* reasons (Feddersen and Pesendorfer 1996) or because of disutility associated with *uncertainty* (Matsusaka 1995), *regret* (Merlo 2006; Degan and Merlo Forthcoming), or aversion to *ambiguity* (Ghirardato and Katz 2002; Ashworth 2011). The possibility that poorly informed people are more likely to abstain has been adduced as a possible explanation for well-known voting regularities, including the tendency of those with lower levels of education to vote less, and for "roll-off", which is voting on only a subset of elections on the same ballot (Bullock and Dunn 1996; Degan and Merlo, Forthcoming).

Empirical analysis linking turnout to information has been an active area of recent research. Various observational studies find that citizens with more political information are more likely to vote (Palfrey and Poole 1987; Wattenberg et al. 2000; Coupe and Noury 2004). Laboratory studies in which research subjects are assigned information, have found similar evidence (Battaglini, Morton, and Palfey 2010). The importance of the media as citizens' principal source of political information in the real world is demonstrated by Snyder and Stromberg (2010), who show that voters living in areas with less media coverage know less about their representatives, who in turn are exert less effort on their constituents' behalf. Several recent analyses, studying how turnout is affected by the media and information it provides, show that voting rises with exogenous expansions and declines with exogenous contractions in the media (Gentzkow 2005; Lassen 2005; Dellavigna and Kaplan 2007; Gentzkow, Shapiro, and Sinkinson forthcoming; and Stromberg 2004).

This paper examines a potential connection between information and voting that has received little previous attention. We study how labor market activity affects turnout, and assess the extent to which this relationship is attributable to information. Our argument applies familiar results from labor supply theory and from work on information and voting, and yields a number of testable predictions. When voters devote more of their time to activities related to market

[1] Blais (2000) provides a comprehensive review of variation in turnout across countries, within countries across election types and population sub-groups, and over time for the same election.

work they necessarily spend less time on leisure. Since the mechanisms by which voters are exposed to political information, such as discussions with friends and family or using the media, are complementary with leisure, increased labor market activity should leave voters less informed and thus less likely to vote, if information accounts of voting are correct.[2] This negative relationship between work activity and voting should be smaller when reduced exposure to information sources has only a scant effect on people's political information, as should be true for elections covered so extensively that knowledge about them is ubiquitous and nearly unavoidable. An implication of this argument is that roll-off in voting between "bigger" and "smaller" elections (as suggested by the amount of information generally available about them) should increase as voters' work-related activity increases.

The first part of this paper tests these three predictions about turnout. Our analysis presumes that voters consume less leisure, and are correspondingly less exposed to political information, when they have abundant opportunities in the labor market – that is, when wages and employment in their local labor market are high. We treat the relatively small spatial area of a *county* as a labor market, and relate county wage and employment information to county-level turnout data that we have assembled. Much of our analysis focuses on state gubernatorial and presidential elections, which are both chief executive elections for which voters in all counties in a state can participate, but which likely receive vastly different amounts of media attention. Using OLS difference estimators that account for latent county-specific effects and unobserved state-specific factors that vary across election years, we find that increases in mean county per capita earnings and mean per capita employment are associated with *lower* gubernatorial turnout, but are *not* related to turnout in presidential contests in a statistically significant way.

To address potential endogeneity bias arising from compositional differences across counties (due, for example, to better informed voters locating in faster growing counties) and problems arising from possible mis-measurement of the county labor market variables, we also estimate Two Stage Least Squares (TSLS) models. In these models, we instrument for changes

[2] We do not argue that people receive *no* political information at work. Instead, we implicitly assume that information received through media exposure or other leisure-intensive activities is superior to whatever is gleaned from discussions with a relatively small group of co-workers. Evidence that leisure time is spent on activities likely to provide information is found in recent work by Krueger and Mueller (2008), who show that consumption of a particular form of media (television viewing) constitutes fully one third of the "awake" time of the unemployed in the United States, and that compared to the employed, the unemployed spend nearly twice as much time viewing TV during the weekday (201 minutes vs. 106 minutes per day) and about 30% more on weekends (209 minutes vs. 162 minutes).

in the two labor market measures using exogenous shocks to world oil supply, interacted with the prior importance of oil, natural gas, or coal production in a county. Shocks to international oil supplies, arising from exogenous geo-political events, induced sharp changes in domestic production of oil, natural gas, and coal. Differences in endowment across counties ensured that international oil shocks affected labor market activity differently across counties within the same state. Consistent with our expectation that any bias in the OLS estimates is towards zero, the TSLS results for the gubernatorial elections are of the same sign but are larger in absolute value than their OLS counterparts. The effects are economically significant, with the preferred TSLS estimates implying that a 10 percent increase (roughly a standard deviation) in county labor market activity between elections lowers voter turnout by between 3 to 4 percent. For presidential election results, by contrast, we find that both the OLS and TSLS estimates are statistically insignificant and close to zero in magnitude. This pattern of results is robust to a number of specification checks, including alternative definitions of the instruments and different metrics for defining labor markets.

The third prediction about turnout we test is whether roll-off varies positively with labor market activity, a prediction we have not seen previously made or empirically tested. To conduct this analysis, we need data on two types of elections that typically occur on the same ballot. Most state gubernatorial elections occur in the U.S. in non-presidential election years so there are fewer than nine states in which these two types of elections are on the same ballot. For this part of our analysis, we therefore use data on presidential and House of Representatives elections, which in every state occur at the same time. Using the county-level data we have assembled, and accounting for the econometric concerns mentioned above, we find that roll off between these two types of elections is indeed larger when wages and employment are high. This result, combined with the evidence about a negative effect of labor market activity on gubernatorial elections but no effect for the much more intensely covered presidential elections, strongly suggests that labor market activity affects turnout in a manner predicted by information-based accounts of voting.

What other explanations might account for the finding that labor market activity negatively affects turnout in some elections but not others? Transitory cross-state migration is one possibility. People moving temporarily into a new state might abstain from the state's gubernatorial election because their future non-residence in that state assures that the outcome of

the state's gubernatorial contest has no bearing on their future well being. At the same time, given the national purview of the office, voters might regard the outcome of a presidential election as important wherever they live and so might vote in those contests even when living in a state to which they have only temporarily moved. If migrants move towards areas with relatively better labor market opportunities, this type of voting behavior among them might generate the different gubernatorial and presidential effects we estimate.[3] This possibility is particularly important for our preferred TSLS analyses, where the large changes in labor market opportunities across counties associated with energy price shocks might have been especially likely to induce substantial migration. We use five-year migration patterns from Census county-level summary file data to directly study cross-state migration. We show that the migration associated with the energy shocks over the years studied cannot explain the difference between gubernatorial and presidential results. This is true even if we assume that *all* out of state migrants voted in one type of election but not the other.[4]

Another possible explanation for the finding that increased labor market activity lowers turnout for gubernatorial elections comes from Downs' (1957) classic calculus of voting theoretical framework. In that framework, citizens vote only if their logistical costs are exceeded by the utility payoff they receive from being the pivotal voter who changes the outcome of an election, plus any psychic payoff they get from participating in the democratic process.[5] Since the time cost of standing in line to vote or driving to the polls are higher when the opportunity cost of time is high, turnout might vary negatively with labor market activity because of logistical cost reasons.[6] But a logistical costs argument would suggest that there should be negative effects of labor market activity on turnout in *all* elections, including those for the President – something we consistently fail to find. Even more persuasive, perhaps, is our finding

[3] Notice that migration that permanent, within-state migration, if turnout is governed by a mechanism like the one described, cannot account for our findings. If migration is permanent, then the outcome of the gubernatorial election in the receiving states should matter to voters, which suggests they should turnout out to vote by the logic of this argument. For migration that occurs across counties within a state, voters presumably care as much about the outcome of the election for state-wide office of governor.

[4] Our analysis is limited to the population of U.S citizens, so international immigration likely has little effect on our estimated results.

[5] The instrumentalist concern noted by Downs has long been known to poorly explain observed voting behavior in elections with large number of voter, as each voter's probability of being pivotal in such cases is vanishingly small (see Palfrey and Rosenthal 1985). Recent theoretical work on turnout attempts to explain the presence of both substantial turnout and substantial abstention in the typical election with many voters.

[6] Some logistical costs, such as variation in weather, the costs of voter registration and the time of poll hours have been shown to affect turnout (See Woldfinger and Rosenstone 1980).

that the share of people who vote on only a subset of elections on the very same ballot gets larger in times when labor market opportunities are abundant. Since people who leave a portion of their ballot blank have already incurred the logistical costs necessary to vote, larger roll-off when citizens are more engaged in work related activity *could not* be due to logistical costs, raising confidence in an explanation based on information exposure.

The paper next studies the effect of labor supply on media exposure, political information, and voting using individual-level data from the American National Election Study (ANES). Analagous to the county-level analysis, it is necessary to account for the fact that individuals who have high labor supply also have high political participation or information. Therefore, we exploit the rarely-used panel feature of the ANES to estimate individual fixed effects models and find that individual-level *changes* in employment are negatively related to *changes* in voters' political information, precisely as an argument of information exposure would predict. Various limitations of the self-reported ANES voting data prevent us from testing some turnout predictions with the ANES data, but such predictions as we are able to test corroborate the rest of our findings. We also present some corroborative associational evidence. After reproducing the familiar result that less-informed citizens are less likely to vote, we show that this effect is largest for political moderates – persons who existing theories of partisanship suggest rely most on new information for resolving political uncertainty (Achen 2005). We also show that voter information about presidential elections is nearly always perfect and is much better than for all other contests. This common sense result implies that exposure to information probably has little effect on voters' information for presidential compared to other types of elections and reconciles our presidential turnout results.

Our analysis extends several branches of existing literature. Most obviously, we add to the very active research program on voter information and turnout. The paper also adds to the literature relating market outcomes to voting behavior. An older literature in political science studies turnout specifically, and tries to distinguish between two hypotheses that are based on more psychological arguments than the mechanisms emphasized in the economics literature. The "withdrawal hypothesis" (Brody and Sniderman 1977) posits that turnout should fall when voters experience reductions in their material circumstances because their preoccupation with

financial concerns leads them to ignore acts like voting.[7] The "mobilization hypothesis" (Scholzman and Verba 1979) argues, by contrast, that voters feeling aggrieved because of economic difficulty will use the political mechanism to relieve those grievances and thus vote more. Previous empirical work relating labor market activity to turnout has generally yielded mixed results, perhaps because it has not addressed the econometric issues we raise, and has relied on state or national correlations.[8] Our turnout results directly contradict the withdrawal hypothesis. Moreover, whereas all of our turnout results, including the gubernatorial and presidential estimates and the results about roll-off, are explicable with the information-based account we emphasize, only the negative gubernatorial estimates are readily explained by an argument based on mobilization.

Our work may also be read as adding to the literature on "economic voting." This literature has been contributed to by both economists and political scientists and studies the relationship between labor market outcomes and one specific dimension of voting behavior: the incumbent's share of total votes cast.[9] [10] Turnout has not been studied in this work. The focus on the voter share is prompted by the controversial argument (see Stigler 1973, 1979) that voters may, perhaps irrationally, reward or punish politicians for the performance of the labor market. The information exposure argument we emphasize in our analysis of turnout is a potentially less controversial theoretical mechanism by which voting and labor market activity might be related.

The remainder of the paper proceeds as follows. Section 2 describes the empirical specifications for the analyses of turnout. Section 3 discusses county-level data. Section 4 presents the county-level results, including OLS and TSLS estimates of turnout, results for migration and the analysis of roll-off. Section 5 presents individual level tests of information and labor supply and Section 6 concludes.

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[7] Why agents should be more likely to turn to political action to correct or seek redress for bad outcomes than they are to turn to political action to reward politicians for good outcomes is not clear, although Lau (1985) suggests that there might be "negativity bias" in political behavior.

[8] Rosenstone (1982) finds that turnout varies positively with economic conditions at the national level, but other work finds that the positive relationship between turnout and labor market performance is unstable in the US (Southwell 1988; Arcelus and Meltzer 1975) and is non-monotone in national time series data (Radcliff 1992).

[9] See Lewis-Beck and Stegmaier (2000), and Hibbs (2005) for reviews of the economic voting literature.

[10] While not part of the economic voting literature, there is also a growing literature studying how economic conditions affect support for redistributive and other policies. A notable example of recent work in this area is Brunner, Ross and Washington (2011), who study official ballot returns in California.

2. Organizing Framework and Empirical Specification for Analysis of Turnout

This section provides a conceptual overview of our reduced form analysis of turnout and labor market activity. Our framework assumes that a citizen votes when his various benefits from voting exceeds his various voting costs. Existing theory suggests what these costs and benefits are. Downs' (1957) classic framework posits two possible benefits from voting: an instrumentalist payoff equal to the voter's utility from changing the election outcome times the probability that his vote is pivotal in determining the outcome (a payoff which must be zero in large elections like those we study in this paper); and a psychic payoff from discharging his civic obligation. Recent theoretical models of voting, including models with strategically sophisticated but differentially informed voters (Feddersen and Pesendorfer 1996) and models of expressive voting with disutility from uncertainty, regret, or ambiguity aversion (see Matsusaka 1995, Merlo 2006, Ghirardato and Katz 2002, Ashworth 2011) stress the role of limited information. In these models, voters derive utility from participation but will abstain unless they are sufficiently well informed. The theoretical literature therefore suggests that voters confront *logistical* cost of voting, C_L, such as standing in line or driving to the polls, and various costs associated with the lack of knowledge associated with derive from limited information, which we refer to by the catch-all term *uncertainty* costs, C_U.

Our empirical analysis hinges on the hypothesis that the two types of voting costs identified in the theoretical literature depend on a voter's labor market activity, $0 < h < 1$. We assume that at date t in location k there are specific set of logistical challenges $g(t,k)$, such as traffic or weather. The logistical cost faced by a voter at that place and time may be written as

$$C_L = ahg(t,k) + \varphi, \tag{1}$$

where φ represents idiosyncratic individual factors. The parameter $a > 0$ captures the fact that logistical hurdles impose greater burdens on persons more heavily involved in market work. A voters' uncertainty costs depend on the information generally available about an election e occurring at a given time and place, $I(e,t,k)$; and by the voter's exposure to that political

information, which we assume varies negatively with his labor market activity.[11] We write uncertainty costs as

$$C_U = -(1+bh)I(e,t,k) + \zeta, \tag{2}$$

where ζ represents idiosyncratic determinants of uncertainty. The parameter $-1 < b < 0$, implying that increases in available information lower uncertainty costs, with an effect that is smaller the more time the voter spends on market work rather than on leisure-intensive activities like consuming media.

Let $\omega = \zeta + \varphi$ represent all the idiosyncratic determinants of a voter's voting costs. Recalling that a citizen votes if $D > C_L + C_U$, the probability that the citizen votes (or, equivalently, the share of citizens who vote), V_{kt}^e, is given by

$$V_{kt}^e = F_\omega\left(D - h\left(ag(t,k) - bI(e,t,k)\right) - I(e,t,k)\right), \tag{3}$$

where F_ω is the cdf of the distribution from which ω is drawn. For illustrative simplicity, assume ω is distributed Uniform $[0,1]$. Then, from (3), the marginal effect of greater labor market activity on turnout is

$$\frac{\partial V_{kt}^e}{\partial h} = -\left(ag(t,k) - bI(e,k,t)\right) \leq 0. \tag{4}$$

Expression (4) says that there should be a negative (or at least, non-positive) relationship between labor market activity and turnout in a given local area, and that this effect should differ across elections for which there is *a priori* reason to suppose there are differences in available information, as summarized by $I(e,k,t)$.

In general, (4) combines the effect of work activity operating through logistical costs and the effect operating through information exposure. Notice, however, that if there were two elections, say e and $\bar{e}$, occurring at precisely the same time within a given location and appearing on the same ballot, the effect of an increase in labor market activity on the *difference* in turnout across those elections could not be a function of any logistical voting hurdles since these are precisely the same for the two elections. That is,

[11] It bears repeating that we do not deny that people may get information at politics about work. Instead, we merely argue that leisure intensive activities like watching the media, talking with friends or attending political events are likely provide *more* information than that from co-workers.

$$\frac{\partial\left(V_{kt}^{e}-V_{kt}^{\bar{e}}\right)}{\partial h}=-\left(ag\left(t,k\right)-bI\left(e,k,t\right)\right)+\left(ag\left(t,k\right)-bI\left(\bar{e},k,t\right)\right)=b\left(I\left(e,k,t\right)-I\left(\bar{e},k,t\right)\right) \quad (5)$$

depends only on the difference in available information about the two elections. The number of people who "roll-off" between a "bigger" and "smaller" election on the same ballot – the difference $V_{kt}^{e}-V_{kt}^{\bar{e}}$ - should get larger as labor market activity increases. This effect is due exclusively to the effect of labor market activity on uncertainty costs and would have nothing to do with logistical costs.

To study these predictions empirically, we choose the following empirical specification for voter turnout, V_{kst}, among citizens in county k and state s for an election at date t for which all voters in the state are eligible to participate:

$$V_{kst}=\beta_0+\beta_1 X_{kst}+\beta_2 h_{kst}^{*}+\delta_k+\pi_{st}+\eta_{kst}. \quad (6)$$

In (6), X_{kst} is a vector of observed county-level determinants of turnout, and η_{kst} is an independent, mean-zero statistical error. The vector δ_k represents unobserved, fixed characteristics of counties that determine turnout. The state×year fixed effects π_{st} capture the effect of state-specific factors for a given election; the quality of challengers, or the amount of advertising dollars spent in the state, are examples of the type of confound that apply for every county in a state, differentially by election year. It is worth noting that the ability to account for state×year specific factors is an important advantage of using county-level rather than state-level data. These various fixed effects, and the vector of observable controls are meant to account for most systematic determinants of turnout.

We wish to estimate β_2: how labor market activity, h_{kst}^{*}, in a county causally affects turnout, net of anything occurring elsewhere the state or nationally. We expect this relationship to be non-positive for any given election, and more negative for elections with smaller amounts of general information, $I\left(e,t,k\right)$. We do not observe information generally available about any election, but we study two types of elections with state-wide voting for which it is plausible to assume large differences in information generally available about them: gubernatorial and presidential elections. One problem with estimating β_2 using (6) is endogeneity bias, arising from the fact that labor market activity in a county is likely systematically associated with unobserved county-specific factors, δ_k. Latent political activism or the political sophistication of

persons in the county are examples of this type of confound. In addition, as denoted by the star on the variable in (6), indicators of county labor market activity are likely measured with error, leading to possible attenuation bias in the estimates of bias β_2.

Consider a differenced version of (6), where Δ_k represents the county-level difference across consecutive election years in a given variable:

$$\Delta_k V_{kst} = \beta_0 + \beta_1 \Delta_k X_{kst} + \beta_2 \Delta_k h^*_{kst} + \Delta_k \pi_{st} + \Delta_k \eta_{kst}. \quad (7)$$

Equation (7) is purged of the effects of any latent county-specific factors that are fixed over time, and also controls for state-specific considerations that change across elections. The difference estimator (7) might not deal with all endogeneity concerns if *changes* in local labor market activity are correlated with *changes* in unmeasured local factors that affect voting behavior $\left(\text{cov}\left(\Delta_k E^*_{kst}, \Delta_k \eta_{kst}\right) \neq 0\right)$. This could arise, for example, if counties experiencing growth in earnings see changes in the political sophistication of their residents. Another potential problem with (7) is that it might exacerbate attenuation bias associated with the mis-measurement of county labor market conditions relative to the level regression (see Bound et al. 2001).

To deal with both of these potential concerns, we conduct a Two Stage Least Squares (TSLS) analysis of the difference estimate in (7), in which we instrument for county-specific changes in labor market activity, $\Delta_k h^*_{kst}$. There are two pieces to our instrument: exogenous shocks to international oil supply; and the degree to which, because of exogenous endowments, counties differ with respect to their historical production of oil, natural gas, and coal. Shocks to world oil supply associated with international geo-political events, spur changes in U.S. production of oil, natural gas, and coal. Differences in endowment across counties ensure that international supply shocks generate differential changes in county labor market activity across counties in the same state. Our instruments for between-election changes in county-level labor market activity are variants of the product

$$\Delta R \times I\left(county\_size\right) \quad (8)$$

where ΔR is a measure of the international energy shock between election years, and $I\left(county\_size\right)$ is a set of indicator variables denoting the importance of oil or coal production

in the county.[12] The TSLS analysis estimates (7), with the change in measures of local labor activity replaced by the predicted value from the regression

$$\Delta_k h^*_{kst} = a_0 + a_1 \Delta R \times I\left(county\_size\right) + a_2 \Delta_k X_{kst} + a_3 \Delta_k \pi_{st} + \Delta_k e_{kst} \quad (9)$$

The TSLS estimates identify the causal effect of labor market activity on turnout, so long as the exclusion restriction is satisfied – that is, so long as the only mechanism by which exogenous shocks to international oil supply affect turnout in a county is through labor market activity in the county. There is, of course, no way to formally test whether the exclusion restriction holds, but below we discuss and provide direct evidence showing that migration induced by cross-market differences in labor market opportunity - the most likely reason for concern about failure of the exclusion restriction - does not matter importantly for our results.

3. Data for County Turnout Analysis

Throughout, we use two indicators of county labor market activity: log per capita earnings and log total employment per adult. 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 Bureau of Labor Statistics (BLS) compiles REIS wage and salary disbursements and employment data using ES-202 filings collected as part of the state unemployment insurance program. The REIS data on county wages and employment are known to be measured with some error.[13] Our analysis controls for both the level and age distribution of county population. We obtain county population information disaggregated by age, sex, and race

[12] Other authors have used energy shocks to isolate variation in local labor market outcomes. Black et al (2002) use only coal shocks to study disability payments; Acemoglu et al (2009) study the impact of oil price shocks on areas in the South with large oil wells to study health spending. Wolfers (2002), using aggregate state data, tests whether voters are rational by observing whether the share of their votes going to an incumbent increase following after energy shocks. Wolfer's analysis differs from our analysis in that does not study turnout and uses aggregate state data.

[13] 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, generating some measurement error. In addition, components of other labor income and proprietors income such as pension plan contributions, health and life insurance contributions, and private worker's compensation contributions are only collected at the state-level and also use an allocation rule to determine county-level totals. Documentation for REIS sources are available on-line http://www.bea.gov/regional/docs/cd.cfm

from the Census Bureau, which reports this data beginning in 1970.[14] Unfortunately, population age distribution information before 1980 is limited to five-year age bands (0 to 4, 5 to 9, etc.). Our estimates of the number of voting age individuals in a county is therefore calculated as the number of individuals aged 20 and over.

Our analysis of state-wide elections focuses on gubernatorial and presidential elections between 1969 and 2000. We compile county-level voting information on these elections from various sources. For elections between 1969-1990 we use data from the Inter-University Consortium for Political and Social Research (ICPSR) "General Election Data for the United States, 1950-1990" (ICPSR study no. 13). For years beyond 1990 we assembled data from the CQ Press Voting and Elections Collection. We have systematically 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 use data for all states except Alaska, Hawaii, and Louisiana which are excluded for reasons detailed in the Data Appendix. 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.

The TSLS part of our analysis focuses on the effect of energy supply shocks in oil and coal producing counties in "coal" and "oil" states. Following Black et al. (2002), we define the "coal" states to be Kentucky, Ohio, Pennsylvania, and West Virginia – the states that span the country's large coal seam. The eleven "oil" states are those with at least 1 percent of annual state wages in the 1974 County Business Patterns (CBP) in the oil/natural gas industry.[15] We create estimates of the oil/natural gas and coal industry employment in these states using two-digit CBP information from 1974.[16] We define counties as "small", "medium" or "large" producers of oil and coal based on whether their 1974 total employment share in oil and gas production or in coal is, respectively, less than 5%, at least 5% and less than 20%, or at least 20%. The maps in Appendix Figure 1, which depict the distribution of "large", "medium" and

[14] These data are available on-line at http://www.census.gov/popest/archives/. We impute 1969 county population information by using the 1970 population data.

[15] These states are Colorado, Kansas, Mississippi, Montana, New Mexico, North Dakota, Oklahoma, Texas, Utah, and Wyoming. As noted earlier, our analysis does not include Alaska so it is not part of this list. See Appendix Table 1 for summary of importance of oil and coal states in "oil" and "coal" states.

[16] See Data Appendix for details of how we construct estimates of importance of oil or coal employment share from CBP data.

"small" oil and coal producing counties, illustrate the tremendous variation in the importance of oil and coal across counties in these states. It is this variation that the TSLS analysis exploits.[17]

We obtain data on energy prices from the Energy Information Administration's *Annual Energy Review*.[18] Our TSLS strategy focuses on the years 1969 to 1990, a period of two large exogenous shocks to 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.[19] These events affected both the prices of oil, coal and natural gas, and the employment in these industries in the U.S., as Figure 1 illustrates.

Oil prices doubled between 1973 and 1974, were stable for several years, then increased fourfold over a two year period. Prices fell sharply over the next four years to levels in the mid-1980s that were slightly lower than those of mid-1970s. Coal prices doubled between 1973 and 1976 but then fell consistently over the next two decades, returning by the late 1990s to 1970s levels. Real natural gas prices followed a very similar pattern to that for oil prices: a fivefold increase between 1970 and the early 1980s, then a decline of more than half over the next 6 years. The second panel of the figure shows that national employment in the oil/natural gas and coal industries closely tracked the movement in prices, with the noteworthy difference that national employment was not kept artificially flat during various periods, as was true for oil prices in two periods in the 1970s because of policy decisions.[20] Since changes in resource prices over our sample period are not exclusively the result of energy supply shocks, we use national employment as our main measure of the energy supply shock when constructing our instrument.[21]

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[17] Importantly, 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 these states, regardless of whether the county falls is in a state we call an oil state or coal state. 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.

[18] Oil prices are the U.S. average first purchase price per barrel, natural 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.

[19] 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.

[20] 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.

[21] Our results are robust to alternative specifications of the instruments, including results using the international oil price as the measure of the shock.

Table 1 summarizes turnout and labor market outcomes for the counties in our sample. For elections between 1969 and 2000, turnout across all counties in the U.S. averaged approximately 55% in presidential elections, and 43% in gubernatorial elections. As can be seen from the number of elections/years observations, the overwhelming majority of gubernatorial elections during this time period occur in non-presidential years. Average turnout in the oil and coal states used for the TSLS portion of our analysis, during the years of the energy "boom" (when prices and production rose sharply) and "bust" (when prices and production sharply fell), is very similar to mean turnout for the country as a whole over the entire 1969-2000 period. The table also shows that both county earnings per capita and employment per adult were slightly lower in the oil and coal sample than in the rest of the country over the entire period studied.

4. County Turnout Results

4.1 OLS Presidential and Gubernatorial Estimates

Table 2 presents estimates of specifications based on equations (6) and (7). This table presents results for all counties in our full sample over the period 1969-2000. The estimates in column (a) are from pooled cross-sectional regressions based on equation (6) in which county voter turnout is regressed on a county labor market measure controlling separately for state fixed effects and year fixed effects, as opposed to state×year fixed effects, and excluding county fixed effects. Column (b) presents difference estimates based on equation (7) in which the change in county voter turnout is regressed on the change in the relevant labor market outcome, in order to account for county fixed effects. These regressions include year effects. The regressions in column (c) estimate the full specification shown in (7) by replacing the year effects from column (b) with state×year fixed effects. The movement from (a) to (b) reflects the importance of controlling for fixed county characteristics; the movement from columns (b) to (c) shows the importance of factors specific to that particular state×year. We present results separately for gubernatorial and presidential elections. Finally, this and all similar tables that follow present separate results for our two measures of labor market conditions (log of per capita earnings and log of employment per adult), so each entry in the table is from a different regression.

The table shows that in pooled OLS models that control separately for state and time effects, more labor market activity in a county is associated with higher voter turnout. The estimated effects are strongly statistically significant for both gubernatorial and presidential

elections and for both indicators of labor market activity. The results in (a) are contrary to the predicted relationship discussed above, but this specification does not account for either fixed local confounds or state-year specific effects.[22] When we account for county-fixed effects in the difference estimator in column (b), the results are quite different. In particular, the positive point estimates from the level equation become negative for the gubernatorial elections in these difference models and both effects are statistically significant. For presidential elections, estimating the difference model effectively takes the point estimates to zero for both labor market measures. Adding state×year effects to the difference model sharply lowers the magnitude of the various effects, as shown in column (c). It is interesting that the point estimates for the gubernatorial elections remain negative (although only one is significant), while those for presidential elections are all positive but very small.[23]

4.2 Two Stage Least Squares Presidential and Gubernatorial Estimates

We have noted that estimates from the difference models above might be contaminated by measurement error and endogeneity bias, which would tend to bias the negative estimates of labor market activity towards 0 (in the case of measurement error) or even *through* 0 (in the case of endogenity bias).[24] Our TSLS analysis addresses both of these concerns.

The validity of the TSLS analysis depends on whether exogenous energy supply shocks affected changes in county labor market outcomes based on the importance of oil or coal production in the county. Table 3 presents the first stage regressions (9) for counties in the oil and coal sample, with each column in the table representing a different regression. The results in panels A and B show that both oil and coal shocks (measured as the change in national employment in these industries) *differentially* affected changes in county log per capita earnings

[22] The pooled results reproduce the finding of the work in political science which is based on more aggregate data and so does not control for unobserved fixed effects (see Rosenstone 1982). As we discuss later, attempts to relate labor market activity and voting using individual level data several problem, but an especially important one is the paucity of individual panel data on voting which makes it difficult to control for latent person fixed effects – the analog of fixed county effects in our analysis.

[23] This basic pattern of results remains the same under various alternative specifications, including those in which we use the mean of labor market conditions in the two years preceding the relevant election as the measure of county labor market performance.

[24] Since better educated and richer people are generally 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.

and log employment per adult in counties where the production of the resource was a "small", "medium" or "large" share of overall employment. For example, the point estimates of 0.108 and 0.281 in the first column of panel A imply that a 10% increase in national coal employment increases per capita earnings in medium and large coal counties by 1 and 3 percent, respectively, compared to small coal counties. We find similar differential effects in large and medium relative to small counties for oil supply shocks (the bottom pair of point estimates in each column); for both measures of labor market performance; and across the different years in which the two types of elections are held. Notice also that *F*-statistics are all larger than the threshold of 10 that has been emphasized in the "weak instruments" literature (Staiger and Stock 1997).

Before moving to the TSLS estimates, we present reduced form results where the change in voter turnout is regressed directly on the exogenous instruments. These results, presented in Panel C of Table 3, demonstrate both that energy shocks have a larger impact on voter turnout in counties in which employment is more dependent on energy production and that these shocks affect gubernatorial turnout but not presidential turnout. We find that positive shocks to both oil and coal production significantly lowered voter turnout in "large" counties relative to "small" or "medium" counties for gubernatorial elections. By contrast, the presidential results indicate that oil shocks had no differential effect on voter turnout across counties; and coal shocks led to increased turnout but did so more in medium than in large counties.

Table 4 presents OLS and TSLS estimates of the effect of changes in the two labor market measures on the change in voter turnout for the sample of oil and coal counties over the energy boom/bust years. The TSLS estimates of labor market performance on turnout are negative and, in all but one case, strongly statistically significant for gubernatorial elections. The fact that these estimates are larger (in absolute value) than the negative OLS point estimates suggests that measurement error and endogeneity bias affect the latter results in the manner earlier described. The difference between the gubernatorial elections and the presidential results 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 OLS results.

How large are these estimated effects? Applying the TSLS estimates to all of the counties in the full sample, the coefficient of -0.138 for log earnings per capita in the gubernatorial regressions implies that a county experiencing a one standard deviation change in log earnings per capita of 0.113 log points has a reduction in voter turnout between elections of

1.6 percentage points. At our estimate of -0.318, a county experiencing a one standard deviation change in log employment per adult of 0.061 log points has a decline in voter turnout of 1.9 percentage points. Alternatively, we can estimate the impact of labor market fluctuations on voter turnout during the energy boom and bust. In large oil producing counties, log per capita earnings changed by 0.4 log points and log employment per adult by roughly 0.2 log points over the boom and bust cycle. In large coal counties, log per capita earnings changed by 0.25 log points and log employment per adult by roughly 0.1 log points. Given these swings in earnings and employment, our estimates imply that changes in labor market activity changed turnout by between 5.5 and 6.3 percentage points, respectively, in large oil counties and between 3.2 and 3.3 percentage points in large coal counties. Relative to the mean of 43% across all gubernatorial elections in the U.S., our implied effects are therefore quite economically significant.

4.3 Roll-Off and Labor Market Activity

As discussed in Section 2, results relating labor market activity and turnout necessarily conflate any effect of labor market activity on logistical (or, shoe-leather) costs, and any effects operating through information exposure and uncertainty. However, if variation in shoe leather costs mattered importantly for our results, increases in this type of cost should lower turnout for *both* gubernatorial and presidential elections, yet we consistently fail to find evidence of any negative effect on presidential turnout in any of the preceding estimates. [25]

To formally assess the possible importance of an effect of market activity on turnout that operates through logistical costs, we study roll-off between presidential and House of Representative elections, which for a given area occur on the same ballot and on the same day. [26] Since logistical costs cannot explain the decision to vote in *only* one of these elections, variation

[25] Another reason to doubt that this type of cost appreciably lowers turnout is plausibility: it not obvious that the few minutes citizens must stand in line to vote are high enough to prevent people from voting.

[26] Recall that to study roll-off we need to focus on different types of elections occurring at the same time and in places where we can use the instrumental variables that allow us to account for measurement error and endogeneity bias in TSLS analyses. These requirements invalidate gubernatorial/Presidential voting for this part of our analysis. During the years under study, gubernatorial and Presidential elections occur at the same time in only 9 states. Moreover, for only 4 of these states do we have measures of the instrumental variables used in our analysis. See the Data Appendix for details about the compilation and cleaning of the data we use for the county-level House of Representatives analysis.

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in exposure to information associated with changes in labor market activity is the most plausible explanation for any finding that roll-off varies systematically with voters' market activity. An information based account of voting predicts that the number of people who vote in the more widely covered election (the presidential contest) but do not vote in the election with less media coverage (the House of Representatives) should increase as labor market activity increases.

We estimate models of the form

$$\Delta_k RO_{kst} = \beta_0 + \beta_1 \Delta_k X_{kst} + \beta_2 \Delta_k h^*_{kst} + \Delta_k \pi_{st} + \Delta_k \eta_{kst}. \quad (10)$$

where $\Delta_k x_{kst}$, $\Delta_k h^*_{kst}$, and $\Delta_k \pi_{st}$ represent, as before, county-level changes in observed county characteristics, labor market conditions, and state×year fixed effects and $\Delta_k \eta_{kst}$ is the change in a mean-zero error. The outcome variable RO_{kst} measures roll-off in a given presidential election year: the difference in share of persons voting in presidential and House elections, or $RO_{kst} = V^P_{kst} - V^H_{kst}$.

Table 5 presents three sets of estimates: OLS county-level difference results on all counties over all the years in our sample; OLS county-level difference results for the oil and coal counties over the "boom" and "bust" years; and TSLS estimates from the latter sample and years. Each entry in the table presents results from a different regression. Taken as a whole, the results indicate that the share of people who vote in a presidential election but not in a House election on the same ballot increases when the labor market is performing better. All of the point estimates are positive and increase when we instrument for the labor market measure, consistent with the likely nature of measurement error and endogeneity biased. For the log per capita earnings measure, the point estimates are statistically significant, and are economically meaningful relative to the mean of the dependent variable. Although none of the point estimates for the employment per adult measure are statistically significant, we find it reassuring that all are positive and generally move in the same direction as the earnings measure. Since this finding that roll-off rises with increases in labor market activity cannot be the result of changes in logistical costs, we think it strongly suggests that the negative effect between turnout and labor market activity (in some elections) mainly reflects the effect of greater labor market activity on agents' exposure to political information and their uncertainty.

4.4 Robustness Analyses

Alternative Specifications

We conduct a number of robustness analyses for the county turnout results. In one set of tests, we use alternative specifications of the instrument. For example, we use changes in coal and oil price instead of changes in national employment in oil and coal. Also, the main specification defines the importance of oil and coal production in a county using CBP data from 1974, which is the earliest years for which two-digit data are available. Because these 1974 numbers might partly reflect endogenous responses to oil shocks (the first of which occurred in 1972/73), we use one-digit 1967 CBP data to create an alternative measure of the importance of county oil and coal employment. Reassuringly, our TSLS results are robust to these alternative ways of defining the instrument.[27]

In another robustness exercise, we estimate OLS difference models similar to those above, but use data for geographic areas larger than counties: State Economic Areas (SEA) and Economic Sub Regions (ESR).[28] There are three benefits to estimating these models. First, OLS difference models using data from these larger geographic areas do not depend on the energy shock instruments in any way. So, unlike the county TSLS estimates, they are impervious to concerns about a violation of exclusion restriction of the energy shock instrument in the turnout estimates (the possibility that energy shocks changed voting through mechanism other than labor market activity). Second, measurement error is certain to be smaller with this more aggregated data than with the smaller county measures. Third, since both SEAs and ESRs are subsumed within states, models using these data can control for state×year fixed effects, which we have shown to be important determinants of turnout.

Of course, there are disadvantages of using data for these larger geographic areas. We are interested in how turnout is affected by voters' *own* labor market activity, for which we believe indicators of labor market opportunity in voters' specific local labor markets provide good proxies. The larger the geographic area used to define a "labor market", the greater the likelihood that our estimates capture how labor activity in areas *outside* of where a voter lives and works affects voting. Economic activity in areas outside of a voter's own labor market does

[27] See Appendix Table 2 for these robustness results.

[28] 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), and ESRs are aggregations of SEAs. Most states have between 6 to 11 times the number of SEAs as counties.

not affect the voter's own time allocation or economic wellbeing, but might still be related to voting – possibly through some "socio-tropic" concerns.[29] Any such effect would have nothing do with changes in voting associated with time allocation, whether because of changes in logistical costs or exposure to information. In addition, in analyses performed on data aggregated to larger geographic areas it is not possible for formally account for the various local (county-specific) unobserved factors, δ_k, that clearly affect turnout. Despite these concerns, it is instructive to see how OLS regressions based on data from larger geographic areas compare to our preferred TSLS county-level results.

Table 6 shows that the OLS difference estimates with SEA and ESR data are, for gubernatorial elections, substantially larger than the corresponding OLS county-level estimates in Table 3. This is precisely what we would expect if the smaller measurement error from using these more aggregate data reduces attenuation bias. Although these point estimates are negative and larger than the corresponding OLS estimates, not all of them are statistically significant. Interestingly, for these regressions as well we find very different results for presidential elections: none of the presidential effects is statistically significant; and most are positive but very small, especially in comparison to the gubernatorial results. It bears repeating that we do not attach the same causal interpretation to these estimates that we do to the preceding results, for the reasons already mentioned. Nonetheless, we find it reassuring that these estimates support the conclusions drawn from the earlier main results: for gubernatorial elections, improvements in labor market conditions lead to reductions in voter turnout; turnout in presidential elections, by contrast, is either unaffected by changes in local labor market conditions or else exhibits a small positive response.

Migration

An important consideration that might affect the causal interpretation we place on the foregoing estimates is transitory internal migration associated with local economic shocks. Suppose that migrants sort temporarily into areas where labor market conditions are improving. Suppose further that migrants do not vote in elections dominated by local concerns because they are either unfamiliar with those concerns or regard them as irrelevant, given their temporary

[29] Unlike the individual-based accounts of voting that dominate the literature, socio-tropic models voting posit that voting decisions are based, in part, how politician performance in office affects people other than the voter himself (Kinder and Kiewiet 1981).

residence in the area. Finally, suppose that migrants continue to vote in elections whose outcomes are relevant to them wherever they live in the future. Local labor market activity and voter turnout would then be negatively related in gubernatorial or House of Representative elections, the outcomes of which are relevant only for people who are from or plan to live in the state in the future. At the same time, these same variables might exhibit no systematic relationship to turnout in presidential elections, about which voters are presumably interested wherever in the country they live. In this scenario, the TSLS estimates could be identifying the effect of migration associated with energy shocks rather than how labor market activity changes turnout among a given set of voters.

We estimate negative effects of labor market activity on turnout for two gubernatorial elections and in the House of Representative roll-off analysis. With respect to the gubernatorial estimates, the concern about migration is important only insofar as migration is from outside the state; migrants across different counties within a state presumably care about the outcomes of the state-wide gubernatorial election. Similarly, for House of Representative elections, the concern is important for migration to or from areas outside the Congressional District, which is spatial unit (typically a collection of several counties) within a state over which these elections are contested. Unfortunately, no data source exists with which it is possible to track migration across Congressional districts. However, we can study within- and out-of-state migration over the period of the energy price shocks.

The interval of what we have called the energy "boom" and "bust" over which the TSLS results are estimated is approximately 1970 to 1990. This is fortunate, as we can use the question from the 1970, 1980 and 1990 Censuses about where the respondent lived five years previously to determine how the share of the population new to a county changed across oil and coal counties over the energy "boom" (1970-1980) and "bust" (1980-1990) periods. These measures are available at the county-level in Census summary files.[30] The first two columns in Table 7 show how the share of residents who had lived in a different county five years previously changed differentially in large and medium, compared to small counties. We find that during the boom, large and medium coal and oil producing counties experienced increases in the share of their residents who had lived in a different county five years previously, and that these counties experienced a reduction in the share of such persons during the period of the energy

[30] See Data Appendix for further details about Census data used in migration analysis.

bust. Again, these estimates represent comparisons to small producing counties – precisely the comparisons on which the TSLS estimates are based.

The results in the second pair of columns, which examine the change in the share of a county's residents who lived in another *state* five years previously, reveal a different pattern. We find that during the energy boom there was no statistically significant change in oil or coal counties' shares of out of state residents compared to changes in small producing counties. And, the reduction in the share of state migration during the bust was only a fraction of the overall relative reduction in the share of persons who from another county. Thus, whereas the energy supply shocks did indeed occasion greater in- and out-migration into the "large" and "medium" oil and coal producing counties compared to "small" ones, the overwhelming majority of that relative migration difference involved people from *within* the state. Given this, and presuming that all residents of a state have an interest in state-wide elections irrespective of which county they live in, these results suggest that the negative gubernatorial county results are not driven by migration.

We can bound the possible effect of migration on the TSLS turnout estimates over the boom (when there was no relative out of state migration difference into small, medium and large counties) and bust (when the statistically significant out-of-state migration differences are small). Suppose we make the unrealistic assumption that *all* out of state migrants into a county do not vote in gubernatorial elections because they did not know or care about these elections in the state to which they move. The results imply that of the 6.3 estimated percentage point change in turnout over the boom/bust cycle in large oil counties from the TSLS estimates, at most only 1.7 percentage points is attributable to migration. Similarly, for large and medium coal states, observed changes in migration cannot explain more than one-third of the 3.3 percentage point swing in gubernatorial turnout arising from changes in labor market activity over the boom/bust cycle in large or medium coal counties. As noted above, we cannot conduct the same sort of analysis for migration across congressional districts for the House of Representatives results, but this direct migration evidence suggests that this is mechanism does not appreciably affect the qualitative conclusions of our analysis.

5. Individual Level Evidence on Information, Voting and Employment

The various county-level turnout results are jointly consistent with the notion that increased labor market activity reduces voters' exposure to political information, increases their uncertainty about some elections, and lowers turnout in those elections. In this section, we present direct evidence on such aspects of this mechanism as we are able to using individual level data.

Suppose that the individual labor supply h_{ist} of a person i living in state s at time t is related to the various outcomes of interest y_{ist} (information or knowledge, media exposure and voting) according to the regression model

$$y_{ist} = \lambda_0 + \lambda_1 x_{ist} + \lambda_2 h_{ist} + \alpha_i + \pi_{st} + \nu_{ist}, \tag{11}$$

where x_{ist} is a vector of individual level observables, α_i is a un-observed fixed person effect, π_{st} is a vector of state×year fixed effects and ν_{ist} is an error term. The person effect α_i is analogous to the fixed county effect δ_k in the county-level turnout analysis, and captures the fact that the outcomes y are partly determined by unobserved individual traits that also affect the likelihood of being employed at any point in time. Failure to account for this would tend to bias estimates of the parameter λ_2. The natural way to account for these latent confounds would be to apply fixed effects methods to (11), which requires that the data source from which we obtain individual level data on labor supply, information, media exposure and voting must have a longitudinal feature, in which the same individual is interviewed across successive election years and asked about the relevant variables in both years.

We use data from the American National Election Studies (ANES) Cumulative Data File (Sapiro et al. 2004). The ANES is primarily a repeated cross-sectional survey, conducted in 1948 and every other year since 1952. ANES respondents are interviewed twice during presidential election years, once in the weeks prior to the election and again in the weeks following the election. In non-presidential election years, individuals are only interviewed once following the election. In addition to some demographic information, the ANES reports respondents' turnout (based on self-reports of voting), employment status, state of residence, and provides measures of voters' political information and their use of various forms of media.

In various years (1956-1960, 1972-1976, and 1992-1996), the ANES has a rarely-used panel component, in which the same people were interviewed across successive presidential

years. During these years the survey provides information about both voters' level of political information and their exposure to media. 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. The interviewer codes his assessment of the respondent's level of political information on a five point scale ranging from "very high" to "very low". We define a respondent as being informed if the interviewer codes their information level as "very high" or "fairly high".

The survey also reports a measure of a respondent's exposure to campaign information from multiple media sources. This measure 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 how many of four different types of media (t.v., radio, magazines, newspapers) respondents report having gotten information from about the recently completed election. We use observations for this index for which there are valid responses for each of the four discrete elements from which this element are constructed. We define the respondent as being informed if they accessed three or more of these media sources.

Table 8 presents estimates of the effect of employment on information and media exposure from individual ANES data.[31] Column (1) presents results from pooled cross-section models across all available years. Column (2) presents pooled cross-section estimates, but only for the ANES panel sample. Column (3) shows the results from a difference model – estimated, of course, on the panel sample.[32] We find that in the pooled cross-section, being employed is very strong and positively correlated with being thought by the interviewer to be a politically

[31] The media exposure index is only asked in a post-election survey, so there is an unavoidable slight discrepancy between the timing of the information questions and the labor supply measures.

[32] Comparisons of sample sizes between the interviewer's assessment of respondent information (Panel A) and the Media Exposure Index (Panel B) must account for the higher rate of missing data for the Media Exposure Index arising from three sources. First, the media measures are based on respondent answers to four items while the interviewers provide an information assessment. Second, the ANES used multiple survey forms in some years such that not all respondents were asked the same questions in a given year. Roughly half of respondents were not asked the media questions in 1972 and roughly one-tenth were not asked in 1992. Third, since 1988 the newspaper media question is only asked for those who first respond that they have read a newspaper during the past. As an example of the impact on the sample sizes, the number of respondents providing valid observations in adjacent panel years for column (3) is 672 in 1972-76 and 574 in 1992-96 in Panel A and 644 in 1956-60, 316 in 1972-76, and 348 in 1992-96 in Panel B.

informed person, and with being rated highly on the Media Exposure index. For the indicator of being politically informed, the pooled result is significant in both the full sample and in the panel sample. For the media exposure measure, the pooled point estimate is significant in the full sample and large but not statistically different from zero in the panel sample. These results seem inconsistent with the argument about information exposure, but they are undoubtedly contaminated by the presence of unobserved person effects, α_i, which likely impart positive bias to the estimates.

The difference-estimator results shown in column (3) differ sharply from the naïve pooled estimates. Once person-specific fixed effects are accounted for, we find that whether a person is informed about politics and his level of media exposure are both negatively related to his being employed, precisely as the argument linking leisure to information exposure predicts. The results suggest that employment and the two measures of information are positively associated with unobserved person-specific factors. The estimated effects for gaining employment on being informed and for having a high level of media exposure are -.09 and -.07, respectively. These are both relatively large compared to the means of the two variables of 0.44 and 0.42. These results provide evidence for the mechanism that we have argued best explains the various county-level turnout results.

Although we have presented various turnout results using official vote totals, it would be appealing to supplement those findings with individual voting results from the ANES that correspond to the specifications in Table 8. Unfortunately, several features of the information the ANES provides about voting makes this difficult or impossible. One problem is that ANES information on turnout is based on respondents' self-reports. Voting self-reports have been shown to be quite unreliable (both in the ANES and elsewhere), with people reporting higher rates of turnout than suggested by official vote totals (see Katz and Katz 2000). Indeed, the well-known unreliability of individual turnout reports is one reason we use official county vote totals to study turnout in the preceding section. The ANES has conducted a validation study, intended in part to measure the degree of mis-reporting of voting in the survey, which partially spans the panel years. However, the validation information only verifies whether someone went to the polls at all; the data cannot be used to ascertain whether someone who reports voting for both, say, the presidential and House of Representative elections actually voted in only one of these elections when he went into the voting booth. This is an important limitation, given our interest

in differential behavior across election types. Indeed, the survey does not inquire about gubernatorial voting at all over the years of the panel.[33]

We conclude our ANES analysis with some simple associational patterns that have not been presented in previous work, as far as we know. The results do not directly test the argument relating labor supply to information, media exposure and turnout but they support the paper's main argument and provide some corroboration of our key hypothesis. The first bit of evidence examines the associational relationship between reported turnout, information and partisanship. Formal definitions of partisanship (Achen 2005) and intuition suggest that the more strongly a person identifies with a given political party the less likely he is to modify his relative preferences over candidates in the face of objective information about politics; candidates' party identification effectively becomes all he needs to know to determine which candidate he most prefers. This implies that, while there should be a negative relationship between reported turnout and how informed the person is judged by an objective observer, the negative gradient should be larger for political moderates compared to more strongly partisan voters.

ANES respondents report their political partisanship in a series of questions which are translated to a seven point scale, ranging from "Strong Democrat"=1, "Independent"=4, through "Strong Republican"=7. The two graphs in Figure 2 show the share of respondents who reported having voted in the election, by the respondent's self-reported partisanship and by their levels of interviewer-assessed political knowledge and media exposure. The top two lines in each figure are the average reported turnout rate 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 better informed persons of each partisanship type were more likely to vote, by between 1.3 and 3.2 statistically significant percentage points. The graphs also show that moderates are more sensitive to political information: the gap in turnout between informed and un-informed moderates is statistically larger than the corresponding gap for voters at the extremes of the partisanship distribution.

[33] Despite these concerns, for robustness we estimated individual first difference results for self-reported ANES presidential voting (where the expected relationship is zero or very small), and self-reported ANES House of Representatives voting (which is likely especially affected by mis-reporting). These individual-level Presidential results line up with the results from official vote totals. The results for reported House of Representatives voting, unlike all of our other results, show no statistically significant relationship with labor market activity. For reasons noted, we place little credence in these results.

These results are subject to the concerns we have raised about the reliability of reported turnout, but they are consistent with an information based account of voting.

The second piece of associational evidence provides some individual-level corroboration for the argument we have made for why voting in presidential elections (unlike other contests) is not affected by labor market activity. We have argued that this result makes sense in the context of an information-based model of voting if peoples' knowledge about presidential contests is vastly superior to that for other elections, and if it is so close to perfect that less exposure to political information has scant effect on what people know. We can test this argument directly.

For different types of elections over several survey years, ANES respondents are asked to rate the candidates in the previous election contest. This "thermometer" scale ranges from 0 to 100. We categorize respondents as not being able to "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 are categorized as "recalling" the candidate. This is the only available measure of information differences across different types of elections and is available for multiple election types beginning in 1978. It is admittedly quite coarse and is not elicited for gubernatorial elections. These shortcomings notwithstanding, we find that the share of respondents who can recall both candidates is 97%, 66% and 45%, respectively, for presidential, Senate and House elections. Reassuringly, as we argued in the county turnout analysis, ignorance about candidates falls the "bigger" (meaning, the likely more intensely covered) the election.

6. Discussion and Conclusion

This paper argues that voters' labor market activity affects voting through two possible mechanisms: by changing the logistical costs associated with those actions; or by changing people's exposure to political information by virtue of changes in their leisure time. These two hypothesized mechanisms yield different predictions. In particular, whereas the logistical cost mechanism suggests that turnout should decline with labor market activity in all elections, the information exposure argument suggests that while this relationship should be negative in general, it should be much smaller or even zero in elections like that for the President for which

information in so widely available that reduced information exposure probably has little effect on knowledge. Moreover, while the logistical cost argument makes no obvious prediction about roll-off (voting in only a subset of elections appearing on a given ballot), the information exposure and leisure mechanism implies that roll-off between any pair of elections should get larger as labor market activity increases.

Analysis of turnout using county-level data and a variety of OLS and TSLS analyses finds evidence that is strongly consistent with the information exposure argument. We find that increases in local per capita earnings and log employment per adult lead to reductions in voter turnout in gubernatorial elections and House of Representative voting, but has no effect on voting in presidential elections. We also show that for elections on the same ballot, for which the shoe-leather cost to vote on one is the same as the cost of voting on both, the gap between presidential and House of Representative voting widens with increases in labor market activity. Our findings are robust to a number of extensions and robustness tests, and we rule out alternative explanations for our findings, such as migration. We also conduct individual-level analyses with data from the ANES. We estimate individual panel models, which find that changes in labor supply are systematically negatively related to changes in media exposure and an objective measure of voters' political knowledge. Simple associational evidence further supports our main argument. We show that the gap in turnout between informed and uninformed persons is largest for political moderates (people who might be expected to rely most on information in forming their political judgments), and that information is, indeed, nearly always perfect in presidential elections compared to other contexts.

Our work extends the burgeoning literature in economics and political economy on information and voting by focusing on a relationship that has received little previous attention. The results also suggest some directions for future work. We have focused on the decision to vote – the most fundamental of voting actions – but our results raise interesting questions about whether the predictable variation in exposure to information occasioned by changes in labor market activity affects the behavior of other actors in the political market place. For example, our analysis treats the information that agents are more or less exposed to because of variation in their labor market activity as exogenous. However, if political actors can predict when voters have time to devote to activities that provide political information, this may have an effect of what information voters observe in equilibrium. For example, the type or amount of political

advertising done might vary with predictable changes in voters' leisure time, or the timing of controversial political decisions might be similarly strategically timed.

The results may also have implications for future work linking labor market outcomes to other aspects of voting behavior. A previous literature on "economic voting" tries to assess whether voters credit or blame political candidates for economic conditions by relating the incumbent's vote share to aggregate measures of labor market activity. Notably, this literature does not address the decision to vote in the first place, even though the vote share going a particular candidate obviously depends on which and how many voters turn out to vote. Our various results suggest that whether economic conditions affect voters' assessments of candidate quality or not, labor market conditions also affect turnout by changing voters' exposure to political information, with effects that may differ between incumbents and relatively unknown challengers. An important future challenge for the economic voting literature will thus be to formally incorporate the turnout decision into both theoretical and empirical analyses of the vote share, and perhaps to explore the importance of the knowledge/uncertainty mechanism we discuss in this paper for why one type of candidate might be preferred relative to another in votes cast.

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Table 1. Average County Voter Turnout by Election Types, and Average County Labor Market Conditions. 1969-2000 Elections, for All Counties and Counties in Oil and Coal States.

	All Counties (All Years)	Counties in Oil and Coal States. (Boom/Bust Yrs. Only)
A: Voter Turnout		
Gubernatorial		
Mean	.427	.417
Std. Dev.	.109	.113
# elections	348	76
# of elections in Pres. Yrs	74	20
Presidential		
Mean	.548	.556
Std. Dev.	.095	.079
# elections	376	70
B: Economic Measures		
Per capita earnings		
Mean	17.5	15.1
Std. Dev.	10.1	5.7
Employment per adult		
Mean	.757	.728
Std. Dev.	.218	.169
# County/Years	47446	11996

Data drawn from multiple sources on aggregate voting data.
"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. See text for details.

Table 2. The Effect of Local Economic Performance on Voter Participation: County-Level Regresssions for All Counties in U.S. for 1969-2000 Elections. Pooled OLS and First-Difference (Change Since Last Election) Models.

	Governor			President		
	Pooled OLS	Difference Models		Pooled OLS	Difference Models	
	(a)	(b)	(c)	(a)	(b)	(c)
Local Labor Market Measure						
(1) Log per capita Earnings	.031	-.032	-.013	.043	.001	.0003
	(.005)	(.012)	(.005)	(.007)	(.007)	(.004)
(2) Log Employment per adult	.041	-.060	-.008	.054	-.016	.009
	(.009)	(.029)	(.007)	(.012)	(.013)	(.007)
Year Effects	Yes	Yes	No	Yes	Yes	No
State Effects	Yes	No	No	Yes	No	No
State*Year Fixed Effects	No	No	Yes	No	No	Yes
Poplulation Share Controls?	Yes	Yes	Yes	Yes	Yes	Yes
Observations	22299	19998	19998	24073	21058	21058

Each point estimate in table represents results from a different regression.
Standard errors account for arbitrary forms of 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 3. OLS Estimates of Effect Oil and Coal Supply 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 log Capita Annual Earnings		B. Δ County log Employment per Adult		C. Δ Voter Turnout	
	Governor	President	Governor	President	Governor	President
Oil/Coal Supply Shock:						
(Δ log Nat. Coal Emp) X I(Medium_Coal_1974)	.108	.205	.080	.067	-.014	.018
	(.029)	(.045)	(.024)	(.015)	(.010)	(.008)
(Δ log Nat. Coal Emp) X I(Large_Coal_1974)	.281	.340	.114	.152	-.022	.004
	(.073)	(.114)	(.014)	(.020)	(.011)	(.006)
(Δ log Nat. Oil Emp) X I(Medium_Oil_1974)	.041	.029	.024	.007	-.006	-.005
	(.011)	(.020)	(.007)	(.018)	(.004)	(.003)
(Δ log Nat. Oil Emp) X I(Large_Oil_1974)	.153	.120	.060	.051	-.035	-.0002
	(.028)	(.033)	(.014)	(.022)	(.005)	(.006)
State*Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
F-Stat on Oil/Coal Instrument	13.4	10.9	19.5	17.4		
Observations	4751	4412	4753	4412	4751	4666

Standard errors account for arbitrary forms of 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 4. OLS and TSLS Estimates of Effect of *Change* in County-Level Labor Market Outcomes on *Change* in Voter Turnout. Gubernatorial and Presidential Elections from 1969-1990 in "Oil" and "Coal" States.

	Governor		President	
	OLS	TSLS	OLS	TSLS
(1) Log per capita Earnings	-.029	-.138	-.004	.033
	(.008)	(.047)	(.003)	(.027)
(2) Log Employment per adult	-.040	-.318	.008	.068
	(.012)	(.105)	(.012)	(.066)
State*Year Fixed Effects	Yes	Yes	Yes	Yes
Instruments for TSLS Models: National Oil/Coal Employment * I(County "medium"/"large")				
Observations	4751	4751	4412	4412

Each point estimate in table represents results from a different regression.
Standard errors account for arbitrary forms of 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. Estimates of Effect of Local Labor Market Conditions on Presidential/Congressional "Roll-Off" (Share of County Voting in Presidential Election - Share of County Voting in House Election on Same Ballot).

Outcome: Presidential - Congressional Vote Share

	OLS First Difference Estimates		TSLS First Difference Estimates
Sample	All Counties	Oil and Coal Counties	Oil and Coal Counties
Local Labor Market Measure			
(1) Log per capita Earnings	.012	.024	.091
	(.004)	(.013)	(.049)
(2) Log per capita Employment	.019	.071	.195
	(.013)	(.039)	(.162)
State*Year Fixed Effects	Yes	Yes	Yes
Years of Data	1972-2000	1972-88	1972-88
Weighted Mean of LEVEL of Dependent Variable	.037	.046	.046
Observations	19056	3836	3836

Each point estimate in table represents results from a different regression.
Standard errors account for arbitrary forms
All regressions control for total Population; % Population Female; % Population: Black, Race "Other"; % Population aged 30s, 40s, 50s, 60s, 70+
See Data Appendix for details about House elections county data.

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

	Governor		President	
	SEA	ESR	SEA	ESR
(1) Log per capita Earnings	-.027	-.046	.002	-.001
	(.011)	(.013)	(.009)	(.010)
(2) Log Employment per adult	-.027	-.047	.010	.021
	(.018)	(.032)	(.014)	(.022)
Year Effects	No	No	No	No
State*Year Fixed Effects	Yes	Yes	Yes	Yes
Observations	3058	1581	3191	1666

Each point estimate represents results from a different regression.
Standard errors account for arbitrary forms of clustering within states.
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 Estimates of Whether Change in Share of County's Residents Living Outside County and Outside State Five Years Before, Differs across "Large", "Medium" and "Small" Production Counties in Oil and Coal States over Energy Shock "Boom" and "Bust".

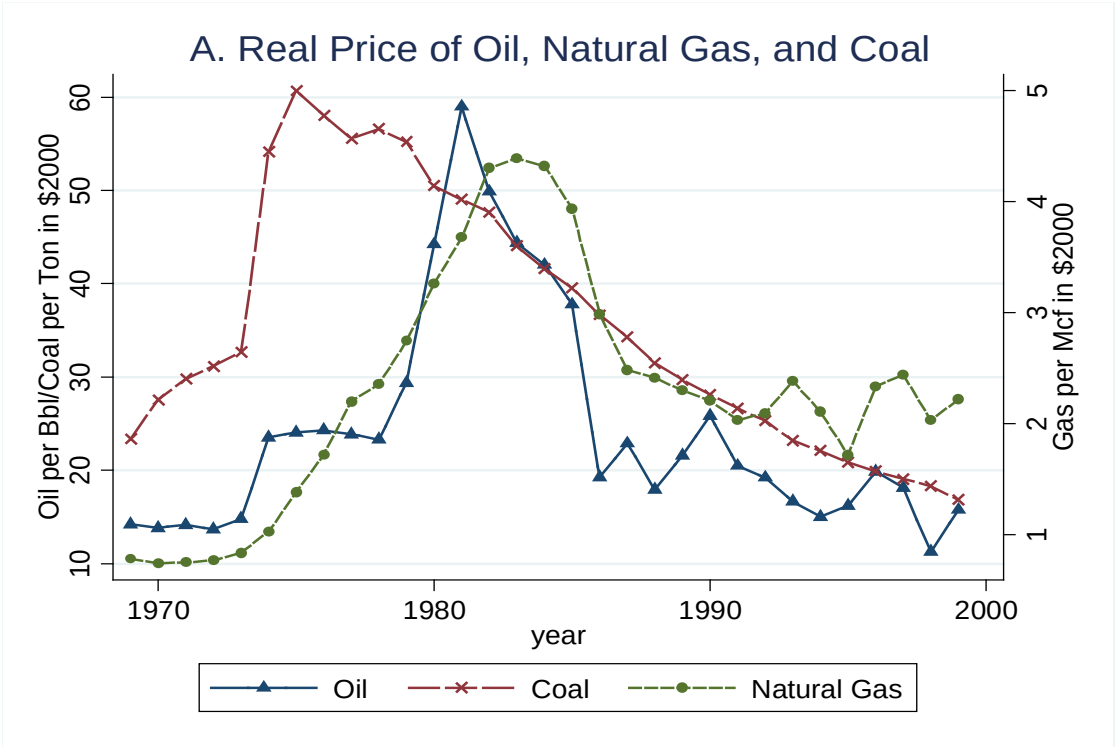
	A. Δ County Residents who Living Outside *County* Five Years Prior		B. Δ County Residents who Living Outside *State* Five Years Prior	
	1970 to 1980	1980 to 1990	1970 to 1980	1980 to 1990
Importance of Oil/Coal in County:				
I(Medium_Coal_1974)	.010	-.022	.001	-.013
	(.005)	(.004)	(.004)	(.002)
I(Large_Coal_1974)	.001	-.025	-.0003	-.016
	(.005)	(.005)	(.006)	(.004)
I(Medium_Oil_1974)	.022	-.020	.001	.002
	(.005)	(.005)	(.007)	(.005)
I(Large_Oil_1974)	-.0001	-.047	.001	-.017
	(.004)	(.005)	(.002)	(.007)
State*Year Fixed Effects	Yes	Yes	Yes	Yes
F-Stat on Oil/Coal Shock (P-Value)	7.2 (0.0028)	33.5 (0.0001)	0.1 (0.9634)	13.1 (0.0002)
Observations	1103	1103	1103	1103

Standard errors account for arbitrary forms of clustering within states.
Counties are "medium" if share of employment in oil/coal at least 5% but less than 20%; "large" if share > 20%

Table 8. OLS and Within Individual-Level Estimates of Effect of Being Employed on Alternative Measures of Political Information and Media Exposure 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**		
	(Mean of Dep. Variable: 0.38 full sample; 0.44 panel sample)		
R. Employed?	**.069**	**.050**	**-.091**
	(.008)	**(.026)**	**(.038)**
Yrs and Sample from ANES	Presidential Election Years 1968-2004	72-'76, '92-'96	72-'76, '92-'96
# Observations	19227	2492	2492
# Unique Respondents			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: 0.49 full sample; 0.50 panel sample)		
R. Employed?	**.030**	**.019**	**-.076**
	(.010)	**(.021)**	**(.043)**
Yrs and Sample from ANES	Presidential Election Years 1952-2004	56-'60, '72-'76, '92-'96	56-'60, '72-'76, '92-'96
# Observations	16802	2616	2616
# Unique Respondents			1308

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.



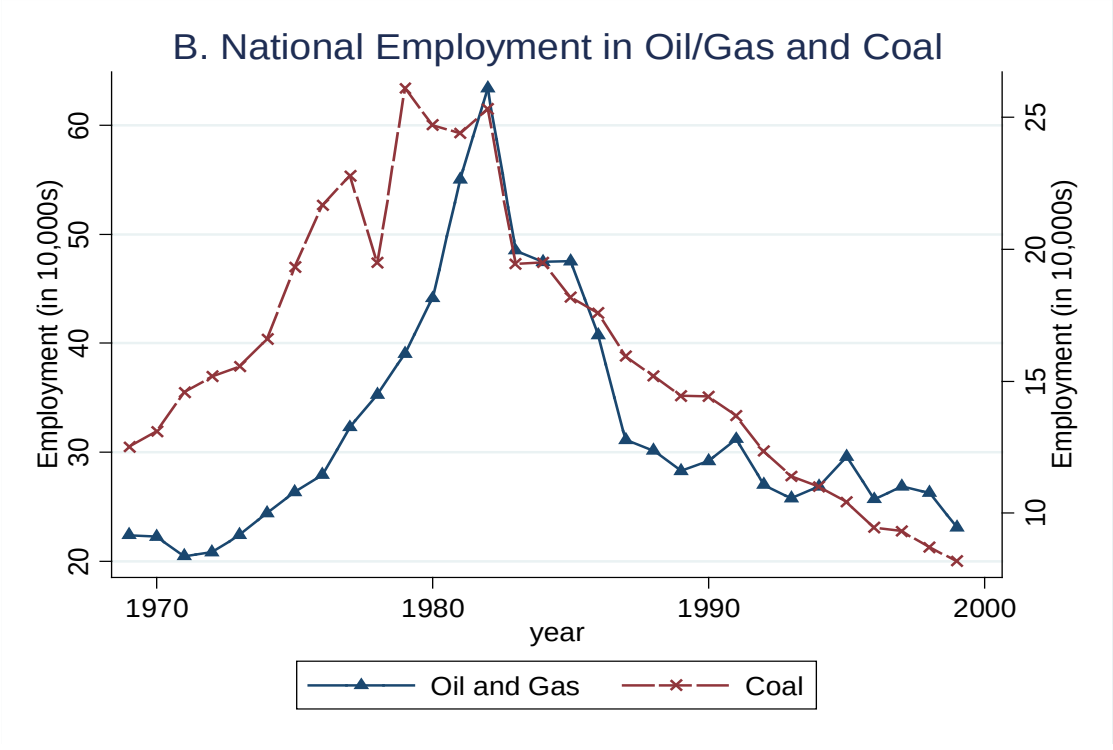
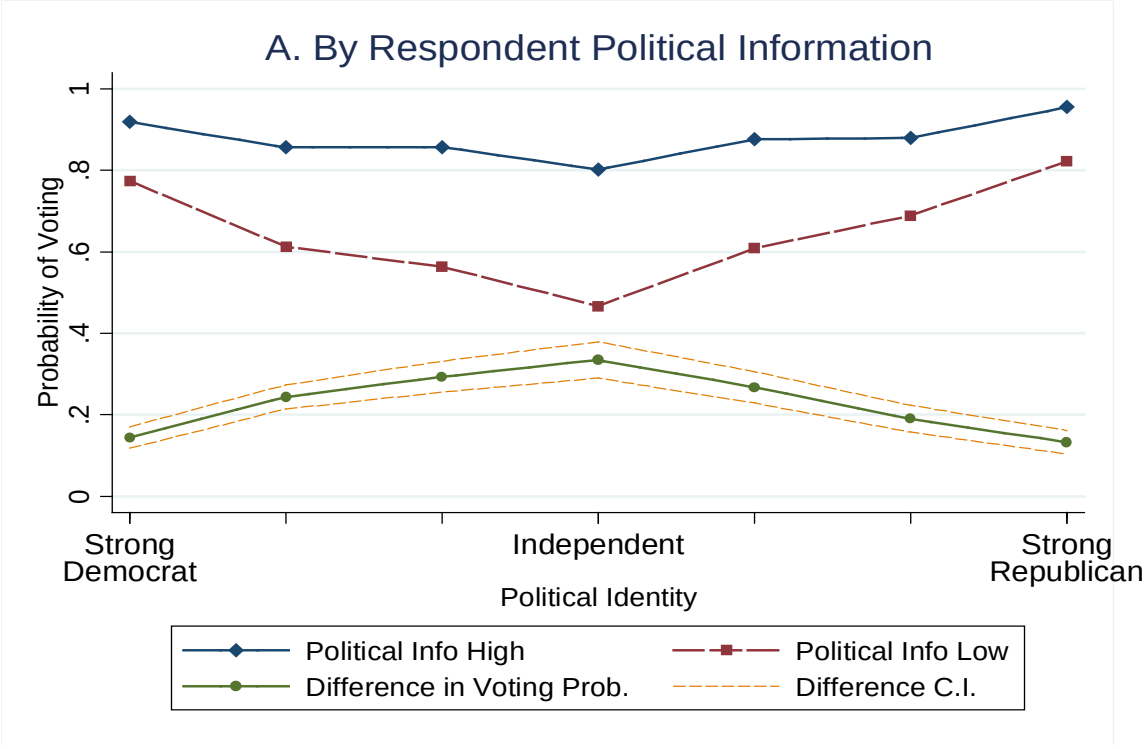


Figure 1: Effect of Energy Supply Shocks on Changes in Price and National Employment in Oil/Gas and Coal.



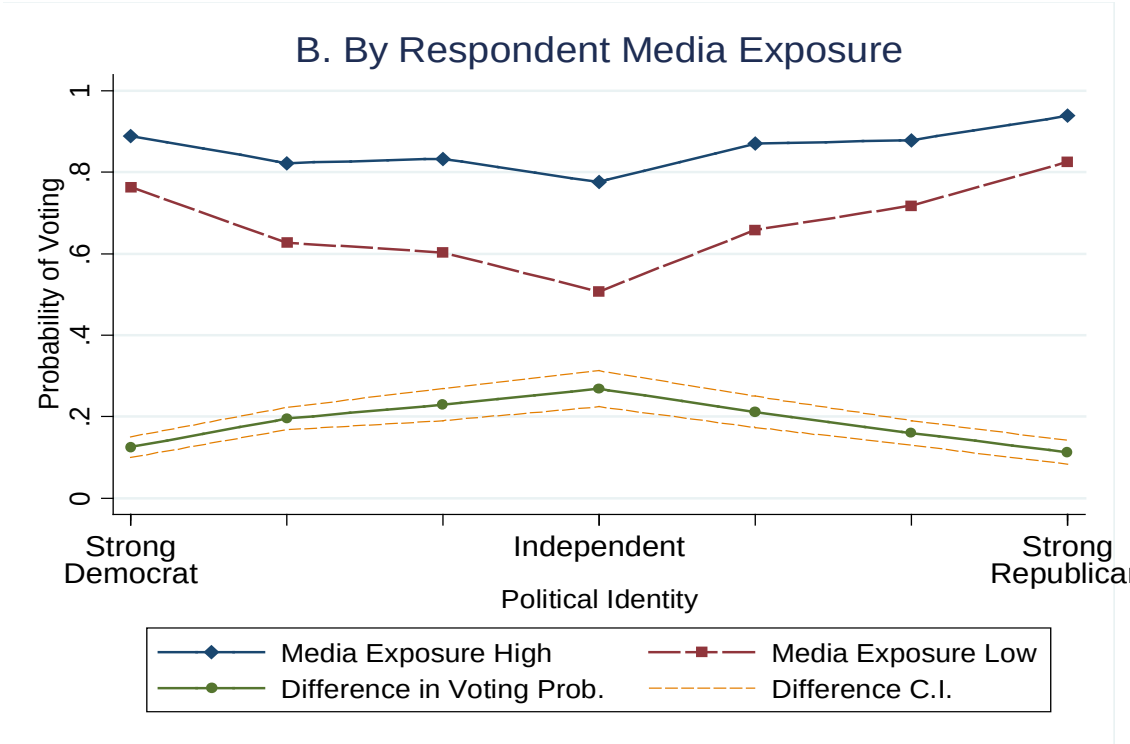


Figure 2: Reported Voter Participation, by Alternative Measures of Individual Partisanship

Data Appendix

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.

To address a small number of counties which are involved in either a merge or a split during our sample period, we only use observations for these counties from the after the merge or split. Counties which split during the sample period are Yuma, AZ (creating La Paz, AZ in 1983) and Valencia, NM (forming Cibola, NM in 1981). Counties which merged during this period are Washbaugh, SD and Jackson, SD in 1976 as well as Nansemond, VA and Suffolk City, VA. In addition, a few cities in Virginia annexed parts of surrounding counties including Roanoke City, VA (annexed part of Roanoke county in 1975), Petersburg City, VA (annexed parts of Dinwiddie, VA and Prince George, VA in 1972), and Lynchburg City, VA (annexed parts of Bedford, VA and Campbell,VA in 1975).

County-Level Voting Data

We use county-level voting totals found from three primary sources. For 1969-1990, we use data from ICPSR study no. 13 "General Election Data for the United States, 1950-1990". For subsequent years, Presidential, Senatorial, and Gubernatorial returns were obtained from the CQ Press Voting and Elections Collection while the Congressional returns were obtained from Election Data Services, Inc.

We made a number of adjustments to these voting returns to account for incomplete and apparent incorrect data. 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 ICPSR 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, in some instances all of the data for a single state for a given election during the period in which the ICPSR data is the primary source was replaced with data from CQ either due to

missing data or numerous suspicious values. A list of these elections is available from the authors.

A small fraction of individual counties in each year have data that apparent most likely because of incorrect coding. In some instances, these errors appear to be due to a switch in values for observations which appear consecutively in the alphabetized county list. In other instances, the data were incorrectly entered. A list of these counties and elections which have individual election problems that were corrected is available from the authors. In addition, for Congressional election data, we investigated the individual county returns whenever the overall total number of votes recorded for a state in a given year did not match the total number found on the Election Information webpage for the Clerk of the House of Representatives (http://clerk.house.gov/member_info/electionInfo/index.html).

We also drop observations due to a lack reported votes on election data. We do not use Louisiana in our analysis since elections in the state during this period except Presidential elections are subject to a non-partisan primary in which all candidates from both parties are on the same primary ballot. A run-off election is held between the top two vote getters, regardless of party, unless one of the candidates receives at least 50% of the vote in the primary in which case they are declared the winner. Since the final election varies may not be held on the standard nationwide general election day, we exclude Louisiana from all of our analysis. We also drop states in the roll-off analysis that do not report vote totals in uncontested elections which include Arkansas, Florida, Louisiana, and Oklahoma. In addition, due to a lack of available county-level Congressional returns in many years, we do not use Pennsylvania in the roll-off elections either.

Finally, there are two elections that meet all of criteria discussed above or earlier in the text that we still drop. The Arkansas 1990 Senate election is dropped because counties do not report vote totals due to the fact under state law election results for unopposed elections need not be reported. The Georgia 1992 Senate final election was a runoff election held on November 24th since no candidate received at least 50 percent of the vote on November 3rd.

Defining Coal and Oil States and County Employment Share in Oil and Coal

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.

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 was obtained from ICPSR with the remaining years being obtained from the U.S. Census Bureau.

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

Microdata from the American National Election Survey (ANES)

We use microdata from the 1948-2004 American National Election Studies (ANES) Cumulative Data File (Sapiro et al. 2004) which is ICPSR Study no. 8475. For our cross-sectional analysis, we restrict the sample to observations with non-missing data for gender, age, and employment status. For our panel analysis, it is important to note that the ANES includes one observation per household in each survey although that the same individual from the household may not be interviewed in all both periods. We use the following approach to link individuals the two observations for an individual which are spaced four years apart. We first use the unique household identifier variable VCF0006A to link observations from the same household across the two survey waves. We then only use linked observations for the same household if a) the respondent's gender is the same across both waves and b) the difference in the respondent's age between the two survey waves is within two years of the expected four year age difference (i.e., the age change is between two and six years). The respondent's gender is the same in over 99 percent of the linked household observations while the age difference is between two and six years in over 96 percent of the linked household observations (over 93% of the age differences are between three and five years while 81% of observations have exactly a four year age difference).

Census County-Level Migration Data

We obtain county-level migration rates using the Summary Tape File 4 data of the 1970, 1980, and 1990 Censuses, made available through the National Historical Geographic Information System. In each of these Census years, a sample of households is asked whether each household member lived in the same house five years before the Census date and, for each one that did not, the county, state, and/or country of residence five years earlier is ascertained. The fraction of the population sampled for this question is 15% in 1970, ~19% in 1980, and ~17% in 1990. The Census Bureau then creates county-level estimates on the number of migrants based on these responses. The full Census is used to enumerate the number of persons ages 5 and over that reside in the county as of the Census date. The Census Bureau imputes age and place of residence five years earlier if the necessary information is missing or incomplete on the Census form. We only use the non-allocated (i.e., non-imputed) counts to compute county-level migration rates.

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

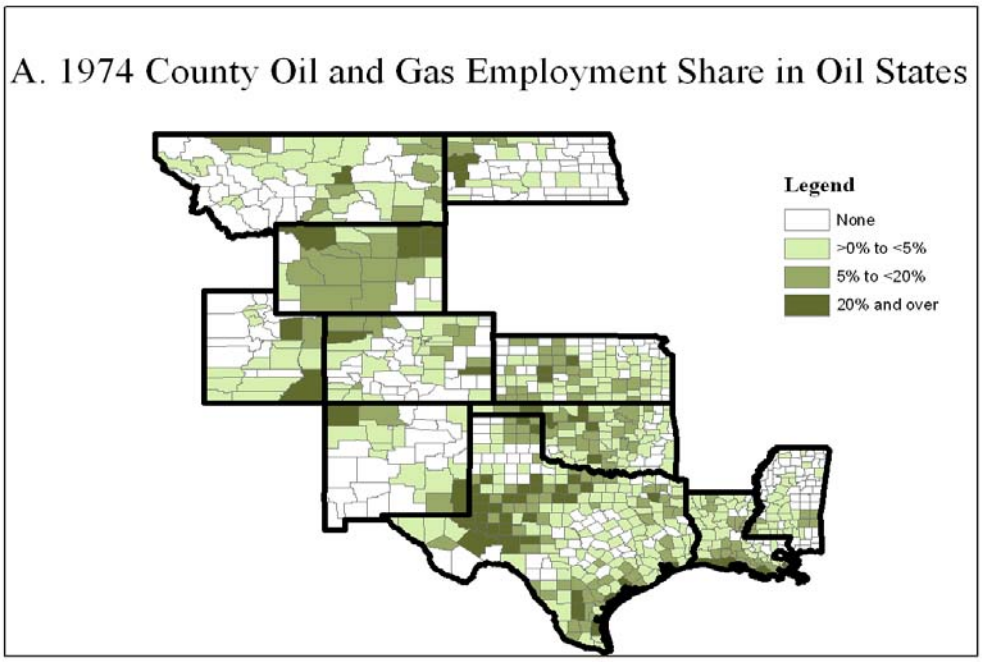
	Percent of 1974 CBP Employment from Mining	1974 CBP Share of mining establishments found in:		1967 CMI Share of mining establishments found in:	
		Oil and Gas	Coal	Oil and Gas	Coal
Wyoming	**15.8%**	**77%**	**5%**	**81%**	**4%**
West Virginia	*13.3%*	*40%*	*53%*	*40%*	*57%*
New Mexico	**7.8%**	**82%**	**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%*

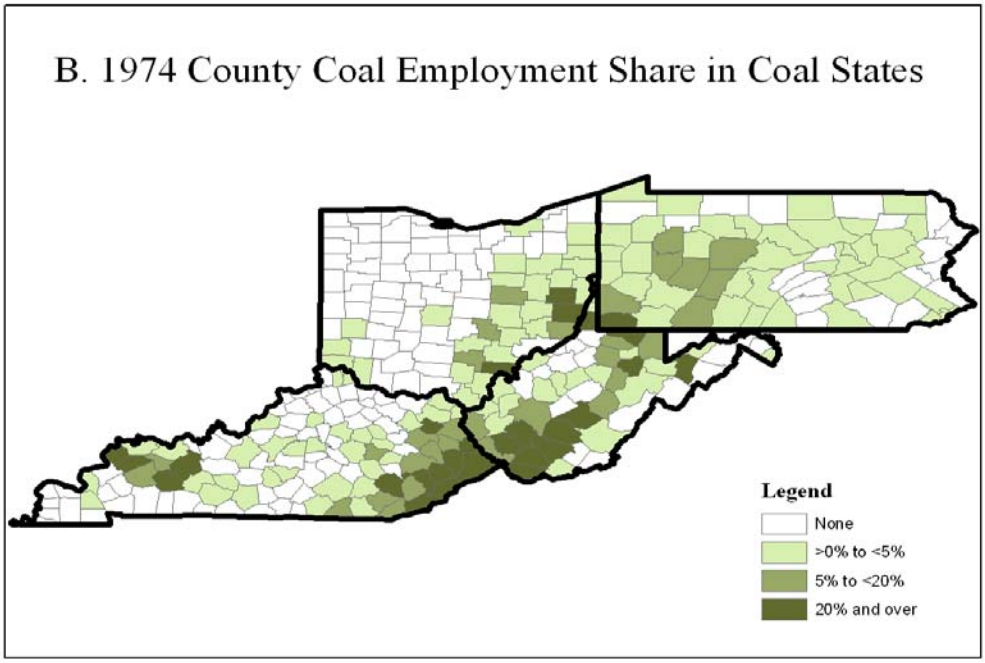
Notes: States in **bold** are "Oil States" in our analysis; States in *italics* are "Coal States"

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.

	Governor		**Senate**		**President**	
	Endogenous Regressor		*Endogenous Regressor*		*Endogenous Regressor*	
Instrument Specification:	Δ County per Capita Annual Earnings	Δ County Log Employment per Adult	Δ County per Capita Annual Earnings	Δ County Log Employment per Adult	Δ County per Capita Annual Earnings	Δ County Log Employment per Adult
1. (Δ National Coal/Oil Employment) X	-.067	-.113	-.042	-.079	.041	.078
I("medium", "large" Oil/Coal 1967)	(.026)	(.064)	(.022)	(.087)	(.024)	(.057)
F-Stat on Excluded Instuments	17.4	49.1	13.3	44.3	6.2	29.8
2. (Δ Coal/Oil Price) X	-.171	-.352	-.044	-.099	.050	.113
I("medium", "large" Oil/Coal 1974)	(.029)	(.080)	(.046)	(.134)	(.048)	(.114)
F-Stat on Excluded Instuments	27.8	38.0	13.7	27.7	15.6	87.2
3. (Δ Oil/Coal Price) X	-.157	-.286	-.027	-.078	.068	.127
I("medium", "large" Oil/Coal 19**67**)	(.033)	(.077)	(.036)	(.078)	(.032)	(.073)
F-Stat on Excluded Instuments	19.4	35.3	16.3	15.0	15.1	47.5
State*Year Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	4751	4753	6014	6016	4412	4414

Each point estimate in table represents results from a different regression. Standard errors account for arbitrary forms of 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+





Appendix Figure 1. Distribution of Small, Medium and Large Producing Counties in "Oil" and "Coal" States.