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ECONOMICS AND IDEOLOGY:
DENCE OF THE IMPACT OF ECONOMIC CONDITIONS ON SUPPORT FOR REDISTRIBUTION AND OTHER BALLOT

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Economics and Ideology: Causal Evidence of the Impact of Economic Conditions on Support
for Redistribution and Other Ballot Proposals
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ABSTRACT

Using California ballot proposition returns and exogenous shifts to labor demand, we provide the first
large-scale causal evidence of the impact of economic conditions on policy preferences. Consistent
with economic theory, we find that positive economic shocks decrease support for redistributive policies.
More notably, we find evidence of a need for cognitive consistency in voting behavior as economic
shocks have a smaller significant impact on voting on non-economic ballot issues. While we also demonstrate
that positive shocks decrease turnout, we present evidence that our results reflect changes to the electorate's
preferences and not simply to its composition.

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I.   INTRODUCTION

How do economic conditions affect political behavior and opinions? The answer to this question is important for understanding the dynamics of policy preference, the evolution of public policy and the optimal timing of the introduction of various types of legislation. Although the pundits speak of "pocketbook politics" we have little understanding of how economic shocks affect political views. We know that a good economy is beneficial for an incumbent, be s/he president or governor, Democrat or Republican (see, for example, Fair, 1978; Peltzman, 1987; Wolfers, 2002). But we have little evidence on the causal impact of economic conditions on support for major party candidates or for particulars of their platforms.

Economic theory predicts that support for redistribution is decreasing in *exogenous* productivity (Meltzer & Richard, 1981). Given proportional taxation and redistribution, the higher one's income, relative to the population, the higher the tax burden the individual must shoulder. As relative income falls, in contrast, the gains from redistribution rise. Thus a positive shock to productivity (which increases potential earnings) should cause a decrease in support for redistribution.

The empirical evidence on the relevance of this theory comes primarily from correlations, relating realized income to political behavior. And that evidence is mixed. On the one hand the red states are less wealthy than the blue. Glaeser and Sacerdote (2007) posit that this relationship is driven by higher income Americans' support for more liberal social policies.[1] On the other hand, in micro data from a variety of countries including the United States, income is negatively related to support for the more liberal

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[1] Alternatively Vigdor (2006) explains the phenomenon by providing empirical evidence that voters consider relative rather than absolute income in choosing a party.

party and for redistributive policies (see, for example, Ravallion & Lokshin, 2000; Corneo & Grüner, 2002; Alesina & La Ferrara, 2005; Leigh, 2005). Clearly these correlations could reflect some omitted variable, rather than a causal link between economic circumstances and voting. For example, perhaps a person with more liberal views is attracted to work in the government sector, which has lower pay. To test whether economic conditions motivate voting behavior, we need to identify the causal link between economic conditions and voting. That is the purpose of this paper.

In the single contribution that we are aware of on the topic that addresses the endogeneity of economic circumstances, Doherty, Gerber, and Green (2006) survey lottery winners of varying amounts about their support for redistributive policies. They find that those with higher lottery-induced affluence display significantly lower support for estate taxes and marginally significantly lower support for redistribution. They find no significant impact of affluence on views on inequality or on the desire to expand the social safety net. However, there are three limitations to the Doherty, Gerber, and Green study as a test of an economic model of voting. First, their small sample size (342 winners) limits their power to detect significant impacts. Second, their study identifies the effect of wealth by comparing winners of lotteries of varying size. Oster (2004) shows that as the jackpot size increases so too does the average income of the players. Thus, winners of differing amounts may not be drawn from identical distributions. Third, and most importantly, even if the lottery treatment were as good as random, the lottery sample lacks generalizeability. Lottery players may respond to economic shocks differently than the average voter. Furthermore, lottery players and non-players may

respond differently to lottery shocks than to the more typical economic shocks, such as variation in employment prospects.

In this paper we investigate the causal impact of a more typical income shock (changes in employment prospects) on a more typical population (all California voters). Our panel of California census tract level voting returns, covering eight elections and 91 state-level ballot propositions, allows us to examine the impact of economic conditions on both redistributive and non-redistributive policies. To measure tract-level economic conditions, we create a predicted employment index by weighting national industry employment by the industry mix of residents in the tract at the beginning of our sample time frame.[2] We then ask how census tract voting patterns change, relative to voting patterns in other tracts, following these plausibly exogenous shocks to labor demand.

To measure voting behavior, we do not rely on survey data, but rather we examine the impact of economic conditions on the true outcome of interest, neighborhood voting returns. This is an important distinction because survey questions, employed frequently in the political economy literature, often do not force respondents to make real tradeoffs. Survey questions ask respondents whether they agree with various policy stances—for example whether education funding should be increased—without actually making respondents consider, let alone potentially face, the implications for their tax bill. Additionally, to the extent that misreporting one's preferences or one's intention to turn out to vote is correlated with local economic conditions, the use of survey data will result in biased estimates of how economic conditions affect actual election returns.

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[2] As we explain in the data section, because of data limitations this is actually tract industry mix at a point during our time series predicted by industry mix at the beginning of (or prior to) our sample time frame.

Because we rely on aggregate data, one concern about our findings is that they may arise from selection rather than from changes in individuals' political behaviors. For example, positive economic shocks may lead relatively more conservative voters to move into a neighborhood. This concern motivates our identification strategy. In addition to neighborhood and year fixed effects, we control for county*year fixed effects and thus our results are not driven by relocation across counties over time. We further control for tract level trends to address within county concerns about neighborhood (d)evolution or composition change across time, as well as isolate our estimates from any within tract correlation between economic and political trends. While the aggregate nature of our data does not allow us to prove the absence of mobility bias, to the extent that non-linear relocation is driving our results, we would expect that our findings would be stronger in the neighborhoods with the most turnover. We find no such evidence. In fact, point estimates indicate that our results are stronger in the more stable neighborhoods.

Using this identification strategy, we find that positive economic shocks decrease support for redistributive policies. Thus we provide causal support for Meltzer and Richard's (1981) economic theory of redistributive politics. Consistent with theory these findings are largest in magnitude in neighborhoods which are most greatly affected by employment shocks. We further find suggestive evidence of two additional mechanisms by which employment shocks may affect voting on redistributive issues: need and sympathy for redistributive policies. First, we see that the link between economic conditions and economic voting is strongest in the poorest neighborhoods, where residents presumably have the most to gain from redistributive policies. Second, we see

that results are stronger in communities with an above median share of Democratic voters, where residents presumably are more amenable to the idea of redistribution.

Beyond the realm of economic theory, we find that economic shocks have a smaller but still significant impact on conservative voting on non-economic issues (e.g., campaign finance, courts and regulation). Consistent with the state proposition results, we find that positive economic shocks increase support for Republican gubernatorial candidates. Thus we find remarkable consistency for economic shocks to shift voting on a variety of issues in a more conservative direction.

While economic theory is silent on the impact of economic conditions on non-economic policy issues, behavioral economists have demonstrated the relevance of cognitive consistency to political opinions. Cognitive dissonance theory (Festinger, 1957) states that there is a cognitive cost to holding inconsistent views; for example, support for a party, but not for various planks of its platform. Gerber, Huber, and Washington (2009) manipulate party registration in a field experiment and show that those who are encouraged to register are not only more likely to call themselves partisans but also more likely to hold more partisan views. Thus if voters use their economic circumstances to determine party preference as our gubernatorial results suggest, then because of a need for cognitive consistency we would expect economic conditions to predict voting on non-redistributive matters as well. Consistent with this view, Branton (2003) finds that partisanship predicts individual voting behavior on a vast array of ballot propositions from economic to moral, despite the fact that ballot measures were originally implemented to lessen the influence of political parties. Further, McCarty, Poole, and

Rosenthal (2006) maintain that increased party polarization in American politics is driven by increased economic inequality.[3]

One concern about our methodology is that it cannot separate to what extent within neighborhoods, individuals are voting based on personal economic circumstances or based on what they observe about their neighbors' economic circumstances. Note that this limitation arises primarily from the aggregate nature of our predicted employment index. Even if we had access to individual level voting data, we would still not be able to discern the effects of individual fortunes from community fortunes because the employment "shock" is at a more aggregated level. A related limitation of the predicted employment index is that it tells us about neighborhood economic conditions generally. We do not know to what extent our results are driven by changes in income or changes in employment on the extensive or intensive margins. We can only estimate the total impact of demand for residents' employment on political preferences.

Finally, we note that that because of the inclusion of tract and county*year fixed effects, the economic changes to job security that we are using for identification are relative to other tracts and to other moments in a tract's history. This is deliberate. An investigation of the impact of relative economic conditions is in line with the Meltzer and Richard (1981) thesis. However, the relative approach means that we cannot use our results to answer questions such as how voting would change if every tract experienced a positive economic shock or if the majority of tracts experienced a negative shock as in the case of our most recent presidential election.

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[3] Our results also speak to the literature on the causes of belief formation. See for example Glaeser (2005), Piketty (1995) and Bénabou and Ok (2001) for theoretical contributions and Di Tella, Galiani, and Schargrodsky (2007) for an empirical investigation.

The remainder of the paper proceeds as follows. In the next section we detail the data, our employment shock measure and our estimation strategy. Section III presents basic results, a discussion of the threat of selection bias, robustness checks, results by tract type and finally a discussion of whether our results are driven by changes in turnout or by changes in preferences. In section IV we conclude.

II.   DATA/METHODOLOGY

California Tract-Level Voting Data

We turn to the state of California for our analysis because the state and its residents make frequent use of the ballot proposition. In the 15 year period, 1990-2004, there were 181 statewide ballot propositions in primary, general and special elections. These propositions spanned the spectrum of political issues from tax and fiscal policy to public good provision to campaign finance regulation to moral issues such as gambling. The great advantage of inferring preferences from propositions, as opposed to candidate choice, is that each proposition asks voters to express their views on a single issue at a time. For example, the "Housing and Emergency Shelter Trust Fund Act of 2002" posed a redistributive question: Should $2.1 billion in bonds be issued to provide temporary and permanent housing or housing improvements for battered women, seniors, the disabled and veterans? In the same year, the "Election Day Voter Registration. Voter Fraud Penalties. Initiative Statute" posed an electoral procedure question: Should voters be allowed to register on Election Day?[4] (The first proposition passed; the second failed.) While on each of these issues voting yes would be considered a more liberal position, inferences about one's willingness to redistribute resources are better drawn from one's vote on the first measure.

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[4] The measure would have also criminalized "conspiracy to commit voter fraud".

Propositions may be placed on a California ballot by either the legislature or by citizen's initiative. The legislature must seek popular approval to issue bonds or to amend the state constitution. An individual may place a proposition on the ballot for either of these purposes or to create a legal statute by collecting signatures equal to five percent of the gubernatorial vote in the last election, or eight percent in the case of a constitutional amendment.[5] Passage of a proposition requires a simple majority. Propositions appear on the ballot without any party identification. Thus, another advantage of propositions for our purposes is that they ask citizens to make real political decisions without being subjected to the immediate influence of a party label.

Prior to Election Day, attentive voters can learn whether a proposition is favored relatively more by Republicans or Democrats by reading official ballot pamphlets. Sent to voters by the state, these pamphlets contain arguments, for and against, signed by high-profile individuals and interest groups. As noted by Gerber and Phillips (2003), these arguments provide voters with "potentially powerful and efficient voting cues" which typically allow readers to discern whether the proposition is being supported or opposed by Republicans or Democrats.[6] In fact, a 1990 poll cited in Bowler and Donovan (1998) finds that 90 percent of California respondents claim to look at the arguments in favor and against the measure, more than report looking at the title or the nonpartisan summary. A second source for political orientation is advertisements which feature party members

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[5] Because individuals may place propositions on the ballot, one might be concerned about a correlation between economic shocks and the type of legislation that is on the ballot. Such simultaneity is not a threat to our identification strategy because we focus only on propositions that are voted on statewide, so that all neighborhoods regardless of relative economic circumstances are voting on the same initiatives at the same time.
[6] Increasingly the California Republican and Democratic Parties themselves take official party stances on ballot proposals and contribute money to the proposition campaigns (Smith & Tolbert, 2001).

or political interest groups.[7] Thus, the political leaning of the proposition can be ascertained by voters willing to do some homework or to read and think critically about the propositions in the voting booth. However, propositions do not allow for a quick and easy "straight ticket" party vote and thus potentially allow us to separate the effects of economic circumstances on party choice from effects on support for various issues.

The Statewide Database, maintained by the Institute of Governmental Studies (IGS) at the University of California at Berkeley, provides data on aggregate vote outcomes and voter registration for all statewide primary and general elections held in California since 1990. The primary unit of analysis in the Statewide Database is the voting precinct. We aggregate to the census tract, at which level employment by industry is available. (The aggregation process is detailed in the Data Appendix.)

To ensure that our biennial employment index has a consistent temporal relationship with our voting variables, we restrict attention to general elections which occur in November of even years in California. To avoid any correlation between regional economic conditions and what appears on the ballot, we focus only on those contests in which all voters in the state may participate. In our eight election years, 1990-2004, we cover four gubernatorial elections and 91 ballot contests. The 91 propositions include all general election ballot items for the years 1992-2004 and 10 of the 28 propositions on the 1990 general election ballot.[8] The most notable propositions in our sample are Proposition 187 in 1994 which denied illegal immigrants access to public

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[7] For example, Governor Arnold Schwarzenegger appeared in television advertisements supporting a set of ballot initiatives he sponsored for the 2005 special election. Similarly Los Angeles Mayor Antonio Villaraigosa narrated a number of television ads that promoted a 2006 ballot initiative that would have provided universal pre-school to California families. In addition, well known special interest groups such as the California Teachers Association and the Howard Jarvis Taxpayers Association commonly sponsor advertisements that either support or oppose various propositions.

[8] In 1990, the first year of data collection, the state collected results for only a sample of propositions.

services and Proposition 209 in 1996 which prohibited public discrimination on the basis

of race, sex, color, ethnicity or national origin and thus ended affirmative action

considerations in admissions to the University of California.[9] (Both propositions passed.)

We use these contests to create our main dependent variable, share voting for the

Democratic (liberal) candidate or issue. For gubernatorial elections, the definition of this

outcome is straightforward: the Democratic share of the two-party vote. The average of

this measure is 53 percent. (See Table 1 for sample means.)

Defining the Democratic side of a proposition is more complicated. To determine

whether yes or no represents the more liberal side, we run regressions of the following

form for each of the 91 propositions:

(1) $yesvote_n = B_1(dem)_n + B_2(rep)_n + B_3(ind)_n + \mu_{yn}$ and

(2) $novote_n = B_4(dem)_n + B_5(rep)_n + B_6(ind)_n + \mu_{nn}$,

where *n* indexes neighborhoods (tracts). *yesvote (novote)* is the share of the tract voting

yes (no) and *dem (rep/ind[10])* is the percent of registered voters who are registered

Democrats (Republicans/Other or Independent). The means of these variables are .49, .34

and .19 respectively. We then calculate the relative propensity of Democrats to vote yes

on a measure as:

(3)   Relative Propensity = $\hat{B}_1 - \hat{B}_2 - (\hat{B}_4 - \hat{B}_5)$.[11]

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[9] Proposition 227, which required that public school instruction be conducted almost exclusively in English, is not in our sample because it appeared on the 1998 primary election ballot.

[10] Independent includes those who are registered unaffiliated and those who affiliate with a party other than Democrat or Republican. As of December 2007, eighty-three percent of registered Californians who are not registered for a major party are registered as "Declined to State", California's term for Independents. http://www.ballot-access.org/2007/12/24/new-california-registration-data-2/

[11] We did not constrain our coefficients to lie between 0 and the share of the party who turned out (predicted in equations of the form of equation 1 substituting *turnout* for *yesvote*). Nonetheless, our predicted coefficients were quite well behaved. Of the 364 coefficients of interest, only 7 were predicted to be negative. In all cases percent Democrats (Republicans) voting yes plus percent Democrats (Republicans)

A score of -2 would mean that in neighborhoods in which all registered voters are

Republican all voters are predicted to vote yes and in neighborhoods in which all

registered voters are Democrats all voters are predicted to vote no. A score of +2 would

predict the reverse. A score of 0 would predict identical voting patterns in districts

regardless of the party composition of their residents. While theoretically this relative

propensity measure varies from -2 to 2, in practice voting is not so lopsided. The measure

ranges from -1.02 to 1.23 with a mean of .16 and a standard deviation of .44.

We check the validity of this measure in three ways. First, the Public Policy

Institute of California (PPIC) surveys state residents about their political leanings and

opinions. Fielded since 1998, the surveys have asked about fourteen of the propositions

in our sample. The survey data allow us to calculate the relative propensity of those who

claim to be Democrats to report voting yes. The correlation between the survey data

measure and the aggregate data measure is .83. Second, there are official proponents and

opponents for each of the propositions.[12] Using Internet resources we were able to

collect party information for at least one proponent and one opponent for 50 of the

propositions in our sample. (The difficulty in collecting this measure is that the official

text of propositions, by design, does not reveal the political affiliation of proponents and

opponents.) We use the party information to calculate the relative propensity of

voting no did not sum to more than a percentage point more than predicted Democratic (Republican) turnout.

[12] Under the California Elections Code, proponents and opponents of a proposition may submit to the Attorney General arguments for or against a proposition. These arguments are included in official ballot pamphlets and are signed by the individuals or groups that submit the arguments. Official sponsors are given the first opportunity to submit arguments in favor of a proposition. If the official sponsor does not submit an argument, the Secretary of State gives first priority to bona fide associations of citizens (e.g. California Teachers Association) and second priority to individual voters. In selecting arguments against a proposition, the Secretary of State gives preference and priority in the following order: (1) legislative body, (2) member of a legislative body, (3) bona fide association of citizens, and (4) individual voters (Gerber & Phillips, 2003). Typically, arguments for or against a proposition are prepared by the official sponsor or by vested interest groups such as the California Teachers Association, the California Taxpayer Protection Committee, the Nature Conservancy, the Howard Jarvis Taxpayers Association, etc.

Democrats to support the yes side of the legislation. This measure correlates .52 (or .59 if

we focus only on the 29 propositions in which our reference states the party of the

individual explicitly[13]) with the relative propensity measure we create using the tract

data. Finally, we follow the money. We examine the relative contributions of the

Democratic and Republican parties to the yes and no sides of the 42 propositions to

which either party contributed money. We find a correlation of .52 between this monetary

support measure and our relative propensity measure. Thus, our measure seems a

reasonable proxy of how liberal leaning a proposition is. We define voting Democratic on

a proposition as voting yes (no) when our relative propensity measure is greater (less)

than zero. Our dichotomous classification yields 100 percent agreement with a

dichotomous classification based on the PPIC survey data, 66-70 percent agreement with

a classification based on official proponent/opponent party and 79 percent agreement

with a classification based on official party donations. The average of the dichotomous

variable is .45. Because of the greater possibility for misclassification amongst those

propositions with a value of the continuous measure near 0, we demonstrate that our

results are robust to excluding those propositions with a relative propensity of -.1 to .1.

    Classifying our votes based on the voting outcomes for the same neighborhoods

whose voting behavior we hope to predict may feel circular. However, our results are

robust to randomly choosing one half of the census tracts to classify the propositions and

the other half to estimate the impact of employment conditions on voting behavior.

    To familiarize the reader with our data, Table 2 shows the relationship between

our outcomes and tract level characteristics. We average Democratic voting on all

gubernatorial and proposition contests. We then merge this collapsed data with 1990

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[13] In the remainder we had to infer party from context.

census data and run regressions of Democratic voting on tract level demographics. Consistent with findings from a variety of countries, higher income predicts more conservative voting in the cross section. This is true for both gubernatorial and ballot contests. Tracts with more minorities (particularly Blacks) and those with more educated residents have a greater propensity to vote Democratic. In the final two columns we examine predictors of voting by proposition type: 1) redistributive propositions which include the categories of social welfare and taxation and fiscal and 2) the remaining non-redistributive propositions which include votes on elections, courts, regulation and transportation. The sign of the income, minority and employment coefficients do not vary across proposition type. However, the income-conservative gradient is steeper for the redistributive propositions.

The ability of the same demographics to predict conservative voting for candidates and propositions of various types is consistent with Branton (2003). While previous studies demonstrated that partisanship predicts voting across two or three unrelated propositions, Branton examines exit polls for 50 ballot propositions covering issues from economic to moral, across more than 20 states and three years. She finds that partisanship (which is strongly predicted by demographics) predicts individual voting behavior across the range of propositions.

Predicted Employment Index

We are interested in the relationship between voting and economic conditions. However we recognize the potential endogeneity of a neighborhood's economic conditions. Employment is a function of both labor demand and labor supply (effort, hours worked, industry employed in). The same characteristics which influence a

person's decisions to work in a particular industry and live in a particular neighborhood may also influence his or her political preferences. We follow the procedure developed by Bartik (1991) and utilized by Blanchard and Katz (1992), Bound and Holzer (2000), and Autor and Duggan (2003) to create an index to isolate exogenous shocks to labor demand. The index, $\hat{\varepsilon}_{n,y}$ is calculated as:

(4) $\hat{\varepsilon}_{n,y} = \sum_k \varphi_{kny=0} \gamma_{ky}$

where φ is the share of tract n residents who are employed in industry k in the initial year and γ is the log share of national employment in industry k in year y. The predicted employment index (PEI) predicts what tract level employment would be if industry composition remained fixed and industry level employment changes occurred uniformly across tracts. Tracts in which a large fraction of residents are working in declining (growing) industries will be predicted to have lower (greater) employment over time. Provided that national employment trends are uncorrelated with tract level supply response, this index isolates exogenous variation in demand for residents' labor. To add to the likelihood that this condition holds, we control for tract level trends in our basic results. Further, we follow Autor and Duggan (2003) and define γ as national employment excluding the state of California, thus excluding the labor supply response of individuals in the focal tract and its labor market. We calculate the index for all tracts located in California Metropolitan Statistical Areas (MSAs) for the years 1990 to 2004. We restrict our attention to tracts which are located in MSAs because our national industry employment data do not contain information for the agricultural sector. Fewer than two percent of the approximately 7000 tracts in the state of California are located outside an MSA. Means for the index are shown in Table 1.

Because of the limitations of tract level employment data by industry, our employment data are coarser than what is available and has been used previously at the state level. Our employment data are grouped into 19 industries listed in the Data Appendix. Because of changes in the industrial classification system over time (also detailed in the Data Appendix) tract level employment data for the year 2000 are compatible with our national time series, but tract level employment data for 1990 and 1980 are not. We do not use the 2000 tract industry employment data as our "initial" year because of the concern that industrial changes during the nineties influenced residential and industry sorting patterns of workers prior to the 2000 census. Instead, we use data from the 1980 or 1990 decennial censuses to predict the share of employment in each identified industry in 2000. Specifically, for the sample of California metropolitan census tracts, the share employed in each of the 19 categories in 2000 is regressed on the share of employment in each of 17 (15) distinct industry categories available in the 1990 (1980) decennial census. We then use these regressions to predict tract level employment in each industry defined in 2000. That the basic pattern of our results is robust to using either 1990 or 1980 industries as our anchor year lends confidence to the notion that our initial employment shares are not endogenous to industrial changes occurring in the 1990s. To be most conservative, we present results using 1980 employment share throughout the paper. We further demonstrate that our results are robust to scaling the employment index by the percent of working age individuals in the tract in 1990.[14] This check ensures that results are not driven by those tracts in which the predicted

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[14] As expected, our effect size increases (in magnitude) when we weight by employment aged population. Similarly, when we split the sample at the median of the share of residents over age 65, we find smaller effects for those tracts with a larger share of older residents, who should be less sensitive to labor market shocks.

employment index should have little power to predict economic health because few residents are of working age.

Previous work has demonstrated that the predicted employment index is correlated with state level employment and earnings (Blanchard & Katz, 1992; Bound & Holzer, 2000) over both the short and long run.[15] Therefore, our estimates will capture the impact of changes in economic circumstances on voting behavior. However, given the nature of our general proxy for economic shocks, we cannot know whether these changes in circumstances operate through increased risk of lay-off and unemployment, short-run declines in earnings, or long-run declines in earnings capacity.

Ideally we would present evidence that the index is predictive of employment at finer levels of geography by showing a "first stage", a regression of employment on our index and tract and year dummies using our biennial tract level data. But as we have stated previously, tract level employment data are not available between censuses. Thus we begin by showing that the index is predictive of biennial employment at the county level and then demonstrate that the index predicts employment at the decennial frequency at the tract level. Results are shown in Table 3. The first cell of the table presents the coefficient on the predicted employment index from a county level regression of employment/population on $\hat{\varepsilon}_{n,y}$ and county and year fixed effects. A ten percent increase in the employment demand index increases the employment rate by over five percentage points. With our coarse industry employment data and a sample of only 37 metropolitan counties across eight years, this result is not significant. The second cell in column 1

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[15] For example, Blanchard and Katz (1992) find that the effect of employment shocks on unemployment disappear within a decade; the effect on wages nearly disappear in about twenty years and employment remains affected twenty years out, leading the authors to conclude that employment shocks "have largely permanent effects on employment". More recently Couch and Placzek (2010) and von Wachter, Song, and Manchester (2009) have documented long run earning impacts from job displacement.

demonstrates that this result is robust to using 1980 industries, in place of 1990 industries, as predictors for 2000 industry tract mix.

In order to compare our "first stage" across levels of geographies, in the next column we re-estimate the specification of column 1 with only two years of county data: 1990 and 2000, to correspond with our tract level census data. Across the ten years, a ten percent increase in the index leads to approximately a two to three percentage point increase in employment.

In the final columns of Table 3 we focus on the level of geography (but not frequency) of data we will employ in our analysis. In column 3 we re-estimate the specification of column 2 substituting tract for county data. Since the counties in column 2 are composed of the tracts in column 3, it is reassuring that point estimates do not differ greatly between the columns. We find in column 3 that a ten percent increase in the predicted employment index (PEI) increases employment by about four percentage points. This result is robust to the addition of county*year fixed effects, as demonstrated in the final column of the table. The results of Table 3 indicate that the PEI is a strong predictor of employment, one of the most prominent measures of economic health, and therefore that our proxy has sufficient power to identify the impact of economic shocks on voting.

Estimation Procedure

Using our predicted employment index and biennial voting data, we estimate an equation of the form:

(5) $\text{Outcome}_{e,n} = \alpha + \pi(\hat{\varepsilon}_{n,y}) + \gamma_n + \delta_e + \chi_{cy} + u_{e,n.}$

where e indexes electoral contests (gubernatorial or ballot contests), c indexes county, n indexes census tracts and y indexes years. *Outcome*, as outlined in a previous section, is share voting the liberal side. γ and δ are vectors of tract and electoral fixed effects respectively. Finally, to hold labor market conditions fixed we control for χ, a vector of county*year effects. These fixed effects further control for any election year shocks at the county level, (e.g., an aggressive advertising campaign in a particular media market).

As we stated earlier our identifying assumption is that national employment trends are uncorrelated with tract level supply response. One threat to identification would be the presence of tract level changes in demographic composition that are correlated both with labor supply and voting preferences. Our county*year fixed effects minimize this threat to the extent that labor supply shocks are spatially correlated. Nonetheless, we are still concerned that different neighborhoods experience different changes in neighborhood demographics and electoral tastes. One approach to addressing such a concern, controlling for election year tract demographics, is unavailable to us given the availability of tract-level census data on a decennial basis only. However, to the extent that neighborhood changes tend to move systematically over time we can address this concern by controlling for tract level trends in our proposition voting regressions.[16] For tractability, rather than adding 6,777 trend variables to equation (5), we employ tract fixed effects in a first difference specification. For this specification we collapse our data to cells by tract/election year or by tract/election year/proposition type and then run:

(6) $\text{Outcome}_{n,\, y-(y-1)} = \beta(\hat{\varepsilon}_{n,\, y-(y-1)}) + \gamma_n + \chi_{cy} + u_{e,n.}$

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[16] We do not control for trends in our gubernatorial specifications in which we have only three or four years of data.

where c, n, and y remain indexes of elections, counties and years respectively and χ continues to be a vector of county*year effects. The tract level fixed effects—γ—in the differences specification control for tract level trends. To increase the precision of our estimates we weight observations by the voting age population in the year. Because of concerns of heteroskedasticity, autocorrelation and the lack of independence of our error term within tracts over time, we cluster standard errors at the tract level.

In the following section we present results on the impact of a change in relative economic circumstance on neighborhood residents' voting behavior based on employment models of the form of equations (5) and (6).

III.   RESULTS

In the first column of Table 4 we show that positive economic conditions increase conservative voting on ballot propositions as a whole. In this analysis which is based on equation (5), an observation is a ballot proposition. The point estimate of -.450 indicates that as a neighborhood's predicted employment index increases by 10 percent, the fraction of voters choosing the Democratic side on the average proposition decreases by 4.5 percentage points. Using our Table 3 findings, we can treat PEI as an "instrument" for employment and scale our column 1 result by the impact of PEI on employment changes. We find that an increase in employment of one percentage point increases conservative voting by over one percentage point, as shown in the squiggly brackets.[17] The second cell in the column shows that this result is robust to a change from 1990 to 1980 weights.

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[17] While the magnitude of the impact may seem large, we note that previous work has found quite sizable correlations between economic conditions and two-party vote share. Because the labor force in California is less than half the size of the population, which we use to scale our employment variable, we can compare our one percentage point change in employment with a two percentage point change in unemployment. Verstyuk's (2004) estimates for U.S. presidential and congressional elections demonstrate that a 2 percentage point increase in unemployment is associated with a reduction in support for Republicans of

In the second column of the table we provide evidence that positive economic conditions also predict more conservative candidate choice. We examine gubernatorial contests to parallel our state level ballot propositions. We find that a one percentage point increase in PEI decreases the share voting for the Democratic candidate by over one percentage point. As discussed earlier, these results do not speak to the impact of a county or state wide shock to economic well-being. In fact, because our identification strategy focuses on relative changes, a relative increase in well-being in one tract must be matched by a relative decrease in well-being in another. Aggregating our results to the state level suggests a very small impact of relative economic conditions on voting. For example, based on tract level changes in PEI our model "predicts" that the aggregate gubernatorial vote should have increased 0.4, 0.8 and 0.4 percentage points in favor of the Republican in 1994, 1998 and 2002 respectively relative to the previous election.[18]

In column 3 we show that an increase in economic well-being decreases residents' propensity to vote.[19] (We define turnout as total number of votes cast in the electoral contest divided by the voting eligible population.)[20] While we present the results for gubernatorial elections which occur in non-presidential election years; our findings hold for presidential election years as well. Our turnout findings are consistent with Hastings

between 1.0 and 1.4 percentage points. Similarly, Gerber (1998) finds that a 2 percentage point increase in unemployment is associated with a 1.0 to 1.2 reduction in support for the incumbent senator.

[18] These small effects arise in spite of large swings in statewide unemployment rates. The unemployment rate increased by 2.7, -2.6, and 0.8 percentage points between the third quarters of 1990 & 94, 1994 & 98, and 1998 & 2002, respectively. In fact, the actual vote swings for those elections were 6.8, -18.6 and 8.2 percentage points towards Republicans.

[19] This specification includes only the years 1994, 1998 and 2002 as turnout was not collected in 1990.

[20] The 1990 census provides citizenship by age and thus we can directly calculate voting age population. For 2000 age by citizenship is no longer available. We predict voting eligible population in 2000 using the following equation: voting age population (2000) = Number of citizens (2000) * Percent of citizens who are adults (1990) *Percent of population that is adult (2000)/Percent of population that is adult (1990). We obtain the voting age population for the remaining years by linear interpolation. Because we are concerned about the endogeneity (and potential measurement error) in our voting eligible population calculations, we also run the turnout specification using the log of total turnout as the dependent variable. Results are robust to this change.

et al. (2007) who find that losing the school choice lottery increases the likelihood that White parents vote in the proximate school board election.

That our results are largely robust to using either 1990 or 1980 industries as our anchor year lends confidence to the notion that our initial employment shares are not endogenous to industrial changes occurring in the 1990s. The one point of concern is in the specifications of column 4 which examine the impact of economic well being on the propensity to vote for the incumbent. Previous literature has shown that willingness to vote for the incumbent party is increasing in macro level economic prosperity. (See Fiorina (1978), for a review of the time series macro data literature. Fiorina (1978) and Markus (1988) are examples of the micro data approach.) The insignificant -.238 coefficient in the final cell of the table suggests that conditional on statewide economic conditions, relative community conditions do not have any additional impact on willingness to support the incumbent. However, the insignificance of this result is not robust to moving from the 1980 to the 1990 weights. This anomaly appears to be due to chance rather than endogeneity. When we run the incumbent specification using industry composition in the year 2000—a year in which endogeneity concerns would be greater than in 1990—we find a coefficient of -.31, closer to the results we obtain using the likely exogenous 1980 industrial shares, than to those we obtain using 1990 industries. Nonetheless, to be conservative, we will present results using 1980 industries for the remainder of the paper.

While the county*year fixed effects control for spatially correlated changes in labor supply and electoral preferences, we are still concerned that our Table 4 results may simply reflect concurrent neighborhood trends in employment and conservatism. Because

of the aggregate nature of our data, concurrent trends in employment and the political

leaning of who moves in and out of a census tract could also bias our results. Thus in

Table 5 we re-estimate the Democratic proposition voting equation allowing for tract

trends. For tractability, as we explain in the methods section, we move from a tract fixed

effects to a first difference model. To do so we collapse our data to tract/year cells—vote

share is now the average vote for a tract on all propositions on the ballot in that year—

and then first difference these cells.

As shown in the first column of Table 5 this specification, absent trend controls,

yields a coefficient of -.572 on PEI. In columns 2-5 we add tract fixed effects to the first

differences model to control for neighborhood trends. The relationship between PEI and

conservative voting is not only robust, but is strengthened by this additional control. As

we show in the second column of the table a ten percent increase in PEI decreases

Democratic voting by 7.8 percentage points. We are hesitant to scale this result by our

findings on employment. With only two years of employment data, we cannot control for

trends in these specifications. If we scale our Table 5 column 2 results by those of Table

3, we find that an employment increase of one percentage point increases conservative

voting by about two percentage points.[21]

Threats to Identification

A key threat to all difference-in-difference analyses is that the results are driven

by concurrent trends in *y* and *x* (in our case conservative leaning and employment or

movement of conservatives into a neighborhood and employment) rather than the impact

of *x* on *y*. In the remaining columns of the table we look for evidence on whether our

neighborhood trend specification has addressed this threat. We do so by examining

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[21] This estimate is still within the range of previous correlation estimates. (See footnote 17.)

whether the lead of PEI predicts voting in the current period. In the third column of the

table we add the one election lead of PEI to the model. The lead enters with a smaller in

magnitude and positively signed coefficient. The main effect remains large and negative.

While elections occur every two years, elections that are four years apart are greater in

similarity (i.e., both in presidential election years or not). In column 4 we repeat the

column 2 analysis substituting the two election lead for the lead of only one election. The

two election lead also yields a small and positive coefficient. In this specification it is

insignificant.[22] Concurrent PEI remains a large, negative and significant predictor of

voting. In the final column of the table we enter both the one and two election year leads

concurrently. The coefficient on the one election lead grows in magnitude, most likely

due to its correlation with the two election lead.[23] Concurrent PEI remains a large,

negative significant predictor of Democratic voting. Thus our lead PEI analysis suggests

that coefficients on our PEI variable reflect the impact of employment on voting, rather

than concurrent trends in the two variables. Accordingly, we continue to rely on the first

differenced/neighborhood trends specification throughout the remainder of the paper.[24]

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[22] The results of columns 3 and 4 are robust to including the lagged PEI along with the lead. The coefficient on PEI is -.751 (-1.137) in specifications containing 2 (4) year lags and leads.

[23] In principle one might look for a zero coefficient on the PEI leads shown in Table 5. However, our PEI represents only a proxy that measures the economic shock to a tract with error, and for any tract the PEI lead is constructed from the same weights, which when combined with short-run persistence in national employment shocks likely creates a correlation between measurement error in the contemporaneous and lead PEI's. Further, this measurement error is almost certainly exacerbated by the removal of tract fixed effects and trends. Under an assumption of classical measurement error, a positive correlation in measurement error over time implies a positive bias in the lead coefficient for a simple linear model, which is exactly what we find in column 3. (These derivations are available from the authors upon request.) Further, when we lengthen the lead to minimize the correlation between the errors, the estimate of the lead coefficient shrinks and becomes statistically insignificant. Finally, when we put both leads in the model, the coefficient on the one period lead gets very large as expected due to the presence of variables before and after with correlated measurement error.

[24] We also rely on this specification because of its fit. We have examined variations such as including the level of PEI in addition to the difference. The level entered insignificantly. We have also tried entering positive and negative employment shocks separately. While negative employment shocks yield a larger in magnitude coefficient, both are significant predictors of conservative ballot voting.

A second threat to identification arises from our reliance on aggregate data. There is the possibility that rather than reflecting changes in behavior, our results reflect changes in neighborhood resident composition. The concern is that a positive economic shock may draw relatively more conservatives into a neighborhood. This is a nontrivial issue given that in the year 2000 nineteen percent of residents in our sample tracts had moved into their residence within the past two years. This high degree of mobility has motivated our identification strategy. We control for county*year fixed effects so that we identify only off of relative changes in predicted tract employment within county years. Thus to the extent that people respond to changes in employment prospects by relocating to another county we have controlled for that. We control for the possibility of a correlation between within county location and PEI by including tract trends in our regressions. Neighborhoods generally develop or deteriorate over time; the tract level trends account for longer run changes in composition and imply that any compositional bias must arise from short run deviations from the 14 year linear trend.

To the extent that non-linear relocation is biasing our results, we would expect that our findings would be stronger in the neighborhoods with the most turnover. That is, if selection is the driver of our results we would expect economic conditions to have a larger (in magnitude) impact on conservative voting in the least stable neighborhoods. To examine this issue we define neighborhood stability in two ways: (1) by the share of housing whose occupants are short term (less than ten year) residents in 2000, and (2) by the share of owner occupied housing in 1990. Time in residence is the more direct measure of past mobility. Rates of future mobility fall with time in residence. However, if past mobility has been driven by non-economic factors then this measure may not

accurately identify those neighborhoods in which residents are least likely to relocate in response to economic shocks. We note that residence in owner occupied housing dramatically increases the transaction costs associated with moving and therefore should reduce the overall tendency to move for any reason including economic shocks.[25] Consistent with this view, Rosenthal (2008) finds that neighborhoods with higher shares of owner-occupied housing are much less likely to transition through the income distribution over time than neighborhoods with rental housing.

We split the sample at the median of each of the two measures. We define more stable as below median short-term residents and then as above median percent owner occupied. Using either measure of neighborhood stability, the analysis presented in Table 6 indicates that our results do not appear to be driven by unstable neighborhoods. In both cases point estimates are larger for more stable neighborhoods. In fact in the new resident definition, the coefficient from the more stable sample is nearly double that of the less stable.[26] Thus, the findings of Table 6 support the contention that the relationship

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[25] This is particularly true in California due to Proposition 13. Among other things, Proposition 13 prohibits the reassessment of homes for property tax purposes except when the house is sold. As noted by Ferreira (2008), the tax savings associated with this provision can be large. As a result, Proposition 13 creates a "lock-in" effect, since homeowners that choose to move may experience a substantial increase in their tax liability. O'Sullivan, Sexton, and Sheffrin (1995) use a simulation model to examine the impact of Proposition 13 on homeowner mobility and conclude that the magnitude of the "lock-in" effect is relatively large. See Wasi and White (2005) and Ferreira (2008) for empirical evidence that suggests Proposition 13 reduced homeowner mobility rates.

[26] We recognize that this result is only suggestive. More and less stable neighborhoods differ on a variety of dimensions in addition to stability and these other differences may drive the findings of Table 6. Secondly, there is not great variation in neighborhood mobility. For example, in our sample the 25th percentile tract still has 59% of population that are new residents in the last 10 years. Further we note the possibility of aggregation bias. In both the more and less stable samples, results may be driven by movers. Nonetheless the findings of Table 6, in conjunction with our identification strategy and trend analysis, provide support for the contention that results are not driven by mobility. In addition we examined tract demographic changes between the 1990 and 2000 censuses, and based on the voting patterns of different demographic groups the demographic change during that period was consistent with more liberal voting in tracts that saw an increase in PEI. Thus this demographic exercise, like the mobility analysis, suggests mobility may bias against our findings. Finally, we investigate models that control for lags (with and without controls for leads). The PEI lags likely capture the effect of neighborhood demographic change since such changes

between economic conditions and voting is due to changes in individuals' political behavior.[27]

Results by Issue Type

The results of Tables 5 and 6 indicate that positive economic conditions increase the tendency for individuals to vote conservatively. In this section we test economic theory more explicitly by examining how economic conditions affect voting by issue. Meltzer and Richard's (1981) theoretical contribution predicts that economic conditions affect votes on redistributive matters in particular. The theory is silent on non-redistributive matters. However, behavioral economists have demonstrated the relevance of cognitive consistency in political opinions. If voters use their economic circumstances to determine party preference as our gubernatorial results suggest, then we would expect economic conditions to predict voting on non-redistributive matters as well. We note that there is no innate reason why conservative views on redistributive and non-redistributive matters should be correlated. In fact what we in the United States refer to as conservative social views, are often part of a platform that includes what would be referred to as liberal economic views in European countries.

To examine the impact of economic conditions on voting by issue type we first code the 91 propositions by issue area: The first two types we call redistributive: 1) taxation and fiscal policy and 2) social welfare, which includes votes in the subcategories

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take time, and the estimated coefficients on our lags are large and their inclusion increases our main effect estimate again suggesting that mobility leads to bias against our findings.

[27] One caveat to our argument concerning owner-occupied housing is raised by Dorn (2009) who finds that due to concerns about property values, neighborhoods with whites residing primarily in owner-occupied housing tip more quickly towards racial segregation than those where whites reside in rental housing. Building on Card, Mas, and Rothstein's (2008, forthcoming) findings that racial tipping points had increased substantially by 1990 and that tipping appears to be one sided with neighborhoods stable when share white is above the tipping point, we reran the owner-occupied split for a subsample of tract with above median share of white residents and again find similar estimates of economic effects on voting across the two groups.

education, health and welfare. While education and health spending might be more readily thought of as public goods, Besley and Coate (1991) note that as long as the quality of the public good is not too high, some households will choose not to consume the public good, and thus public good provision will in fact be redistributive. The remaining categories are: 3) election, which includes campaigns, elections and public officials; 4) courts, which includes crime and crime adjudication; 5) government regulation, which includes energy, environment, gambling, health (regulations only), labor and miscellaneous regulations; and 6) transportation. The coding of the subcategories is based on "History of California Ballot Initiatives: 2002"[28] which lists citizens' initiatives by category. Appendix Table 1 lists all propositions by category.

Using this coding of propositions, we collapse our data into tract/year/proposition type cells and run a modified version of equation (6) in which we allow separate coefficients for the PEI main effect and PEI's interaction with redistributive propositions. The -.558 coefficient on PEI, shown in column 1 of Table 7, implies that a ten percentage point increase in PEI increases conservative voting on non-redistributive issues by 5.6 percentage points. Summing the main effect and the interaction we see that the impact of employment conditions on redistributive issues is even larger: a ten percentage point increase in PEI increases conservative voting on redistributive issues by over ten percentage points.

One explanation for the positive effect of economic conditions on conservative voting across categories is that issues in a variety of categories can have fiscal or redistributive consequences. For example Proposition 7 in 1998, which we code as environmental regulation, awards tax credits for reductions in air emissions. We consider

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[28] Available at http://www.sos.ca.gov/elections/init_history.pdf.

the possibility that bills with a fiscal impact in various categories are driving our non-redistributive proposition results. To investigate this possibility we recode ballots by whether their official summary, which appears on the ballot, explicitly mentions taxation or the issuance of bonds. As the Proposition 7 example illustrates, these words are not simply proxies for vote category. While the fiscal category is the one whose bills most frequently mention taxes explicitly, there are votes concerning campaign issues, regulation and transportation that also explicitly mention the word "tax" or "bond". (See Appendix Table 1 for a complete list of proposals and their tax/bond classification.)

We once again modify equation (6) to include in addition to the predicted employment index main effect, the interaction of the index with an indicator for the word "tax" or "bond" being mentioned in the bill summary. We see that our results are qualitatively robust to this change in coding (see column 2 Table 7). Once again we see that economic shocks increase conservative voting on votes across the board, but that the impact on redistributive votes is larger than for non-redistributive votes. However, with this alternative coding the difference in impact by vote type is not as large. The Table 7 basic findings suggest that the impact of economic shocks on conservative voting is driven by economic issues, which is consistent with economic theory.[29] In addition we find that economic conditions impact voting on non-economic issues which is consistent with recent work showing the relevance of cognitive consistency to the political arena.

Robustness

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[29] We caution that this result should not be interpreted as saying that the demand for poverty alleviation is decreasing in economic conditions, but more narrowly that the demand for publicly provided poverty alleviation is decreasing in economic conditions. Households may well view public and private giving as substitutes. The charitable giving literature has shown that income increases private giving (see, for example, Auten, Sieg, & Clotfelter, 2002).

The remaining columns of Table 7 examine the robustness of the results reported in columns 1 and 2. First we want to ensure that our results are driven by populations for whom a change in predicted employment should be most relevant. To that end we scale our index using the fraction of 1990 residents of working age (18-64). Reassuringly, as shown in columns 3 and 4, the estimated coefficients increase in magnitude; thus providing further evidence that employment conditions are the driver of our findings.[30]

An additional concern related to our predicted employment index is that it may be correlated spatially. Because of the similarity of their residents' employment patterns, economic shocks may not be independent across tracts. To allow for dependence, we cluster our standard errors at the county, rather than the tract level. This is an extremely conservative correction given that we control in all specifications for county*year fixed effects and thus are identifying solely based on within county variation. Results are shown in columns 5 and 6. While our standard errors increase five-fold, our results using the policy content coding type remain significant at conventional levels. However, the interaction term in the tax/bond specification is no longer significant implying that the impact of economic conditions on voting is uniform across issue type.

We are also concerned that because we classify a proposition as liberal or conservative based on the relative frequency of Democrats to vote yes on the proposition, there is a far greater possibility of misclassification for propositions in which our relative

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[30] While we are uncomfortable scaling our voting results by our employment results to ascertain the magnitude of the impact of employment change on voting change due to the absence of trends in the employment regressions, we do believe such a scaling is useful for comparisons across specifications. We note that when we scale the results of columns 1-2 and 3-4 the two specifications imply nearly identical impacts. Coefficients from basic and employment-scaled regressions of employment change on PEI yield coefficients of .384 (Table 3) and .514 respectively. Thus Table 7 results imply that a one percentage point increase in employment increases conservative voting on fiscal/social propositions by 2.7 (2.7) percentage points and non-fiscal/social propositions by 1.46 (1.5) percentage points in the basic (scaled) specification. The alternative coding yields employment impacts of 2.62 (2.47) percentage points on tax/bond propositions and 1.96 (2.07) on non-tax/bond propositions.

propensity measure is close to zero. In columns 7-8 we demonstrate that our results are qualitatively robust to restricting attention to the 78 of 91 propositions with relative propensity scores of greater than .1 in absolute value. However, restricting attention to these propositions suggests a much larger differential in the impact of economic conditions on redistributive over non-redistributive ballot propositions, regardless of coding method.

In summary, the results of Table 7 provide evidence that voting on economic issues is motivated by economic self-interest. Our finding that positive economic shocks decrease support for redistributive policies is robust to a variety of specifications. We additionally find support for the relevance of cognitive consistency in voting. Positive economic shocks not only increase support for conservative economic policy, but for conservative policies more generally.

Results by Tract Type

We have shown that economic conditions have a causal impact on residents' economic and non-economic policy views in the average neighborhood. But we do not know whether this aggregate homogeneity reflects individual heterogeneity. We are limited in our ability to address this issue because of the aggregate nature of our voting data. Nonetheless we can examine our Table 7 results by census tract type to provide suggestive evidence on heterogeneity and to shed light on the mechanisms by which economic conditions affect voting.

If the causal relationship between economic conditions and economic voting is driven by self-interest, as Meltzer and Richard (1981) posit, then we would expect those who have the most to gain from redistributive programs to be most influenced by

economic shocks. Redistributive programs are targeted primarily at the lower class. Thus,

we divide our tracts into quartiles based on their share of residents in poverty in 1990 and

then examine the impact of economic shocks on employment levels and voting across

proposition type for each of these four groups. As shown in Panel A of Table 8, the

impact of PEI on employment is fairly similar across the four quartiles of poverty. A ten

percent increase in PEI increases employment 3.6 to 4.7 percentage points. Nonetheless

the results of the remaining panels indicate that the impact on voting is not uniform

across tract type. The results reported in Panel B indicate that a ten percent increase in

PEI increases conservative voting an insignificant .7 percentage points in the least poor

tracts, 2.4 and 2.5 percentage points in the middle tracts and 5.3 percentage points in the

most poor tracts.[31] Examining voting by ballot type suggests additional heterogeneity.

Using the policy content coding we find that impacts for tracts in the second, third and

highest quartile of poverty are driven solely by the redistributive votes while the impacts

for the least poor tracts are driven solely by the non-economic votes. This pattern is

robust to a change to the tax/bond coding with one exception: Using this coding,

economic conditions impact voting on both economic and non-economic issues for voters

in tracts in the second quartile. Voters in the more (less) well to do tracts still only see

impacts on non-economic (economic) issues. Table 8 provides suggestive evidence that,

in accordance with economic theory, those who have the most to gain from economic

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[31] Scaling by the impact of PEI on employment (Panel A) does not change the rank ordering of the impact of employment conditions on conservative voting: scaled coefficients from least to most poor quartile are .214, .508, .67 and 1.36.

policy are those whose voting on economic issues is most sensitive to economic conditions.[32]

In addition to being concentrated amongst those voters most likely to benefit from redistribution, we would further expect the impact of economics on economic voting to be most concentrated amongst those who are most comfortable with the idea of redistribution. Democrats are more favorable toward redistributive policies than Republicans. Therefore we divide the tracts into two groups based on the share of residents in 1990 who were registered Democrats. As the final two columns of Panel A indicate, our economic shocks, as measured by PEI, have a significant impact on employment in both types of neighborhoods. However, that impact is about twice as large in areas with an above median share of Democratic residents as compared to neighborhoods with a below median share. In Panel B we see that, despite the significant impact on employment in both types of neighborhoods, on average the impact of economic shocks on voting is only significant in the more Democratic neighborhoods. A ten percent increase in PEI increases conservative voting 8.3 percentage points in Democratic neighborhoods, but has no impact on voting in Republican neighborhoods.[33] When we examine these results by vote type, we learn that the Republican neighborhood story is more nuanced. Economic conditions increase conservative voting on economic issues, but have no significant impact on non-economic issues. The impacts on economic voting in Democratic neighborhoods are more than twice as large, suggesting that more than the simple difference in employment impacts is driving the difference in voting

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[32] This result is robust to redefining the most likely to benefit as the neighborhoods with the most children, in accordance with the fact that children are most often targets of redistributive programs.

[33] Scaled coefficients differ by more than an order of magnitude. They are .106 and 1.63 for below and above median Democrat respectively.

impacts. We argue that a greater comfort with the idea of redistribution amongst Democrats may be a part of the reason for differential impacts. We further find that the Democratic neighborhoods see impacts on both economic and non-economic issues, with larger impacts on the former. A need for cognitive consistency seems to be at play in these more Democratic communities. More generally, the results of the final two columns of Table 8 are comforting in that they suggest that the impacts of economic shocks on economic voting are concentrated not only amongst a population that sees larger swings in employment due to these shocks, but also amongst a population that would actually be receptive to the idea of government intervention in the economy.

Examining results by tract type has provided evidence that the impact of economic conditions on voting is largest among those populations who 1) are most likely to benefit from redistributive economic policy and 2) are most likely to be in favor of redistribution as a concept. But because of the aggregate nature of our data these results are only suggestive. In the future, we hope to obtain panel data on individual level policy positions and economic circumstances in order to better explore issues of heterogeneity.

Voter Turnout

We have found robust evidence that positive economic conditions affect neighborhood residents' tendencies to vote conservatively on both redistributive and non-redistributive issues. But again, because of the aggregate nature of our data, we do not know how the composition of voters varies across years. Are seasoned voters changing their views or are new voters coming to the polls as a community's economic conditions improve? Both mechanisms reflect changing political views and behavior and by either mechanism the result that positive economic conditions increase voters' support for more

conservative policies is policy relevant. Nonetheless, it is interesting to understand whether our results are driven primarily by changing views or changing voter composition.[34]

In Table 9, we present suggestive evidence that our results are driven by the former. The first column of the table demonstrates that the negative impact of PEI on turnout is robust to controlling for tract level trends. We use seven years of election data: the three years of off-year turnout data that we employed in Table 4 and turnout data for our four presidential election years.[35] Thus the column 1 results provide evidence that our proposition voting results may reflect changes in the composition of the electorate.

However, the remainder of the table suggests that this possibility is unlikely. We next examine how our proposition voting results are altered by including a control for the change in turnout. In the second column of the table we estimate our proposition voting equation using only the years 1992-2004, the years for which we have turnout data. We see that a ten percent increase in PEI increases conservative voting 8.5 percentage points in that sample. In the next column we run the same specification but include a control for turnout. If our results are attenuated then that would suggest that the relationship between PEI and conservative voting is mediated through turnout. However, results are little changed. We still find that a ten percent increase in PEI increases conservative voting by about 8.5 percentage points. The coefficient on turnout is positive as expected since increased turnout is generally associated with gains for the Democrats. The evidence of columns 2 and 3 does not suggest that our voting results are driven by changes in turnout.

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[34] We have also explored the impact of economic conditions on party registration using tract trend models, but unfortunately our estimates are not precise enough to be informative.
[35] Recall that we do not have turnout data for 1990.

The remaining columns of the table provide additional evidence to that end. If turnout were driving our results, we would expect to see the largest impact of PEI on turnout in the tracts in which we see the largest impact of PEI on Democratic proposition voting. We find the opposite. In columns 4-7 we examine the impact of PEI on turnout for tracts by poverty quartile. We find that the PEI has the largest (in magnitude) impact on turnout in the lowest poverty tract and in fact has no statistical impact on the highest poverty tracts, despite the fact that we find that the impact of PEI on proposition voting is largest (in magnitude) in the highest poverty tracts and not statistically significant in the lowest poverty tracts. We perform the same test dividing tracts by their share of Democratic residents. Recall that PEI increased conservative voting much more in those tracts with an above median share of Democratic registrants in 1990. However, we show in columns 8 and 9 that the impacts of PEI on turnout are similar across the two tract types, and in fact point estimates suggest a slightly larger (in magnitude) impact on the below median Democratic tracts. Thus, the results of Table 9 suggest that positive economic conditions increase conservative voting by altering voters' views.[36]

IV.   CONCLUSION

We have used employment shocks and a panel of neighborhood voting on various ballot propositions to identify the impact of economic conditions on the voting behavior

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[36] Another question of interpretation is whether people change their preferences or voting in direct response to the economic shock or in response to local actions of political parties that change as the economic circumstances of residents change. We believe that our estimates likely capture the direct effect of economic shocks on voting because our model is identified by within county differences in changes in the economic circumstances. Thus, for party behavior to affect our estimates the parties must be acting at the neighborhood level through grass roots actions, with little across neighborhood spillover, rather than media based campaigns. Further, given the inclusion of linear trends, these changes in grass root organizing would have to be very reactive to neighborhood economic circumstances in order to create short run increases and decreases in resources expended that co-vary with non-linear changes in economic circumstances.

of neighborhood residents. We show that positive employment shocks increase support for more conservative state ballot propositions concerning redistribution, particularly in neighborhoods that are most likely to benefit from redistribution. Thus our results provide empirical support for Meltzer and Richard's (1981) theoretical prediction that due to self-interest, support for redistribution decreases in economic well-being. We further find that economic conditions increase the tendency for residents' to vote conservatively on non-economic ballot issues. We therefore add to a small, but growing literature, demonstrating the relevance of cognitive consistency to the voting arena.

DATA APPENDIX

Converting precinct to tract level voting data

  For statewide elections that occurred between 1992 and 2000, the Institute of Governmental Studies (IGS) at the University of California at Berkeley matched precinct-level vote returns and voter registration information to 2000 census blocks and then aggregated the data to the 2000 census tract level.[37] For the 1990 general election, the IGS matched precinct-level vote returns and voter registration information to 1990 census blocks. Consequently, we use census block relationship files, provided by the U.S. Census Bureau, to aggregate the 1990 census block data to the 2000 census tract level. For all statewide elections occurring after 2000, the IGS only makes available precinct-level vote returns and voter registration information. However, the precinct level data can be aggregated to the 2000 census tract level using conversion files that the IGS makes available for each election. We use these election specific conversion files to convert all election results from 2002 forward to the 2000 census tract level.[38]

Obtaining an Inter-Geographic-Level Comparable Time Series on Employment

  Our research design requires both industry data that describe the industrial composition of neighborhood residences at the census tract level at a fixed point in time and that describe changes in industry employment over time at the national and state levels. The United States Bureau of Labor Statistics (BLS) produces a comparable time series of national and state industry annual employment using the North American

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[37] To match voting precincts to census blocks, the IGS used a straight proportional merge. In cases where voting precincts crossed the boundaries of census blocks, the IGS used the proportion of voters assigned to each census block as a weight to allocate vote returns to census blocks.

[38] The number and geographic composition of voting precincts changes from election to election. Thus, election specific "voting precinct to census block" conversion files are needed to match precinct level vote returns to 2000 census tracts.

Industry Classification System (NAICS) definitions. However, BLS does not provide the tract level industrial employment data we need.

The United States Census Bureau's decennial censuses provide the only information on industrial composition of resident workers down to the census tract level. A further complication is that because of the changes in industrial classification systems over time, the 2000 censuses rely on the NAICS classifications, but the 1980 and 1990 censuses are based on the previous classification system, The Standard Industrial Classification (SIC) system. Thus only the 2000 tract level industry codes match our 1990-2004 annual state and national employment data industry codes. Hence, in order to obtain a pre-period measure of tract level employment, we are forced to predict 2000 industrial employment shares using the 1990 (or 1980) industrial employment shares.

The industries identified in each year are identified in the following table:

	1980 tract (SIC codes)	1990 tract (SIC codes)	2000 tract (NAICS codes)	National annual data
Agriculture, Forestry and Fishery		√		
Agriculture, Forestry, Fishery and Mining	√			
Agriculture, Natural Resource and Mining			√	
Natural Resources and Mining				√
Mining		√		
Construction	√	√	√	√
Manufacturing			√	√
Manufacturing—nondurables	√	√		
Manufacturing—durables	√	√		
Wholesale Trade	√	√	√	√
Retail Trade	√	√	√	√
Transportation	√	√		
Transportation and Warehousing			√	√
Communication and Other Public Utility	√	√		
Utilities			√	√
Information			√	√
Finance and Insurance			√	√
Real Estate, and Rental and Leasing			√	√
Finance, Insurance and Real Estate	√	√		
Business and Repair Services	√	√		
Personal Services		√		
Personal Entertainment and Recreation Services	√			
Professional, Scientific and Technical Services			√	√
Management of Companies and Enterprises			√	√
Administrative and support and Waste Management Services			√	√
Educational Services	√	√	√	√
Health Care and Social Assistance			√	√
Health Services	√	√		
Entertainment and Recreation Services		√		
Arts, Entertainment and Recreation			√	√
Accommodation and Food Services			√	√
Other Professional and Related Services	√	√		
Other Services			√	√
Public Administration	√	√	√	√

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Table 1: Summary Statistics

	Gubernatorial Voting Panel (6777 tracts*4 elections=27,108)	Ballot Propositions Voting Panel (6777 tracts*91 propositions=616,707)
Dependent Variables		
Voting for Democrat/Democratic side	.53 (.18) [27096]	.45 (.16) [616516]
Turnout	.33 (.16) [20331]	.39 (.16) [616707]
Voting for Incumbent (of two party voting)	.49 (.18) [27096]	
Independent Variables		
Predicted Employment Index, 1990 weights	-2.91 (.14) [27076]	-2.91 (.14) [615979]
Predicted Employment Index, 1980 weights	-2.91 (.13) [27056]	-2.91 (.13) [615524]
Years	1990, 1994, 1998, 2002	1990-2004, even years

Notes: Means are weighted by tract voting age population. Voting for Democrat is fraction of two-party voting. Standard deviations are in parentheses and sample sizes are in brackets. Turnout can only be calculated for the Gubernatorial elections of 1994, 1998, and 2002 because total number of votes cast was not collected in 1990. All sample sizes exhibit minor variation within columns because of data availability.

Table 2: Descriptive Look at Tract Voting Patterns, Dependent Variable is Share Voting Democratic

Variable	Means	Governor	All	Propositions Social/Fiscal	Other
Income ($10,000)	4.55	-.04**	-.01**	-.02**	-.01**
	(1.61)	(.00)	(.00)	(.00)	(.00)
Urban	.93	.05**	.02**	.03**	.02**
	(.22)	(.01)	(.00)	(.00)	(.00)
Black	.07	.72**	.25**	.29**	.23**
	(.13)	(.01)	(.00)	(.00)	(.00)
Asian	.09	.29**	.07**	.10**	.05**
	(.10)	(.03)	(.01)	(.01)	(.01)
Hispanic	.21	.39**	.12**	.14**	.11**
	(.19)	(.02)	(.04)	(.01)	(.00)
White	.63				
	(.26)				
Other race	.01	.62**	.12**	.14**	.11**
	(.01)	(.17)	(.04)	(.05)	(.04)
17 and under	.25	-.41**	-.13**	-.12**	-.14**
	(.08)	(.04)	(.01)	(.01)	(.01)
65 and over	.11	.06	.01	.02	.01
	(.08)	(.04)	(.01)	(.01)	(.01)
Foreign born	.19	.17**	.09**	.08**	.09**
	(.13)	(.03)	(.01)	(.01)	(.01)
College	.24	.41**	.14**	.19**	.10**
	(.16)	(.02)	(.01)	(.01)	(.01)
Employed	.63	.13**	.04**	.04**	.05**
	(.11)	(.04)	(.01)	(.01)	(.01)
Owner occupied	.59	.04**	-.01*	-.01**	-.00
	(.23)	(.01)	(.00)	(.00)	(.00)
Ethnic heterogeneity	.42	-.09**	-.02**	-.03**	-.02**
	(.17)	(.01)	(.00)	(.00)	(.00)

Notes: In column 2 standard deviations in parentheses; in remaining columns standard errors in parentheses. The 1990 census tract variables are defined as percent of population, except in the case of owner occupied and income which are normalized by housing units, and ethnic heterogeneity which is defined, as in Alesina

and La Ferrara (2000) as $1 - \sum_{k} s_{k}^{2}$ where k are the five racial groups and s is the share of the tract population who belong to the racial group. Regressions also

control for percent poverty. The sample size for the regressions is 6769. Regressions weighted by tract voting age population.
**denotes significance at the 1 percent level, * at the 5 percent level.

Table 3: Relationship Between Predicted Employment Index and Employment

	Employment/population, metropolitan counties biennially, 1990-2004	Employment/population, metropolitan counties, 1990 and 2000	Employment/population, metropolitan census tracts, 1990 and 2000	Employment/population, metropolitan census tracts, 1990 and 2000
Predicted Employment Index, 1990 weights	.552 (.462) [296]	.16 (.234) [74]	.397** (.044) [13538]	.408** (.048) [13538]
Predicted Employment Index, 1980 weights	.556 (.436) [296]	.299 (.222) [74]	.399** (.044) [13528]	.389** (.048) [13528]
Mean (SD) Dependent Variable in Sample	.61 (.04)	.63 (.04)	.61 (.11)	.61 (.11)
County*year fixed effects	No	No	No	Yes

Notes: Each cell in the first two rows presents the estimated coefficient on the PEI from a different regression. All specifications control for county (or tract in columns 3-4) and year. Column 4 also includes county*year fixed effects. Sample size in brackets. Robust standard errors clustered by county (or tract in columns 3-4). Regressions weighted by voting age population. **denotes significance at the 1 percent level, * at the 5 percent level.

Table 4: Impact of Changes in Predicted Employment on Voting Outcomes

	Share Voting Democratic on Propositions	Share Voting for Democratic Gubernatorial Candidates	Share Turning Out in Gubernatorial Elections	Share Voting for Incumbent Party Gubernatorial Candidates
Predicted employment index, 1990 weights	-.450** (.012) {-.011} [615788]	-.523** (.034) {-.013} [27064]	-.380** (.111) {-.009} [20307]	-.699** (.124) {-.017} [27064]
Predicted employment index, 1980 weights	-.450** (.012) {-.012} [615362]	-.474** (.036) {-.012} [27045]	-.380** (.108) {-.010} [20292]	-.238 (.126) {-.006} [27045]

Notes: Each cell presents the estimated coefficient on the PEI from a different regression using a panel of metropolitan census tract voting returns. In column 1 each observation is a proposition; in the remaining columns each observation is an election. All specifications control for tract and county*year effects. Robust standard errors clustered by tract in parentheses. The figure immediately below the standard errors is the implied change in outcome that results from a one percentage point increase in employment. Sample size in brackets. Regressions weighted by tract voting age population. **denotes significance at the 1 percent level, * at the 5 percent level.

Table 5: Impact of Changes in Predicted Employment on Democratic Proposition Voting, First Difference Estimates

	(1)	(2)	(3)	(4)	(5)
Predicted employment index	-.572**	-.777**	-.865**	-1.061**	-1.079**
	(.02)	(.064)	(.074)	(.097)	(.101)
One election lead of predicted employment index			.243*		.613**
			(.107)		(.115)
Two election lead of predicted employment index				.158	.25
				(.19)	(.184)
Tract fixed effects to control for tract trends	no	yes	yes	yes	yes
N	47331	47331	40577	33813	33813

Note: Each cell represents a different regression specification. All specifications estimated in first differences using 1980 PEI, controlling for county*year and tract fixed effects to allow for tract specific trends. Robust standard errors clustered by tract in parentheses. Regressions weighted by tract voting age population. **denotes significance at the 1 percent level, * at the 5 percent level.

Table 6: Impact of Changes in Predicted Employment on Voting Democratic, by Neighborhood Stability

Definition of Stable:	More Stable	Less Stable
Below Median New Residents, 2000	-.991**	-.537**
	(.088)	(.081)
	[23666]	[23609]
Above Median Percent Owner Occupied, 1990	-.704**	-.698**
	(.08)	(.086)
	[23666]	[23630]

Note: Each cell represents a different regression specification. All specifications estimated in first differences using 1980 PEI, controlling for county*year and tract fixed effects to allow for tract specific trends. Robust standard errors clustered by tract in parentheses. Regressions weighted by tract voting age population. **denotes significance at the 1 percent level, * at the 5 percent level.

Table 7: Impact of Changes in Predicted Employment on Democratic Proposition Voting, by Proposition Type

	Basic		PEI scaled by employment aged population		Cluster by county		Most partisan propositions	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Predicted employment index	-.558**	-.754**	-.771**	-1.064**	-.558	-.754*	-.408**	-.486**
	(.065)	(.065)	(.108)	(.103)	(.332)	(.268)	(.074)	(.075)
Predicted employment index*fiscal/social	-.475**		-.596**		-.475**		-.776**	
	(.033)		(.051)		(.163)		(.031)	
Predicted employment index*tax/bond		-.257**		-.228**		-.257		-.936**
		(.044)		(.068)		(.28)		(.056)
Tract trends	yes	yes	yes	yes	yes	yes	yes	yes

Note: Each column represents a different regression specification. All specifications estimated in first differences using 1980 PEI, controlling for county*year and tract fixed effects to allow for tract specific trends. Robust standard errors clustered by tract (or county in columns 5 and 6) in parentheses. Regressions weighted by tract voting age population. Sample size is 94661. **denotes significance at the 1 percent level, * at the 5 percent level.

Table 8: Impact of Changes in Predicted Employment on Employment and Democratic Voting, By Tract Type

	Poverty				Democratic Registration	
	Lowest Quartile *Below 4 percent poverty*	Second Quartile *Four to eight percent poverty*	Third Quartile *Eight to fifteen percent poverty*	Fourth Quartile *Above 15 percent poverty*	Below Median *Up to 56 percent registered Democrats*	Above Median *56-98 percent registered Democrats*
Outcome: Employment						
Panel A						
Predicted Employment Index	.355**	.474**	.373**	.387***	.235**	.51**
	(.108)	(.123)	(.083)	(.096)	(.074)	(.061)
	[3372]	[3384]	[3386]	[3378]	[6748]	[6772]
Outcome: Democratic Voting						
Panel B						
Predicted employment index	-.076	-.241*	-.251*	-.526**	-.025	-.831**
	(.116)	(.113)	(.12)	(.101)	(.078)	(.085)
	[11798]	[11839]	[11848]	[11818]	[23606]	[23701]
Panel C						
Predicted employment index	-.218*	-.18	.195	-.004	.115	-.533**
	(.11)	(.108)	(.129)	(.106)	(.079)	(.09)
Predicted employment index*fiscal/social	.28**	-.301**	-.975**	-.956**	-.238**	-.71**
	(.049)	(.058)	(.075)	(.08)	(.045)	(.047)
	[23596]	[23678]	[23695]	[23636]	[47212]	[47401]
Panel D						
Predicted employment index	-.339**	-.387**	-.028	-.082	.077	-.84**
	(.112)	(.121)	(.13)	(.122)	(.077)	(.094)
Predicted employment index*tax/bond	.363**	.017	-.564**	-.907**	-.27**	-.246**
	(.069)	(.079)	(.094)	(.113)	(.058)	(.066)
	[23596]	[23678]	[23695]	[23636]	[47212]	[47401]

Note: Each column represents a model specification, and each panel in a column represents a separate regression. All specifications estimated in first differences using 1980 PEI, controlling for county*year and tract fixed effects to allow for tract specific trends. Robust standard errors clustered by tract in parentheses. Sample size in brackets. Regressions weighted by tract voting age population. **denotes significance at the 1 percent level, * at the 5 percent level.

Table 9: Impact of Predicted Employment Index on Democratic Voting, Exploring the Role of Turnout

	Turnout	Democratic Proposition Voting				Turnout			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Predicted Employment Index	-.871** (.194)	-.854** (.071)	-.848** (.071)	-.951* (.459)	-.841* (.41)	-.786* (.335)	-.011 (.315)	-.697* (.288)	-.67* (.263)
Turnout			.007* (.002)						
N	[40584]	[40577]	[40577]	[10116]	[10152]	[10158]	[10134]	[20250]	[20310]
Sample	All tracts	All tracts	All tracts	Lowest Poverty Quartile	Second Poverty Quartile	Third Poverty Quartile	Highest Poverty Quartile	Below Median Democrats	Above Median Democrats

Note: Each column represents a different regression specification. All specifications estimated in first differences using 1980 PEI, controlling for county*year and tract fixed effects to allow for tract specific trends. Robust standard errors clustered by tract in parentheses. Sample years: 1992-2004; sample size in brackets. Regressions weighted by tract voting age population. **denotes significance at the 1 percent level, * at the 5 percent level.

Appendix Table 1: propositions on California General Election Ballots, 1990-2004

Year	#	Description	Sub-Category	Politics	Outcome	Initiative	Bond	Tax
		Campaigns, Elections and Public Officials						
1996	208	Limits campaign contributions.	campaign reform	Republican	Passed	yes	no	no
1996	212	Repeals law limiting gifts and honoraria for public officials.	campaign reform	Democratic	Failed	yes	no	no
2000	34	Limits campaign contributions and loans to state candidates and parties.	campaign reform	Democratic	Passed	no	no	no
1990	131	Limits terms, gifts and behaviors of various statewide offices.	elected officials	Democratic	Failed	yes	no	no
1990	140	Term limits for various offices.	elected officials	Republican	Passed	yes	no	no
1992	164	Establishes congressional term limits.	elected officials	Republican	Passed	yes	no	no
2000	33	Allows legislatures to participate in the Public Employees' Retirement System.	elected officials	Democratic	Failed	no	no	no
~~1990~~	~~137~~	~~Requires voter approval for changes to initiative or referendum procedure.~~	~~elections~~		~~Failed~~	~~yes~~	~~no~~	~~no~~
1994	183	Allows longer between signatures and recall to consolidate elections.	elections	Republican	Passed	no	no	no
1998	3	Establishes partisan primary for president.	elections	Democratic	Failed	no	no	no
2002	52	Allows for election day registration.	elections	Democratic	Failed	yes	no	no
2004	60	Top vote getter from each party primary advances to general election.	elections	Democratic	Passed	no	no	no
2004	62	Establishes non-partisan primaries.	elections	Republican	Failed	yes	no	no
2004	59	Allows public access to meetings of government bodies.	public officials	Republican	Passed	no	no	no
		Courts						
~~1990~~	~~129~~	~~Funds for drug enforcement, treatment and gang-related purposes.~~	~~courts~~		~~Failed~~	~~yes~~	~~yes~~	~~no~~
~~1990~~	~~133~~	~~Establishes funds for drug education, treatment and enforcement.~~	~~courts~~		~~Failed~~	~~yes~~	~~no~~	~~yes~~
1990	139	Allows public entities, businesses and others to contract for inmate labor.	courts	Republican	Passed	yes	no	yes
~~1990~~	~~144~~	~~Construction to relieve overcrowding of state prisons.~~	~~courts~~		~~Failed~~	~~no~~	~~yes~~	~~no~~
~~1990~~	~~147~~	~~Funds for correctional facilities.~~	~~courts~~		~~Failed~~	~~no~~	~~yes~~	~~no~~
~~1990~~	~~150~~	~~Funds for physical infrastructure of county courthouses.~~	~~courts~~		~~Failed~~	~~no~~	~~yes~~	~~no~~
1994	184	Increases sentences for felons with prior convictions.	courts	Republican	Passed	yes	no	no
1994	189	Adds felony sexual assault to crimes excepted from right to bail.	courts	Republican	Passed	no	no	no
1994	190	Transfers authority to discipline judges to commission.	courts	Republican	Passed	no	no	no
1994	191	Eliminates justice courts; elevates existing justice courts to municipal courts.	courts	Republican	Passed	no	no	no
1996	205	Funds for correctional facilities.	courts	Democratic	Failed	no	yes	no
1996	207	Prohibits restrictions on negotiation of attorneys' fees.	courts	Democratic	Failed	yes	no	no
1996	211	Prohibits restrictions on attorney-client fee arrangements.	courts	Democratic	Failed	yes	no	no
1996	213	Denies damage recovery to felons whose injuries were caused during felony.	courts	Republican	Passed	yes	no	no
2000	36	Requires probation and drug treatment, not incarceration, for some drug crimes.	courts	Democratic	Passed	yes	no	no
2002	48	Amends constitution to delete outdated references to municipal courts.	courts	Democratic	Passed	no	no	no
2004	64	Allows "unfair business" lawsuits only if actual loss suffered.	courts	Republican	Passed	yes	no	no
2004	66	Limits "Three Strikes" Law to violent and/or serious felonies.	courts	Democratic	Failed	yes	no	no
2004	69	Requires collection of DNA samples from all felons and certain arrestees.	courts	Republican	Passed	yes	no	no

Appendix Table 1: propositions on California General Election Ballots, 1990-2004 (continued)

Year	#	Description	Sub-Category	Politics	Outcome	Initiative	Bond	Tax
		Regulation						
1998	9	Regulates charges of electric companies.	energy	Democratic	Failed	yes	yes	no
1990	128	Regulates pesticides.	environment		Failed	yes	yes	no
1990	130	Allows public acquisition of forests providing wildlife habitat.	environment	Democratic	Failed	yes	yes	no
1990	132	Establishes marine protection zone.	environment		Passed	yes	no	no
1990	135	Regulates pesticides.	environment		Failed	yes	no	no
1990	138	Funds for forestry projects and restoration.	environment		Failed	yes	yes	no
1990	141	Prohibits business from discharging carcinogens into water.	environment		Failed	yes	no	no
1990	148	Funds for water conservation.	environment		Failed	no	yes	no
1990	149	Funds for recreation, greenbelt, wildland, coastal, historic or museum purposes.	environment		Failed	no	yes	no
1996	204	Funds to ensure safe drinking water.	environment	Democratic	Passed	no	yes	no
1998	7	Awards state credits to encourage air-emissions reduction.	environment	Democratic	Failed	yes	no	yes
2002	50	Bonds for water and wetland projects.	environment	Democratic	Passed	yes	yes	no
1998	5	Specifies terms of mandatory compacts for Indian gambling casinos.	gambling	Democratic	Passed	yes	no	no
2004	68	Authorizes tribal gambling or non-tribal if tribes do not accept.	gambling	Democratic	Failed	yes	no	no
2004	70	Tribes entering state gambling compact would pay state based on gambling income.	gambling	Democratic	Failed	yes	no	no
1994	188	Bans public smoking with significant exceptions.	government regulati	Democratic	Failed	yes	no	no
1998	4	Prohibits trapping certain types of animals and use of certain methods.	government regulati	Democratic	Passed	yes	no	no
1998	6	Prohibits sale/slaughter of horses for horsemeat for human consumption.	government regulati	Democratic	Passed	yes	no	no
1990	124	Local hospital districts may own stock in health care related businesses.	health regulation		Failed	no	no	no
1992	161	Allows for physician assisted death.	health regulation	Democratic	Failed	yes	no	no
1996	215	Legalizes marijuana for medical use.	health regulation	Democratic	Passed	yes	no	no
2004	71	Establishes institute to regulate and fund stem cell research.	health regulation	Democratic	Passed	yes	yes	no
1992	166	Requires employers to provide health care coverage for employees.	labor	Democratic	Failed	yes	no	no
1996	209	Prohibits public discrimination on race, sex, color, ethnicity or national origin.	labor	Republican	Passed	yes	no	no
1996	210	Increases the state minimum wage.	labor	Democratic	Passed	yes	no	no
2004	72	Requires health care coverage for employees.	labor	Democratic	Failed	yes	no	no

Appendix Table 1: propositions on California General Election Ballots, 1990-2004 (continued)

Year	#	Description	Sub-Category	Politics	Outcome	Initiati	Bond	Tax
		Social Welfare						
1990	143	Funds for physical infrastructure of colleges and universities.	education	Democratic	Failed	no	yes	no
1990	146	Funds for physical infrastructure for public schools.	education	Democratic	Passed	no	yes	no
1990	151	Funds for child care facilities.	education	Democratic	Failed	no	yes	no
1992	155	Funds for physical infrastructure for public schools.	education	Democratic	Passed	no	yes	no
1998	8	Creates permanent fund for reducing class size.	education	Democratic	Failed	yes	no	no
1998	10	Creates commission for early childhood smoking prevention programs.	education	Democratic	Passed	yes	no	yes
1998	1A	Relieve public school overcrowding. Repair older schools.	education	Democratic	Passed	no	yes	no
2000	38	Authorizes annual state per pupil payments to private/religious schools.	education	Republican	Failed	yes	no	no
2000	39	Bonds for repair or construction of school facilities.	education	Democratic	Passed	yes	yes	yes
2002	47	Relieves public school overcrowding. Repair older schools.	education	Democratic	Passed	no	yes	no
2002	49	Increases state grant funds for before/after school programs.	education	Democratic	Passed	yes	no	no
1994	186	Establishes state health insurance system	health	Democratic	Failed	yes	no	yes
1996	214	Prohibits health care business from denying care without examination.	health	Democratic	Failed	yes	no	no
1996	216	Imposes new taxes on health care businesses.	health	Democratic	Failed	yes	no	no
2004	61	Grants to children's hospitals for physical structural improvements.	health	Democratic	Passed	yes	yes	no
2004	63	Establishes 1% tax on income above $1 million for mental health services.	health	Democratic	Passed	yes	no	yes
2004	67	Increases telephone surcharge and allocates other funds for emergency services.	health	Democratic	Failed	yes	no	yes
~~1990~~	~~142~~	~~Farm and home aid for veterans.~~	~~social welfare~~		~~Passed~~	~~no~~	~~yes~~	~~no~~
~~1990~~	~~145~~	~~Funds for first time home buyers and earthquake safety.~~	~~social welfare~~		~~Failed~~	~~no~~	~~yes~~	~~no~~
1992	162	Grants board of public employee retirement system investment authority.	social welfare	Democratic	Passed	yes	no	no
1994	187	Makes illegal aliens ineligible for public social services.	social welfare	Republican	Passed	yes	no	no
1996	206	Farm and home aid for veterans.	social welfare	Democratic	Passed	no	yes	no
2000	32	Farm and home aid for veterans.	social welfare	Democratic	Passed	no	yes	no
2002	46	Provides housing assistance.	social welfare	Democratic	Passed	no	yes	no

Appendix Table 1: propositions on California General Election Ballots, 1990-2004 (continued)

Year	#	Description	Sub-Category	Politics	Outcome	Initiative	Bond	Tax
		Taxation and Fiscal Policy						
1992	158	Replaces Legislative Analysis with California Analyst.	fiscal	Democratic	Failed	no	no	no
1992	159	Establishes auditor general as a constitutional office.	fiscal	Democratic	Failed	no	no	no
1992	165	Allows governor to declare "fiscal emergency" when budget not balanced.	fiscal	Republican	Failed	yes	no	no
1994	185	Increases tax on gas to go to transit and highway funds.	fiscal	Democratic	Failed	yes	no	yes
1998	11	Authorizes local governments to enter into sales tax revenue sharing by vote.	fiscal	Republican	Passed	no	no	no
2000	35	Eliminates restrictions on state, local, contracting.	fiscal	Republican	Passed	yes	no	no
2004	60A	Requires proceeds from surplus state property be used to pay off bonds.	fiscal	Republican	Passed	no	yes	no
1990	126	Adds alcohol beverage excise tax rates to constitution.	taxation	Democratic	Failed	no	no	yes
~~1990~~	~~127~~	~~Excludes earthquake safety improvements from property tax assessment.~~	~~taxation~~		~~Passed~~	~~no~~	~~no~~	~~yes~~
1990	134	Establishes alcohol surtax.	taxation	Democratic	Failed	yes	no	yes
1990	136	Regulations for property, special and general taxes.	taxation	Republican	Failed	yes	no	yes
1992	160	Allows property tax exemption for home of veteran killed in duty.	taxation	Democratic	Passed	no	no	yes
1992	163	Amends constitution to prohibit sales tax on exempt foods, adds exemptions.	taxation	Democratic	Passed	yes	no	yes
1992	167	Increases top state tax rates.	taxation	Democratic	Failed	yes	no	yes
1996	217	Increase top income bracket.	taxation	Democratic	Failed	yes	no	yes
1996	218	Requires vote to approve tax increase.	taxation	Republican	Passed	yes	no	yes
1998	1	Allows repair of contaminated structures without increasing tax value.	taxation	Republican	Passed	no	no	yes
2000	37	Requires 2/3 legislature vote to establish certain regulatory changes.	taxation	Republican	Failed	yes	no	yes
2004	65	Requires voter approval for reduction of local fee/tax revenues.	taxation	Democratic	Failed	yes	no	yes
2004	1A	Ensures local property and sales tax revenues remain with local government.	taxation	Republican	Passed	no	no	no
		Transportation						
~~1990~~	~~125~~	~~Allows motor vehicle fuel tax to be spent on railways.~~	~~transportation~~		~~Failed~~	~~no~~	~~no~~	~~no~~
1992	156	Funds for passenger rail.	transportation	Democratic	Failed	no	yes	no
1992	157	Leased toll roads shall be toll free at expiration of lease or after 35 years.	transportation	Democratic	Failed	no	no	yes
1994	181	Funds for passenger rail.	transportation	Democratic	Failed	no	yes	no
1998	2	Requires loans of transportation funds be repaid in the same fiscal year.	transportation	Republican	Passed	no	no	no
2002	51	Portion of state motor vehicle sales/lease revenues to transportation.	transportation	Democratic	Failed	yes	no	no

Notes: The rows that are struck out are the 18 1990 propositions that do not appear in our sample. Initiative indicates a proposition on the ballot by a citizen's initiative. Bond/tax indicates whether the proposition mentions bonds/taxes specifically.