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# Political Alignment, Attitudes Toward Government and Tax Evasion

## Abstract

We ask whether attitudes toward government play a causal role in the evasion of U.S. personal income taxes. We first use individual-level survey data to demonstrate a link between sharing the party of the president and trust in the administration generally and opinions on taxation and spending policy, more specifically. Next, we move to the county level, and measure tax behavior as elections, decided by the voting behavior in swing-states, push voters in partisan counties into and out of alignment with the party of the president. Using IRS data, we find that reported taxable income increases as a county moves into alignment, with the increases concentrated in income sources that are easily evaded, due to lack of third-party reporting. Corroborating the view that evasion falls, potentially suspect EITC claims and audit rates also fall. Our results provide real-world evidence that a positive outlook on government lowers tax evasion.

JEL-Codes: D720, H240, H260, H300.

Keywords: tax evasion, tax morale.

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*If a thousand men were not to pay their tax bills this year, that would not be a violent and bloody measure, as it would be to pay them, and enable the State to commit violence and shed innocent blood.*

*-Henry David Thoreau, Resistance to Civil Government*

As long as there has been taxation, there has been tax resistance—the refusal to pay based on disapproval of how the funds would be spent. There are numerous examples of tax resistance in U.S history. In 1846, Henry David Thoreau famously refused to pay taxes because of his opposition to both the Mexican-American war and to slavery, as reflected in the quote above. In the 1960s, antiwar protestors advocated nonpayment of federal taxes to defund the Vietnam War. What is the extent of tax resistance today? We address this question in this paper, viewing tax evasion as a modern version of tax resistance.

Tax evasion lowered federal tax revenue in the United States by $419 billion on average across tax years 2008-2010.[1] This translates to a voluntary compliance rate of 83 percent (with another 2 percentage points recovered through enforcement). The vast majority of losses (roughly 70 percent or $290 billion) come from evasion of the personal income tax. This reflects both the heavy reliance on this form of taxation (which accounts for roughly half of federal receipts) as well as the greater scope for evasion of personal income taxes as compared to other forms of taxation, such as corporate and payroll taxes.

Failure to pay taxes impacts the efficiency, equity and incidence of the tax system and alters the distribution of resources to and across economic activities. Given the widespread consequences of evasion, economists have a long history of studying the behavior. The classic model (e.g., Allingham and Sandmo, 1972) characterizes tax evasion as a financial gamble that the agent undertakes if the benefits exceed expected costs. In this framework, the impact of the

[1] See Figures 1 and 2 for more details and sources for the facts in this paragraph.

marginal tax rate on evasion is ambiguous,[2] but the model clearly predicts and the empirical evidence generally supports the idea that evasion is decreasing in the cost (i.e., audit and penalty rates).[3] Given currently low levels of enforcement, we are then left with a puzzle: why is tax compliance so high?

We build on the literature that argues that the benefits of tax compliance are broader than simply avoiding a penalty in expectation. Among the factors that might affect willingness to pay is the perceived value of government spending. Falkinger (1988) presents the direct self-interest version, extending the basic model to allow the agent to value the share of public goods she receives. More generally, Congdon, Kling and Mullainathan (2009) propose that tax behavior may be affected not only by public goods received but also by one's attitudes toward government and its policies. The U.S. federal government also asserts that sentiments could have real consequences on tax collections; the Internal Revenue Service (IRS) mentions "socio-political" factors as one of the primary influences on voluntary tax compliance.[4] Yet, to date there is little empirical evidence regarding the importance of these factors. Our innovation is to test their importance in a real world setting where there is plausibly exogenous variation in attitudes, allowing us to gauge how changes in approval of government impact tax evasion at the county level.

Our approach is designed to overcome two key data challenges. The first is the well-known difficulty of quantifying an illegal activity. To address this challenge, we construct several proxies for tax evasion using aggregated IRS data. We use measures of income reported

[2] If the penalty depends on the amount of tax evaded, the marginal rate plays no role, but there are competing income and substitution effects if the penalty depends on the amount of under-reporting. The empirical relationship between the marginal tax rate and evasion is similarly non-robust, with, for example, Clotfelter (1983) and Kleven et al. (2011) finding a positive relationship, and Feinstein (1991) finding a negative one.
[3] See Barbuta-Misu (2011) for a review of this literature.
[4] See the IRS's "Reducing the federal tax gap: a report on improving voluntary compliance" (August 2, 2007).

to the IRS as our dependent variables, presuming reductions in reported amounts conditional on observed economic activity reflect evasion. We compare categories of income that differ in the extent to which they are third-party verified, with greater reporting sensitivity in the less visible categories suggesting evasion. We also consider more direct proxies for evasion, namely the number of returns claiming suspect Earned Income Tax Credits (EITCs) and the number of returns that are subject to audits. For the former, we draw on the findings in Chetty et al. (2012) that the self-employed are particularly likely to report income levels that maximize EITC refunds and that this "sharp bunching" is likely to reflect evasion. The use of audit rates as a proxy for evasion is motivated by the fact that most personal income tax audits are initiated when reported amounts are discrepant with norms for similar returns in ways that correlate with detected evasion from prior audits.[5]

The second data challenge we face is measuring approval of government. The proxy for approval we choose is political alignment—a match between own party and presidential party. To support the validity of this proxy, we use nationally representative data from the General Social Survey (GSS) to confirm that an individual who is in political alignment with the president has more positive views of government and taxes and spending relative to an individual who is not aligned. We then construct an analogous county-level measure of political alignment from voting records, equal to the share of the two-party vote cast for the current president.[6] In light of evidence that voters' preferences are sensitive to current economic conditions (e.g., Brunner, Ross and Washington, 2011), rather than using the vote share from the most recent election, we use the average over several elections.

[5] Historical information on how returns are selected for examination was accessed at https://www.irs.gov/newsroom/the-examination-audit-process on February 6, 2018.

[6] It is important to note that we are unable to link individual-level IRS data to other sources that might capture person-specific attitudes, so instead we use residential location to form groups of potentially like-minded taxpayers.

Our empirical analyses then track changes in evasion for partisan counties—those that vote consistently for one party—that are either shifted into or out of alignment by turnover elections based on the voting outcomes in other counties. Given the time frame covered by our data, we focus on the years just before and after the 2000 and 2008 presidential elections in which the party of the president changed. These natural experiments allow us to observe the same counties under different regimes, with both Democratic and Republican counties observed moving into and out of alignment.[7]

Overall, our results provide novel evidence for an attitudinal component to tax compliance.[8] Combing evidence from our survey (GSS) and administrative (IRS) data, we demonstrate that when a higher fraction of county residents hold a positive view of government, a lower fraction of individual income tax is evaded. Gross income reported increases by 0.4 percent as a county moves into alignment, and the majority of the increase is concentrated in categories that are subject to little third-party reporting, such as income from small businesses. While reported income in these less visible categories expands by 3 percent, we find no elasticity of third-party reported income to alignment. Corroborating the view that evasion falls, EITC claiming among the self-employed and audit rates also fall, by 1 and 4 percent, respectively. These effects are sizable, particularly if attributable to the approximately 30 percent of the population shifted into alignment on net.

In addition to conducting extensive robustness tests to ensure we have adequately controlled for underlying economic conditions, we conduct a limited set of heterogeneity

[7] As part of our data agreement with the IRS, we do not attempt to estimate differential impacts by party affiliation.
[8] Ours is among the first studies to consider the role of political alignment in tax evasion. Previous work has looked at the relationship between a CEO's political affiliation and corporate tax avoidance, with conflicting results. Christensen et al. (2015) find that firms led by CEOs who donate more to the Republican party are less likely to avoid taxes, while Francis, Hasan and Sun (2016) find these are exactly the firms that are more likely to avoid taxation. Besley, Jensen and Persson (2015) rely on election-induced shifts in the single-majority party status of local governments to provide shocks to the tax enforcement regime, and, in their UK setting, it is the unpopular shift to a poll tax to fund local government that alters intrinsic motives to pay taxes.

analyses. These reveal that evasion responses to alignment with the president are muted when federal income tax reports are direct inputs to state tax returns, so that evasion at one level is tied to evasion at the other and attitudes toward government are mediated by another layer of alignment. Similarly, evasion responses are magnified when the county is aligned with both the president and governor.

The remainder of the paper proceeds as follows. Section 1 reviews the recent literature on tax morale, and provides evidence that political alignment is a meaningful proxy for the component of tax morale that operates through government approval. The data and methods are presented in Section 2, and the results in Section 3. Finally, in Section 4 we offer a brief conclusion.

## 1. Tax morale and the role of political alignment

### *1.1 Literature on tax morale*

There is a growing literature exploring mechanisms underlying differences in the willingness to pay taxes, or "tax morale." In their review, Luttmer and Singhal (2014) provide a typology for classifying these mechanisms. In addition to other categories, such as intrinsic motivations (e.g., guilt) and peer influences (e.g., social image and norms), they define "reciprocity" to refer to those that depend on the individual's relationship to the state. Attitudes towards government and alignment with the president's party fall under the reciprocity category. Being aligned with the president's party might increase trust in the administration in general, as well as approval of the government's tax and spending activities.

There is both survey and experimental lab evidence in support of the idea that taxes paid are a positive function of the payee's trust in and approval of government. Webley et al. (1991) demonstrate a correlation between negative attitudes toward government and evasion in the lab,

while Scholz and Lubell (1988) and Torgler (2003) show that trust in government is correlated with reported compliance in surveys. Reported compliance is also increasing in an individual's level of patriotism (Konrad and Qari, 2009) and exposure to war threats against the state (Feldman and Slemrod, 2009). Further, experimental economists have found that individuals are more likely to be tax compliant the more they value the public good (Alm, Jackson and McKee, 1992) and when those individuals have selected that public good (Alm, McClelland and Schulze, 1992). Torgler (2005) and Hanousek and Palda (2004) find complementary evidence that tax morale is higher when individuals have direct democratic rights and view the quality of government services to be high, respectively. Authors have also repeatedly found that perceptions that the tax system is fair increase reported compliance (e.g., Cummings et. al, 2009; Fortin, Lacroix and Villeval, 2007; Steenbergen, McGraw and Scholz, 1992).

Our study moves beyond the lab and surveys, to quantify the impact of quasi-experimental variation in attitudes on evasion as measured by IRS administrative data. From this perspective, the most closely related predecessor is Cebula (2013), showing that the IRS time series on aggregate evasion is predicted by the public's dissatisfaction with government. Using more plausibly exogenous variation in attitudes, we confirm a causal link.

*1.2 Linking political alignment to tax morale*

While the use of IRS administrative data, rather than self-reported survey data or data generated in the lab, distinguishes our paper, the IRS data, which we describe below, are not without limitation. An important shortcoming of IRS data for our purposes, is that they do not contain a measure of tax morale. We instead proxy tax morale with political alignment. In this subsection, we use survey data to show that alignment is a valid proxy for government approval, and like other measures of approval used in the literature, it predicts self-reported tax morale. For

this exercise, we employ data from the GSS.[9] Begun in 1972, the GSS is an annual or biannual repeated cross section of the political and social attitudes of adults. Relevant for our purposes, the survey includes questions on confidence in government and views on government spending and taxation as well as respondent partisanship.

Using the pooled 1972-2014 samples, weighted to be representative of the non-institutionalized English speaking population,[10] we run models of the form:

(1) $$\textit{Government attitude}_{it} = \beta_1 \times \textit{Presalignment}_{it} + \beta_2 \times \textit{Congalignment}_{it} + \mathbf{X}_{it}\Omega + \varepsilon_{it} \ ,$$

where *Government attitude* is a measure of confidence in a government institution or support for government activities. *Presalignment*, how well one's own party identification corresponds with the presidential party, is calculated from a party identification variable whose values range from 0 (strong Democrat) through 6 (strong Republican). We create a "party id" index by rescaling this variable to range from 0 to 1, for ease of interpretation. Then, we define alignment to be equal to party id during Republican administrations, and to 1 – party id during Democratic administrations.[11] We define *Congalignment* analogously. It is equal to party id when the House and Senate are both majority Republican, 1 – party id when the House and Senate are both majority Democrat, and ½ when the chambers are split. The vector **X** is a detailed set of individual controls, including party id and an ideological index (ranging from 0 for extremely liberal to 1 for extremely conservative). The reported standard errors are robust to clustering by party id-by-presidential term.

Before we move to the relationship between alignment and tax morale, we first

[9] Smith, Tom W; Marsden, Peter V; Michael Hout. General Social Surveys, 1972-2014. [machine-readable data file]. Principal Investigator, Tom W. Smith; Co-Principal Investigators, Peter V. Marsden and Michael Hout, NORC ed. Chicago: National Opinion Research Center, 2015.
[10] See Appendix A for more detail on our GSS sample and variable construction.
[11] The GSS is administered during February through April and presidents take office in January following election years, so that the relevant administration is always that of the most recently elected president.

demonstrate that our constructed measure of presidential alignment predicts feelings toward the executive, namely confidence in the federal executive branch. Table 1a shows these results. The outcome variable, rescaled from the original, increases with confidence and ranges from 0 to 1 so that 0 is "hardly any" and 1 is "a great deal." Thus, the .227 in column 1 of Table 1a indicates that when aligned, the strongest partisans (whose values of alignment are 0 or 1) are 23 percentage points more likely to say they have a great deal of confidence in the executive branch.[12] For the moderate partisans (who answer 1 or 5 on the original scale), the difference in confidence across aligned and unaligned administrations is 15 percentage points. Results are robust to controlling for how liberal or conservative a respondent is (column 2) and demographics (column 3).

In our main analysis, we construct our alignment measure from county vote shares rather than individuals' party identification. The small samples sizes and sampling frame in the GSS preclude creating representative county aggregates of respondents' political views. However, we explore robustness by moving from self-reported partisanship to self-reported vote choice.[13] In column 4, we restrict attention to respondents who answered the question on their choice for president in the most recent election. Amongst that sample, we find having favored the president in the last election is associated with an increase of 17 percentage points in the likelihood of great confidence. The association is strengthened when in column 5 we limit the sample to those who report having voted for their preferred candidate.

Across columns in Table 1a, the relationship between congressional alignment and confidence in the executive branch is consistently positive, but either marginally or non-

[12] In this section, all of the dependent variables take on values between 0 and 1. For simplicity, we describe the estimates for those that take on intermediate values as if they were indicator variables.

[13] We recognize that self-reported vote choice is influenced by party identification (Gerber, Huber and Washington 2010) and the election winner. Across the years 1950-1988, Wright (1993) finds over reports of voting for the winner in the American National Election Study only for the 1964 Goldwater-Johnson election.

significant and an order of magnitude smaller than presidential alignment. While we find this comforting, we recognize that there may be some concern that political alignment may predict some general sense of satisfaction and not specifically satisfaction with the executive branch. We address this concern in Table 1b in which we examine the relationship between alignment and a cross-section of institutions. In the first column of the table we repeat our preferred specification from Table 1a for comparison. In columns 2 and 3 we demonstrate how confidence in Congress and the Supreme Court varies with alignment. In both cases, we find that presidential alignment is associated with much smaller increases in the likelihood of approval. For Congress we find congressional alignment has a greater impact than presidential alignment, as expected. In column 4, interestingly, we find a negative (but again small) relationship between approval of the press and presidential alignment. Perhaps this reflects some frustration at the press "attacking" the respondent's president. In the remainder of the table, we find no relationship between presidential alignment and confidence in major companies (column 5) or the church (column 6). (We do, however, find that congressional alignment has small predictive power for the church.) All in all, the evidence of Table 1a demonstrates that presidential party alignment predicts first and foremost feelings about the executive branch.

In the final GSS table, Table 1c, we further ask whether presidential alignment predicts support for federal government taxation and spending. The answer is yes. On the tax side, while fewer than 1 percent of respondents say their taxes are too low, presidential alignment is associated with a six percentage point decrease in responding that taxes are "too high" over "just right" and "too low".[14] In the next two columns we examine feelings about government spending. To create the outcome measures, we sum across a series of questions that ask whether

[14] While there are questions even more directly related to tax morale, such as whether it is okay to cheat on taxes, these are asked in too few years to identify the role of alignment conditional on party identification.

spending in a particular area is too much, just right or too little to create variables on the fraction of categories for which the respondent held a given view.[15] In column 2, we see that presidential alignment is negatively and significantly associated with feeling there is too much spending. We do not find that the too little spending margin moves with alignment. These findings are echoed in respondents' attitudes toward government action. We find that alignment negatively and significantly predicts the view that the government should do less. However, alignment is not significantly associated with the view that government should do more. Across these measures of tax morale, the fact that we find no predictive power of congressional alignment motivates our focus on presidential approval as the key independent variable in the specifications that follow.[16]

In summary, the results from the GSS analysis provide support for party alignment being a meaningful proxy for approval of the executive branch. There is also an elasticity of disapproval for taxation and spending with respect to alignment, but not an elasticity of approval. In other words, respondents have a *less negative view* of government tax and spending policies when aligned. In the main analysis, we ask whether these less negative attitudes toward taxation and spending translate into a higher willingness to comply with taxation.

## 2. Methodology and data

### *2.1 Methodology*

Our goal is to estimate the impact of political alignment on evasion, a behavior that is difficult to measure due to illegality. A variety of methods have been used to measure evasion in the literature. In rare instances, data from random audits are available (e.g., Kleven et al., 2011).

[15] The spending categories are education, health, welfare, the environment, law enforcement, drug rehabilitation, assistance to big cities, assistance to blacks, defense, space exploration and foreign aid.

[16] Political scientists have long documented that voters assign credit or blame for the macro economy to the president (Key, 1966). Gomez and Wilson (2001) provide evidence that only sophisticated voters understand that there are multiple players, including Congress, in macroeconomic conditions, and thus vote accordingly. Other evidence of the greater attribution assigned to the President include the fact that Presidential approval predicts the outcomes of Congressional midterm elections (Kernell, 1977) and that voters assign greater responsibility for sub-national economic conditions to the President than to state elected officials (Stein, 1990).

More typically, evasion is inferred from discrepancies between what is observed and what is expected. For example, Feldman and Slemrod (2007) compare the estimated elasticity of charitable giving across different sources of taxable income. Absent evasion, their presumption is that the propensity to donate would be constant across more and less visible income sources.

In this paper, we use several approaches to infer evasion at the county by year level. The first approach that we follow is known as the tax gap approach. We use reported taxable income measures as our dependent variables, presuming reductions in reported amounts conditional on observed economic activity reflect evasion. The categories of income differ in the extent to which they are third-party verified, so are differentially susceptible to evasion and would be expected to be differentially responsive to shifts in attitudes for this reason.

Second, we use suspect claims of the EITC. In part due to its complexity, the EITC is subject to high rates of over-claiming. Based on audit studies, the IRS estimates about one-third of credit payments reflect overpayments (IRS 2014), with most of the errors due to claiming an ineligible child, filing as a single or head of household when legally married and over- or under-reporting income or business expenses. Further, Saez (2010) demonstrates that those who report self-employment income have a propensity to report the least amount of income that qualifies for the maximum EITC, while Chetty et al. (2012) provide evidence from audits that this "sharp bunching" is driven by noncompliance. Thus, we consider both the rates at which the self-employed claim the EITC at all, as well as the propensity to bunch near the minimum earnings level that qualifies for the maximum credit. Following Chetty, Friedman and Saez (2013), we identify these "sharp bunchers" as returns with dependents and non-zero Schedule C income that report net earnings within $500 of the level associated with the end of the credit phase-in range applicable to the tax unit.

Finally, we use the audit rate. Audits are triggered under the personal income tax primarily by automated computer algorithms that are periodically updated based on stratified random audits. If the statistical analysis of a return suggests a high probability of inaccurate information or omitted income, the return is flagged for audit. We do not consider the outcome of the audit, such as the extent of misreporting detected, since this could be affected by changes over time in the size and allocation of the enforcement budget.

With these proxies for evasion in hand, we exploit presidential turnover elections to provide the quasi-experimental equivalent to manipulating tax morale in the lab. Partisan counties that consistently vote for one party over the other in presidential elections are not the counties that determine the election outcome. As a result, partisan counties come into and out of alignment with the party of the president based on the voting outcomes of swing counties, those counties where voters do not consistently support one particular party. By tracking behavior of residents of partisan counties under different regimes imposed on them by swing counties, we attempt to hold all else constant and isolate the associated shift in tax morale.

Our data span two turnover elections: 2000 and 2008. In 2000, George Bush (Republican) took over from Bill Clinton (Democrat). In 2008, Barak Obama (Democrat) was elected, changing the party in the Whitehouse once again. For our primary regression analyses, we choose a window sample bracketing these two elections. Specially, we include the years 1999 and 2001 for the 2000 election and the years 2007 and 2009 for the 2008 election.[17] An advantage of our window analysis is that it balances the number of years each county is in versus out of alignment. We omit the election year because of its ambiguity. During election years, income is earned under one president and reported (in the following April) under another. For

[17] If we restrict our GSS analysis to similar window years (1998, 2002, 2006 and 2010, due to the biennial design), we find qualitatively similar evidence for the relationship between presidential alignment and approval of government, taxes and spending.

these years, it is not obvious how to define alignment as long as any real costs to evasion are borne in advance of filing.

We run the following ordinary least squares specification relating one of our proxies for evasion for county *c* in state *s* in year *t* to the county's political alignment in that year:

(2) $$Proxy_{cst} = \beta \times alignment_{cst} + \mathbf{X}_{cst}\Omega + \alpha_c + \delta_{st} + \varepsilon_{cst} \ ,$$

The sample is restricted to partisan counties and the four window years around the two turnover elections. The specification includes county fixed effects and state-by-year fixed effects, so that relative changes in alignment within a county over time provide the identifying variation. To ensure these changes are exogenous to time-varying county characteristics, alignment is based on the average vote share across many elections. If 80 percent of the two-party vote typically goes to the Democratic candidate, then the county's alignment measure will be 80 percent when the president is a Democrat, and 20 percent when the president is a Republican. The vector **X** includes time-varying factors that flexibly control for the amount and types of income generated in a county. Standard errors are clustered by county. Our identifying assumption is that economically similar counties facing common state and federal tax systems would behave similarly in the absence of differential changes in alignment.

The key threat to interpreting $\beta$ as identifying the causal effect of alignment on evasion is omitted time varying factors correlated with both alignment and evasion. For example, if the economic controls do not adequately capture true generated taxable income, reported income and alignment may be positively related through a shared correlation with unobserved components. One channel for such a link is studied in Gerber and Huber (2009). The authors use the same definition of alignment as we do, showing that it predicts optimism about the future of the economy in survey data. They then demonstrate increased sales tax collections from the quarter

before to the quarter after the election when a county moves into alignment, consistent with increased consumption (though also perhaps with reduced evasion).[18] Another channel that has been documented is federal spending targeted to counties on the basis of political alignment.[19] In addition to these mechanisms, Figure 3 shows that we face the challenge that swings in alignment for Democratic and Republican counties occur under contrasting economic environments. The first of our turnovers coincides with an economic recovery, while the second coincides with the onset of the Great Recession. Reassuringly, the figure shows that these counties exhibit similar trends in unemployment. To address concerns about omitted time-varying factors, we test sensitivity to more and less rich control sets. We also compare results across more and less visible income sources and use proxies that are less dependent on accurately measuring true taxable income, such as suspect EITC claims and audits.

A second limitation of our approach stems from our use of aggregate data to make inference about individual behavior. Particularly given the low levels of turnout in the United States, we can never prove that the county residents' whose alignment changes are the same individuals who subsequently change their tax-paying behavior. In addition to this problem, which has been termed the ecological fallacy, it is important to recognize that the treatment is a group treatment. That is, attitudes of networks are shocked at the same time as own attitudes. Thus, in the conclusion, we discuss implications for policies that would be targeted to populations, not individuals.

[18] In contrast, Mian, Sufi and Khoshkhou (2015) find no evidence of an effect on consumer spending, also using a quite similar strategy to us but studying each election in turn. Interestingly, to support their strategy, they document that alignment is not correlated with systematic changes in either IRS adjusted gross income or wage aggregates. We too find no detectable effect on wages, but do find reported AGI tends to increase, though our measure differs in that it excludes capital gains.

[19] Dynes and Huber (2014) is a current study that shows an explicit link between voter alignment with the president and federal government transfers. Prior work has demonstrated a link that is moderated by congressional representation. For example, Albouy (2013) finds that representation by a member of the majority party predicts greater transfers, and Berry, Burden and Howell (2010) find the same for House representation by the party of the president.

A more subtle interpretation issue that we cannot rule out is that taxpayers perceive the probability or cost of audit as varying inversely with alignment. Since this would serve to increase evasion during alignment (which is the opposite of our findings), we interpret our estimates as providing lower bounds on the role of improved morale.

*2.2 Data and sample*

To measure income tax reporting behaviors, we rely on administrative tax data from the IRS. We create our own aggregations from the population returns, collapsed to the county year level.[20] We access the underlying individual income tax data from the Compliance Data Warehouse (CDW). These data are available beginning in 1996 and include information on nearly every line of the 1040 and most supporting schedules filed, as well as records of audits.

The broadest measure we create is total income reported on the tax return net of capital gains, which we refer to as "gross income." We exclude capital gains since these are highly volatile and realizations can be timed strategically with respect to changes in tax rates. We also separately consider components of gross income differentiated by third-party reporting. The components we consider, from least to most easy to evade, are: i) information reported and withheld income (wages and salaries), ii) income that is subject to substantial information reporting (financial and retirement income), and iii) income that is subject to little information reporting (Schedule C proprietor income and Schedule E pass-through and rental income). Figure 2 shows that this categorization aligns well with evasion rates found in IRS audit studies. Additionally, we investigate how reported income changes as we move from gross income to adjusted gross income (AGI) and then from AGI to taxable income. With each step there is increased scope for evasion because of successively allowed adjustments and deductions, many of which are not third-party reported, though for these we are less able to disentangle evasion

[20] See Appendix B for more details.

from avoidance. To complement the income measures, we calculate filing rates overall and for various combinations of Schedule C and EITC claims, as well as audit rates.

We construct two versions of the CDW income and filing variables, one that includes all returns, and another that includes only the subset filed by "policy-constant" tax filers. The set of policy constant filers is determined by applying the 1996 tax law (adjusted for inflation) to later years. Intuitively, we attempt to hold fixed the tax filing population by limiting the sample to taxpayers who would have filed under 1996 policy.[21] This helps guard against the possibility that changes in reported income that we attribute to evasion actually result from differential impacts of tax policies, such as expansions to existing tax credits or the introduction of temporary tax credits that induce filing among those not otherwise required to file. We prioritize results from the policy constant sample, but demonstrate robustness to using the full sample of filers instead.

The CDW also includes information returns submitted by third-party reports starting in 1999. These include W2 forms for wages and salaries and 1099 forms for interest and dividends, distributions from retirement accounts and unemployment compensation.[22] We use these forms to create some of our controls for generated taxable income. We also link the W2 forms to various business tax returns using the employer identification number. This allows us to construct the share of W2 wages attributable to employees of partnerships, S-corporations, and C-corporations. These shares control for the composition of business activity, and possible shifting between personal and corporate tax bases.

The other controls for county economic activity come from a variety of government sources. Though the Bureau of Economic Analysis (BEA) estimates proprietorship income and other components of personal income, these estimates incorporate IRS tax return data, so are not

[21] See Appendix B for more details.
[22] Unfortunately, data from the 1099-MISC, which would capture some forms of self-employment income, are missing in window year 1999.

independent of reported taxable income. We do draw government employment from the BEA, but use the Census County Business Patterns (CBP) reports for the number of establishments with employees and the breakdown by industry. The unemployment rate and number of housing starts are from the Bureau of Labor Statistics and the Census building permits survey, respectively. Annual county population estimates are produced by the Census, while other slow-moving demographic controls are linearly interpolated from decadal Census values.

The sample of counties and years included in the analysis is based on those that have available data. Starting from an unbalanced panel of the 3,149 counties that ever existed 1989 to 2012, we drop counties that are:

i) not represented in the voting data (34 counties, including all 33 Alaska counties),

ii) deleted over the period (3 counties),

iii) not the primary county for any zip codes (4 counties),

iv) missing whole zip codes of returns deleted from the CDW in 1999 (53 counties),

v) combined with other areas for reporting by the BEA (50 counties).

The remaining sample is a balanced panel of 3,005 counties, representing more than 95 percent of ever existing counties and 93 percent of the population in a typical year.

*2.3 Descriptive and summary statistics*

We characterize a county's partisanship status by the average two-party vote shares in either the medium term (across the 1996 to 2008 presidential elections) or the long term (across the 1988 to 2008 presidential elections).[23] Classifying counties according to average two-party vote shares in the medium term, 15 percent are always majority Democratic, 48 percent are

[23] County vote returns were purchased from http://uselectionatlas.org/. See Appendix C for details on the distribution of vote shares by year and persistence over time within counties, as well as partisan and swing county shares by state.

always majority Republican, and the remaining counties are swing counties.[24] In the longer term, more are classified as swing counties and fewer as partisan counties, with 11 percent Democratic and 43 percent Republican. Figure 4 shows the geographic distribution of counties by partisan status (in the medium term). While many states have large majorities of supporters of one party, most states still have heterogeneity across counties in party learning. (See Appendix C for a list of the types of counties by state.)

Tables 2a and 2b report means and standard deviations for the dependent and control variables, respectively, by the partisan status of the county. Note that all financial variables have been converted to real per capita 2010 dollars. The reported income statistics show that the most visible form of income is also the most common, with wage and salary income making up three-quarters of gross income. The least visible forms make up less than 10 percent. Republican counties tend to have higher shares self-employed and more income from less visible sources, highlighting the importance of comparing reporting behavior within county over time.

## 3. Results

### *3.1 Graphical event-study evidence*

Before moving to the results from our main "window" strategy, we show graphical evidence that county-level evasion decreases when counties move into political alignment. Here, we expand the sample period to include the two years before and after our focal turnover elections in 2000 (Democrat to Republican) and 2008 (Republican to Democrat), as well as the transitional election year. This allows us to provide a more dynamic view, but comes at the cost of our not being able to rely on the richer set of controls for economic activity, available beginning in 1999. We include only partisan counties, classified based on the medium-term

[24] Democratic counties tend to be more urban and populous, so that the population-weighted shares are 43 percent Democrat and 29 percent Republican.

1996-2008 presidential election vote shares. We pool across the two elections, so that the Republican counties are the ones moving into alignment in 2000, while the Democratic counties are in 2008.

Figure 5 plots the coefficients from event study analysis of the impact of moving into alignment on three types of income that vary in the level of third-party reporting and automatic withholding.[25] The figure includes wages and salary income that is subject to substantial information reporting and withholding, financial and retirement income that is subject to substantial information reporting but no withholding, and Schedule C&E small business income that is subject to little or no information reporting and no withholding. The regressions control for the demographic and economic variables detailed in Table 2b, as well as county and state-by-year fixed effects. Standard errors are robust to clustering at the level of the county.

As the diamond line in the figure shows, partisan counties that are out of alignment report significantly less Schedule C&E income prior to the election relative to the counties that are in alignment. Behavior converges during the election year and, one year after the election, counties that have moved into alignment are now reporting significantly more Schedule C&E income relative to their counterparts that are now unaligned. On the other hand, there is no response of income sources that have third-party verification and/or automatic withholding. The coefficients on wage and salary and financial and retirement incomes do not change meaningfully throughout the five-year sample period. Overall, the results of Figure 5 are consistent with the idea that being in alignment with the president increases tax compliance.

*3.2 Baseline "window" analysis*

In the remainder of this section, we provide more systematic evidence of a causal link

[25] See Appendix D for alternative event studies that use swing counties as controls and separately consider moving in vs. moving out of alignment.

running from alignment to evasion using our "window" strategy from equation (2). The estimation samples include only the two years that surround each of the turnover elections (i.e., 1999, 2001, 2007 and 2009), with each partisan county spending two of these years in and two of these years out of political alignment. We study impacts of alignment on tax paying behavior on the intensive and extensive margins, including amounts reported by category, returns filed by type and number of audits.

Table 3a reports baseline results for (log real per capita) income measures, while Table 3b reports results for measures based on (log per capita) counts of returns. In these tables, each cell contains the coefficient and standard error on an alignment measure from a different specification. The dependent variable varies across columns, while the alignment measure varies across rows and is described in the first column. The dependent variables in these two tables are defined based on the subset of tax returns filed by policy constant filers, who would have been expected to file under time-invariant tax provisions. All specifications include the same set of controls – county and state-by-year fixed effects and the demographic and economic variables shown in Table 2b.

Starting with Table 3a, the first row presents results for the baseline alignment measure, derived from average county vote shares 1996 to 2008 interacted with the party of the current administration, which is the continuous analog of the binary version underlying Figure 5. The 0.012 in the first cell indicates that as alignment increases by one, reported gross income (exclusive of capital gains) increases by 1.2 percent. An increase of one in alignment would occur for a county that voted unanimously for the Democratic Presidential candidate from 1996 to 2008, at the time when a Democratic president succeeds a Republican. In our data, the average Democratic (Republican) county gives 63 percent (35 percent) of its vote to the Democrat;

therefore the average alignment change is only about 30 percentage points. Taking this into account, the results of the first cell indicate that, for the average partisan county, moving into alignment would increase the gross income reported by 0.4 percent, or about $80 per person per county moving into alignment.

In the next three columns of the table, we revisit the components of income, where we expect to observe greater underreporting for sources of income that are not third-party verified, as suggested by Figure 5. Consistent with that figure, we find that alignment has no significant impact on the reporting of income for which there is little scope for evasion either because of information reporting (financial & retirement income) or both withholding and information reporting (wages & salary income). However, we find that alignment predicts a significant 3.5 percent increase in the self-reporting of less visible schedule C & E income for the average county. Notably, underreporting of business income accounts for nearly 1/3 of the IRS estimation of the tax gap (IRS, 2016).

In the first four columns, the dependent variables capture evasion by means of failure to report income. Another route is to erroneously report activities that entitle one to additional exemptions and deductions. In the remaining columns of the table we look for evidence of this behavior by examining the impact of alignment on AGI (reported gross income minus adjustments) and taxable income (AGI minus additional deductions). We find that for the average county alignment increases AGI by 0.5 percent and taxable income by 1 percent. Thus, as the tax code offers additional scope for misreporting, we find additional income as counties move into alignment, a pattern consistent with a causal impact of alignment on evasion. A caveat, however, is that AGI and taxable income might also embed legal avoidance that is unobserved, so while the finding of increased reported income with alignment is still interesting,

the interpretation is less clear than for the gross income components.[26]

In the remaining rows of the Table 3a, we examine the robustness of our results to alternative measures of alignment. In the second row, we calculate alignment from the average vote shares across more elections. This gives us a better measure of a county's long run partisanship status, but also leaves fewer counties by which to estimate the impact of alignment, as some counties that were formerly classified as partisan move to the swing column. The pattern of results is robust to this change.

In the next two rows we move away from exploiting the intensity of alignment and rely solely on the aligned/unaligned margin for identification. As in Figure 5, we model alignment as a binary variable that takes the value 1 for a county that has voted for the current president's party over the focal time period, either 1996 to 2008 (row 3) or 1988 to 2008 (row 4). The qualitative patterns remain, though some of the estimates lose statistical significance.[27] There is once again no significant impact of alignment on the reporting of those forms of income that are withheld or information reported. However alignment increases the reporting of the most easily evaded income—that with little information reporting—by about 3 percent. Impacts on AGI and taxable income are in the neighborhood of 0.5 percent with the coefficients on taxable income failing to achieve significance.

Up to this point, we have ignored turnout, effectively assuming that voters and non-voters are affected by the treatment similarly. However, there are at least two problems with this

[26] A second caveat is that is that the definitions of gross income, taxable income and AGI change over time. In Appendix B, we therefore create policy-constant versions that are purged of the influence of changes in the tax law, only allowing exemptions and deductions that would have been allowed in a base year. The results for gross income are unaffected, while the estimated sensitivity of AGI falls slightly but retains statistical significance. The most notable change is for taxable income, where the impacts fall by about half and lose statistical significance (though the point estimate remains larger than that for AGI). This decrease is likely in part because some of the potential channels for avoidance and evasion have been closed down by our calculation.

[27] As indicated in Appendix C, 12 (15) states have partisan counties from only one side of the aisle over the medium (long) term, so that these states do not contribute to identification when using this binary measure.

assumption. First, we do not know that the views about government of non-voters correspond with those of voters. Second, even if voters and non-voters hold similar views of the candidates ex-ante, the literature on cognitive dissonance and voting suggests they would have differing views ex-post, as those who are able to exercise the vote have stickier views (Beasley and Joslyn, 2001; Mullainathan and Washington, 2009). Therefore, in the next two rows of Table 3a, we interact our continuous alignment measure with average turnout over the same period that alignment is evaluated. This scales the magnitude of the changes into and out of alignment by the share of county eligible voters that exercise their franchise. Average turnout is 0.52, so therefore the swing in alignment in the average county shrinks from 0.3 to 0.16. Coefficients increase commensurately leaving the impact of alignment on the average county little different from the base specification.

The last two rows of Table 3a address another possible source of slippage in our alignment measure due to timing. Our window approach effectively compares reported taxable income in the third year of the old president's term to the first year of the new president's term. As presidential approval often dips throughout a president's tenure, vote share could be a less accurate measure of presidential approval in the third year than in the first. In the final rows of the table we use as our key independent variable Gallup's measure of national presidential approval averaged across the tax year, stratified by party. We assign the Democratic (Republican) approval measure to the Democratic (Republican) counties. We see swings (from unaligned to aligned) double in size for approval (approximately 0.6) as compared to continuous alignment (0.3). Coefficients then fall by ½ or more, suggesting a somewhat smaller impact of alignment on the reported taxable income of the average county. However, the qualitative pattern of the results remains unchanged.

Across all measures of alignment, we find robust evidence that reported income increases with alignment for the most easily-evaded forms of income, Schedules C & E. We find smaller and insignificant, and sometimes wrong-signed, impacts on other sources of gross income subject to withholding and information reporting. We also find that adjusted gross income, which can be manipulated by claiming deductions one is not entitled to, is impacted by alignment. The magnitudes of the point estimates for taxable income, which has scope for even further deductions, are consistent with yet greater responsiveness, though not always statistically significant.

In Table 3b, our dependent variables move from measures of the intensive reporting margin to measures of the extensive margin. The first two columns help us to describe how the number and character of returns changes with alignment. In the final five columns, we consider additional proxies for evasion. The alignment measures (in the rows) continue to be the same as in Table 3a.

When a county is aligned the filing rate increases, we learn in the first results column. And given that incentives to file vary from year to year, it is important to recall that we are estimating results for the aggregates that are derived from the set of policy constant filers (who would have also chosen to file under 1996 policy). [28] Although the IRS estimates that it loses some $32 billion due to nonfiling (IRS, 2016), increased returns do not necessarily imply increased tax collections. In fact, if we assume alignment decreases evasion, then its impact on filing is ambiguous, as those who owe taxes have an incentive not to file while those whom the government owes have an incentive to file.

[28] Results for the income measures calculated for this subsample of returns are nearly identical to the results for the measures based on all returns. This is not surprising since the population induced to file by temporary incentives typically has minimal taxable income. Tables E2a and E2b in Appendix E show both the insensitivity of the income measures, and the sensitivity of the overall filing rate.

When a county is aligned the rate of filing using a paid preparer decreases, we see in the next column of Table 3b. While Erard (1993) finds that paid tax preparers increase tax evasion, the literature as a whole has failed to reach this consensus and thus we characterize the results of this column as descriptive.[29]

In the remaining columns of the Table 3b, we examine outcomes that provide further evidence on the link between alignment and evasion. The sharp bunchers among Schedule C filers that others have associated with evasion are a subset of Schedule C and EITC filers. We explore the broader sets of filers, as well as the rare sharp bunchers (where rates in our sample are about 1 per 1,000 residents). First, column 3 echoes the finding for amounts, showing that rates of reporting positive Schedule C income increase by 0.8 percent when the average county moves into alignment. In contrast to filing, claiming the EITC unambiguously decreases tax liability. There is an incentive to claim the benefit erroneously because the maximum credit is potentially large.[30] Further, there is the potential for the credit to be claimed erroneously because it is not possible to perfectly observe eligibility. For example, self-employment income, which is not third-party verified, counts as earnings for the EITC. In column 4 we show that EITC claiming robustly decreases with alignment, and in column 5 we find specifically that EITC claiming on returns with non-zero self-employment income decreases. Bunching decreases with alignment, we see in column 6, however the bunching results are never statistically significant. Finally, consistent with decreased evasion, column 7 shows that alignment predicts a significant decline in the audit rate on the order of 3-4 percent. Note that the samples for both the bunching

[29] Chetty, Friedman and Saez (2013) find that EITC sharp bunching is correlated with the availability of local tax preparers across areas, while Hite and Hasseldin (2003) find fewer audit adjustments to paid preparer returns than to self-prepared returns. Perhaps reconciling these, Klepper and Nagin (1989) find that returns filed by paid preparers are more likely to adhere to legally unambiguous rules and to circumvent ambiguous ones.

[30] The value of the EITC depends on the number of qualifying children. In 2017, values ranged from $510 for returns with no children to over $6,300 for returns with three or more children.

and audit measures are restricted to counties with populations over 10,000, since counts for less populous counties are often censored due to IRS masking requirements.

Thus, all of our Table 3b markers of evasion point to decreased evasion for residents of counties in alignment. This finding coupled with the increases in income reporting observed in Table 3a are supportive of a causal link between alignment and tax compliance.

*3.3 Robustness tests*

In Appendix E, we report results from two sets of robustness tests for both the income measures in Table 3a and the return counts in Table 3b.[31] The first set considers alternative control sets, and we briefly describe here the ones that are richer than our baseline. As we demonstrated in figure 3, the Democratic and Republican county swings into alignment occur under very different economic circumstances. Thus, the key threat to our identification strategy is a failure to control adequately for a county's earnings. Our baseline control set is relatively rich, but includes indirect indicators of income such as employment and establishments, rather than direct indicators. Perhaps our best available measures of a county's taxable income are the information returns filed by employers, governments and financial institutions on wage and salary, financial, retirement and unemployment income. We show that controlling flexible for these does not affect the results much.[32] Nor does adding county-by-election fixed effects, where the comparison of a county's post-election tax behavior is made solely to that county's pre-election behavior, just two years prior. Although we find that when a county moves into alignment it is more likely to receive federal grants and procurement, controlling for these has

[31] In results not shown, we also test sensitivity to clustering standard errors at the state rather than county level. Though standard errors tend to be somewhat larger, statistical significance is rarely affected and is always maintained at least at the 10-percent level for estimates that are statistically significant at any level in Tables 3a and 3b. The only exception is for total gross income, where some of the marginally significant estimates in column 1 of Table 3a become insignificant (and some of the statistically insignificant estimates gain significance).

[32] Notably, these returns do not capture sources of income that are not subject to information reporting. As supplementary evidence against unobserved shifts in economic activity, we find no relationship between alignment and self-employment in the GSS.

little effects.

The second set of tests explores robustness to alternative samples. The first alternative sample we consider adds swing counties—counties that do not consistently vote Democratic or Republican over our focal period. Because the share of the vote going to Democrats and Republicans is generally close to 50-50, these counties do not see the large changes in alignment that their partisan counterparts do. We abstract from even these small changes, by setting swing county alignment to zero. This allows these counties to help to identify the coefficients on the numerous state-by-year effects and other controls, without contributing to the estimated impact of alignment. The remaining samples are all subsets of the original sample. First, we exclude small and then medium-size counties in turn, to match the samples for the sharp bunching and audit analyses. Next, given the concern that Democratic and Republican counties differ such they may not be good controls for one another, we use propensity score trimming to limit the sample to partisan counties that are similar economically. Specifically, we predict whether a partisan county is Republican based on 1990 values for the economic variables included in the baseline regression, and then keep only those counties for whom the predicted propensity lies between 0.1 and 0.9. Finally, we exclude counties that are likely to have greater divergence between measured economic activity and resident incomes, such as capital cities and those with large commuting flows, and counties hit hard by the housing crisis. Throughout the results are relatively stable.

*3.4 Heterogeneity*

While we have demonstrated the impact of alignment on the payment of federal income taxes, in the vast majority of states residents must pay state income taxes as well. In the final section of the paper we ask how the impact of alignment varies with state tax codes and other

state characteristics, focusing on our preferred proxies for evasion: the EITC and audit measures.

In the first section of Table 4 we ask about the relative tax behavior of people living in minority counties, partisan counties in which the majority of the county's voting population has consistently voted for the opposite party than that which the state's majority favors. The coefficients in a single column in the section come from the same regression. The coefficient in the first row is on the main effect of alignment and the coefficient in the second row is on alignment interacted with minority county status. We find that in minority counties while the impact of alignment on Schedule C&E and claiming EITC is muted, the latter significantly so, results are mixed so one cannot draw conclusions on whether these counties are more or less responsive.

In the "State income tax piggybacking" section of Table 4 we incorporate variation across states in the degree to which alignment with the president would be expected to matter for evasion under the federal personal income tax. Some states closely tie their own income tax calculations to amounts reported on the federal return. In these cases, taxpayers may be less sensitive to approval of the federal government when deciding how much to report, since it is necessary to evade at the federal level to evade at the state level, and vice versa. To test this, an interaction between the baseline alignment measure and an indicator for states that piggyback on the federal income tax is added to the specification. The interaction term is of the opposite sign from the main effect in all columns, in two cases significantly so. The results are consistent with these ties moderating the responsiveness to alignment for some of our proxies.

In the final section of the table we show that the impact of alignment is greater when a county is doubly-aligned, aligned both with the president and with the governor, as determined from gubernatorial vote shares across many elections. It would be interesting to explore dual

alignment with the executive and the legislative branches, but there is too little independent variation across our four window years.

## 4. Discussion and conclusion

We find real-world evidence consistent with taxpayer's approval of government affecting evasion. We first provide evidence from national survey data that people's attitudes towards government are correlated with their partisan alignment. When individuals are of the same political party as the incumbent president, they express less negative views on government tax and spending policies. Using population-level tax and voting outcomes at the county-level, we then examine how tax return behaviors evolve over the turnover presidential elections in 2000 and 2008 for residents of partisan counties that move into party alignment relative to those that move out.

We find that gross income reported increases by 0.3 percent on average as a county moves into alignment. The majority of the increase is concentrated in categories that are subject to little third-party reporting, such as income from small businesses. While we find no elasticity of third-party reported income, we find that the non-third-party reported Schedule C&E income increases by about 3 percent on average. By comparison, DeBacker et al. (2015) track individual taxpayers and find that reported Schedule C income increases by roughly 15 percent in the first year after an audit. If the behavioral responses we identify are scaled by the net change in an individual's likelihood of alignment, sensitivity to alignment is about two-thirds as great as the effect of an audit. Relative to our findings for gross income, we see that AGI and taxable income tend to exhibit greater responses, as there is scope for additional evasion when claiming deductions. However, we caution that our estimates for these categories may also reflect avoidance, such as true increases in charitable contributions, which we are unable to observe and

for which we do not have good proxies. Corroborating the view that alignment decreases evasion, potentially suspect EITCs and audit rates fall when counties are aligned. Finally, there is some evidence that the responses are muted when federal income tax reports are direct inputs to state tax returns and magnified when the county is aligned with both the president and governor.

Overall, our pattern of results suggests that individuals who disapprove of government tax and spending policies evade more, relative to comparable individuals who have a more positive outlook about the government. This fact is cause for concern given the inefficiencies of evasion, yet it also suggests that there may be scope for remedying evasion through simple interventions, such as information campaigns. Americans are unclear about how the government spends their money and who bears the burden of taxes. Ballard and Gupta (2017) find that in a random sample of Michigan residents, roughly 85 percent overstate their average federal tax rate, and that respondents who believe that tax dollars were spent ineffectively overstate their average tax rate by a greater extent compared to those who believe their tax dollars were spent effectively. Confusion also persists in how the federal government spends tax dollars. A recent Pew survey (Pew Research Center, 2013) showed that 33 percent of respondents believe the national government spends more on foreign aid than on interest on the debt, Social Security or transportation. In reality, the government typically spends about 17 times what it spends on foreign aid on just Social Security (Ingraham, 2014). Further, information affects perceptions of government programs. When Kaiser told poll respondents that the U.S. spent less than 1 percent on foreign aid, the fraction of respondents saying that too much was spent on aid fell in half, from 56 percent to 28 percent (Rutsch, 2015). Conveying similar information about taxation may change individuals' perceptions about their tax burdens and alter their inclination to evade taxes.

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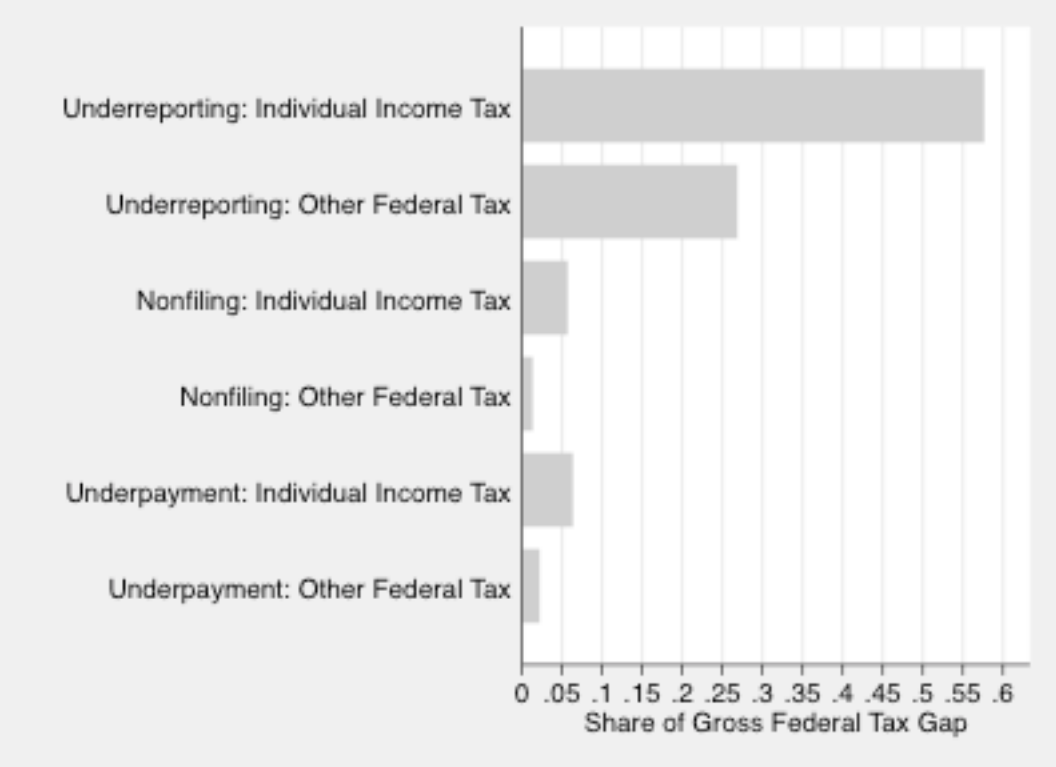
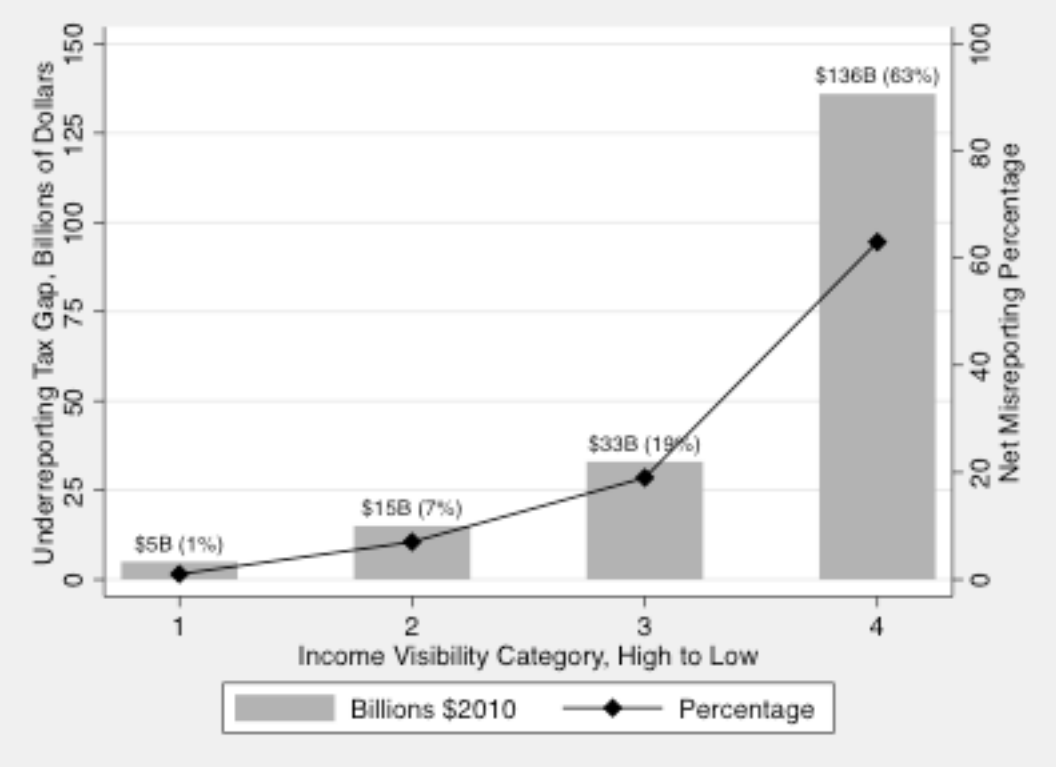
Figure 1. Role of the individual income tax in federal tax noncompliance, 2008-2010



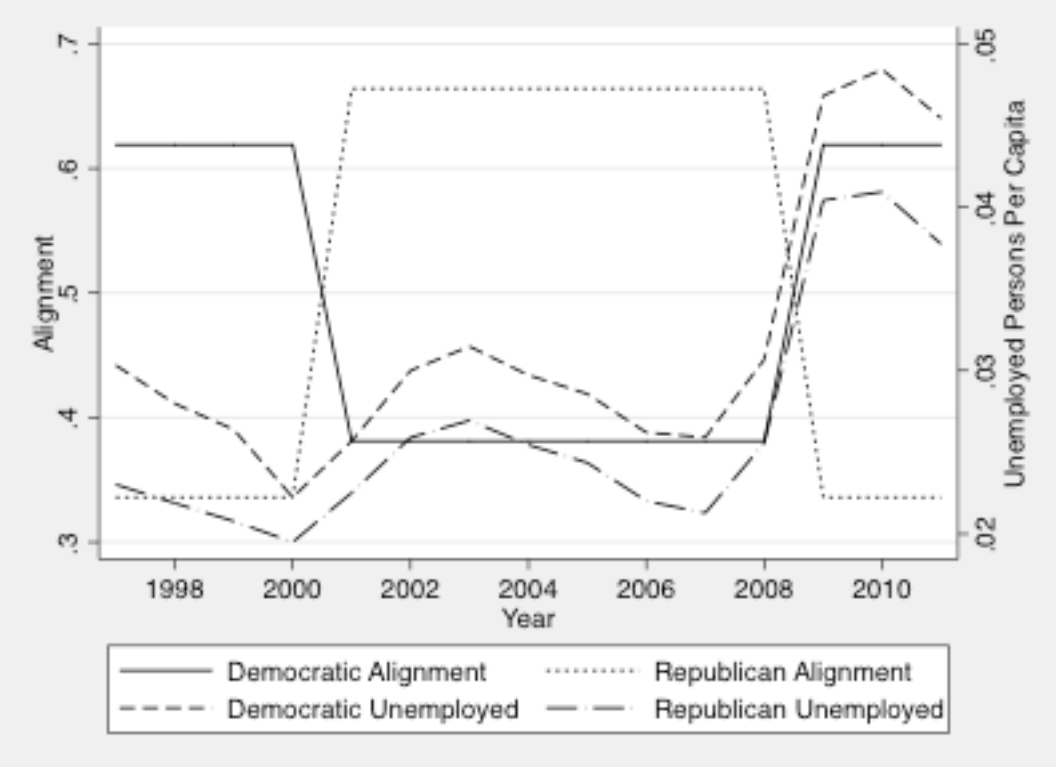

Notes: These statistics are from the Internal Revenue Service "Federal Tax Compliance Research: Tax Gap Estimates for Tax Years 2008-2010" (https://www.irs.gov/pub/irs-soi/p1415.pdf). On average for tax years 2008-2010, the total estimated federal tax liability including all major taxes (i.e., individual income, corporate income, FICA payroll, unemployment, self-employment, estate and excise taxes) was $2.5 trillion dollars, with a gross tax gap of $458 billion. The gross tax gap is the amount owed that is not paid voluntarily and on time, and exceeds the net tax gap by $52 billion. The bars show the percent of the gross tax gap attributable to the individual income tax vs. the other major federal taxes by type of noncompliance. Evasion consists of the first two categories of noncompliance – underreporting ($387 billion) and nonfiling ($32 billion).

Figure 2. Underreporting by the extent of withholding and information reporting, 2008-2010



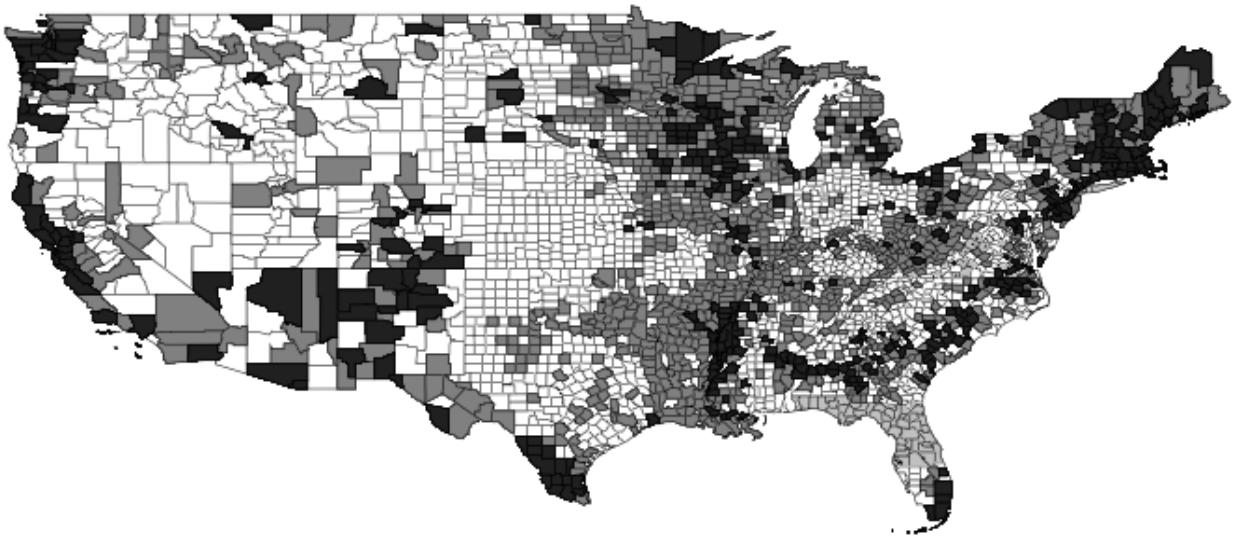

Notes: These statistics are from the Internal Revenue Service "Federal Tax Compliance Research: Tax Gap Estimates for Tax Years 2008-2010" (https://www.irs.gov/pub/irs-soi/p1415.pdf). The bars show the average annual underreporting tax gap by type of income, while the markers show the associated net misreporting percentages (NMP). The NMP is the ratio of the net misreported amount (i.e., understatements less overstatements) to the sum of the absolute values of the amounts that should have been reported, expressed as a percentage. Income is grouped into categories by type according to the degree of visibility. Category 1 includes amounts subject to substantial information reporting and withholding, and consists of wages and salaries. Category 2 includes amounts subject to substantial information reporting but no withholding, such as pensions and annuities, unemployment compensation, dividend income, interest income and Social Security benefits. Category 3 includes amounts subject to some information reporting including partnership and S-corporation income, capital gains and alimony. Category 4 includes amounts subject to little or no information reporting, such as nonfarm proprietor income, rents and royalties, farm income and other income.

Figure 3. Time series for alignment and macroeconomic conditions, by county partisan status



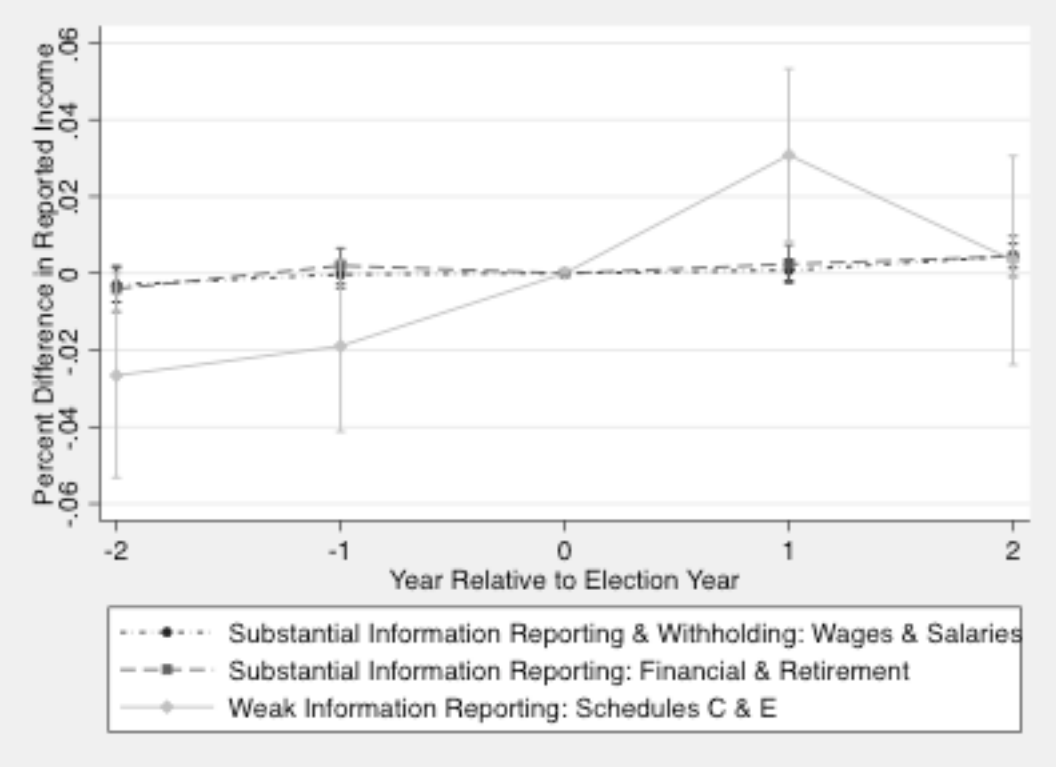

Notes: Partisan counties are classified as Democratic (454 counties) or Republican (1,453 counties) based on the two-party vote shares across the 1996-2008 presidential elections. County partisan alignment is the average share cast for the current president's party across these elections, so only changes following turnover elections. Also shown are the average numbers of unemployed persons per capita across counties of each type by year.

Figure 4. County partisanship status, 1996 through 2008 elections



Notes: The black shading indicates Democratic counties, identified as those that have a minimum Democratic share of the two-party vote across the 1996, 2000, 2004 and 2008 elections above 0.5. The white shading indicates Republican counties, where the maximum is always below 0.5. The dark grey shading indicates swing counties, where the share does not always fall on the same side of the threshold of 0.5. The light grey shading indicates counties (located primarily in Florida and Virginia) that are excluded from our analysis sample due to data quality issues, as described in the text. Three Hawaii counties included in the analysis are not depicted.

Figure 5. Estimated relative impact of moving into vs. out of alignment, 2000 and 2008 elections




Notes: This figure depicts the results from event study regressions for three different dependent variables: reported wages and salaries, financial and retirement income, and incomes from Schedules C & E (all expressed as log per capita amounts). Depicted are the estimated coefficients and 95 percent confidence intervals for indicators for years from the election for counties that move into alignment. The specifications pool the five-year windows surrounding the 2000 and 2008 elections and the sample is restricted to counties that are classified as partisan across the 1996-2008 elections. In the 2000 election, Republican counties are classified as moving into alignment after the election year (t=0), while Democratic counties are for the 2008 election. In addition to the baseline controls (the demographic and economic variables shown in Table 2b), the regressions include county x election fixed effects and year fixed effects, and standard errors are robust to clustering by county.

Table 1a. Alignment and confidence in government, General Social Survey

| *Independent variables* | Dep. var. = Level of confidence in the people running the federal executive branch | | | | |
|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) |
| Party-alignment with president | 0.227*** | 0.225*** | 0.226*** | | |
| | (0.018) | (0.018) | (0.018) | | |
| Choice for president | | | | 0.168*** | 0.190*** |
| | | | | (0.011) | (0.012) |
| Party-alignment with Congress | 0.032* | 0.034* | 0.034* | 0.020 | 0.021 |
| | (0.018) | (0.018) | (0.018) | (0.021) | (0.022) |
| Republican party identification index | 0.031* | 0.028 | 0.035* | 0.044** | 0.034 |
| | (0.018) | (0.017) | (0.019) | (0.021) | (0.023) |
| Conservative views index | | 0.004 | 0.001 | -0.008 | -0.009 |
| | | (0.020) | (0.018) | (0.018) | (0.021) |
| Includes additional controls | No | No | Yes | Yes | Yes |
| Restricted to voters | No | No | No | No | Yes |
| Mean of dependent variable | 0.429 | 0.425 | 0.425 | 0.426 | 0.427 |
| Number of observations | 37,357 | 33,992 | 33,992 | 31,610 | 22,236 |

Notes: Data are drawn from the 1972-2014 General Social Survey. Each column reports the results from a separate ordinary least squares regression. The dependent variable is the reported level of confidence and takes on three values: 0 is hardly any, 0.5 is some, and 1 is a great deal. All specifications include survey version by interview year fixed effects. Standard errors are clustered at the level of party identification x presidential term. The additional controls are a comprehensive set of respondent characteristics (as detailed in Appendix Table A2).

Table 1b. Alignment and confidence in government and institutions, General Social Survey

| *Independent variables* | Dependent variable = Level of confidence in: Executive branch | Congress | Supreme Court | Press | Major companies | Church |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Party-alignment w/ president | 0.226*** | 0.027*** | 0.044*** | -0.021*** | -0.002 | 0.006 |
| | (0.018) | (0.009) | (0.009) | (0.006) | (0.007) | (0.010) |
| Party-alignment w/ Congress | 0.034* | 0.069*** | 0.012 | 0.001 | -0.000 | 0.018** |
| | (0.018) | (0.012) | (0.011) | (0.009) | (0.007) | (0.009) |
| Republican party identification index | 0.035* | -0.002 | 0.018* | -0.071*** | 0.087*** | 0.040*** |
| | (0.019) | (0.011) | (0.010) | (0.008) | (0.007) | (0.009) |
| Conservative views index | 0.001 | -0.014 | -0.045** | -0.125*** | 0.068*** | 0.069*** |
| | (0.018) | (0.011) | (0.011) | (0.013) | (0.010) | (0.011) |
| Mean of dep. var. | 0.425 | 0.411 | 0.593 | 0.418 | 0.551 | 0.534 |
| Number of obs. | 33,992 | 33,985 | 33,576 | 34,198 | 33,647 | 33,676 |

Notes: Data drawn from 1972-2014 General Social Survey. Each column reports the results from a separate ordinary least squares regression. The dependent variable is the reported level of confidence in the people running the institution shown in the column heading, and takes on three values: 0 is hardly any, 0.5 is some, and 1 is a great deal. The control set includes fixed effects for survey version by year and a comprehensive set of respondent characteristics (as detailed in Appendix Table A2). Standard errors are clustered at the level of party identification x presidential term.

Table 1c. Alignment and tax and spending morale, General Social Survey

| | Dependent variables | | | | |
|---|---|---|---|---|---|
| *Independent variables* | Own income tax too high | Gov. spends too much | Gov. spends too little | Gov. should do less | Gov. should do more |
| | (1) | (2) | (3) | (4) | (5) |
| Party-alignment with president | -0.055*** | -0.024*** | -0.002 | -0.070*** | 0.007 |
| | (0.011) | (0.004) | (0.004) | (0.016) | (0.015) |
| Party-alignment with Congress | 0.020 | 0.008 | -0.004 | -0.011 | 0.010 |
| | (0.012) | (0.006) | (0.004) | (0.014) | (0.012) |
| Republican party identification index | 0.040*** | 0.028*** | -0.084*** | 0.250*** | -0.169*** |
| | (0.012) | (0.005) | (0.005) | (0.015) | (0.017) |
| Conservative views index | 0.108*** | 0.064*** | -0.090*** | 0.253*** | -0.145*** |
| | (0.018) | (0.007) | (0.006) | (0.025) | (0.019) |
| Mean of dep. var. | 0.628 | 0.241 | 0.432 | 0.324 | 0.273 |
| Number of obs. | 29,301 | 46,362 | 46,362 | 25,493 | 25,493 |

Notes: Data drawn from 1972-2014 General Social Survey. Each column reports the results from a separate ordinary least squares regression. The dependent variable is shown in the column heading, and the controls include fixed effects for survey version by year and a comprehensive set of respondent characteristics (as detailed in Appendix Table A2). Standard errors are clustered at the level of party identification x presidential term.

Table 2a. Summary statistics for dependent variables, IRS aggregates for policy constant filers

| | 1996 to 2008 elections | | |
|---|---|---|---|
| | D | R | Swing |
| *Reported income, per capita $2010* | | | |
| Gross income less capital gains | 20,575 | 18,186 | 17,457 |
| | (9,246) | (6,152) | (5,818) |
| Information-reported and withheld (wages, salaries and tips) | 15,578 | 13,592 | 13,251 |
| | (6,538) | (4,674) | (4,268) |
| Substantial information reporting (interest, dividend and retirement income) | 3,078 | 2,882 | 2,662 |
| | (1,783) | (1,312) | (1,211) |
| Little information reporting (Schedules C and E income) | 1,831 | 1,886 | 1,520 |
| | (1,726) | (1,506) | (1,130) |
| Adjusted gross income less capital gains (subtracts adjustments to income from gross income) | 20,246 | 17,883 | 17,195 |
| | (8,985) | (6,069) | (5,633) |
| Taxable income (subtracts deductions to income from AGI) | 15,606 | 13,398 | 12,753 |
| | (10,041) | (5,977) | (7,261) |
| *Filing rates, per capita* | | | |
| Filed an income tax return | 0.398 | 0.382 | 0.376 |
| | (0.056) | (0.057) | (0.052) |
| Used a paid tax preparer | 0.233 | 0.244 | 0.238 |
| | (0.042) | (0.044) | (0.040) |
| Filed a Schedule C with positive income | 0.045 | 0.051 | 0.046 |
| | (0.018) | (0.013) | (0.012) |
| Claimed the EITC | 0.096 | 0.077 | 0.086 |
| | (0.048) | (0.025) | (0.029) |
| Filed a Schedule C and claimed the EITC | 0.016 | 0.016 | 0.016 |
| | (0.010) | (0.007) | (0.007) |
| Filed a return exhibiting sharp bunching | 0.001 | 0.001 | 0.001 |
| | (0.002) | (0.001) | (0.001) |
| Return was audited | 0.004 | 0.002 | 0.002 |
| | (0.003) | (0.001) | (0.002) |
| Number of observations (county x year) | 1,816 | 5,812 | 4,392 |

Notes: The sample is the 3,005 analysis counties for the four years (1999, 2001, 2007, 2009) bracketing the turnover elections in 2000 and 2008. Means are shown for counties by partisan status, with standard errors in parentheses. The county aggregates are derived from returns filed by the subset of filers who would have been expected to file absent policy changes, such as the stimulus credits, as described in the text (and Appendix B). Sharp bunchers are those with dependents who both filed a Schedule C and reported net earnings within $500 of the first kink in the relevant EITC schedule. For both sharp bunching and audits, the sample is restricted to counties with populations of at least 10,000 for which the data are rarely censored due to nondisclosure rules.

Table 2b. Summary statistics for independent variables

| | 1996 to 2008 elections | | |
|---|---|---|---|
| | D | R | Swing |
| *Partisanship measures* | | | |
| Average Democratic two-party vote share, 1996-2008 | 0.619 | 0.336 | 0.475 |
| | (0.071) | (0.074) | (0.048) |
| Average Democratic two-party vote share, 1988-2008 | 0.609 | 0.354 | 0.490 |
| | (0.070) | (0.067) | (0.044) |
| *Demographic variables* | | | |
| Share of households single parent | 0.190 | 0.135 | 0.153 |
| | (0.069) | (0.036) | (0.040) |
| Share of households non-family | 0.333 | 0.295 | 0.310 |
| | (0.063) | (0.043) | (0.042) |
| Share of population under 18 years of age | 0.246 | 0.251 | 0.243 |
| | (0.039) | (0.033) | (0.029) |
| Share of population over 64 years of age | 0.137 | 0.156 | 0.155 |
| | (0.033) | (0.044) | (0.038) |
| Share of population foreign born | 0.065 | 0.036 | 0.029 |
| | (0.080) | (0.042) | (0.038) |
| Share of population living in urban areas | 0.575 | 0.364 | 0.370 |
| | (0.338) | (0.291) | (0.284) |
| Share aged 25+ with no high school diploma | 0.199 | 0.190 | 0.208 |
| | (0.103) | (0.080) | (0.084) |
| Share aged 25+ with high school diploma only | 0.318 | 0.353 | 0.369 |
| | (0.074) | (0.062) | (0.063) |
| Share aged 25+ with a BA or higher | 0.218 | 0.174 | 0.162 |
| | (0.114) | (0.066) | (0.076) |
| *Economic variables* | | | |
| Private nonfarm wage employment, per capita | 0.332 | 0.276 | 0.275 |
| | (0.167) | (0.137) | (0.113) |
| Government employment, per capita | 0.096 | 0.085 | 0.082 |
| | (0.054) | (0.043) | (0.048) |
| Number unemployed, per capita | 0.031 | 0.026 | 0.030 |
| | (0.014) | (0.013) | (0.013) |
| Number of private housing permits, per capita | 0.003 | 0.004 | 0.003 |
| | (0.003) | (0.005) | (0.004) |
| Private nonfarm establishments, per capita | 0.024 | 0.025 | 0.023 |
| | (0.010) | (0.008) | (0.008) |
| *Share of establishments by sector* | | | |
| Agriculture, forestry, fishing, hunting | 0.012 | 0.011 | 0.014 |
| | (0.019) | (0.019) | (0.023) |
| Mining | 0.004 | 0.013 | 0.009 |
| | (0.014) | (0.029) | (0.018) |
| Construction | 0.100 | 0.120 | 0.109 |
| | (0.038) | (0.048) | (0.039) |
| Manufacturing | 0.044 | 0.049 | 0.052 |

| | | | |
|---|---|---|---|
| | (0.019) | (0.026) | (0.024) |
| Transportation, utilities | 0.043 | 0.051 | 0.050 |
| | (0.030) | (0.033) | (0.026) |
| Wholesale trade | 0.048 | 0.050 | 0.045 |
| | (0.022) | (0.026) | (0.020) |
| Retail trade | 0.178 | 0.179 | 0.186 |
| | (0.044) | (0.039) | (0.037) |
| Finance, insurance, real estate | 0.095 | 0.093 | 0.092 |
| | (0.024) | (0.024) | (0.021) |
| Number of observations (county x year) | 1,816 | 5,812 | 4,392 |

Notes: The sample is the 3,005 analysis counties for the four years (1999, 2001, 2007, 2009) bracketing the turnover elections in 2000 and 2008. Means are shown for counties by partisan status, with standard errors in parentheses. The sources for the demographic variables are the Census (1980, 1990, 2000, 2010) and the ACS (2007, mid-year of 5-year average), and annual values are assigned based on linear interpolation. Information on private nonfarm employment and number of establishments and sector (NAICS) shares is from the Census County Business Patterns. Government employment is from the Bureau of Economic Analysis, the number unemployed is from the BLS, and private housing permits are from the Census Building Permits Survey.

Table 3a. Results for reported income, window analysis for 2000 and 2008 turnover elections

| Key independent variable | Log per capita gross income less capital gains | | | | AGI less capital gains | Taxable income |
|---|---|---|---|---|---|---|
| | Total | Wages & salaries | Financial & retirement | Schedules C&E | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ***Continuous alignment measures*** | | | | | | |
| Baseline alignment measure (calculated from average vote share 1996 to 2008) | 0.012** | -0.003 | 0.006 | 0.105*** | 0.015** | 0.033* |
| | (0.006) | (0.004) | (0.006) | (0.026) | (0.006) | (0.018) |
| Long-run alignment measure (calculated from average vote share 1988 to 2008) | 0.014** | -0.004 | 0.008 | 0.110*** | 0.016** | 0.038* |
| | (0.007) | (0.005) | (0.007) | (0.030) | (0.007) | (0.021) |
| ***Binary alignment measures*** | | | | | | |
| Indicator for party alignment, based on county partisanship status 1996 to 2008 | 0.002 | 0.000 | 0.000 | 0.026*** | 0.003** | 0.003 |
| | (0.001) | (0.001) | (0.002) | (0.007) | (0.001) | (0.005) |
| Indicator for party alignment, based on county partisanship status 1988 to 2008 | 0.003 | 0.000 | 0.000 | 0.030*** | 0.004* | 0.006 |
| | (0.002) | (0.001) | (0.002) | (0.008) | (0.002) | (0.006) |
| ***Continuous alignment x turnout*** | | | | | | |
| Baseline alignment measure x average 2-party turnout per capita 1996 to 2008 | 0.024* | -0.008 | 0.018 | 0.164*** | 0.030** | 0.079** |
| | (0.012) | (0.008) | (0.012) | (0.049) | (0.013) | (0.032) |
| Long-run alignment measure x average 2-party turnout per capita 1988 to 2008 | 0.030** | -0.010 | 0.022* | 0.185** | 0.035** | 0.091** |
| | (0.015) | (0.009) | (0.013) | (0.057) | (0.015) | (0.037) |
| ***Presidential approval*** | | | | | | |
| Party-specific presidential approval rating, based on partisanship status 1996 to 2008 | 0.004 | -0.000 | -0.001 | 0.048*** | 0.005** | 0.004 |
| | (0.003) | (0.002) | (0.003) | (0.012) | (0.003) | (0.009) |
| Party-specific presidential approval rating, based on partisanship status 1988 to 2008 | 0.005 | -0.000 | 0.000 | 0.057*** | 0.007** | 0.010 |
| | (0.003) | (0.002) | (0.003) | (0.014) | (0.003) | (0.011) |
| Dependent variable mean (in levels) | 18,283 | 13,769 | 2,832 | 1,744 | 17,990 | 13,498 |
| Dependent variable standard deviation | 6,683 | 4,929 | 1,367 | 1,429 | 6,530 | 7,258 |

Notes: Each cell reports results from a separate regression. The estimated coefficient on the political alignment measure is shown, with standard errors robust to clustering at the level of the county in parentheses. Rows indicate the alignment measure, while columns indicate the dependent variable. All dependent variables are expressed as log real ($2010) per capita amounts, and are the amounts reported for the subset of policy constant filers. In addition to county and state-by-year fixed effects, the control set includes non-farm private employment, government employment, unemployment, number of establishments, number of housing starts and the share of establishments by industry. Variables defined as shares are included in levels per capita, while all other variables enter in log per capita form. An exception is number of housing starts, which is included in levels due to the high rates of zero values. The regressions pool the years on either side of the two turnover elections (1999, 2001, 2007 and 2009) and include only partisan counties. There are 1,907 partisan counties in the medium run (over the 1996-

2008 elections), and 1,618 partisan counties in the long run (over the 1988-2008 elections). Due to missing or negative values for some of the outcomes, the number of observations varies slightly across cells, but the samples are always balanced in the sense that every county included in the estimation is represented all four years.

Table 3b. Results for number of returns, window analysis for 2000 and 2008 turnover elections

| **Key independent variable** | Log per capita number of returns | | | | | | |
|---|---|---|---|---|---|---|---|
| | All | Using Paid Preparer | With Positive Sched C | Claiming EITC | Sched C & EITC | Bunching | Audited |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| ***Continuous alignment measures*** | | | | | | | |
| Baseline alignment measure (calculated | 0.007** | -0.019*** | 0.027*** | -0.039*** | -0.030** | -0.019 | -0.118*** |
| from average vote share 1996 to 2008) | (0.002) | (0.004) | (0.006) | (0.004) | (0.009) | (0.025) | (0.023) |
| Long-run alignment measure (calculated | 0.005** | -0.022*** | 0.026*** | -0.044*** | -0.035*** | -0.032 | -0.113*** |
| from average vote share 1988 to 2008) | (0.003) | (0.004) | (0.007) | (0.005) | (0.011) | (0.029) | (0.027) |
| ***Binary alignment measures*** | | | | | | | |
| Indicator for party alignment, based on | 0.002** | -0.005*** | 0.007*** | -0.008*** | -0.007** | -0.003 | -0.032*** |
| county partisanship status 1996 to 2008 | (0.001) | (0.001) | (0.002) | (0.001) | (0.002) | (0.007) | (0.007) |
| Indicator for party alignment, based on | 0.001 | -0.006*** | 0.007*** | -0.010*** | -0.011*** | -0.007 | -0.031*** |
| county partisanship status 1988 to 2008 | (0.001) | (0.001) | (0.002) | (0.001) | (0.003) | (0.009) | (0.008) |
| ***Continuous alignment x turnout*** | | | | | | | |
| Baseline alignment measure x average 2- | 0.012** | -0.039*** | 0.043*** | -0.067*** | -0.049** | -0.042 | -0.199*** |
| party turnout per capita 1996 to 2008 | (0.004) | (0.007) | (0.013) | (0.012) | (0.018) | (0.052) | (0.049) |
| Long-run alignment measure x average 2- | 0.010** | -0.045*** | 0.042** | -0.078*** | -0.060** | -0.070 | -0.190*** |
| party turnout per capita 1988 to 2008 | (0.005) | (0.008) | (0.014) | (0.013) | (0.021) | (0.061) | (0.057) |
| ***Presidential approval*** | | | | | | | |
| Party-specific presidential approval rating, | 0.003** | -0.009*** | 0.012*** | -0.015*** | -0.016*** | -0.003 | -0.065*** |
| based on partisanship status 1996 to 2008 | (0.001) | (0.002) | (0.003) | (0.002) | (0.004) | (0.012) | (0.011) |
| Party-specific presidential approval rating, | 0.002 | -0.012*** | 0.011*** | -0.018*** | -0.021*** | -0.010 | -0.062*** |
| based on partisanship status 1988 to 2008 | (0.001) | (0.002) | (0.003) | (0.002) | (0.005) | (0.015) | (0.013) |
| Dependent variable mean (in levels) | 0.388 | 0.236 | 0.046 | 0.084 | 0.016 | 0.001 | 0.003 |
| Dependent variable standard deviation | 0.049 | 0.037 | 0.012 | 0.031 | 0.007 | 0.001 | 0.002 |

Notes: The notes to Table 3a apply, except that i) the dependent variables are expressed as log counts per capita, and ii) the sample is restricted to the subset of partisan counties with populations over 10,000 for the bunching and audit counts.

Table 4. Heterogeneity, window analysis for 2000 and 2008 turnover elections

| **Key controls** | Schedule C&E Income | Log per capita number of returns | | | |
|---|---|---|---|---|---|
| | | Claiming EITC | Sched C & EITC | Bunching | Audited |
| | (1) | (2) | (3) | (4) | (5) |
| ***Minority county in state*** | | | | | |
| Baseline continuous alignment | 0.163*** | -0.053*** | -0.021 | -0.022 | -0.072 |
| | (0.042) | (0.009) | (0.017) | (0.051) | (0.048) |
| Interaction with indicator for minority county within state | -0.142 | 0.036** | -0.023 | 0.006 | -0.106 |
| | (0.102) | (0.018) | (0.036) | (0.105) | (0.100) |
| ***State income tax piggybacking*** | | | | | |
| Baseline continuous alignment | 0.100*** | -0.056*** | -0.043** | -0.059 | -0.199*** |
| | (0.051) | (0.007) | (0.017) | (0.046) | (0.041) |
| Interaction with indicator for state tax system tied to reports on the federal return | 0.007 | 0.028** | 0.020 | 0.061 | 0.124** |
| | (0.057) | (0.009) | (0.020) | (0.055) | (0.049) |
| ***Dual alignment with president and governor*** | | | | | |
| Aligned with president only | 0.018** | -0.006*** | -0.005* | -0.002 | -0.041*** |
| | (0.007) | (0.002) | (0.003) | (0.009) | (0.008) |
| Aligned only with governor | 0.016 | -0.002 | -0.035** | -0.015 | -0.049** |
| | (0.019) | (0.005) | (0.013) | (0.025) | (0.020) |
| Aligned with both president and governor | 0.064*** | -0.016*** | -0.051*** | -0.021 | -0.059** |
| | (0.018) | (0.004) | (0.013) | (0.026) | (0.019) |

Notes: Each pair of cells in a panel reports results from a separate regression. The estimated coefficients on the key variables of interest are shown, with standard errors robust to clustering at the level of the county in parentheses. All dependent variables, indicated in the column headings, are the amounts or counts reported for the subset of policy constant filers. Other than the key variables of interest, the specifications are the same as in Tables 3a and 3b, and those notes apply. The presidential alignment variables are calculated based on county partisan status over the 1996 to 2008 presidential elections. Minority counties (16 percent of partisan counties) are counties in states where the average two-party vote across the 1996 to 2008 presidential elections has favored the other party. States whose systems are tied to reports on the federal return are those that begin state tax calculations with federal AGI, taxable income or tax liability. Nearly three quarters (72 percent) of counties are in such states. Party alignment with the governor is based on the governor's party and the average two-party vote share in gubernatorial elections over the same period as for the presidential alignment measure. In this sample, 35, 16 and 15 percent of county-years are aligned with the president only, the governor only, and both, respectively.

**Appendix A. General Social Survey sample and variables**

The General Social Survey (GSS) has been conducted during February, March and April for the years 1972-1978, 1980, 1982-1991, 1993-1994 and then for even years 1996-2014. We use the General Social Surveys 1972-2014 Cross-Sectional Cumulative Data File (Release 4, September 22, 2015).

We restrict our sample to interviews that either were or could have been conducted in English. The GSS did not begin interviewing in Spanish until 2006, so we first drop the Spanish language interviews (SPANENG equals 2) that would not otherwise have been conducted to maintain consistency. Specifically, we drop cases where the interviewer reported that the respondent would have been excluded due to lack of English proficiency (SPANINT equals 2). Since this variable is not available in 2010, for this year we exclude Spanish interviews where the respondent self-reported that it would have been difficult or impossible to do the interview in English (SPANSELF equals 2 or 3). In years when both variables exist, 94.5% of these individuals would have been deemed ineligible by the interviewer, while only 11.3% of those believing it would have been easy would have been deemed ineligible. Across the 2006 through 2014 surveys, 84.6% of the Spanish interviews are dropped, amounting to 4.0% of all interviews conducted in those years. After these restrictions are imposed, the total sample size falls from 59,599 to 59,073.

Starting from this sample, we drop an additional 923 respondents who report a party affiliation that does not fall on the scale between Republican and Democrat (i.e., PARTYID equals 7 for “Other party”), as well as 349 respondents failing to answer the party affiliation question. This reduces the number of observations by 2.2%, leaving 57,801 observations. Finally, we drop another 1,346 observations with missing information for demographic variables

(e.g., gender, years of education, household composition, work status, religion). This reduces the sample size by another 2.3%, resulting in a maximum potential sample size for the analyses of 56,455. The number of observations differs depending on the outcome considered, since some attitudinal questions are asked in a subset of years and/or to a (random) subset of respondents. All specifications control for a full set of fixed effects for the year-specific versions of the survey administered.

Observations are weighted to be representative of the non-institutionalized English speaking adult population within each year. We begin with the weight (WTSSALL) that takes into account the number of adults in the household, since the GSS only interviews one adult per household, as well as the sub-sampling of non-respondents starting in 2004. We then interact this weight with multipliers that adjust for oversamples conducted in 1982 and 1987 (OVERSAMP) and imperfect randomization of survey forms in 1978, 1980 and 1982-85 (FORMWT). The composite weight maintains the original sample size for the weighted sample, by design.

Table A1 provides details on the political and attitudinal variables we use in the analysis, while Table A2 provides details on the demographic and interview variables included in the control set.

Table A1. GSS political and attitudinal variables

| Analysis variable | GSS variable and question | Years |
|---|---|---|
| Republican party affiliation index (PARTYID rescaled to range from 0 to 1, treating 7 as missing) | PARTYID: Do you usually think of yourself as a Republican, Democrat, or Independent?<br>0 = Strong Democrat<br>1 = Not very strong Democrat<br>2 = Independent, close to Democrat<br>3 = Independent<br>4 = Independent, close to Republican<br>5 = Not very strong Republican<br>6 = Strong Republican<br>7 = Other party | All |
| Conservative views index (POLVIEWS rescaled to range from 0 to 1) | POLVIEWS: Where would you place yourself on this scale?<br>1 = Extremely liberal, 2 = Liberal<br>3 = Slightly liberal, 4 = Moderate<br>5 = Slightly conservative, 6 = Conservative<br>7 = Extremely conservative | 1974+ |
| Voted for current president | PRES68, PRES72 … PRES12: Which candidate did you vote for (if voted)? | All |
| Would have voted for the current president | IF68WHO, IF72WHO, … IF12WHO: Who would you have voted for if you had voted? | All |
| Confidence in institutions | As far as the people running these institutions, how much confidence do you have in them?<br>1 = A great deal, 2 = Only some, 3 = Hardly any | 1973-84, 1986+ |
| Federal executive branch | CONFED | |
| Congress | CONLEGIS | |
| U.S. Supreme Court | CONJUDGE | |
| Press | CONPRESS | |
| Major companies | CONBUS | |
| Organized religion | CONCLERG | |
| Own income tax too high | TAX: Do you consider the amount of federal income tax you have to pay as too high, about right, or too low?<br>1 = Too high, 2 = About right, 3 = Too low<br>4 = Pays no income tax (vol.) | 1976-77, 1980-82, 1984-85, 1987+ |
| Share of categories, among those with a valid response, that reports are spending too little, about right, and too much | For the following programs, are we spending:<br>1 = Too little<br>2 = About right<br>3 = Too much | 1973+ |
| Space exploration | NATSPAC: Space exploration program<br>NATSPACY: Space exploration<br>NATSPACZ: Advancing space exploration | |
| Environment | NATENVIR, NATENVIZ: Improving and protecting the environment | |

| | | |
|---|---|---|
| | NATENVIY: The environment | |
| Health | NATHEAL, NATHEALZ: Improving and protecting the nation's health | |
| | NATHEALY: Health | |
| Big cities | NATCITY, NATCITYZ: Solving the problems of big cities | |
| | NATCITYY: Assistance to big cities | |
| Crime | NATCRIME: Halting the rising crime rate | |
| | NATCRIMY: Law enforcement | |
| | NATCRIMZ: Reducing crime | |
| Drug addiction | NATDRUG: Dealing with drug addiction | |
| | NATDRUGY: Drug rehabilitation | |
| | NATDRUGZ: Reducing drug addiction | |
| Education | NATEDUC, NATEDUCZ: Improving the nation's education system | |
| | NATEDUCY: Education | |
| Assistance to blacks | NATRACE, NATRACEZ: Improving the conditions of blacks | |
| | NATRACEY: Assistance to blacks | |
| Military | NATARMS: The military, armaments and defense | |
| | NATARMSY: National defense | |
| | NATARMSZ: Strengthening national defense | |
| Foreign aid | NATAID: Foreign aid | |
| | NATAIDY: Assistance to other countries | |
| | NATAIDZ: Helping other countries | |
| Welfare | NATFARE: Welfare | |
| | NATFAREY: Assistance to the poor | |
| | NATFAREZ: Caring for the poor | |
| Government should do more (HELPNOT equals 1 or 2); Government should do less (HELPNOT equals 4 or 5) | HELPNOT: Some think the government in Washington is doing too many things that should be left to individuals and private businesses. Others think it should do more to solve our country's programs. Where are you on this scale? 1 = I strongly agree it should do more, 2 ... 4 5 = I strongly agree it is doing too much | 1975, 1983-84, 1986+ |

Notes: For the spending questions, rather than the standard versions, Y and Z versions were each asked of a third of the sample in 1984, and then Y versions were asked of a subset 1985 onward. We pool responses from all three versions, which differ slightly in the wording used to describe the spending program.

Table A2. GSS demographic and interview control variables

| Control variable | GSS variable |
|---|---|
| *Respondent demographic variables* | |
| Male | SEX equals 1 |
| Age in years | AGE (89 is 89 and over) |
| White | RACE equals 1 |
| Indicators for 2, 3, and 4+ household members 18 years and older | ADULTS |
| Indicators for 1, 2, 3 and 4+ earners in family | EARNRS |
| Indicators for 1, 2, 3, 4, 5 and 6+ children | CHILDS |
| Years of completed education | EDUC |
| Indicators for married, widowed, divorced, and separated | MARITAL |
| Indicators for Protestant, Catholic, Jewish, and Other Religion | RELIG |
| Indicators for respondent's current work status (8 categories) | WRKSTAT |
| Ever worked | WRKSLF not equal to N/A |
| Self-employed currently (or most recently if ever worked) | WRKSLF equal to 1 |
| Log of real family income (in 2000 dollars) | CONINC |
| Indicators for size of place (10 categories) | XNORCSIZ |
| Indicators for region (9 categories) | REGION |
| *Interview variables* | |
| A respondent incentive or fee was used | FEEUSED equals 1 or 2 |
| Interview done by phone | MODE equals 2 |
| Respondent was friendly and interested | COOP2 equals 1 (1972)<br>COOP equals 1 (1973+) |
| Respondent was cooperative but not particularly interested | COOP2 equals 2 (1972)<br>COOP equals 2 (1973+) |
| Respondent's understanding of the questions was good | COMPREND equals 1 |
| Indicators for version administered × year | 1972-87: FORM<br>1988-93: FORM×BALLOT<br>1994+: FORM×VERSION |

Notes: All of these variables are available in all survey years, other than FEEUSED (available starting 1998) and MODE (available starting 2004). For the interview variables, "no answers" and "refusals" are grouped with non-affirmative responses (so set to 0). For the other variables, "no answers" and "refusals" are set to missing, other than for log of real family income which has higher rates of missing values. For this variable, a separate indicator is included for missing income information, and log income is set to 0 in these cases.

## Appendix B. IRS tax return sample and variable construction

We begin with the universe of unedited population-level income tax returns (Form 1040) for tax years 1996 through 2012. From these, we pull detailed income, adjustment, deduction, and credit amounts and merge on information from all closed audits. We retain one tax return per primary taxpayer in each year, selecting the most recent form in the case of duplicates. To mitigate the effect of large outliers due to transcription and taxpayer reported errors, we compare the amounts against annual minimum and maximum values obtained from an edited nationally representative sample of returns (where the stratified random sampling technique samples high-income and high-loss returns with a rate approaching one).

Then, in order to minimize changes in the composition of taxpayers induced by the stimulus and other less dramatic tax policy changes, we identify the subset of tax returns filed by "policy constant" taxpayers. We define these taxpayers as those who would be likely to file taxes if the 1996 tax law were held fixed and extended to all later years. This set includes three groups. The first is taxpayers that meet minimum income thresholds for filing. The thresholds are based on total income less social security benefits and vary by tax filing status and age. To identify this group, we apply the relevant 1996 threshold, indexed for inflation. The second is taxpayers who report negative total income. These cases tend to be high wealth taxpayers experiencing business losses in a given year, and who are likely to file taxes in most years. The third group of taxpayers includes people below the filing threshold who are likely to file in order to claim the Earned Income Tax Credit (EITC), which is refundable. Specifically, these individuals are identified as having positive earnings (wages and self-employment income) and adjusted gross income below the inflation adjusted income limit in 1996. Notably, the overall structure of the EITC was quite

stable over this period, so that inflation adjusting the 1996 policy closely mimics the actual policy.

For both all taxpayers and policy-constant taxpayers, we then aggregate the amounts and counts from the 1040s to the county level (using the zip codes from the tax returns and a crosswalk from zip codes to counties that we constructed using mappings available from the Census, US Postal Service and the IRS). We exclude the counties in Northern Florida and New York that include zip codes for which a large number of records were deleted from the system in 1999.

The main consequence of limiting the sample to policy-constant tax returns is to eliminate the spike in returns attributable to the 2007 and 2008 stimulus programs. This is demonstrated in Figure B1. The figure plots the average number of tax returns per capita by year for all filers and policy constant filers, as well as gross income attributable to each type of filer. Across all years, returns filed by policy constant filers (0.38 per capita) represent about 90 percent of all returns (0.43 per capita). In 2007, overall filing rates per capita jump up by nearly 30 percent, and rates are also slightly elevated in the following year. Our strategy effectively screens out those (typically elderly) individuals with low income and earnings induced to file in those years in order to claim refundable credits, yielding a much smoother series. As the graphs for gross income per capita show, however, restricting the sample to policy constant filers has little impact on the aggregate amount of income reported. Since those pulled into filing are by definition low income, there is no comparable spike in all filers' gross income in the stimulus years, and nearly 99 percent of total gross income is attributable to policy constant filers in every year.

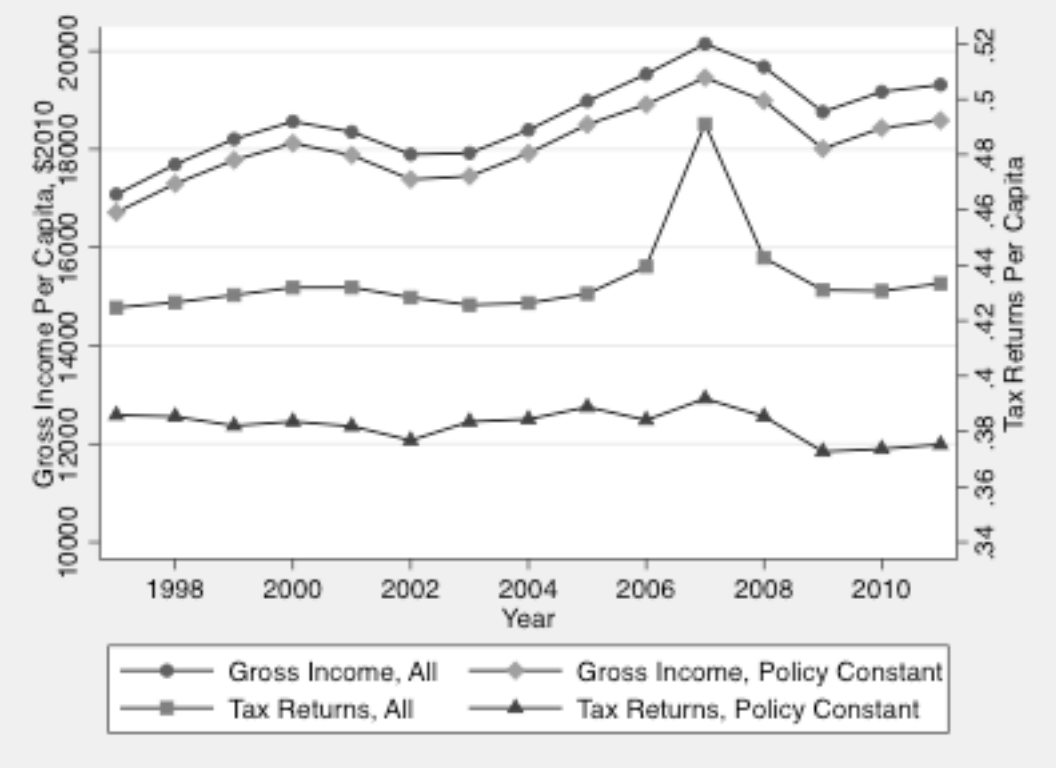
Though most of the evasion proxies we use in the analysis are consistently defined across the sample period, three are susceptible to policy changes. These three are gross income (less capital gains), adjusted gross income (AGI) and taxable income. For these three measures, we create policy constant versions based on provisions in 1996. These measures rule out some legitimate behavioral responses, but also do guard against confounding changes to the tax code.

The policy constant version of gross income (less capital gains) subtracts state income tax refunds and unemployment compensation. The logic is that state income tax refund amounts may reflect tax policy and compliance, and taxable unemployment compensation is net of the first $2,400 only in 2009. When subtracting adjustments to arrive at policy constant AGI, we then only allow adjustments that existed in 1996 (i.e., IRA deduction, moving expenses, self-employment tax, SEP/SIMPLE plans, penalty on early withdraw of savings and alimony paid). For those that existed, we apply the 1996 parameters and adjust for inflation when relevant. In practice, the policy constant version of AGI is almost always within 3% of the original version.

We use an analogous strategy to arrive at policy constant taxable income, applying the 1996 rules to personal exemptions, standard deductions and itemized deductions. Under our approach, taxpayers who itemize their deductions based on current tax law may not itemize under the policy constant measure, and we reassign them accordingly. Due to the way the data were processed, we unfortunately cannot truncate taxable income at zero return-by-return. Thus, aggregate potential exemptions and deductions are subtracted from aggregate policy constant AGI, leading to a policy constant taxable income measure that is conceptually different from the original. Exemptions and deductions that would push taxable income to zero, and thus no longer be counted as part of taxable income, are allowed to lower our policy constant measure of taxable income.

The first three columns of Table B1 reproduce results shown in Table 3a for our original income measures, while the next three show results from the same specifications for our policy constant income measures. The bottom rows document the relatively minor alterations to gross income and AGI, and the more major change to taxable income. The magnitudes of the point estimates for the coefficients on the alignment measures fall some for AGI and further for taxable income. This is likely in part because some of the potential channels for avoidance and evasion have been closed down and/or muted by our calculation.

Figure B1. Average reported gross income and tax returns per capita




Notes: The sample is the 3,005 counties in our full analysis sample. Average county per capita gross income (exclusive of capital gains) and number of tax returns are shown by year for all filers and for the subset that would be expected to file holding tax policy constant.

Table B1. Results for reported income, window analysis for 2000 and 2008 turnover elections

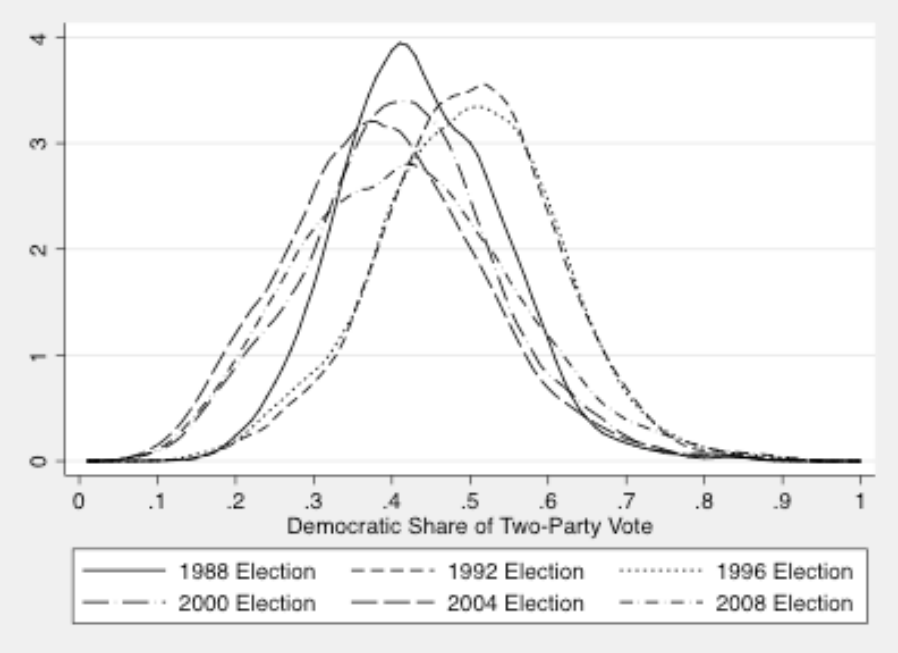
| **Key independent variable** | Original measure | | | Policy constant measure | | |
|---|---|---|---|---|---|---|
| | Gross Income | AGI | Taxable income | Gross Income | AGI | Taxable income |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| ***Continuous alignment measures*** | | | | | | |
| Baseline alignment measure (calculated from average vote share 1996 to 2008) | 0.012** | 0.015** | 0.033* | 0.012* | 0.011** | 0.019 |
| | (0.006) | (0.006) | (0.018) | (0.006) | (0.006) | (0.014) |
| Long-run alignment measure (calculated from average vote share 1988 to 2008) | 0.014** | 0.016** | 0.038* | 0.014** | 0.014** | 0.019 |
| | (0.007) | (0.007) | (0.021) | (0.007) | (0.007) | (0.017) |
| Dependent variable mean (in levels) | 18,283 | 17,990 | 13,498 | 18,107 | 17,942 | 10,730 |
| Dependent variable standard deviation | 6,683 | 6,530 | 7,258 | 6,642 | 6,578 | 5,168 |

Notes: Each cell reports results from a separate regression. The estimated coefficient on the political alignment measure is shown, with standard errors robust to clustering at the level of the county in parentheses. Rows indicate the alignment measure, while columns indicate the dependent variable. All dependent variables are expressed as log real ($2010) per capita amounts, and are the amounts reported for the subset of policy constant filers. The first three rows are based on the baseline income measures, and the last three use policy constant versions. For other details, see the notes to Table 3a.

## Appendix C. County partisan status

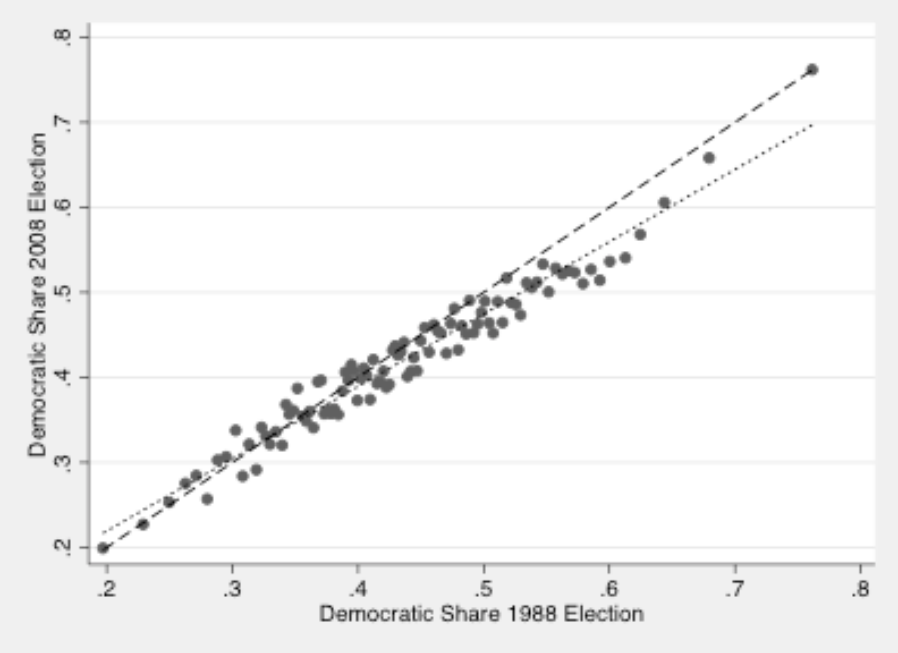
The longest time period we consider for classifying counties according to partisan status spans the 1988 to 2008 presidential elections. We base our classification on the Democratic share of the two-party vote. Figure C1 shows the probability distribution of this share for counties for each of the six elections in this period. National swings toward higher Democrat shares are apparent in the 1992 and 1996 elections. This trend reverses in 2000 and 2004, after which the pendulum swings back again. Despite these underlying time patterns, Democratic vote shares are highly persistent over time within counties. For example, Figure C2 shows the high correlation in vote shares across the 1988 and 2008 elections. Finally, Table C1 shows the shares of counties by partisan status using both the medium and longer run definitions.

Figure C1. Probability distribution of county Democratic vote shares by election




Notes: The sample underlying the distribution in each election is the 3,005 counties in our full analysis sample.

Figure C2. Correlation between 1988 and 2008 county Democratic vote shares




Notes: The sample is the 3,005 counties in our full analysis sample. Counties are binned by percentiles of the 1988 Democratic vote share distribution, and averages within those bins are plotted for both years. The thin dashed line shows the predicted value from a linear regression of the 2008 vote share on the 1988 vote share and a constant (which yields a coefficient of 0.85 (standard error 0.02) and an adjusted R-squared of 0.39).

Table C1. Types of counties by state

| State | Counties | 1996 to 2008 elections | | | 1988 to 2008 elections | | |
|---|---|---|---|---|---|---|---|
| | | D | R | Swing | D | R | Swing |
| Alabama | 67 | 0.16 | 0.52 | 0.31 | 0.15 | 0.51 | 0.34 |
| Arkansas | 75 | 0.12 | 0.12 | 0.76 | 0.09 | 0.05 | 0.85 |
| Arizona | 15 | 0.27 | 0.40 | 0.33 | 0.07 | 0.33 | 0.60 |
| California | 58 | 0.34 | 0.41 | 0.24 | 0.24 | 0.31 | 0.45 |
| Colorado | 63 | 0.19 | 0.56 | 0.25 | 0.17 | 0.54 | 0.29 |
| Connecticut | 8 | 0.88 | 0.00 | 0.13 | 0.13 | 0.00 | 0.88 |
| D.C. | 1 | 1.00 | 0.00 | 0.00 | 1.00 | 0.00 | 0.00 |
| Delaware | 3 | 0.33 | 0.00 | 0.67 | 0.00 | 0.00 | 1.00 |
| Florida | 18 | 0.28 | 0.56 | 0.17 | 0.00 | 0.56 | 0.44 |
| Georgia | 159 | 0.15 | 0.49 | 0.36 | 0.10 | 0.39 | 0.51 |
| Hawaii | 3 | 1.00 | 0.00 | 0.00 | 1.00 | 0.00 | 0.00 |
| Iowa | 99 | 0.27 | 0.20 | 0.53 | 0.27 | 0.16 | 0.57 |
| Idaho | 44 | 0.02 | 0.89 | 0.09 | 0.00 | 0.80 | 0.20 |
| Illinois | 102 | 0.15 | 0.28 | 0.57 | 0.12 | 0.18 | 0.71 |
| Indiana | 92 | 0.02 | 0.70 | 0.28 | 0.01 | 0.66 | 0.33 |
| Kansas | 105 | 0.02 | 0.96 | 0.02 | 0.01 | 0.90 | 0.09 |
| Kentucky | 120 | 0.02 | 0.51 | 0.47 | 0.01 | 0.38 | 0.61 |
| Louisiana | 64 | 0.13 | 0.17 | 0.70 | 0.11 | 0.17 | 0.72 |
| Massachusetts | 14 | 1.00 | 0.00 | 0.00 | 0.79 | 0.00 | 0.21 |
| Maryland | 24 | 0.21 | 0.63 | 0.17 | 0.13 | 0.63 | 0.25 |
| Maine | 16 | 0.69 | 0.00 | 0.31 | 0.00 | 0.00 | 1.00 |
| Michigan | 83 | 0.17 | 0.19 | 0.64 | 0.11 | 0.18 | 0.71 |
| Minnesota | 87 | 0.17 | 0.11 | 0.71 | 0.15 | 0.11 | 0.74 |
| Missouri | 115 | 0.03 | 0.44 | 0.52 | 0.03 | 0.29 | 0.69 |
| Mississippi | 82 | 0.27 | 0.56 | 0.17 | 0.21 | 0.55 | 0.24 |
| Montana | 56 | 0.09 | 0.71 | 0.20 | 0.09 | 0.66 | 0.25 |
| North Carolina | 100 | 0.18 | 0.55 | 0.27 | 0.17 | 0.47 | 0.36 |
| North Dakota | 53 | 0.04 | 0.72 | 0.25 | 0.04 | 0.72 | 0.25 |
| Nebraska | 93 | 0.00 | 0.95 | 0.05 | 0.00 | 0.95 | 0.05 |
| New Hampshire | 10 | 0.40 | 0.00 | 0.60 | 0.00 | 0.00 | 1.00 |
| New Jersey | 21 | 0.57 | 0.19 | 0.24 | 0.14 | 0.19 | 0.67 |
| New Mexico | 33 | 0.36 | 0.36 | 0.27 | 0.27 | 0.36 | 0.36 |
| Nevada | 17 | 0.06 | 0.76 | 0.18 | 0.00 | 0.65 | 0.35 |
| New York | 58 | 0.29 | 0.19 | 0.52 | 0.14 | 0.19 | 0.67 |
| Ohio | 88 | 0.17 | 0.55 | 0.28 | 0.13 | 0.53 | 0.34 |
| Oklahoma | 77 | 0.00 | 0.51 | 0.49 | 0.00 | 0.45 | 0.55 |
| Oregon | 36 | 0.22 | 0.56 | 0.22 | 0.19 | 0.47 | 0.33 |
| Pennsylvania | 67 | 0.15 | 0.52 | 0.33 | 0.06 | 0.51 | 0.43 |
| Rhode Island | 5 | 1.00 | 0.00 | 0.00 | 1.00 | 0.00 | 0.00 |
| South Carolina | 46 | 0.30 | 0.43 | 0.26 | 0.26 | 0.41 | 0.33 |
| South Dakota | 66 | 0.08 | 0.61 | 0.32 | 0.08 | 0.55 | 0.38 |
| Tennessee | 95 | 0.06 | 0.40 | 0.54 | 0.03 | 0.31 | 0.66 |
| Texas | 254 | 0.07 | 0.65 | 0.28 | 0.07 | 0.54 | 0.39 |

| | | | | | | | |
|---|---|---|---|---|---|---|---|
| Utah | 29 | 0.00 | 0.83 | 0.17 | 0.00 | 0.83 | 0.17 |
| Virginia | 81 | 0.16 | 0.59 | 0.25 | 0.12 | 0.59 | 0.28 |
| Vermont | 14 | 0.71 | 0.00 | 0.29 | 0.36 | 0.00 | 0.64 |
| Washington | 39 | 0.28 | 0.36 | 0.36 | 0.23 | 0.33 | 0.44 |
| Wisconsin | 72 | 0.35 | 0.13 | 0.53 | 0.29 | 0.11 | 0.60 |
| West Virginia | 55 | 0.09 | 0.22 | 0.69 | 0.09 | 0.20 | 0.71 |
| Wyoming | 23 | 0.00 | 0.87 | 0.13 | 0.00 | 0.78 | 0.22 |

Notes: The second column shows the number of counties included in the full analysis sample from each state. The next three columns show the share of counties that are classified as Democratic (D), Republican (R), and swing based on two-party vote shares across the 1996 through 2008 elections. The last three columns show the same shares based on vote shares across the 1988 through 2008 elections.

## Appendix D. Supplementary event study analyses, swing counties as controls

Figure D1a. Impact of moving into alignment

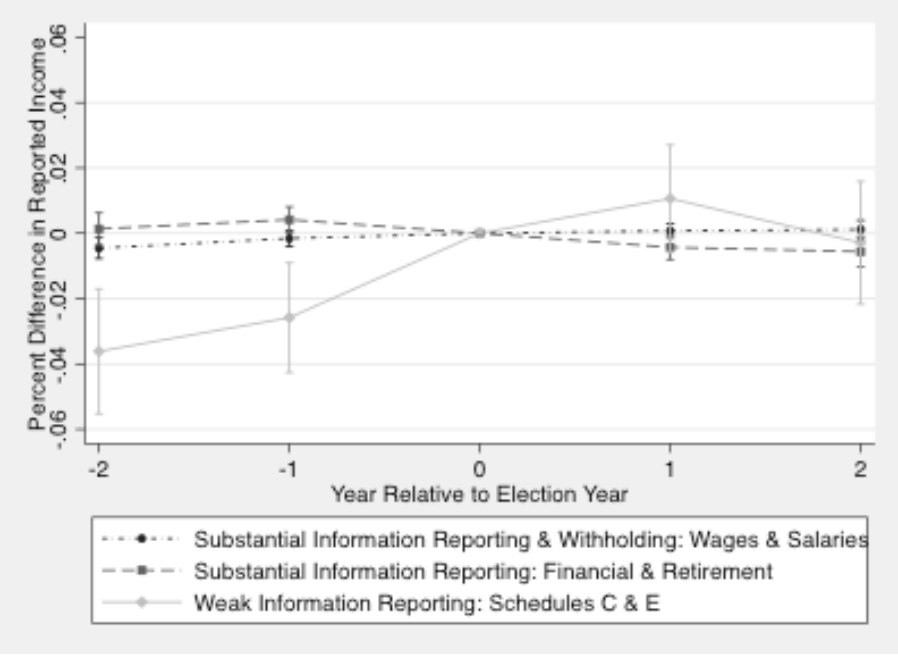
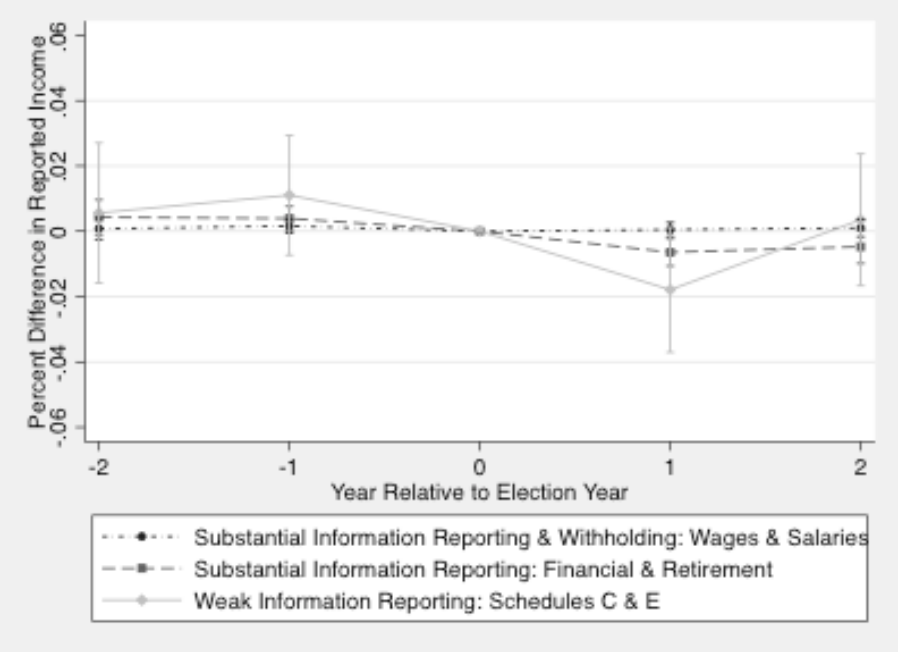



Figure D1b. Impact of moving out of alignment




Notes: These estimates are from specifications analogous to those presented in Figure 5 and those notes apply, except that the control group is swing counties rather than partisan counties moving out of alignment. In panel a), the sample is restricted to swing counties and partisan counties that will be moving into alignment in the upcoming election (i.e., Republican counties for the 2000 election period, and Democratic counties for the 2008 election period). In panel b), the sample is restricted to swing counties and partisan counties that will be moving out of alignment in the upcoming election.

## Appendix E. Robustness analyses

Table E1. Summary statistics for variables used in robustness analyses

| | 1996 to 2008 elections | | |
|---|---|---|---|
| | D | R | Swing |
| *Additional control variables, per capita $2010* | | | |
| Wages, tips and other compensation (W2 box 1) | 15,752 | 13,778 | 13,417 |
| | (6,508) | (4,665) | (4,253) |
| Share linked to S-corporation by employer id | 0.151 | 0.160 | 0.158 |
| | (0.045) | (0.050) | (0.047) |
| Share linked to C-corporation by employer id | 0.209 | 0.205 | 0.205 |
| | (0.074) | (0.067) | (0.069) |
| Share linked to partnership by employer id | 0.049 | 0.051 | 0.047 |
| | (0.027) | (0.029) | (0.028) |
| Financial income (1099INT taxable interest, 1099DIV | 839 | 791 | 706 |
| ordinary dividends) | (687) | (401) | (386) |
| Retirement income (1099R gross pension distributions; | 5,185 | 5,280 | 5,223 |
| 1099SSA Social Security net payments) | (1,695) | (1,640) | (1,435) |
| Unemployment compensation (1099G UI payments) | 206 | 150 | 184 |
| | (173) | (160) | (166) |
| Federal government grants and procurement contracts | 3,292 | 2,031 | 2,420 |
| | (3,895) | (3,183) | (4,823) |
| *Variables identifying subsamples* | | | |
| Population ever <1,000 (1990-2012) | 0.002 | 0.021 | 0.004 |
| | (0.047) | (0.142) | (0.060) |
| Population ever <10,000 (1990-2012) | 0.154 | 0.311 | 0.216 |
| | (0.361) | (0.463) | (0.411) |
| County contains the state capital | 0.055 | 0.006 | 0.013 |
| | (0.228) | (0.078) | (0.112) |
| Net commuter income flow >1/3 personal income | 0.108 | 0.163 | 0.099 |
| | (0.310) | (0.369) | (0.299) |
| Median home values fell by >10% 2007-2010 | 0.183 | 0.087 | 0.107 |
| | (0.387) | (0.282) | (0.309) |
| Number of observations (county x year) | 1,816 | 5,812 | 4,392 |

Notes: The sample is the 3,005 analysis counties for the four years (1999, 2001, 2007, 2009) bracketing the turnover elections in 2000 and 2008. Means are shown for counties by partisan status, with standard errors in parentheses. The first panel shows statistics for variables that are added to the control set in sensitivity analyses. The wage, financial, retirement and unemployment amounts are derived from the sums of all information return amounts received by county residents using the IRS CDW. Federal government grants and procurement contract amounts are from the Census Governments Division. The second panel shows variables used to identify subsamples. Annual population estimates are from the Census. The BEA estimates the ratio of net commuter income flow to personal income generated, and the indicator is set to 1 if the absolute value of the ratio ever exceeds 1/3 in the period 1990-2012. Median home values are from the ACS in 2007 and the Census in 2010.

Table E2a. Robustness of results to alternative control sets, window analysis for 2000 and 2008 turnover elections

| **Specification** | Log per capita gross income less capital gains | | | | AGI less capital gains | Taxable income |
|---|---|---|---|---|---|---|
| | Total | Wages & salaries | Financial & retirement | Schedules C&E | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Baseline specification and sample | 0.012** | -0.003 | 0.006 | 0.105*** | 0.015** | 0.033* |
| | (0.006) | (0.004) | (0.006) | (0.026) | (0.006) | (0.018) |
| ***Alternative control sets*** | | | | | | |
| Adding log per capita amounts from information returns | 0.010** | -0.004 | 0.008* | 0.101*** | 0.013** | 0.029 |
| | (0.005) | (0.003) | (0.005) | (0.027) | (0.005) | (0.018) |
| Adding quintics in per capita amounts from information returns | 0.009* | -0.003 | -0.005 | 0.102*** | 0.012** | 0.026 |
| | (0.005) | (0.003) | (0.004) | (0.025) | (0.005) | (0.018) |
| Adding log per capita amounts interacted with indicators for each election | 0.010* | -0.004 | 0.006 | 0.101*** | 0.013** | 0.027 |
| | (0.005) | (0.003) | (0.005) | (0.027) | (0.005) | (0.018) |
| Adding county x election fixed effects | 0.011* | -0.002 | 0.005 | 0.089*** | 0.014** | 0.035** |
| | (0.006) | (0.004) | (0.006) | (0.027) | (0.006) | (0.018) |
| Adding federal grants and procurement spending per capita | 0.012* | -0.003 | 0.004 | 0.096*** | 0.015** | 0.033* |
| | (0.006) | (0.004) | (0.006) | (0.026) | (0.006) | (0.018) |
| Excluding demographic controls | 0.012** | -0.003 | 0.005 | 0.104*** | 0.015** | 0.033* |
| | (0.006) | (0.004) | (0.006) | (0.026) | (0.006) | (0.018) |
| Excluding number of establishments and industry composition of establishments | 0.012** | -0.002 | 0.002 | 0.107*** | 0.015** | 0.033* |
| | (0.006) | (0.004) | (0.008) | (0.026) | (0.006) | (0.018) |
| ***Alternative dependent variables*** | | | | | | |
| Aggregates based on all filers | 0.010* | -0.004 | 0.006 | 0.099*** | 0.013** | 0.032* |
| | (0.006) | (0.004) | (0.006) | (0.025) | (0.006) | (0.018) |

Notes: The first row reports results for the baseline specifications shown in row 1 of Table 3a. The remaining rows report the coefficient and standard error (robust to clustering at the county level) on the baseline alignment measure but either add or subtract variables from the baseline specification. Details on the additional variables are provided in Table E1.

Table E2b. Robustness of results to alternative control sets, window analysis for 2000 and 2008 turnover elections

| **Specification** | Log per capita number of returns | | | | | | |
|---|---|---|---|---|---|---|---|
| | All | Using Paid Preparer | With Positive Sched C | Claiming EITC | Sched C & EITC | Bunching | Audited |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Baseline specification and sample | 0.007** | -0.019*** | 0.027*** | -0.039*** | -0.030** | -0.019 | -0.118*** |
| | (0.002) | (0.004) | (0.006) | (0.004) | (0.009) | (0.025) | (0.023) |
| ***Alternative control sets*** | | | | | | | |
| Adding log per capita amounts from information returns | 0.009*** | -0.015*** | 0.034*** | -0.034*** | -0.014 | -0.009 | -0.107*** |
| | (0.002) | (0.004) | (0.006) | (0.004) | (0.009) | (0.025) | (0.024) |
| Adding quintics in per capita amounts from information returns | 0.006** | -0.019*** | 0.027*** | -0.036*** | -0.025** | -0.016 | -0.089*** |
| | (0.002) | (0.004) | (0.006) | (0.004) | (0.010) | (0.025) | (0.024) |
| Adding log per capita amounts interacted with indicators for each election | 0.009*** | -0.015*** | 0.034*** | -0.034*** | -0.015 | -0.011 | -0.111*** |
| | (0.002) | (0.004) | (0.006) | (0.004) | (0.009) | (0.025) | (0.024) |
| Adding county x election fixed effects | 0.008*** | -0.020*** | 0.025*** | -0.033*** | -0.034*** | -0.033 | -0.126*** |
| | (0.002) | (0.004) | (0.006) | (0.004) | (0.010) | (0.027) | (0.024) |
| Adding federal grants and procurement spending per capita | 0.006** | -0.020*** | 0.027*** | -0.041*** | -0.031** | -0.020 | -0.120*** |
| | (0.002) | (0.004) | (0.006) | (0.004) | (0.010) | (0.026) | (0.024) |
| Excluding demographic controls | 0.007** | -0.018*** | 0.028*** | -0.039*** | -0.029** | -0.015 | -0.114*** |
| | (0.002) | (0.004) | (0.006) | (0.004) | (0.009) | (0.025) | (0.023) |
| Excluding number of establishments and industry composition of establishments | 0.007** | -0.018*** | 0.028*** | -0.038*** | -0.032*** | -0.024 | -0.124*** |
| | (0.002) | (0.004) | (0.006) | (0.004) | (0.009) | (0.024) | (0.023) |
| ***Alternative dependent variables*** | | | | | | | |
| Aggregates based on all filers | 0.000 | -0.023*** | 0.025*** | -0.042*** | -0.034*** | -0.019 | -0.130*** |
| | (0.003) | (0.004) | (0.006) | (0.004) | (0.009) | (0.025) | (0.023) |

Notes: The first row reports results for the baseline specification shown in row 1 of Table 3b. Otherwise, see the notes to Table E2a.

Table E3a. Robustness of results to alternative samples, window analysis for 2000 and 2008 turnover elections

| **Sample** | Log per capita gross income less capital gains | | | | AGI less capital gains | Taxable income |
|---|---|---|---|---|---|---|
| | Total | Wages & salaries | Financial & retirement | Schedules C&E | | |
| | (1) | (2) | (3) | (4) | (5) | (6) |
| Baseline specification and sample | 0.012** | -0.003 | 0.006 | 0.105*** | 0.015** | 0.033* |
| | (0.006) | (0.004) | (0.006) | (0.026) | (0.006) | (0.018) |
| ***Alternative samples*** | | | | | | |
| Including swing counties (alignment set to 0) | 0.016** | 0.001 | 0.007 | 0.118*** | 0.019*** | 0.043* |
| | (0.005) | (0.003) | (0.005) | (0.023) | (0.005) | (0.016) |
| Excluding counties with population <1,000 | 0.007 | -0.004 | 0.005 | 0.101*** | 0.010* | 0.030 |
| | (0.005) | (0.004) | (0.006) | (0.026) | (0.005) | (0.018) |
| Excluding counties with population <10,000 | 0.005 | -0.001 | -0.008 | 0.114*** | 0.008 | 0.008 |
| | (0.005) | (0.005) | (0.005) | (0.023) | (0.005) | (0.018) |
| Restricting sample to economically similar counties (via propensity score trimming) | 0.005 | -0.004 | 0.005 | 0.110*** | 0.007 | 0.035 |
| | (0.006) | (0.005) | (0.006) | (0.030) | (0.006) | (0.021) |
| Exclude counties containing capital cities | 0.014** | -0.003 | 0.008 | 0.107*** | 0.016** | 0.035* |
| | (0.006) | (0.004) | (0.006) | (0.026) | (0.006) | (0.019) |
| Exclude counties with large commuter flows | 0.013** | -0.005 | 0.011* | 0.130*** | 0.016** | 0.046** |
| | (0.007) | (0.004) | (0.006) | (0.026) | (0.007) | (0.021) |
| Exclude counties hit hard by the housing crisis | 0.011* | -0.003 | 0.010 | 0.099*** | 0.013** | 0.044** |
| | (0.006) | (0.004) | (0.006) | (0.028) | (0.006) | (0.020) |

Notes: The first row reports results for the baseline specification shown in row 1 of Table 3a. The remaining rows report the coefficient and standard error (robust to clustering at the county level) on the baseline alignment measure. Details on the variables used to identify the alternative samples are provided in Table E1.

Table E3b. Robustness of results to alternative samples, window analysis for 2000 and 2008 turnover elections

| **Sample** | Log per capita number of returns | | | | | | |
|---|---|---|---|---|---|---|---|
| | All | Using Paid Preparer | With Positive Sched C | Claiming EITC | Sched C & EITC | Bunching | Audited |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) |
| Baseline specification and sample | 0.007** | -0.019*** | 0.027*** | -0.039*** | -0.030** | -0.019 | -0.118*** |
| | (0.002) | (0.004) | (0.006) | (0.004) | (0.009) | (0.025) | (0.023) |
| ***Alternative samples*** | | | | | | | |
| Including swing counties (alignment set to 0) | 0.005** | -0.016*** | 0.021*** | -0.041*** | -0.035*** | -0.021 | -0.088*** |
| | (0.002) | (0.003) | (0.005) | (0.004) | (0.008) | (0.023) | (0.022) |
| Excluding counties with population <1,000 | 0.006** | -0.018*** | 0.030*** | -0.040*** | -0.032*** | -0.021 | -0.088*** |
| | (0.002) | (0.004) | (0.006) | (0.004) | (0.009) | (0.023) | (0.022) |
| Excluding counties with population <10,000 | 0.008*** | -0.010** | 0.028*** | -0.034*** | -0.029** | -0.021 | -0.088*** |
| | (0.002) | (0.004) | (0.005) | (0.004) | (0.009) | (0.023) | (0.022) |
| Restricting sample to economically similar counties (via propensity score trimming) | 0.007** | -0.013*** | 0.031*** | -0.030*** | -0.019* | 0.004 | -0.140*** |
| | (0.002) | (0.004) | (0.006) | (0.004) | (0.010) | (0.027) | (0.025) |
| Exclude counties containing capital cities | 0.006** | -0.019*** | 0.026*** | -0.040*** | -0.032*** | -0.024 | -0.121*** |
| | (0.002) | (0.004) | (0.006) | (0.004) | (0.010) | (0.026) | (0.024) |
| Exclude counties with large commuter flows | 0.005** | -0.019*** | 0.026*** | -0.038*** | -0.029** | -0.031 | -0.147*** |
| | (0.002) | (0.004) | (0.007) | (0.005) | (0.010) | (0.029) | (0.026) |
| Exclude counties hit hard by the housing crisis | 0.006** | -0.022*** | 0.021*** | -0.045*** | -0.042*** | -0.026 | -0.123*** |
| | (0.002) | (0.004) | (0.006) | (0.005) | (0.010) | (0.028) | (0.026) |

Notes: The first row reports results for the baseline specification shown in row 1 of Table 3b. Otherwise, see the notes to Table E3a.