POLITICAL ALIGNMENT, ATTITUDES TOWARD GOVERNMENT AND TAX EVASION

JULIE BERRY CULLEN, NICHOLAS TURNER & EBONYA WASHINGTON*

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ABSTRACT: We ask whether attitudes toward government play a causal role in the evasion of U.S. personal income taxes. We 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 counties, push voters in partisan counties into and out of alignment with the party of the president. We provide three types of evidence of alignment causally reducing evasion. As a county moves into alignment we find 1) conditional on county economic activity, taxpayers report more easily-evaded Schedule C&E income, with no changes in income that is third-party reported; 2) sharp-bunching around the EITC threshold decreases; and 3) audits, generally instigated by computer algorithms that identify suspect returns, decrease as do audits that result in additional tax liabilities assessed. Our results provide real-world evidence that a positive outlook on government lowers tax evasion.

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* University of California, San Diego; Federal Reserve Board; Yale University
E-mail addresses: jbcullen[at]ucsd.edu; Nick.Turner[at]frb.gov; ebonya.washington[at]yale.edu
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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] 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. Speaking to that second point, the IRS estimates that individuals fail to report only one percent of the most visible income—income that is both withheld and third-party reported. However, taxpayers fail to report some 63% or $136 billion of the least visible income—income subject to no withholding and little to no third-party reporting—such as proprietor income.

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

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

the agent undertakes if the benefits exceed expected costs. In this framework, the impact of the 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 on less visible income so high?[4]

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) extends the basic model to allow the agent to value the share of public goods received. 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.[5] Yet, to date there is little empirical evidence regarding the importance of these factors. Our innovation is to study 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. We address this challenge in three ways. First,

[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] Alm, McClelland and Schulze (1992) calibrate the Allingham and Sandmo model for the United States. They find that even a coefficient of relative risk aversion of 3, which is on the high end of estimates (Gandelman and Hernández-Murillo, 2014) only predicts a 14% compliance rate, far lower than even the 37% compliance rate for the least visible income.

[5] See the IRS's "Reducing the federal tax gap: a report on improving voluntary compliance" (August 2, 2007).

we follow a tax gap approach. We use measures of income reported 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. Second, we identify suspect claims of the Earned Income Tax Credit (EITC). Prior research (Chetty et al. 2012) suggests that the self-employed taxpayers who report the least amount of income that qualifies for the maximum EITC, a pattern Chetty, Friedman and Saez (2013) term "sharp bunching," is likely to reflect evasion. Most personal income tax audits are initiated by computer when reported amounts are discrepant with norms for similar returns in ways that correlate with prior detected evasion.[6] Therefore our third grouping of evasion proxies are audit rates and the rate of audits that yield additional tax liabilities.

The second data challenge we face is measuring government approval. 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 party of the current president.[7] 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.

[6] 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.

[7] 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 tax 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.[8]

Overall, our results provide novel evidence for an attitudinal component to tax compliance.[9] Combining evidence from our survey (GSS) and administrative (IRS) data, we demonstrate that when a higher fraction of county residents holds a positive view of government, a lower fraction of individual income tax is evaded. As a county moves out of alignment, conditional on economic activity, we find no change in the reporting of visible third-party reported income but that reporting of less visible income decreases by about 2.6 percent. We also show that sharp-bunching around the EITC threshold and the rate of audits that result in additional tax liabilities increase, which we interpret as further evidence of tax evasion.

In addition to conducting extensive robustness tests to ensure we have adequately controlled for underlying economic conditions, we perform a limited set of heterogeneity analyses. These reveal that evasion responses to alignment with the president are muted when the costs of evasion increase because the 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

[8] As part of our data agreement with the IRS, we do not attempt to estimate differential impacts by party affiliation.
[9] 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 et al. (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.

are mediated by another layer of alignment. Similarly, evasion responses are magnified when the county is unaligned 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 discussion and 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, 2012) 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.[10] 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,

[10] 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.

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,[11] 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.[12] 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 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

[11] See Appendix A for more detail on our GSS sample and variable construction.
[12] 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.

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.[13] 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.[14] 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-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

[13] 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.
[14] 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.

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".[15] 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 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.[16] In column 2, we see that presidential alignment is negatively and significantly associated with feeling there is too much spending. We

[15] 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.
[16] 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.

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.[17]

In summary, the results from the GSS analysis provide support for party alignment being a meaningful proxy for approval of the executive branch. Further we note that there is 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 Measuring evasion

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). 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

[17] 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).

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 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.[18] 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. 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.

Our second approach to identifying evasion is to identify 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. 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. Guyton et al.

[18] We create our own aggregations from the population returns, collapsed to the county year level. 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. See Appendix B for more details on IRS variable creation.

(2018) provide additional evidence that many self-employed returns that claim the EITC are suspect. Exploiting the random assignment of audits, they show that among EITC taxpayers with self-employment income, those that are randomly audited are roughly 40 percent less likely to claim the EITC in the year following the audit, compared to returns with similar audit risk scores that were not audited. 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 as markers of evasion. 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 minimum income required for the maximum credit.

Finally, we infer evasion using 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. In addition to the audit rate, we look at the fraction of returns adjudicated via audit to owe additional tax.

For all tax outcome variables, we are concerned about selection. Namely, there is 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. To guard against this possibility we rely on a subset of returns 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.[19]

[19] See Appendix B for more details and Appendix E for results that use the full, unrestricted sample and yield qualitatively similar results.

2.2 Methodology

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. Instead, 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, Barack Obama (Democrat) was elected, changing the party in the White House once again. For our primary regression analyses, we employ 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.[20] An advantage of our window analysis is that it balances the number of years each county is in versus out of alignment, and accounts for the constraint that the IRS information returns data we use to capture the level of economic activity is first available in 1999. We omit the election year because of the difficulty in defining alignment for that tax year. For election years, income is earned under one president and reported (the following April) under another. Because evasive behavior may begin in advance of filing, for example by increasing one's share of less visible earnings, alignment is not well defined for these transition years.

We restrict the sample to partisan counties and the four window years around the two

[20] 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.

turnover elections.[21] 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:

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

where α is a vector of county fixed effects and δ is a vector of 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 presidential elections 1996 to 2008 in our base specification. 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. To account for correlation over time, reported standard errors are clustered at the county level. 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 β as the causal effect of alignment on evasion is omitted time varying factors correlated with alignment and evasion, the most obvious being varying economic conditions. 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).[22] A second

[21] We demonstrate robustness of our results to the inclusion of swing counties in Appendix Table E4.
[22] 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

channel that has been documented is federal spending targeted to counties on the basis of political alignment.[23]

Figure 3 demonstrates this key challenge for our sample period where 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. To control for varying economic environments, the vector **X** includes time-varying factors drawn from IRS third-party information reports that control for the amounts and types of income generated in a county. Specifically, we include log per capita information return amounts (wages from W2 forms and financial, retirement and unemployment income from 1099 forms) and the shares of wages paid by different types of businesses (S-corporations, C-corporations and partnerships).[24] These shares control for the composition of business activity, and possible shifting between personal and corporate tax bases. Finally, to allow for the differential economic cyclicality of less visible income sources, we interact per capita unemployment compensation with the pre-period share of self-employed in the county as recorded in 1990. It is important to allow for this flexibility since Republican

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too find no detectable effect on AGI or wages.

[23] Dynes and Huber (2014) show an explicit link between voter alignment with the president and federal government transfers in the United States. 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. In the Portuguese context, Migueis (2013) demonstrates an impact of municipal government alignment with the federal government on federal transfers to the municipality. Brollo and Nanncini (2012) and Bracco et al. (2015) find that pre-election transfers increase to aligned municipalities in Brazil and Italy, respectively. Dell (2015) demonstrates that violence increases in Mexican municipalities following a close mayoral election in which the PAN party is victorious, attributing this to increased transfers from the PAN federal government allowing mayors to crack down on the drug trade.

[24] We create the wage share variables by linking the W2 forms to various business tax returns by employer identification number. Appendix B provides more details on the IRS variable construction. Unfortunately, data from the 1099-MISC, which would additionally capture some visible forms of self-employment income, are not available in window year 1999.

counties tend to have higher shares self-employed.[25] We provide evidence that this share succinctly captures the key economic differences between otherwise similar Republic and Democratic counties by demonstrating robustness of our findings to additionally interacting our unemployment intensity measure with a county's propensity to vote Democratic (as predicted by economic variables drawn from a variety of government sources excluding the IRS) and with a county's average vote share. We further show robustness to both adding these alternative economic variables or substituting them for our information reported variables.

Our variety of dependent variables also addresses concerns that results may be driven by economic activity. As we noted previously, we compare results across more and less visible income sources and use proxies that are less dependent on accurately measuring true taxable income generated, 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. This problem is known as the ecological fallacy. Because attitudes of networks are shocked at the same time as own attitudes, we are unable to discern whether our impacts are due to changes to own tax morale or due to changes in the attitudes of peers that would operate through social multipliers. Therefore, in the conclusion we discuss implications for policies that would be targeted to populations, not to individuals.

A final concern is that taxpayers may perceive the probability or cost of audits as varying inversely with alignment. The three cross-sectional surveys that we were able to locate that ask

[25] Allowing for differential cyclicality is the primary innovation relative to the NBER working paper version of this paper, Cullen, Turner and Washington (2018).

both about party identification and audit perception, suggest that this is not a concern. We find that Republican and Democratic respondents do not have significantly different expectations of audits at any of our three survey time points, two during a Republican administration and one during a Democratic presidency (see Appendix C for details). Further to the extent that there are differential audit probabilities that we were not able to detect, they would serve to drive our results toward zero as the increase in evasion from being out of alignment would be tempered by a decrease in evasion due to its perceived costs.

2.3 Sample and summary statistics

We characterize a county's partisanship status by the average two-party vote shares across the 1996 to 2008 presidential elections.[26] Fifteen percent are always majority Democratic, 48 percent are always majority Republican, and the remaining counties are swing counties.[27] Figure 4 shows the geographic distribution of counties by partisan status. Our analysis focuses on the 1,907 partisan counties for which we have needed data.[28] While many states have large majorities of supporters of one party, most states still have heterogeneity across counties in party leaning.

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

[26] County vote returns were purchased from http://uselectionatlas.org/. See Appendix D 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.

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

[28] 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.

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 relatively more income from less visible sources. Larger shares of residents of Democratic counties claim the EITC. However, sharp-bunching is a rare event in both types of counties.

3. Results

3.1 Baseline "window" analysis

The first row of Table 3 presents our baseline estimates of the relationship between alignment with the president and evasion. Each cell of the table contains the coefficient and standard error on alignment from a different specification of 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. The dependent variable, which varies across columns, is 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. As described above, in addition to county and state-by-year fixed effects, the control set in the first row includes controls for income generated based on variables constructed from the information returns as well as the interaction between unemployment and self-employment intensity. The subsequent rows present results for more and less restrictive versions of the control set, which are discussed in the next subsection.

Our first evidence of a causal link between alignment and evasion comes from the tax gap approach in the first three columns of Table 3. The small and insignificant point estimate in the first cell of the table indicates that the amount of wage and salary income reported, conditional on our controls for income generated, does not vary as a county moves into

alignment. Since our presumption is that reductions in reported amounts conditional on observed economic activity reflect evasion, this null finding is reassuring since there is little scope for evasion on this type of income.[29] Similarly, we find no responsiveness of financial and retirement income in the second cell, which is also largely visible to the government. However, moving to the third cell, we find that as alignment increases (decreases) by one, reporting of the less visible Schedule C&E increases (decreases) by a significant 0.088 log points. 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 62 percent (34 percent) of its vote to the Democrat; therefore, the average change in alignment is about 30 percentage points. Normalized by this average change in alignment, we find that moving into alignment increases the amount of Schedule C& E income reported in the average partisan county by 2.6 percent, or about $50 per person per county moving into alignment.[30] 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. Notably, underreporting of business income accounts for nearly a third of the IRS estimation of the tax gap (IRS, 2016).[31]

Our second type of evidence for a causal link between alignment and evasion is suspect EITC claims. Claiming the EITC unambiguously decreases tax liability. Depending on family,

[29] We also find no impact of alignment on the more aggregate reported income measure of gross income less capital gains, which we omit for brevity.

[30] Throughout the discussion of our results, we refer to effects in percent changes adjusted for the average change in county alignment rather than log points for the zero to one change expressed in the table. This involves first scaling the effects by the average difference in vote shares of roughly 30 percentage points and then uses the simplification that log(1+x) is roughly equal to x when x is small.

[31] In the first three columns of Table 3, 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 NBER working paper version of this paper, Cullen, Turner and Washington (2018), 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 omit analysis of these outcomes here because they embed unobserved legal avoidance so map less directly to evasion.

structure, the maximum credit is potentially large.[32] 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, both gains and losses, count towards earnings for the EITC and are not third-party verified. 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).[33] Alignment decreases the rate of EITC claims by about 0.9% in the average partisan county as shown in the first row of column 4. Even more suggestive of decreased evasion, moving into alignment decreases the rates of filing both Schedule C and the EITC by a significant 1.5%. Finally, despite both the rarity of the event and the reduction in our sample size,[34] we find further evidence that moving into alignment decreases evasion when analyzing sharp-bunching returns. As the average county moves into alignment, there is a 2.4% reduction in sharp bunching returns.

Our final two dependent variables are related to audits. We see in the first cell of column 7 that residents of the average county that moves into alignment are nearly 4% less likely to submit returns that are audited. And, not only are those in alignment less likely to be audited conditional on economic activity, they are less likely to be audited and found to have underreported income (column 8).

3.2 Robustness to varying economic controls

In summary, the first row of Table 3 presents results from three types of tests (tax gap,

[32] 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.

[33] This rate is lower than the 2.1% rate reported in Chetty, Friedman and Saez (2013) because of how the denominator is constructed. Our rate is relative to the county year population as opposed to the number of EITC returns with children that have income in the EITC-eligible range.

[34] For both the sharp bunching and audit outcomes, the sample is restricted to partisan counties with populations over 10,000 to avoid missing data due to masking for nondisclosure. We demonstrate the robustness of our other outcomes to limiting the sample to larger counties in Appendix Table E4.

sharp bunching and audits) that consistently point to an economically and statistically significant causal impact of alignment on evasion. The greatest threat to this interpretation is unobserved economic activity that is correlated with alignment. The remainder of Table 3 addresses this concern.

Recall that our control set includes measures of amounts and types of income earned by county residents drawn from IRS information returns and the interaction of the best proxy for cyclicality from these (unemployment benefits received) with self-employment intensity (the 1990 share of residents self-employed). We argue that the interaction is necessary to allow for differential cyclicality of less visible economic activity for Democratic and Republican counties. That is, while controls such as wages and unemployment benefit amounts capture conditions for households with wage earners well, they mail fail to capture the dynamics of earnings for small business owners. The interaction allows small business activity to evolve with the local business cycle according to its importance as a sector. In row 2 of Table 3 we omit this interaction. This serves to decrease some of our estimates in magnitude, leaving the sharp bunching result insignificant and the found underreporting in an audit result only marginally so.

In the remaining two rows, we explore whether the interaction we have included is not only necessary but also sufficient, by instead expanding the control set to allow for additional differential cyclicality. We use non-IRS baseline economic variables to predict the propensity for a county to be partisan Democratic as opposed to Republican.[35] We then add an additional interaction between this propensity and our measure of the local business cycle. Results, shown in row 3, are largely unchanged from the baseline. To push even further we then also add an

[35] Predictors include non-farm private employment, government employment, unemployment, and number of establishments, as well as number of housing starts and the share of establishments by industry (as detailed in Appendix Table E1). All are from 1990 and all but housing starts (which has high rates of zeroes) are expressed in log per capita. The prediction equation is run in the sample of medium-run partisan counties.

analogous interaction using the county average vote share, which is the variable that is used to construct alignment each year. While some coefficients increase and others decrease in magnitude, as demonstrated in row 4, all three tests still yield significant evidence of a causal link between political alignment and evasion. We choose the more parsimonious specification in row 1 as our baseline specification to avoid over-controlling for differences across partisan counties that are not economic.

We further explore robustness of our result to additional economic controls in Appendix Table E3. We demonstrate that we still find tax-gap, bunching and audit evidence of the impact of moving out of alignment on evasion when in comparison to our baseline specification we 1) allow for greater flexibility of information return controls by interacting them with indicators for the second election; 2) include directly as controls time-varying versions of the non-IRS economics variables used as predictors for county partisan status; 3) include these prediction variables and remove the information return variables; 4) compare a county's post-election tax behavior to only its pre-election tax behavior from the same election by including county by election fixed effects and 5) include county demographic controls. And, although we find that when a county moves into alignment it is more likely to receive federal grants and procurement, controlling for these has little effect.

We additionally address concerns about economic differences among counties through propensity score trimming and eliminating counties hit hard by the housing crisis, as shown in Appendix Table E4. In other specifications in the table, we exclude counties that are likely to have greater divergence between measured economic activity and resident incomes including those that are the location of capital cities and those with large commuting flows.[36]

[36] In results not shown, we 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. For example, in the baseline

Thus, we have provided robust evidence of a causal link between alignment and evasion using three sets of outcomes. The tax gap approach, that defines evasion as reductions in reported amounts conditional on generated amounts, has the most stringent requirements for controlling adequately for a county's economy, something that our robustness tests suggest we have done. Within the tap gap approach, however, what is most indicative of evasion is not any single specification, but the fact that we find an impact of alignment on the more invisible Schedule C&E income, but not on information reported income, all using the same control set. And, notably, the complementary approaches using sharp bunching and audit measures also reveal that alignment reduces evasion.

3.3 Robustness to varying measures of alignment

In our baseline model, we define alignment as the average presidential vote share across the 1996 to 2008 elections. We investigate in Appendix Table E5 how dependent our results are on this baseline definition. The pattern and magnitude of our results are robust to increasing the time span over which we take the vote share average, substituting a binary measure for our continuous measure, scaling our continuous measure by average county turnout and substituting presidential approval for vote share out of a concern that vote share is not as good a measure of sentiment toward the president three years after the election as compared to only one year.[37]

3.4 Event study

Another concern readers may have about our baseline specification is that we include only the years immediately before and after the election year. We do not include the election

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specification in the first row of Table 3, all estimated coefficients retain the same level of statistical significance other than that for Schedule C&E reported income, which falls from the 1% to the 5% level.

[37] To compare the magnitudes of the estimates, note that the average change in the key independent variable associated with moving into alignment is 0.3 for the continuous alignment measures, 1 for the binary measure, 0.16 for the interactions with average turnout and 0.6 for presidential approval. The implied impacts of moving into alignment are quantitatively similar across measures, since the magnitude of the estimated coefficients shrink (grow) in proportion to increases (decreases) in the magnitude of the swings in the alignment measures.

year because the treatment status is unclear as income is earned under an administration of one party but taxes are paid under the administration of the other. We do not use a larger window around the election year because of the lack of information return data prior to 1999.

While the window study is our preferred specification, in Figure 5 we provide a more dynamic view of our results, using an event study timeframe. We substitute the economic controls from Appendix Table E1 for the information return variables, allowing us to include all years t-2 to t+2 of around the 2000 and 2008 turnover elections. We continue to control for county and state-by-year fixed effects and to cluster standard errors by county.

In Figure 5, we plot coefficients estimating the conditional differences from two years before through two years after the two elections for counties moving into alignment relative to those moving out of alignment for the three tax gap outcomes. The election year difference for each is normalized to zero. In the figure we use a binary alignment variable for ease of interpretation.[38] The flat lines through zero for wage and salary and financial and retirement income indicate that the reporting of these forms of income, subject to substantial information reporting, does not change meaningfully throughout the five-year sample period. However, as the diamond line in the figure shows, partisan counties that are out of alignment report significantly less Schedule C&E income two years prior to the election relative to the counties that are in alignment. The year after moving into alignment these same counties report significantly more Schedule C&E income relative to their counterparts that are now unaligned.

While the figure indicates that the impact of moving into alignment lasts only one year post-election, the interpretation for the years not directly surrounding the election is less clear. The response in t+2 of our turnover elections is concurrent with t-2 of the next election, which

[38] Our baseline specification using binary alignment can be found in Table E5 row 1. The continuous version of the figure can be found in Figure E1.

although not a turnover election may still be a salient event for shaping attitudes towards the government. Despite this difficulty, the results of Figure 5 are consistent with the idea that moving into alignment (effect at t+1) with the president increases tax compliance relative to being out of alignment (effect at t-1).

3.5 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 results we ask how the impact of alignment varies with state tax codes and alignment with state executives.

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, we substitute a binary alignment measure for our continuous measure and add an interaction between that binary measure and an indicator for states that piggyback on the federal income tax. (Estimates from the specification that includes just the binary alignment main effects are shown in the first row for comparison.) The interaction term is of the opposite sign from the main effect in all but one column, and in three cases significantly so. The results are consistent with these ties increasing the costs of evasion and therefore moderating the responsiveness to alignment.

In the "Dual alignment with president and governor" section, we show that the impact of alignment, again captured by a binary variable, 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. Being doubly unaligned increases the benefit of evasion as it allows one to express displeasure with, or at least withhold funds from, two administrations.[39]

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.

We then use tax and voting outcomes at the county-level and an identification strategy based on partisan counties being moved into and out of alignment by voters in swing counties, to provide three types of evidence all supporting a causal impact of alignment on tax evasion. First, using the tax gap approach, 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 2.5 percent as the average county is moved into alignment.

Second, we find evidence of sharp bunching of income around the EITC phase-in level. As the average county moves out of alignment its population is 2.4 percent more likely to file returns that note dependents, non-zero Schedule C income and net earnings within $500 of the level associated with the end of the EITC credit phase-in range applicable to the tax unit.

Thirdly, we demonstrate that audits significantly decrease when a county moves into evasion. As audits are predominantly instigated by algorithms designed to detect those likely to be evading, it is not surprising that we find that the fraction of returns adjudicated in audit to owe

[39] Another type of heterogeneity that would be interesting to explore is by county party. However, we remind readers that our data use agreement precludes exploring differential impacts by party.

additional taxes decreases as well.

Finally, we provide evidence that all three responses are muted when the cost of evasion increases because federal income tax reports are direct inputs to state tax returns. Responses are magnified when the benefits of evasion increase because county residents are doubly unaligned, with 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.[40]

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[40] While field experiments measuring the impact of information on government spending on evasion have shown mixed results, we note that the mailing interventions are quite weak and thus we conclude that the impact of a large-scale government information campaign on evasion in the United States is still unknown. See Blumenthal et al. (2001) and Castro and Scartascini (2015) for null results in Minnesota and Argentina, respectively, and Bott et al. (2017) and Hallsworth et al. (2017) for the positive impacts in Norway and the United Kingdom. Dorrenger and Peichl (2017) find large positive impacts in a survey experiment in Germany.

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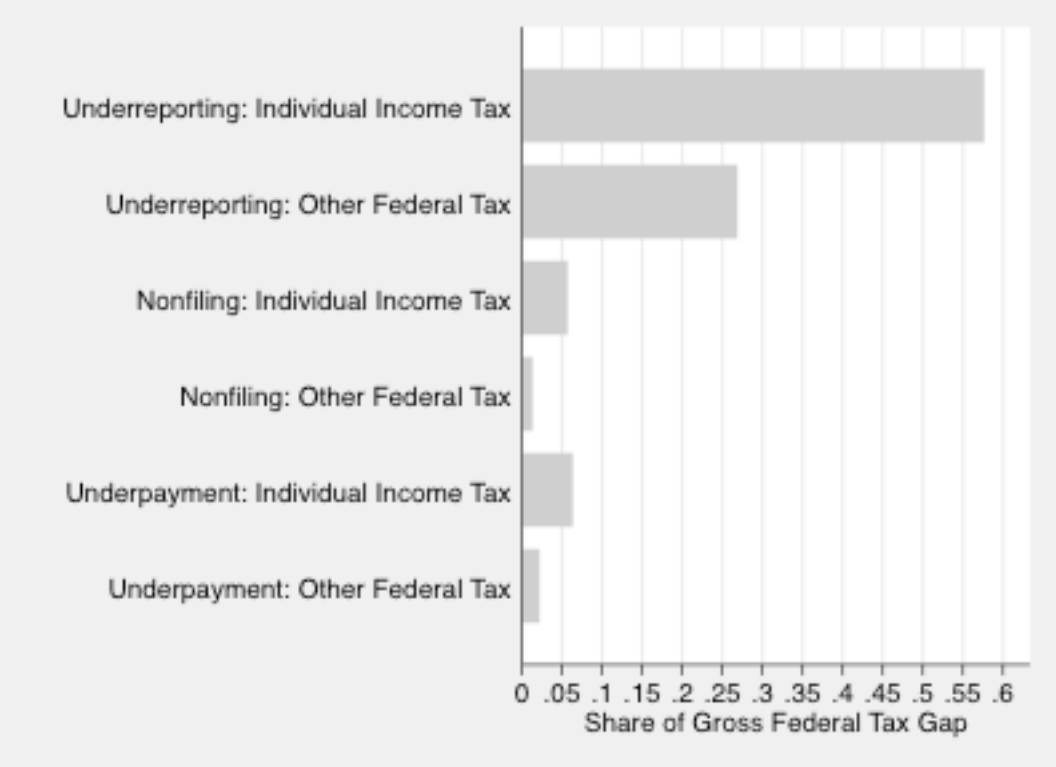
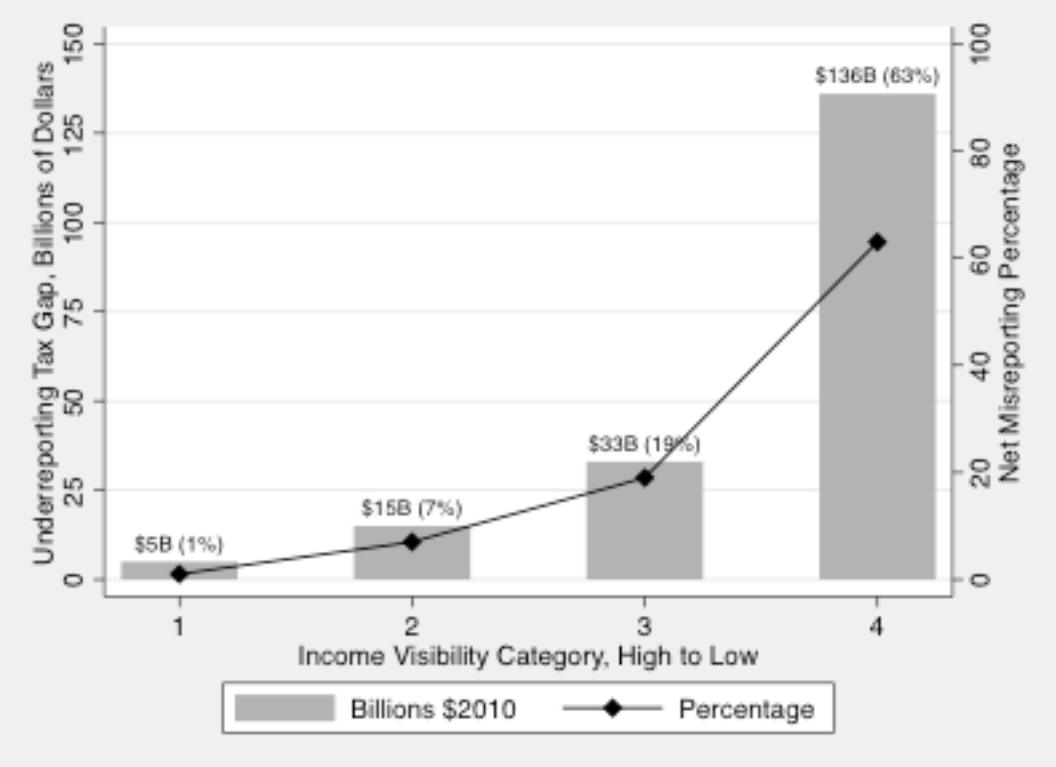
Figure 1. Role of the individual income tax in federal tax noncompliance, 2008-2010



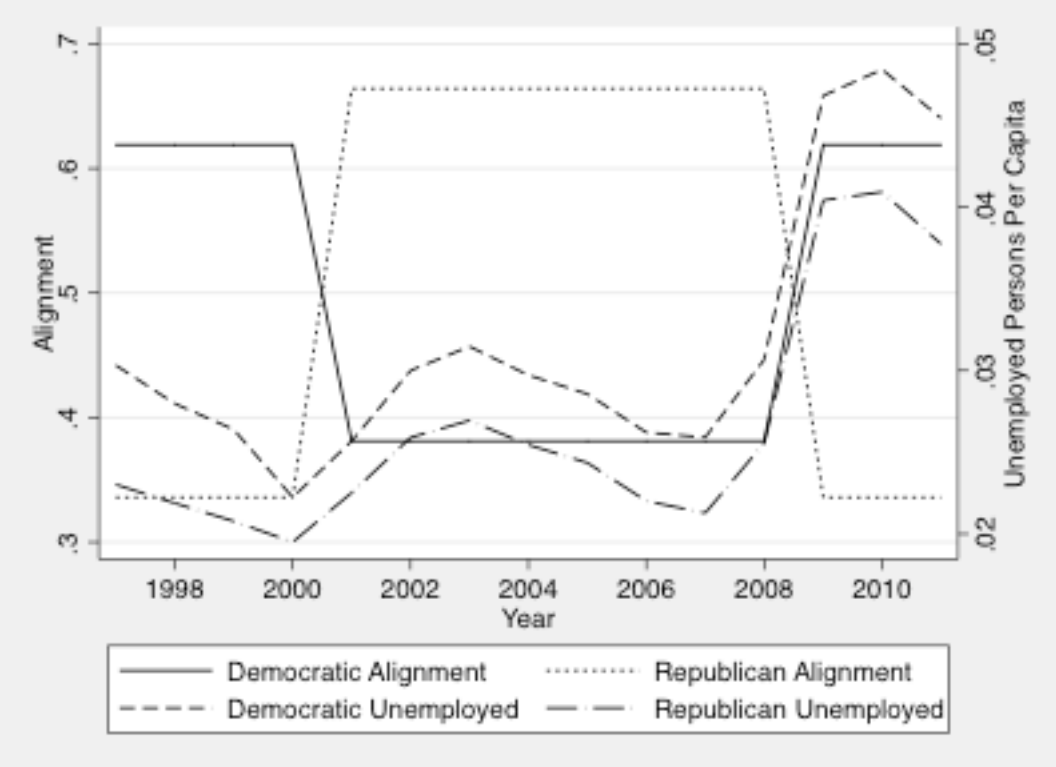
Notes: These statistics are from the Internal Revenue Service "Federal Tax Compliance Research: Tax Gap Estimates for Tax Years 2008-2010" (https://www.irs.gov/pub/irs-soi/p1415.pdf). On average for tax years 2008-2010, the total estimated federal tax liability including all major taxes (i.e., individual 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



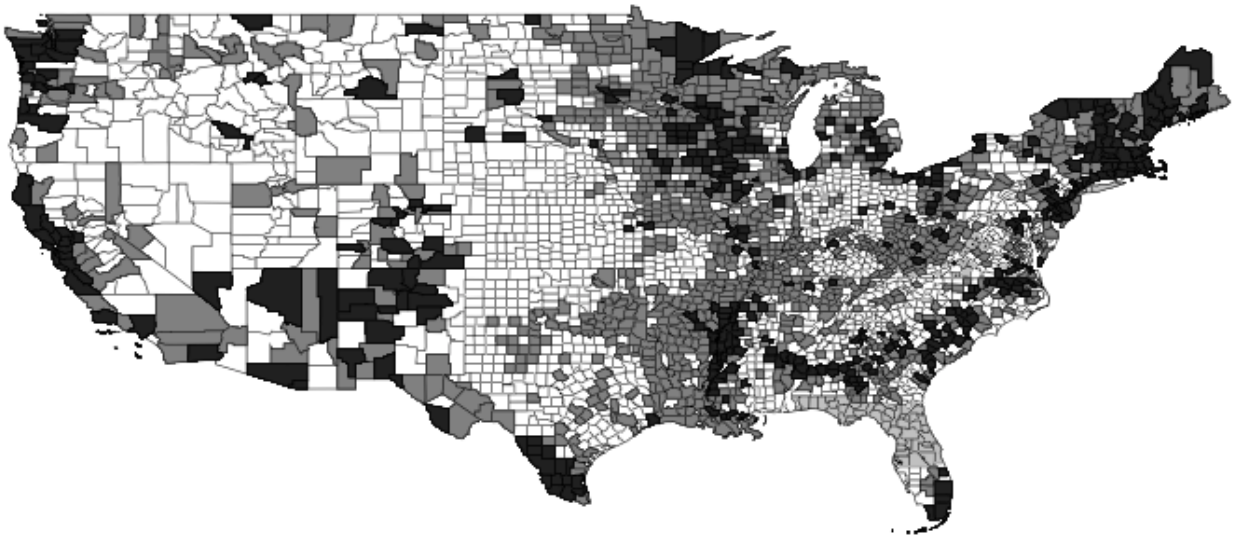
Notes: These statistics are from the Internal Revenue Service "Federal Tax Compliance Research: Tax Gap Estimates for Tax Years 2008-2010" (https://www.irs.gov/pub/irs-soi/p1415.pdf). The bars show the average annual underreporting tax gap by type of income, while the markers show the associated net 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



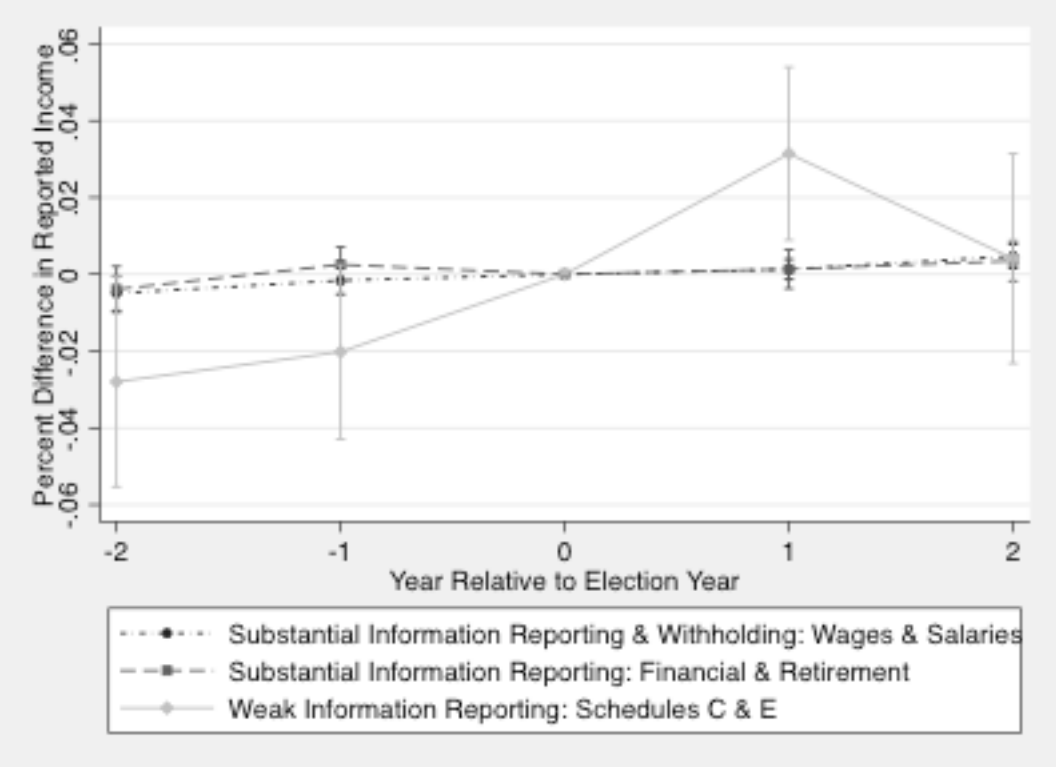
Notes: Partisan counties are classified as Democratic (454 counties) or Republican (1,453 counties) based on the two-party vote shares across the 1996-2008 presidential elections. County partisan alignment is the average share cast for the current president's party across these elections, so only changes 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, binary alignment measure



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 county-by-election and state-by-year fixed effects, the control set includes an alternative set of economic controls since the information returns are not available before 1999. That set includes (log per capita) non-farm private employment, government employment, unemployment, and number of establishments, as well as (log per capita) unemployment interacted with self-employment per capita in 1990, number of housing starts and the share of establishments by industry. Details on the sources for these variables are provided in Appendix E. 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-by-presidential term. The additional controls are a comprehensive set of respondent characteristics (as detailed in Appendix Table A2). *** p<0.01, ** p<0.05, * p<0.10

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 with 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 with 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-by-presidential term. *** p<0.01, ** p<0.05, * p<0.10

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-by-presidential term. *** p<0.01, ** p<0.05, * p<0.10

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

	2000 and 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)
Filing rates, per capita			
Filed an income tax return	0.398	0.382	0.376
	(0.056)	(0.057)	(0.052)
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)
Return was audited and found to owe taxes	0.003	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 deviations 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 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. Annual population estimates are from the Census.

Table 2b. Summary statistics for independent variables

	2000 and 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)
Information return amounts, 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)
Differential cyclicality			
Self-employed, per capita in 1990	0.038	0.061	0.048
	(0.020)	(0.035)	(0.024)
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 deviations in parentheses. The wage, financial, retirement and unemployment incomes are derived from the sums of all information return amounts received by county residents using the IRS CDW. The 1990 self-employment count and annual population estimates are from the Census.

Table 3. Estimates of the impact of alignment on proxies for tax compliance, baseline alignment measure

Control set	Log per capita reported income			Log per capita number of returns				
	Wages & salaries	Financial & retirement	Sched C&E	Claims EITC	Sched C & EITC	Sharp Bunch	Audit	Audit Owe
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline specification	-0.003	-0.001	0.088***	-0.031***	-0.051***	-0.081**	-0.132***	-0.075**
	(0.003)	(0.005)	(0.025)	(0.004)	(0.010)	(0.026)	(0.024)	(0.026)
More restrictive control set								
Omitting interaction with 1990 self-employment per capita	-0.005	0.006	0.090***	-0.030***	-0.024**	-0.021	-0.111***	-0.041*
	(0.003)	(0.005)	(0.025)	(0.004)	(0.009)	(0.024)	(0.024)	(0.025)
More expansive control sets								
Adding interaction with predicted propensity to be partisan Democrat	-0.004	-0.000	0.078**	-0.028***	-0.065***	-0.101***	-0.140***	-0.088***
	(0.003)	(0.005)	(0.025)	(0.004)	(0.010)	(0.027)	(0.025)	(0.026)
Adding interactions with predicted propensity and with average vote share	-0.003	-0.005	0.056**	-0.012**	-0.153***	-0.209***	-0.136***	-0.104***
	(0.003)	(0.006)	(0.024)	(0.005)	(0.013)	(0.033)	(0.029)	(0.030)
Dependent variable mean (in levels)	13,875	2,818	1,944	0.084	0.016	0.001	0.003	0.002
Dependent variable standard deviation	5,142	1,416	1,785	0.031	0.008	0.001	0.002	0.002

Notes: Each cell reports results from a separate regression. 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 to 2008 elections). The estimated coefficient on the political alignment measure is shown, with standard errors robust to clustering at the level of the county in parentheses. Political alignment is based on the average two-party vote shares across the medium run, and is equal to the share cast for the party of the current president. Rows indicate the control set, while columns indicate the dependent variable. The dependent variables in columns 1-3 are expressed as log real ($2010) per capita amounts, and those in columns 4-8 are log counts of returns per capita. Both the amounts and counts are based on the subset of policy constant filers. 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. Further, for the bunching and audit counts in columns 6-8, the sample is restricted to the subset of partisan counties with populations over 10,000 to avoid missing data due to masking for nondisclosure. In addition to county and state-by-year fixed effects, the baseline specification includes log per capita information return amounts (wages, financial income, retirement income and unemployment compensation), the shares of wages paid by different types of businesses (S-corporations, C-corporations and partnerships), and an interaction between log per capita unemployment compensation and self-employment intensity (i.e., self-employed per capita in 1990). The more restrictive control set drops this final interaction. The more expansive control sets add similar interactions with i) the predicted propensity for a county to be partisan Democrat (based on 1990 (log per capita) non-farm private employment, government employment, unemployment, and number of establishments, as well as number of housing starts and share of establishments by industry), and then also ii) the average Democrat two-party vote share across the medium run. *** p<0.01, ** p<0.05, * p<0.10

Table 4. Estimates of the impact of alignment on proxies for tax compliance, heterogeneity

Key controls	Log per capita reported income			Log per capita number of returns				
	Wages & salaries	Financial & retirement	Sched C&E	Claims EITC	Sched C & EITC	Sharp Bunch	Audit	Audit Owe
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline indicator for binary alignment	-0.000	-0.002	0.022***	-0.008***	-0.011***	-0.017**	-0.036***	-0.019**
	(0.001)	(0.001)	(0.007)	(0.001)	(0.003)	(0.007)	(0.007)	(0.007)
State income tax piggybacking								
Baseline indicator for binary alignment	-0.003	-0.001	0.016	-0.012***	-0.018***	-0.031**	-0.073***	-0.054***
	(0.002)	(0.003)	(0.014)	(0.002)	(0.005)	(0.014)	(0.012)	(0.012)
Interaction with indicator for state tax system tied to reports on the federal return	0.004**	-0.002	0.009	0.005**	0.009	0.020	0.051***	0.048***
	(0.002)	(0.003)	(0.016)	(0.002)	(0.006)	(0.016)	(0.014)	(0.015)
Dual alignment with president and governor								
Aligned with president only	0.000	-0.002	0.017**	-0.006***	-0.010***	-0.017*	-0.045***	-0.032***
	(0.001)	(0.001)	(0.007)	(0.002)	(0.003)	(0.009)	(0.008)	(0.009)
Aligned only with governor	0.002	0.001	0.008	0.005	-0.045***	-0.014	-0.049**	-0.059**
	(0.002)	(0.004)	(0.020)	(0.005)	(0.013)	(0.025)	(0.020)	(0.023)
Aligned with both president and governor	0.001	-0.001	0.046**	-0.008	-0.064***	-0.034	-0.062**	-0.048**
	(0.002)	(0.004)	(0.018)	(0.005)	(0.013)	(0.026)	(0.019)	(0.021)

Notes: The top row shows the baseline specification with a binary version of the continuous alignment variable used in Table 3.. The bottom two panels report results for the key variables of interest from otherwise identical specifications that allow for heterogeneity based on state income tax systems or politics. The first adds an interaction with whether the state income tax "piggybacks" on the federal system. 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. The second decomposes alignment with the president according to whether the county is also aligned with the governor. 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. *** p<0.01, ** p<0.05, * p<0.10

Online Appendices

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, and/or 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? 0 = Strong Democrat 1 = Not very strong Democrat 2 = Independent, close to Democrat 3 = Independent 4 = Independent, close to Republican 5 = Not very strong Republican 6 = Strong Republican 7 = Other party	All
Conservative views index (POLVIEWS rescaled to range from 0 to 1)	POLVIEWS: Where would you place yourself on this scale? 1 = Extremely liberal 2 = Liberal 3 = Slightly liberal 4 = Moderate 5 = Slightly conservative 6 = Conservative 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? 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? 1 = Too high 2 = About right 3 = Too low 4 = Pays no income tax (volunteered)	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: 1 = Too little 2 = About right 3 = Too much	1973+
Space exploration	NATSPAC: Space exploration program	

	NATSPACY: Space exploration 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 (they are at 5). Others think it should do even more to solve our country's programs (they are at 1). Where are you on this scale? Range: 1 to 5 (3 = Agree with both)	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) COOP equals 1 (1973+)
Respondent was cooperative but not particularly interested	COOP2 equals 2 (1972) COOP equals 2 (1973+)
Respondent's understanding of the questions was good	COMPREND equals 1
Indicators for version administered by year	1972-87: FORM 1988-93: FORM*BALLOT 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 truncate the amounts according to 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).

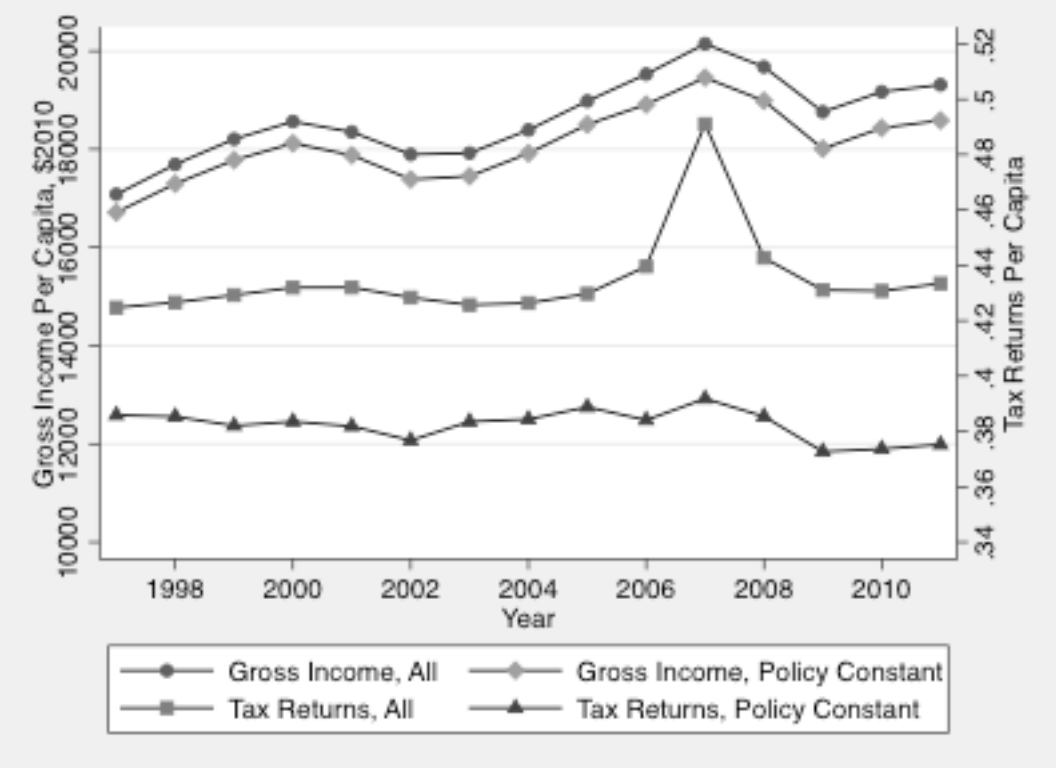
Then, in order to minimize changes in the composition of taxpayers induced by the stimulus and other less dramatic tax policy changes, we identify the subset of tax returns filed by "policy-constant" taxpayers. We define these taxpayers as those who would be likely to file taxes if the 1996 tax law were 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 group 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% of all returns (0.43 per capita). In 2007, overall filing rates per capita jump up by nearly 30%, 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% of total gross income is attributable to policy-constant filers in every year.

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.

Appendix C. Subjective audit rates and partisan alignment

To understand if taxpayers' perceptions of audit and detection probabilities are systematically related to partisan alignment we would ideally analyze panel data over years that span turnover elections. However, we know of no such data. Instead, we analyze cross-sectional data using separate surveys from years that include both Democrat and Republican administrations. We first explore the relationship between partisan alignment and subjective audit, evasion and detection rates in 2016 when Democrats controlled the White House using data from the American Life Panel survey administered by the Rand Corporation. We then quantify the relationship between partisan alignment and audit perception in 2002 and 2003 under a Republican president using data from polls administered by Gallup/CNN/USA Today and by International Communications Research (ICR)/Associated Press (AP).

While there are differences in the measurement of audit perceptions and political affiliation across these surveys, we find no meaningful differences between Democrats and Republicans under either Republican or Democratic presidents. We interpret the results as evidence that voters' perceptions of audit probabilities are not related to political alignment.

Perceptions under Democratic administration in 2016

The American Life Panel (ALP) is a probability-based panel of over 4,900 individuals ages 18 and older routinely interviewed over the internet since 2006, though not all are interviewed in each segment. We use data from interviews conducted in 2016 in months prior to the election. These are the only data we could find that include both political affiliation and perceptions about audits under a Democratic president.

We combine two segments from the ALP. First, we use information on political alignment from the 2016 presidential election segment in the field from 3/8/2016 to 4/7/2016.

The survey asks three questions to determine the likelihood the respondent would vote Democrat, Republican or other. From this segment, we retain respondents who indicate the greatest probability of voting for either a Democrat or a Republican in the 2016 presidential election (we drop those who say they are most likely to vote for an Independent). Results are virtually identical if we keep only the subset that indicates a likelihood of over 50% for either party. This sample restriction removes 8% of weighted observations. Second, we add information from the tax evasion segment that asks respondents about their perceptions of the likelihood of being audited, the share of people who evade taxes and the likelihood that people who evade are caught. This segment was in the field from 7/14/2016 to 9/2/2016. In addition, we make use of demographic information in these surveys to control for family income, educational attainment, self-employment status and race. Table C1 provides details on the relevant survey questions and the construction of the analysis variables.

Since our analysis sample is based on the subset of respondents in both the presidential election and tax evasion segments, it is not obvious how to weight the sample to be nationally representative and we choose to use the weights from the tax segment. We confirmed that the characteristics that Rand uses to construct weights do not differ between the entire tax sample and the merged tax-election sample. Across gender and age (10 categories), gender and race (6 categories), gender and education (6 categories) and household size and income (12 categories), we find only one statistically significant difference. In the merged sample, the share of the population that is female and Hispanic/other is 0.038, compared to 0.058 in the full tax sample.

Using these data, we quantify whether Democrats have different subjective probabilities of audit, evasion and detection relative to Republicans. For ease of interpretation, these outcomes are scaled to range from 0 to 1. We then regress the subjective probabilities on Democratic party

status and report results in Table C2.

Overall, we interpret our findings as showing that taxpayers' perceptions of audits, evasion and detection are not materially different between Democrats and Republicans in 2016, a year in which the Democrats controlled the White House. Panel A reveals no statistically significantly differences in perceived audit rates. Interestingly, the mean subjective audit rate is 23%, over 40 times the actual rate of 0.5% in 2016 (https://www.irs.gov/statistics/enforcement-examinations). Panels B and C suggest that there are also no significant differences in the perception of the evasion rate or in the share of tax evaders that is caught. Panel D shows the similarity in the subjective probability of detection also holds after controlling for the subjective assessment of the evasion rate. Although not shown, we find that the same partisan results hold among respondents who indicate they are not self-employed, though these individuals surprisingly have meaningfully lower perceptions of audit and detection risk.

Perceptions under Republican administration in 2002 and 2003

We combine information from two separate telephone surveys that ask respondents both about their political affiliation and their perceptions of being audited. For 2002, we use a Gallup/CNN/USA Today poll. This poll was in the field 4/5/2002-4/7/2002. For 2003, we use an ICR/AP poll. This poll was in the field 4/2/2003-4/6/2003. In our baseline sample, we retain respondents who indicate they are either Democrats or Republicans. This removes 37% of weighted observations in 2002 and 42% of weighted observations in 2003 identifying as independent, other or failing to provide a valid response. In 2002, the survey further asks these respondents which party they lean towards. As a robustness check, we also report results that retain individuals who lean Republican or Democrat, in which case only 10 percent of weighted observations are lost. The surveys include similar questions on family income, education and

race, allowing us to control for these factors. Full details of the survey questions and the construction of the analysis variables are in Tables C3 and C4.

The surveys both ask about audits but in slightly different ways. The 2002 poll asks how concerned respondents are that the IRS will audit their returns in that year, while the 2003 poll asks about the likelihood of being audited. To combine these measures, we recode the categories to range from 0 (not concerned/not likely) to 1 (very concerned/very likely) in order to pool the polls. The transformed variable has different means across years (0.190 in 2002 compared to 0.216 in 2003), which may reflect differences in the questions. Using z-scores instead of this transformation, we find qualitatively similar results. As a robustness check, we also report the results for each poll separately. To ensure the sample is nationally representative, we use weights from the surveys after normalizing weights in each poll by the sum of the weights to transform them into relative shares.

To quantify the difference in subjective audit probabilities across Democrats and Republicans under a Republican president, we regress the subjective audit probability on Democrat status. We show these results in Table C5. Like the results for the ALP in 2016, these findings suggest that there are not meaningful differences between Democrats and Republicans in their perceptions of audit probabilities. Panel A shows the results using the pooled data, while panels B and C show the results for each poll separately. Panel D uses a broader set of respondents, including both those who respond that they are Democratic or Republican in addition to respondents who lean towards one of the two parties. In all cases, we find no significant differences between Democrats and Republicans. The differences are generally substantively small as well. As in 2016, respondents in 2002-2003 pooled data overestimate the likelihood of audit. In 2002, the actual audit rate was 0.5% (see https://www.irs.gov/pub/irs-

soi/03databk.pdf, Table 10).

Table C1. American Life Panel variables

Analysis variable	**ALP variable and question**
Audit rate (rescaled to range from 0 to 1)	Ms456_PerceivedAuditRate: In a typical year, what percent of taxpayers in the U.S. will have their income tax return audited by the IRS? Range: 0 to 100
Evasion rate (rescaled to range from 0 to 1)	Ms456_PerceivedEvasionRate: In a typical year, out of 100 people like you, how many intentionally underreport their taxes? Range: 0 to 100
Detection rate (rescaled to range from 0 to 1)	Ms456_PerceivedCaught: You previously stated that [Ms456_PerceivedEvasionRate]% of taxpayers in the US will intentionally underreport their taxes. In a typical year, what percent of these people are caught by the IRS? Range: 0 to 100
Democrat party affiliation (coded to 1 when Ms452_whovote_democrat is the maximum)	Ms452_whovote_democrat, Ms452_whovote_republican, Ms452_whovote_someoneelse: If you vote in the election, what is the percent chance that you will vote for a Democrat? A Republican? And for someone else? (Must sum to 100% or question is asked again.) Range: 0 to 100
Family income	Ms90002_familyincome: Which category represents the total combined income of all your family (living here) during the last 12 months? 1 = Less than $5,000 2 = $5,000 to $7,499 3 = $7,500 to $9,999 4 = $10,000 to $12,499 5 = $12,500 to $14,999 6 = $15,000 to $19,999 7 = $20,000 to $24,999 8 = $25,000 to $29,999 9 = $30,000 to $34,999 10 = $35,000 to $39,999 11 = $40,000 to $49,999 12 = $50,000 to $59,999 13 = $60,000 to $74,999 14 = $75,000 or more
Self employed	Ms456_SelfEmployed: Do you work for someone else, are you self-employed or what? 1 = Work for someone else, 2 = Self-employed, 3 = Other
Birth year (used to construct age groups 20-39, 40-59, 60-79 and 80+ years of age)	Ms90002_birthyear: What is your birth year?
Born in the United States	Ms90002_borninuns: Were you born in the US? 1 = Yes, 2 = No

Education (used to construct education groups HS grad or less, some college but no degree, 2-year degree, 4-year degree and graduate degree)	Ms90002_highesteducation: What is the highest level of school you have completed or the highest degree you received? 1 = less than first grade 2 = first through fourth grade 3 = fifth through sixth grade 4 = seventh or eighth grade 5 = ninth grade 6 = tenth grade 7 = eleventh grade 8 = twelfth grade, no diploma 9 = high-school grad or equivalent 10 = some college, no degree 11 = AA in occupational program 12 =AA in academic program 13 = BA/BS 14 =MA/MS 15 = professional degree 16 = doctoral degree
White	Ms90002_ethnicity: Do you consider yourself primarily white, black, American Indian or Asian? 1 = White, 2 = Black, 3 = American Indian, 4 = Asian, 5 = Other

Table C2. Tax perceptions under Democratic president, 2016

	Panel A: Audit rate		
Democrat	0.028		0.005
	(0.052)		(0.019)
Demographic controls	No		Yes
N		802	
Mean audit rate		0.230	
	Panel B: Evasion rate		
Democrat	0.019		-0.002
	(0.037)		(0.022)
Demographic controls	No		Yes
N		797	
Mean evasion rate		0.209	
	Panel C: Detection rate		
Democrat	0.019		-0.019
	(0.057)		(0.022)
Demographic controls	No		Yes
N		795	
Mean detection rate		0.227	
	Panel D: Detection rate		
Democrat	0.015		-0.019
	(0.052)		(0.019)
Demographic controls	No		Yes
Evasion rate control	Yes		Yes
N		795	
Mean detection rate		0.227	

Notes: The analysis sample includes all respondents with valid responses for the outcome and control variables. Demographic controls include indicators for family income, self-employment, age group, born in the US, education and race as detailed in Table C1. Samples sizes differ slightly because of differences in non-response rates. Standard errors are clustered at the income group-by-Democrat status level and are reported in parentheses.

Table C3. Gallup/CNN/USA Today variables

Analysis variable	**Gallup/CNN/USA Today variable and question**
Audit rate (reversed and rescaled 0 to 1, so 0 = not at all and 1=very)	Q49: How concerned are you that the IRS will audit your return this year? 1 = Very concerned 2 = Somewhat concerned 3 = Not concerned 4 = Not at all concerned
Democratic party affiliation (coded to 1 when D7 = 2)	D7: In politics, as of today, do you consider yourself a Democrat, Republican, or Independent? 1 = Republican 2 = Democrat 3 = Independent 4 = Don't know, 5 = Other, 6 = Refused
Democratic party leaning (coded to 1 when D7 = 2 or D8 = 1)	D8: If D7 is 3, 4, 5 or 6, ask: As of today do you lean more to the Democratic Party or the Republican Party? 1 = Democrat 2 = Republican 3 = Neither/other 4 = Don't know, 5 = Refused
Family income	D5: Total family income before taxes is 1 = Less than $10,000 2 = $10,000 to $14,999 3 = $15,000 to $19,999 4 = $20,000 to $29,999 5 = $30,000 to $49,999 6 = $50,000 to $74,999 7 = $75,000 or more 8 = Don't know, 9 = Refused
Male	S3: What is your gender? 1 = Male, 2 = Female
Education (Used to construct education groups less than high school graduate, high school graduate, some college, graduated college, graduate school or more, technical/trade school and don't know/refused)	D3: What is the highest level of school you have completed or the highest degree you received? 1 = None or grades one through four 2 = Fifth through seventh grade 3 = Eighth grade 4 = Ninth through eleventh grade 5 = High school graduate 6 = Technical/trade school after high school 7 = Some college 8 = College graduate or higher 9 = Don't know/refused
White	D4A: What is your race? 1 = Other, 2 = Don't know 6 = White, 7 = Black, 8 = Hispanic, 9 = Asian

Table C4. ICR/AP variables

Analysis variable	**ICR/AP variables and question**
Audit rate (rescaled to range from 0 to 1)	AP3: How would you describe your chances of being audited this year? 1 = Not at all likely 2 = Not too likely 3 = Somewhat likely 4 = Very likely
Democratic party affiliation	Z11A: Generally speaking, do you consider yourself a Democrat, Republican, or Independent? 1 = Republican 2 = Democrat 3 = Independent 4 = Other 5 = Don't know, 6 = Refused
Family income	Z9: Is your total family income from all sources before taxes: 1 = Less than $10,000 2 = $10,000 to $14,999 3 = $15,000 to $19,999 4 = $20,000 to $29,999 5 = $30,000 to $49,999 6 = $50,000 to $74,999 7 = $75,000 or more 8 = Don't know, 9 = Refused
Male	Sex: What is your gender? 1 = male, 2 = female
Education	Z8: What is the last grade of school you completed? 1 = Less than high school graduate 2 = High school graduate 3 = Some college 4 = Graduated college 5 = Graduate school or more 6 = Technical/trade school after high schools 9 = Don't know/refuse
White	Z10A: What is your race? 1 = Other, 2 = Don't know 6 = White, 7 = Black, 8 = Hispanic, 9 = Asian

Table C5. Tax perceptions under Republican president, 2002-03

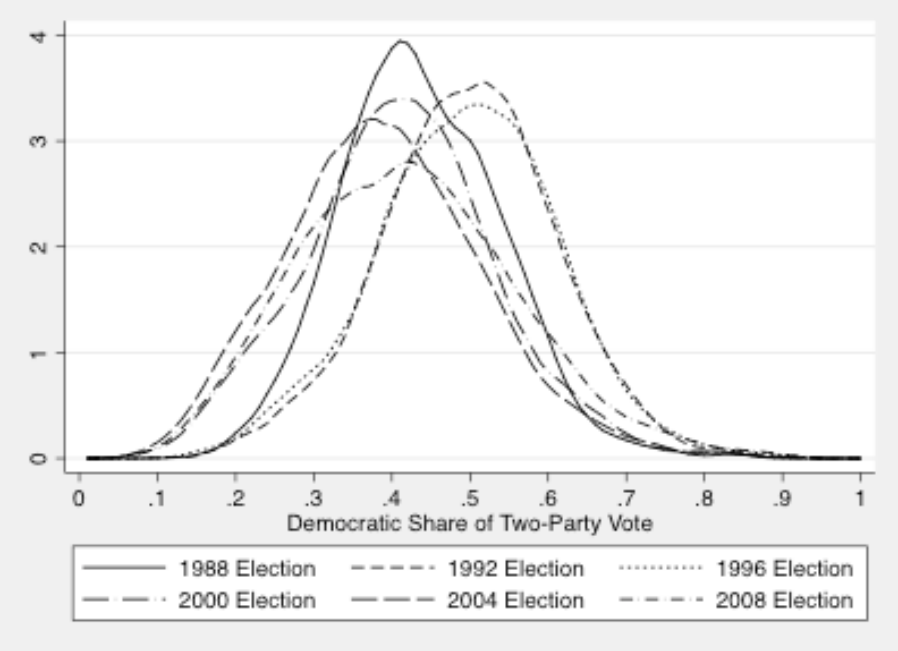
Panel A: Audit Likelihood/Concern, 2002-2003			
Democrat	0.016		0.015
	(0.025)		(0.020)
Demographic controls	No		Yes
N		862	
Mean Audit Likelihood/Concern		0.223	
Panel B: Audit Concern, 2003			
Democrat	-0.004		-0.007
	(0.027)		(0.026)
Demographic controls	No		Yes
N		439	
Mean Audit Concern		0.258	
Panel C: Audit Likelihood, 2002			
Democrat	0.036		0.029
	(0.043)		(0.022)
Controls	No		Yes
N		423	
Mean Audit Likelihood		0.190	
Panel D: Audit Likelihood, with weak partisans 2002			
Democrat	0.042		0.038
	(0.024)		(0.019)
Demographic controls	No		Yes
N		623	
Mean Audit Likelihood		0.197	

Notes: The analysis sample includes all respondents with valid responses for the outcome and control variables. Both columns in panel A include indicators for survey year. Demographic controls include indicators for family income, gender, education and race as detailed in Tables C3 and C4. Standard errors are clustered at the income-group and Democrat status and year level and are reported in parentheses.

Appendix D. County partisan status

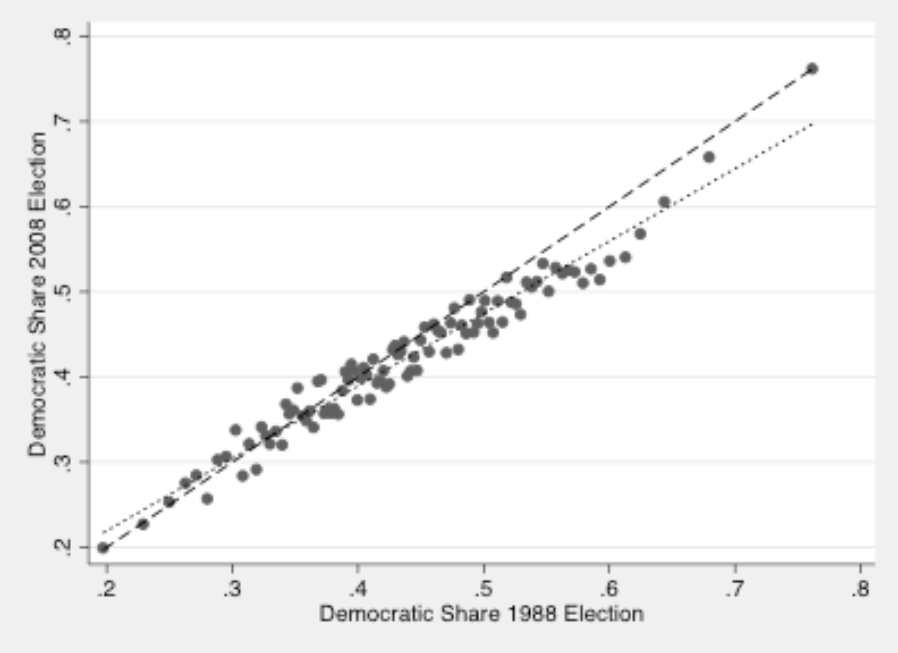
The longest time period we consider for classifying counties according to partisan status spans the 1988 to 2008 presidential elections. We base our classification on the Democratic share of the two-party vote. Figure D1 shows the probability distribution of this share for counties for each of the six 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 D2 shows the high correlation in vote shares across the 1988 and 2008 elections. Finally, Table D1 shows the shares of counties by partisan status using both the medium and longer run definitions.

Figure D1. 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 D2. 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 D1. Types of counties by state

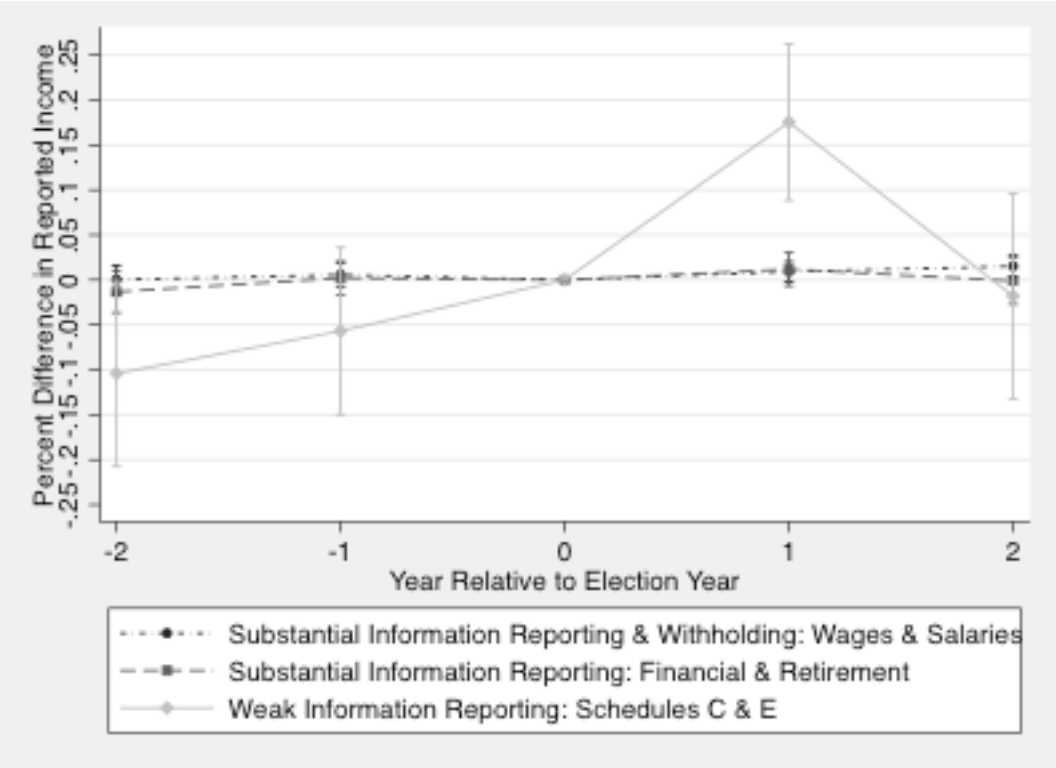
State	Counties	1996 to 2008 elections			1988 to 2008 elections		
		D	R	Swing	D	R	Swing
Alabama	67	0.16	0.52	0.31	0.15	0.51	0.34
Arkansas	75	0.12	0.12	0.76	0.09	0.05	0.85
Arizona	15	0.27	0.40	0.33	0.07	0.33	0.60
California	58	0.34	0.41	0.24	0.24	0.31	0.45
Colorado	63	0.19	0.56	0.25	0.17	0.54	0.29
Connecticut	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 E. Robustness analyses

Figure E1. Estimated relative impact of moving into vs. out of alignment, 2000 and 2008 elections, continuous alignment measure



Notes: This figure depicts results from event-study-style 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 $2010 amounts). 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 to 2008 elections. Shown are the estimated coefficients and 95 percent confidence intervals for indicators for years from the election interacted with the level of alignment that characterizes the county following the election. For example, for the 2000 election, the years relative to election indicators are interacted with the county's average Republican vote share across elections. Standard errors are robust to clustering by county.

Table E1. Summary statistics for additional control variables used in robustness analyses

	1996 to 2008 elections		
	D	R	Swing
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)
Political variables			
Federal government grants and procurement	3,292	2,031	2,420
contracts, per capita $2010	(3,895)	(3,183)	(4,823)
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 deviations 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. Federal government grants and procurement contract amounts are from the Census Governments Division.

Table E2. Summary statistics for variables identifying subsamples used in robustness analyses

	2000 and 2008 elections		
	D	R	Swing
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)
Propensity to be partisan Democrat between 0.1 and 0.9	0.941	0.677	NA
	(0.237)	(0.468)	
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 deviations in parentheses. Annual population estimates are from the Census. The predicted propensity for a county to be partisan Democrat is based on a Probit specification that includes 1990 (log per capita) non-farm private employment, government employment, unemployment, and number of establishments, as well as number of housing starts and share of establishments by industry. 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 E3. Robustness of results to alternative control sets, window analysis for 2000 and 2008 elections

Control set	Log per capita reported income			Log per capita number of returns				
	Wages & salaries	Financial & retirement	Sched C&E	Claims EITC	Sched C & EITC	Sharp Bunch	Audit	Audit Positive Adjust.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline specification	-0.003 (0.003)	-0.001 (0.005)	0.088*** (0.025)	-0.031*** (0.004)	-0.051*** (0.010)	-0.081** (0.026)	-0.132*** (0.024)	-0.075** (0.026)
Alternative control sets								
Adding information return variables interacted with 2008 election indicators	-0.004 (0.003)	-0.007 (0.006)	0.058** (0.025)	-0.009* (0.005)	-0.129*** (0.012)	-0.161*** (0.032)	-0.113*** (0.029)	-0.076** (0.030)
Adding county-by-election fixed effects	-0.003 (0.003)	-0.001 (0.005)	0.101*** (0.025)	-0.027*** (0.004)	-0.032*** (0.010)	-0.058** (0.025)	-0.100*** (0.025)	-0.045* (0.026)
Adding demographic variables	-0.003 (0.003)	-0.001 (0.005)	0.086*** (0.026)	-0.029*** (0.004)	-0.050*** (0.010)	-0.078** (0.026)	-0.130*** (0.024)	-0.073** (0.026)
Adding federal grants and procurement spending per capita	-0.003 (0.003)	0.001 (0.005)	0.081** (0.026)	-0.034*** (0.004)	-0.048*** (0.010)	-0.080** (0.026)	-0.134*** (0.024)	-0.077** (0.026)
Adding economic variables	-0.003 (0.003)	0.000 (0.005)	0.097*** (0.027)	-0.038*** (0.005)	-0.045*** (0.010)	-0.080** (0.027)	-0.133*** (0.025)	-0.075** (0.026)
Including economic variables while excluding information return variables	0.004 (0.004)	0.002 (0.006)	0.107*** (0.026)	-0.042*** (0.005)	-0.053*** (0.010)	-0.083** (0.027)	-0.142*** (0.024)	-0.085*** (0.025)
Alternative dependent variables								
Aggregates based on all filers	-0.004 (0.004)	0.006 (0.006)	0.099*** (0.025)	-0.038*** (0.004)	-0.032*** (0.009)	-0.024 (0.024)	-0.124*** (0.023)	-0.052*** (0.025)

Notes: The first row reports results for the baseline specifications shown in row 1 of Table 3. 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. *** p<0.01, ** p<0.05, * p<0.10

Table E4. Robustness of results to alternative samples, window analysis for 2000 and 2008 elections

Control set	Log per capita reported income			Log per capita number of returns				
	Wages & salaries	Financial & retirement	Sched C&E	Claims EITC	Sched C & EITC	Sharp Bunch	Audit	Audit Positive Adjust.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Baseline specification	-0.003 (0.003)	-0.001 (0.005)	0.088*** (0.025)	-0.031*** (0.004)	-0.051*** (0.010)	-0.081** (0.026)	-0.132*** (0.024)	-0.075** (0.026)
Alternative samples								
Including swing counties (alignment set to 0)	-0.002 (0.003)	0.000 (0.004)	0.101*** (0.022)	0.004** (0.002)	-0.031*** (0.004)	-0.051*** (0.008)	-0.081*** (0.023)	-0.105*** (0.022)
Excluding counties with population <1,000	-0.004 (0.003)	-0.002 (0.005)	0.089*** (0.025)	-0.031*** (0.004)	-0.055*** (0.010)	-0.081** (0.026)	-0.132*** (0.024)	-0.075** (0.026)
Excluding counties with population <10,000	-0.000 (0.004)	-0.013** (0.005)	0.100*** (0.023)	-0.032*** (0.004)	-0.056*** (0.010)	-0.081** (0.026)	-0.132*** (0.024)	-0.075** (0.026)
Restrict sample to economically similar counties (via propensity score trimming)	-0.006 (0.004)	-0.001 (0.005)	0.097*** (0.029)	-0.025*** (0.004)	-0.045*** (0.011)	-0.054** (0.027)	-0.157*** (0.025)	-0.085** (0.027)
Exclude counties containing capital cities	-0.004 (0.003)	-0.000 (0.005)	0.089*** (0.026)	-0.031*** (0.004)	-0.052*** (0.010)	-0.084** (0.026)	-0.134*** (0.025)	-0.080** (0.026)
Exclude counties with large commuter flows	-0.005 (0.004)	0.003 (0.005)	0.111*** (0.026)	-0.029*** (0.005)	-0.052*** (0.011)	-0.092** (0.029)	-0.163*** (0.027)	-0.106*** (0.028)
Exclude counties hit hard by the housing crisis	-0.005 (0.004)	0.002 (0.005)	0.085** (0.027)	-0.037*** (0.004)	-0.062*** (0.010)	-0.090** (0.029)	-0.128*** (0.027)	-0.068** (0.028)

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

provided in Table E2. *** p<0.01, ** p<0.05, * p<0.10

Table E5. Estimates of the impact of alignment on proxies for tax compliance, alternative alignment measures

Key independent variable	Log per capita reported income			Log per capita number of returns				
	Wages & salaries	Financial & retirement	Sched C&E	Claims EITC	Sched C & EITC	Sharp Bunch	Audit	Audit Owe
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Continuous alignment measures								
Baseline alignment measure, vote share 1996 to 2008	-0.003	-0.001	0.088***	-0.031***	-0.051***	-0.081**	-0.132***	-0.075**
	(0.003)	(0.005)	(0.025)	(0.004)	(0.010)	(0.026)	(0.024)	(0.026)
Long-run alignment measure, vote share 1988 to 2008	-0.005	0.001	0.090**	-0.034***	-0.055***	-0.092**	-0.125***	-0.080**
	(0.004)	(0.005)	(0.029)	(0.005)	(0.011)	(0.029)	(0.028)	(0.029)
Binary alignment measures								
Indicator for party alignment, baseline partisanship status	-0.000	-0.002	0.022***	-0.008***	-0.011***	-0.017**	-0.036***	-0.019**
	(0.001)	(0.001)	(0.007)	(0.001)	(0.003)	(0.007)	(0.007)	(0.007)
Indicator for party alignment, long-run partisanship status	-0.001	-0.001	0.026***	-0.009***	-0.015***	-0.021**	-0.035***	-0.021**
	(0.001)	(0.001)	(0.008)	(0.001)	(0.003)	(0.009)	(0.008)	(0.009)
Continuous alignment x turnout								
Baseline alignment measure x avg. two-party turnout 1996 to 2008	-0.005	-0.000	0.134**	-0.056***	-0.099***	-0.170**	-0.230***	-0.116**
	(0.006)	(0.010)	(0.048)	(0.009)	(0.019)	(0.054)	(0.051)	(0.052)
Long-run alignment measure x avg. two-party turnout 1988 to 2008	-0.009	0.004	0.146**	-0.062***	-0.108***	-0.197**	-0.220***	-0.130**
	(0.007)	(0.011)	(0.056)	(0.011)	(0.022)	(0.063)	(0.060)	(0.061)
Presidential approval								
Party-specific presidential approval rating, baseline partisanship status	-0.001	-0.004*	0.044***	-0.014***	-0.024***	-0.030**	-0.072***	-0.042***
	(0.001)	(0.002)	(0.012)	(0.002)	(0.004)	(0.012)	(0.011)	(0.012)
Party-specific presidential approval rating, long-run partisanship status	-0.002	-0.002	0.050***	-0.016***	-0.029***	-0.037**	-0.069***	-0.044**
	(0.002)	(0.003)	(0.014)	(0.002)		(0.015)	(0.013)	(0.014)

	(0.005)

Notes: Each cell reports results from a separate regression. The top row reproduces results for the baseline specification from the top row of Table 3. The other rows match this specification except for variations in the measure of political alignment and corresponding changes in the set of partisan counties. There are 1,907 partisan counties in the medium run (over the 1996 to 2008 elections), and 1,618 partisan counties in the long run (over the 1988-2008 elections). The estimated coefficient on the political alignment measure is shown, with standard errors robust to clustering at the level of the county in parentheses. Presidential approval is based on 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. To compare the magnitudes of the estimates, note that the average change in the key independent variable associated with moving into alignment is 0.3 for the continuous alignment measures, 1 for the binary measure, 0.16 for the interactions with average turnout and 0.6 for presidential approval. The implied impacts of moving into alignment are quantitatively similar across measures, since the magnitude of the estimated coefficients shrink (grow) in proportion to increases (decreases) in the magnitude of the swings in the alignment measures. *** $p<0.01$, ** $p<0.05$, * $p<0.10$