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POLITICAL ALIGNMENT, ATTITUDES TOWARD GOVERNMENT AND TAX EVASION

Julie Berry Cullen
Nicholas Turner
Ebonya L. 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 turnover elections push voters in partisan counties into and out of alignment with the party of the president. We provide three types of evidence that alignment reduces evasion. As a county moves into alignment we find 1) taxpayers report more easily-evaded forms of income; 2) suspect EITC claims decrease; and 3) audits triggered and audits found to owe additional tax decrease. Our results provide real-world evidence that a positive outlook on government lowers tax evasion.

Julie Berry Cullen
Department of Economics - 0508
University of California, San Diego
9500 Gilman Drive
La Jolla, CA 92093-0508
and NBER
jbcullen@ucsd.edu

Ebonya L. Washington
Yale University
Box 8264
37 Hillhouse, Room 36
New Haven, CT 06520
and NBER
ebonya.washington@yale.edu

Nicholas Turner
Federal Reserve Board
Nick.Turner@frb.gov

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If a thousand men were not to pay their tax bills this year, that would not be as 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]

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 (IRS 2007). Economists have attempted to manipulate tax morale in the lab and in the field, as we detail in our literature review. 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, we follow a tax gap approach that relates reported income to generated income, presuming that

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[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. Given that enforcement is low, some authors posit that there is a puzzle as to why compliance on less visible sources of income is so high. 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.

the difference reflects evasion. We bolster this interpretation by comparing the reporting sensitivity of income sources with differing degrees of third-party reporting and, hence, scope for evasion. Second, we identify suspect claims of the Earned Income Tax Credit (EITC). Prior research (Chetty et al., 2012) suggests that when self-employed taxpayers report the least amount of income that qualifies for the maximum EITC, a pattern Chetty, Friedman and Saez (2013) term "sharp bunching," this 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.[4] 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.[5] 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.

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

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[4] 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.

[5] 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.

elections. Given the time frame covered by our tax data, we focus on the years just before and after the 2000 and 2008 presidential elections, when 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.[6]

Overall, our results provide novel evidence for an attitudinal component to tax compliance.[7] 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.[8] 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. First, we consider how results vary by election, finding that the 2000 election drives impacts on our EITC proxies while the 2008 election drives impacts on the tax gap and audit proxies. Second, we consider differences across states according to their tax systems and politics. We find that the evasion response is muted in states where the cost of evasion is higher since the federal tax return is a direct input into the state return. The response is magnified in counties where the benefit is increased because of a lack of alignment with both the governor and the

[6] As part of our data agreement with the IRS, we do not attempt to estimate differential impacts by party affiliation.
[7] 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.
[8] In our specifications, we estimate the differential effect of moving into, relative to moving out of, alignment. Throughout the text when we refer to movements into alignment we mean relative to moving out of alignment.

president.

The remainder of the paper proceeds as follows. In Section 1 we review the recent literature on tax morale and provide 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 mechanisms 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 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 (1998) 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 in the lab 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. Researchers 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).

Outside of the lab, experimental economists have tried to manipulate tax morale through mailings or other interventions that prime reciprocal motives by highlighting the public goods that tax dollars provide. The impacts of these relatively weak interventions – that do not change the allocation of revenues or political circumstances – on tax compliance have been mixed. While De Neve et al. (2019) find that messages of reciprocity were effective in increasing Belgium income taxpayers' knowledge and appreciation of public goods, respondents were not more likely to say that taxes should be reported honestly and these messages failed to increase compliance.[9] Among firm owners subject to the VAT in Uruguay (Bergólo et al., 2019) and individuals subject to the property tax in Argentina (Castro and Scartascini, 2015), the income tax in Minnesota (Blumenthal et al., 2001) and the church tax in Germany (Dwenger et al., 2016), varying teams of researchers similarly find no impact on tax collections of randomized mailings emphasizing the beneficial use of revenues. In contrast, such mailings are found to raise taxes collected on foreign income in Norway (Bott et al., 2017) and from overdue personal

[9] Unlike the Belgian null results for reported tax morale, Dorrenger and Peichl (2017) find that injecting reminders that taxes support public education coupled with information about evasion levels reduces the likelihood that respondents report that it is justifiable to evade taxes in a German survey experiment.

income taxes in the UK (Hallsworth et al., 2017). Notably, however, in the former setting the reciprocity message was coupled with moral suasion, and in the latter the behavior is a form of tax compliance that is exclusive of evasion.

Our study moves beyond the attempts to experimentally manipulate morale to quantify the impact of naturally occurring quasi-experimental variation in attitudes toward government 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.[10]

1.2 Linking political alignment to tax morale

Our proxy for tax morale under the federal personal income tax is sharing the same party as the president. It is the president who is the head of the executive branch, which houses the IRS. Further, political scientists have long documented that voters assign credit or blame for the macroeconomy 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.[11] It seems likely that taxpayers similarly focus heavily on the president when forming attitudes related to tax morale.

In order to provide empirical evidence in support of this contention, we use GSS[12] data to

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[10] The only other study we are aware of that exploits a natural experiment to provide a shock to tax morale is Besley, Jensen and Persson (2019). These authors show that the adoption of an unpopular poll tax to fund local government in the UK had an immediate negative impact on tax payments that persists long after the tax was repealed due to changes to social norms.

[11] 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 subnational economic conditions to the president than to state elected officials (Stein, 1990).

[12] 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.

show that sharing the same party as the president predicts government approval, and like other measures of approval used in the literature, predicts self-reported tax morale. Our key findings are summarized in Figure 3, with details provided in Appendix A. Conditional on observable characteristics, respondents whose party identification matches that of the president are significantly more likely to have confidence in the executive branch and significantly less likely to state that their income taxes are too high, that the government spends too much, and that the government should do less. We do not find that alignment with the executive branch predicts agreement with the idea that the government spends too little or should do more. In other words, there is an elasticity of disapproval for taxation and spending with respect to alignment, but not an elasticity of approval. Though not shown in the figure, party alignment with congress does not predict tax morale, which supports our focus on the president. In the main analysis, we ask whether less negative attitudes toward taxation and spending induced by party alignment with the president 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 its illegality. A variety of methods have been used to measure evasion in the literature. In rare instances, data from random (e.g., Kleven et al., 2011) or near complete (e.g., Dwenger et al.. 2016) audits are available. More typically, evasion is inferred from discrepancies between what is observed and what is expected. For example, Feldman and Slemrod (2007) compare the estimated elasticity of charitable giving across different sources of taxable income. Absent evasion, their presumption is that the propensity to donate would be constant across more and less visible income sources.

In this paper, we use several approaches to infer evasion at the county by year level. The first 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.[13] 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 discrepancy 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 and Chetty et al. (2012) provide evidence from audits that this "sharp bunching" is driven by noncompliance. Guyton et al. (2018) provide additional evidence that many returns filed by the self-employed claiming the

[13] 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.

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 the subset of returns filed by "policy constant" tax filers. The subset of policy constant filers is determined by applying the 1996 tax law (adjusted for inflation) to later years.[14] This strategy effectively screens out those (typically elderly) individuals with low income and earnings induced to file in 2007 and 2008 in order to claim

[14] See Appendix B for more details and Appendix E (Table E3) for results that use the full, unrestricted sample and yield qualitatively similar results.

refundable credits as part of the stimulus program. Not surprisingly, since so little of aggregate reported income is screened out, results are robust to expanding the sample to include all returns and not simply the policy-constant filers.

2.2 Methodology

With these proxies for evasion in hand, we exploit presidential turnover elections to provide the quasi-experimental variation. Our focus is on partisan counties, those counties that vote consistently for one party over the other in presidential elections. By tracking the behavior of residents of partisan counties under different regimes, we attempt to hold all else constant and isolate the alignment-induced shift in tax morale.

We characterize counties based their two-party vote shares across the 1996 to 2008 elections. We define partisan Democratic counties as those for which the Democratic share of the two-party vote is always above 50 percent, while we label as Republican counties those for which this share is always below 50 percent. Alignment is then defined as the average share of the two-party vote cast for the party of the president.[15] Therefore, alignment only changes when the party of the president changes. For example, 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. We focus on partisan counties as they see the largest swings in the share aligned following a turnover election.[16] These counties, which always fall on one side of the 50 percent threshold, are also least likely to have their latent partisanship misclassified by average vote share across presidential elections.

[15] We demonstrate robustness to varying the definition of alignment, including basing it on a longer-run average of the two-party vote share in Table 3.

[16] Given nonpartisan counties' small swings in the share party-aligned, it is not surprising that including these counties in the analysis leaves the results largely unchanged as we demonstrate in Appendix Tables E5-E8.

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.[17] 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 (by the following April) under another. Alignment is not well-defined for these transition years as evasive behavior may occur not only at the time of, but also well in advance of, tax filing. For example, a contractor may ask for cash payments in order to be able to evade taxes on income.

Our window analysis 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 are first available in 1999. Most importantly, this strategy isolates the variation in political sentiment that our alignment measure is designed to capture. As we demonstrate in the top of Figure 4, both Democratic and Republican approval for the president (measured at the national level) vary considerably even within an administration. Remarkably, Democrats (Republicans) swing more than 50 (nearly 30) points in their approval of George Bush across his eight years in office. In contrast to the approval measures which capture changing national sentiment over time, our alignment measure has the virtue of isolating the large shift in public opinion at the county level following turnover elections. However, it fails to capture within term variations in approval, a point we return to Section 3.4.

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

Restricting the sample to partisan counties and the four window years around the two turnover elections, 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:

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

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 account for correlation over time, reported standard errors are clustered at the county level. Our identifying assumption is that residents of 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).[18] A second channel that has been documented is federal spending targeted to counties on the basis of political alignment.[19]

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

[19] Dynes and Huber (2015) 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.

Figure 4 demonstrates this key challenge for our sample period where swings in alignment for Democratic and Republican counties (shown at the top of the figure) occur under contrasting economic environments (as demonstrated by the unemployment series in the bottom of the figure). 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).[20] 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 the 1990 Census. It is important to allow for this flexibility since Republican counties tend to have higher shares self-employed. 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

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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.

[20] 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.

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 Democratic vote share. To address the concern that unemployment may not fully characterize the economic cyclicality of counties, particularly during the Great Recession, we demonstrate the robustness of our results to the addition of housing market controls and analogous interactions with these variables.

Our variety of dependent variables also addresses concerns that results may be driven by economic activity. 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. Within the tap gap approach, however, we are able to compare the sensitivity of reporting across more and less visible income sources that are differentially susceptible to evasion, all using the same control set. Further, the complementary EITC and audit specifications are less dependent on accurately measuring true taxable income generated.

A second limitation of our approach stems from our use of aggregate data to make inferences 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 taxpaying behavior. This problem is known as the ecological fallacy. Because attitudes of networks are shocked at the same time as own attitudes, we are also 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 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

As explained above, we characterize a county's partisanship status by the average two-party vote shares across the 1996 to 2008 presidential elections.[21] Fifteen percent are always majority Democratic, 48 percent are always majority Republican, and the remainder we classify as nonpartisan counties.[22] Figure 5 shows the geographic distribution of counties by partisan status. Our analysis focuses on the 1,907 partisan counties for which we have needed data.[23] While many states have large majorities of supporters of one party, most states still have heterogeneity across counties in party leaning.

Tables 1a and 1b 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

[21] 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 nonpartisan county shares by state.
[22] Democratic counties tend to be more urban and populous, so that the population-weighted shares are 43 percent Democratic and 29 percent Republican. Regression results are robust to weighting by log population.
[23] 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 (partisan and nonpartisan) counties, representing more than 95 percent of ever existing counties and 93 percent of the population in a typical year.

been converted to real per capita 2010 dollars. The reported income statistics show that the most visible form of income is also the most common, with wage and salary income making up three-quarters of gross income. The least visible forms make up less than 10 percent. Republican counties tend to have higher shares self-employed and 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 2 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 1. In order to isolate exogenous variation in alignment our estimation samples include 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, we control for income generated based on variables constructed from the information returns as well as the interaction between unemployment benefits received 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 2. 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.[24] Similarly, in the second cell, we find no responsiveness of financial and retirement income, 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[25] increases (decreases) by a significant 0.086 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.[26] 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).

Our second type of evidence for a causal link between alignment and evasion is suspect

[24] 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.

[25] When we examine the impact of alignment on Schedule C&E income separately, results are economically and statistically significant for each. Coefficients (standard errors) are 0.075 (0.024) for Schedule C and 0.083 (0.035) for Schedule E income.

[26] 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.

EITC claims. Claiming the EITC unambiguously decreases tax liability. Depending on family structure, the maximum credit is potentially large.[27] 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. In the next three columns of Table 2, 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).[28] 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 1.2%. Finally, despite both the rarity of the event and the reduction in our sample size,[29] we find further evidence that moving into alignment decreases evasion when analyzing sharp bunching. As the average county moves into alignment, there is a 2.3% reduction in this behavior.

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, those that are audited are less likely to be found to have underreported income (column 8).

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[27] The value of the EITC depends on the number of qualifying children. In 2019, values range from $529 for returns with no children to over $6,557 for returns with three or more children. (Accessed at https://www.irs.gov/credits-deductions/individuals/earned-income-tax-credit/eitc-income-limits-maximum-credit-amounts-next-year on May, 9 2019).

[28] 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.

[29] 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.

3.2 Robustness to varying economic controls

In summary, the first row of Table 2 presents results from three types of tests (tax gap, 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 2 addresses this concern.

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 may 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 2 we omit this interaction. This serves to decrease some of our estimates in magnitude, leaving both the sharp bunching and the found underreporting results insignificant.

In the next two rows of the table, 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.[30] We then add an additional

[30] 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 form. The prediction equation is run for the sample of medium-run partisan counties.

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 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.

Our macroeconomic proxy, the unemployment rate, may not fully capture income dynamics, particularly during the Great Recession when housing price dynamics were key. Thus, in the remainder of Table 2, we explore the sensitivity of our results to augmenting the specification to include measures of economic cyclicality drawn from the housing market, namely the number of mortgage-months serviced per capita and the median home value. In row 4, we demonstrate the robustness of our baseline specification to adding these two controls. In the final rows of the table, we then interact these new macroeconomic proxies with first the predicted propensity to be Democratic (row 5) and then additionally with the average Democratic vote share (row 6). For both of these augmented interaction models, we find significant evidence of evasion across all three tests of the illegal behavior. 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 results 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; and 3) 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, 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.[31] Finally, we demonstrate robustness to including all counties, whether classified as partisan in or not, by repeating all analyses to this point with this expanded sample in Appendix Tables E5-E8.

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. In Table 3 we investigate how dependent our results are on this baseline definition. In the first row of the table we repeat our baseline specification from the first row of Table 2 for reference.

In the second row, we calculate alignment from the average vote share across more

[31] In results not shown, we test sensitivity to varying the treatment of our standard errors. First, we cluster at the state rather than the county level. Though standard errors tend to be somewhat larger, statistical significance is rarely affected. For example, in the baseline specification in the first row of Table 2, all estimated coefficients retain the same level of statistical significance other than that for audits found to owe, which falls from the 1% to the 5% level. Second, we implement the Conley (1999) adjustment for spatial correlation using Stata code provided by Hsiang (2010). We model the adjustment assuming a 100-year serial correlation (which well approximates the baseline that allows for clustering at the county level) and a Bartlett spatial weighting kernel that we allow to decay over 150 miles, which is over 4 times the average distance between county centroids. (The average county land area is 2,584 square kilometers which, assuming a circle, translates to a diameter of 35 miles.) With this admittedly arbitrary adjustment, audits owed again falls to the 5% level, sharp bunching falls from the 1 to 10% level and Schedule C & EITC lose significance. The other three measures of evasion retain the same significance levels.

elections. This gives us a better measure of a county's long run partisanship status but leaves fewer counties by which to estimate the impact of alignment, as some counties that were formerly classified as partisan no longer have consistent partisanship and so are classified as nonpartisan. The pattern of results is robust to this change.

In the next two rows we move away from exploiting the intensity of alignment and rely solely on the aligned/unaligned margin for identification. We model alignment as a binary variable that takes the value 1 for a county that has voted for the current president's party over the focal time period, either 1996 to 2008 (row 3) or 1988 to 2008 (row 4).[32] The tax gap, EITC and audit results are robust to this change in terms of both significance levels and effect sizes in the average county. Recall that the average change in alignment in a turnover election is 0.3 (1) for the continuous (binary) measure.

Up to this point, we have ignored turnout, effectively assuming that voters and nonvoters are affected by the treatment similarly. However, there are at least two problems with this assumption. First, we do not know that the views about government of nonvoters correspond with those of voters. Second, even if voters and nonvoters hold similar views of the candidates ex-ante, the literature on cognitive dissonance and voting suggests they would have differing views ex-post, as those who are able to exercise the vote have stickier views (Beasley and Joslyn, 2001; Mullainathan and Washington, 2009). Therefore, in the next two rows of Table 3, we interact our continuous alignment measure with average turnout over the same period for which alignment is calculated. This interaction scales the magnitude of the changes into and out of alignment by the share of county eligible voters that exercise their franchise. Average turnout is 0.52, so therefore the swing in alignment in the average county shrinks from 0.3 to 0.16. The

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

magnitude of coefficients generally increases commensurately leaving the impact of alignment on the average county little different from the baseline specification.

The last two rows of Table 3 address another possible source of slippage in our alignment measure due to timing. Our window approach effectively compares taxpayer behavior in the third year of the old president's term to the first year of the new president's term. As presidential approval often dips throughout a president's tenure (see, for example, Figure 4), vote share could be a less accurate measure of presidential approval in the third year than in the first. In the final rows of the table we use as our key independent variable Gallup's measure of national presidential approval averaged across the tax year, stratified by party. We assign the Democratic (Republican) approval measure to the Democratic (Republican) counties. As does the binary alignment measure, approval abstracts from variation within counties of the same party. Swings from unaligned to aligned are double in size for approval (approximately 0.6) as compared to continuous alignment (0.3). Once again, the magnitude of coefficients adjusts commensurately. Thus, Table 3 demonstrates that results are robust across alignment measures.

3.4 Heterogeneity

3.4.1 By year

Given that our approach zeroes in on the year before and the year after a turnover election, it is natural to ask what happens if we widen our lens to include more years on either side of elections. As we demonstrate in Figure 4, even within an administration, there is great variation in presidential approval that may well impact tax morale. However, our alignment measure, while having the virtue of reflecting variation in sentiment across counties, is unable to capture variation in sentiment across time within an administration. Thus, our identification strategy fails to distinguish between year-to-year shifts in tax evasion that are due to changing attitudes towards a particular president and secular trends. Given these limitations, we relegate event study style results to Appendix F.

Here, we present results from our window strategy broken down by election. For completeness, we include the 2004 non-turnover election, when George W. Bush won a second term. We do not view this election as a falsification test, as relative presidential approval by party changed by some 20 percentage points from 2003 to 2005. To present results by election, we amend the basic specification by interacting our alignment measure with election indicators, including in our analysis set the window years around the three elections and treating Republican counties as moving into alignment in 2004.[33] We present results in Table 4 for our baseline specification (in the top panel) and two of the alternative specifications (in the bottom two panels) that add additional controls for differential cyclicality according to initial economic characteristics.

Focusing first on our two turnover elections, the results of Table 4 indicate that the link between alignment and evasion that we see in Table 3 is not strictly driven by either election. However, we do see different types of evasion responses in each of our two elections. In 2000, our EITC findings are consistent with alignment reducing evasion while our tax gap and audit evidence are not. In 2008 we find the reverse, robust evidence of alignment curbing evasion when looking at audits and the tax gap approach, while the EITC evidence is more mixed, less stable and in the case of sharp bunching, significantly wrong-signed in two of three specifications.[34] The differences across the elections may suggest that there is differential ease or

[33] We include county-by-2004 election fixed effects to control for the fact that average county alignment differs from average alignment across the other two elections (which are equated by design).

[34] The year-to-year patterns shown in Appendix F reveal pre-trends local to the 2008 election that are not adequately addressed by our baseline controls when this election is viewed in isolation, as evidenced by the sensitivity to the control set across panels.

knowledge of different forms of evasion across time and space. Secondly, Table 4 results are consistent with the change in (and perhaps greater salience of) presidential approval from before to after the 2004 election resulting in significant changes in evasion as measured by both the tax gap and EITC outcomes.[35]

3.4.2 By state characteristics

In the vast majority of states, residents must pay state income taxes as well as federal income taxes. In this final section of results, we ask how the impact of alignment varies with state income tax codes and alignment with state executives.

In the "State income tax piggybacking" section of Table 5, 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 the medium-run binary alignment measure for our medium-run 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 across the three EITC claimant and two audit columns. In three cases the interaction coefficients are significant at the one percent level and in a fourth at the 10 percent level. These results are consistent with these ties increasing the costs of evasion and

[35] Given the smaller change in approval in 2004, we might expect that the magnitudes of the coefficients in the 2004 row should be smaller than those of the other two rows. And in many cases they are smaller. However, given the differing context expected relative magnitudes are unclear.

therefore moderating the responsiveness to alignment. However, the same pattern is not seen for the tax gap approach.

In the "Dual alignment with president and governor" section, we show that the impact of alignment, again captured by a binary variable, is larger in magnitude when a county is doubly-aligned, aligned both with the president and with the governor. This pattern holds across the tax-gap and audit approaches and for two of three of the EITC outcomes. Being doubly unaligned increases the benefit of evasion as it allows one to express displeasure with, or at least withhold funds from, two administrations.[36]

4. Discussion and conclusion

We find real-world evidence consistent with taxpayers' 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 moving into and out of alignment by turnover elections, to provide three types of evidence all supporting a causal impact of alignment on tax evasion. First, using the tax gap approach that relates reported income to income generated, we find no elasticity of third-party reported income. However, 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.

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[36] Another type of heterogeneity that would be interesting to explore is by county party. However, we remind readers that our data use agreement precludes examining differential impacts by party. We did explore whether sensitivity to alignment varies by the 1997 level of county social capital (Rupasingha, Goetz and Freshwater, 2006), but did not uncover any heterogeneity by this dimension.

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 alignment. As audits are predominantly instigated by algorithms designed to detect likely evasion, it is not surprising that we find that the fraction of returns adjudicated in audit to owe 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 (2018) 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 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). As noted in our literature review, single mailings on these topics produced mixed impacts on tax compliance. However, we speculate that the government's clearly and repeatedly conveying information about how tax dollars are actually spent may change individuals' perceptions about their tax burdens and alter their inclination to evade taxes.

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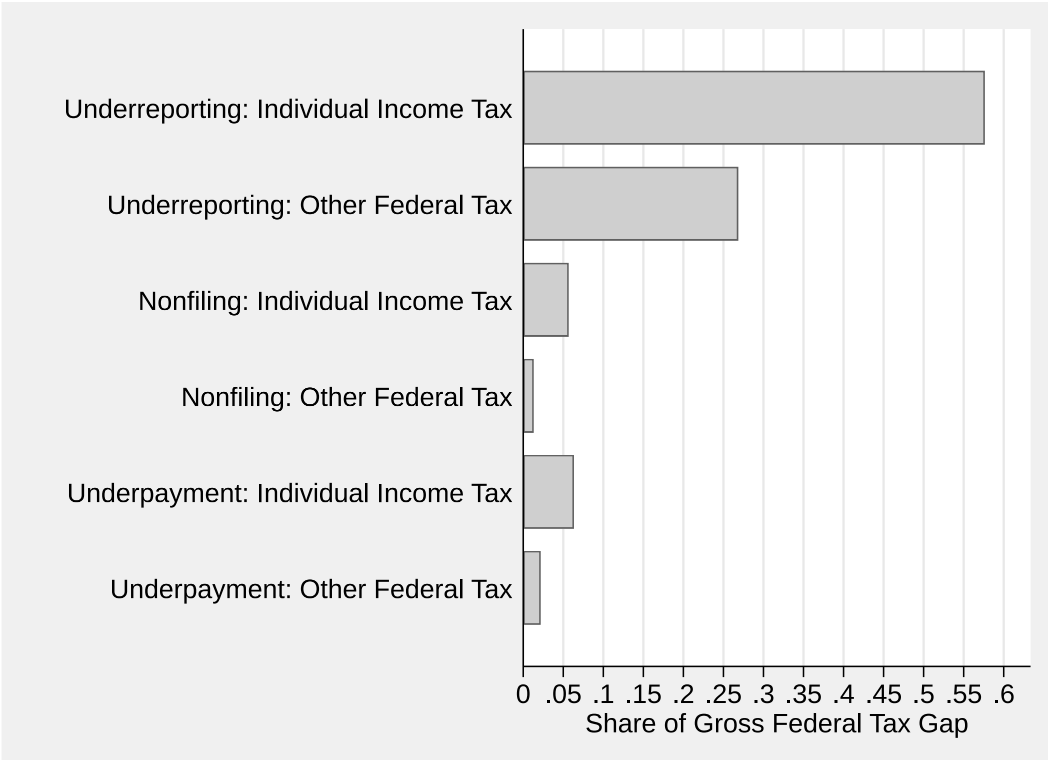
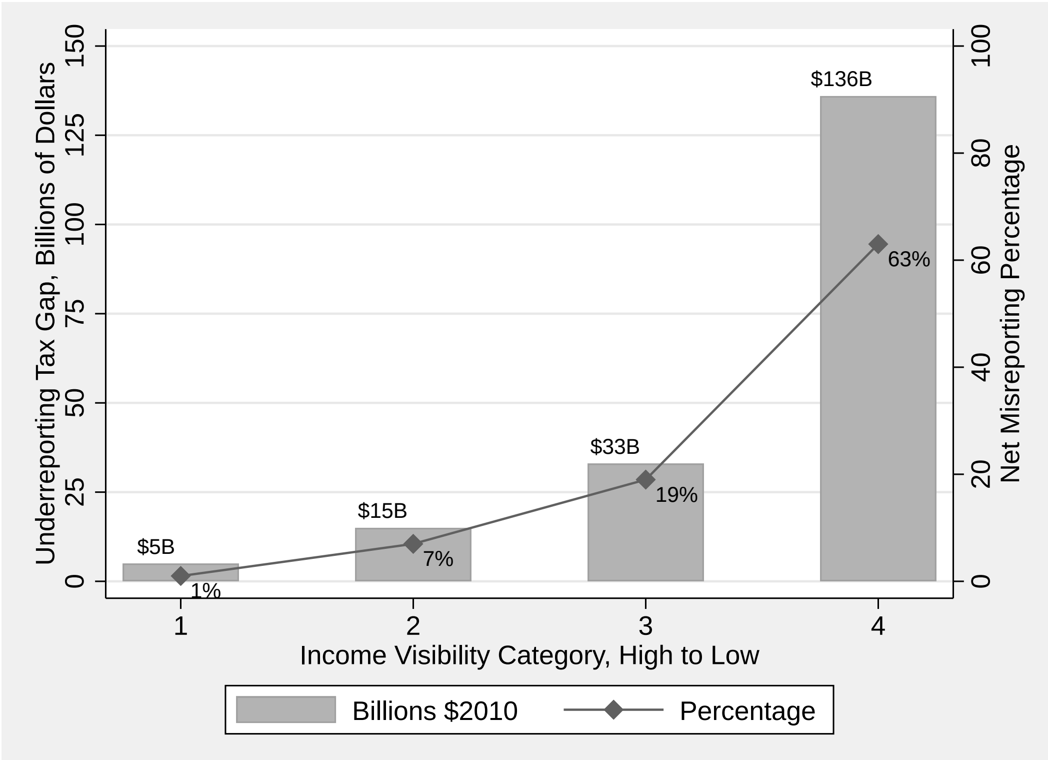
Figure 1. Role of the individual income tax in federal tax noncompliance, 2008-2010



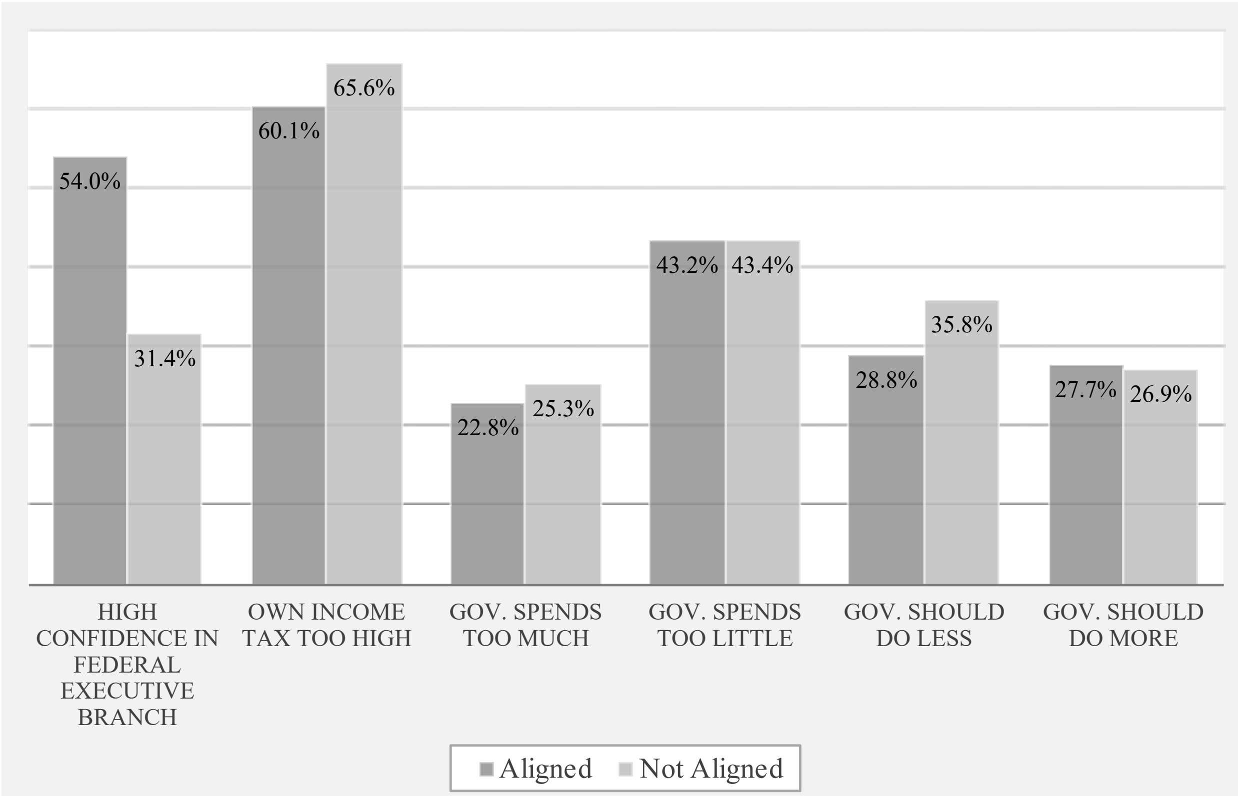
Notes: These statistics are from the Internal Revenue Service "Federal Tax Compliance Research: Tax Gap Estimates for Tax Years 2008-2010" (https://www.irs.gov/pub/irs-soi/p1415.pdf). On average for tax years 2008-2010, the total estimated federal tax liability including all major taxes (i.e., individual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



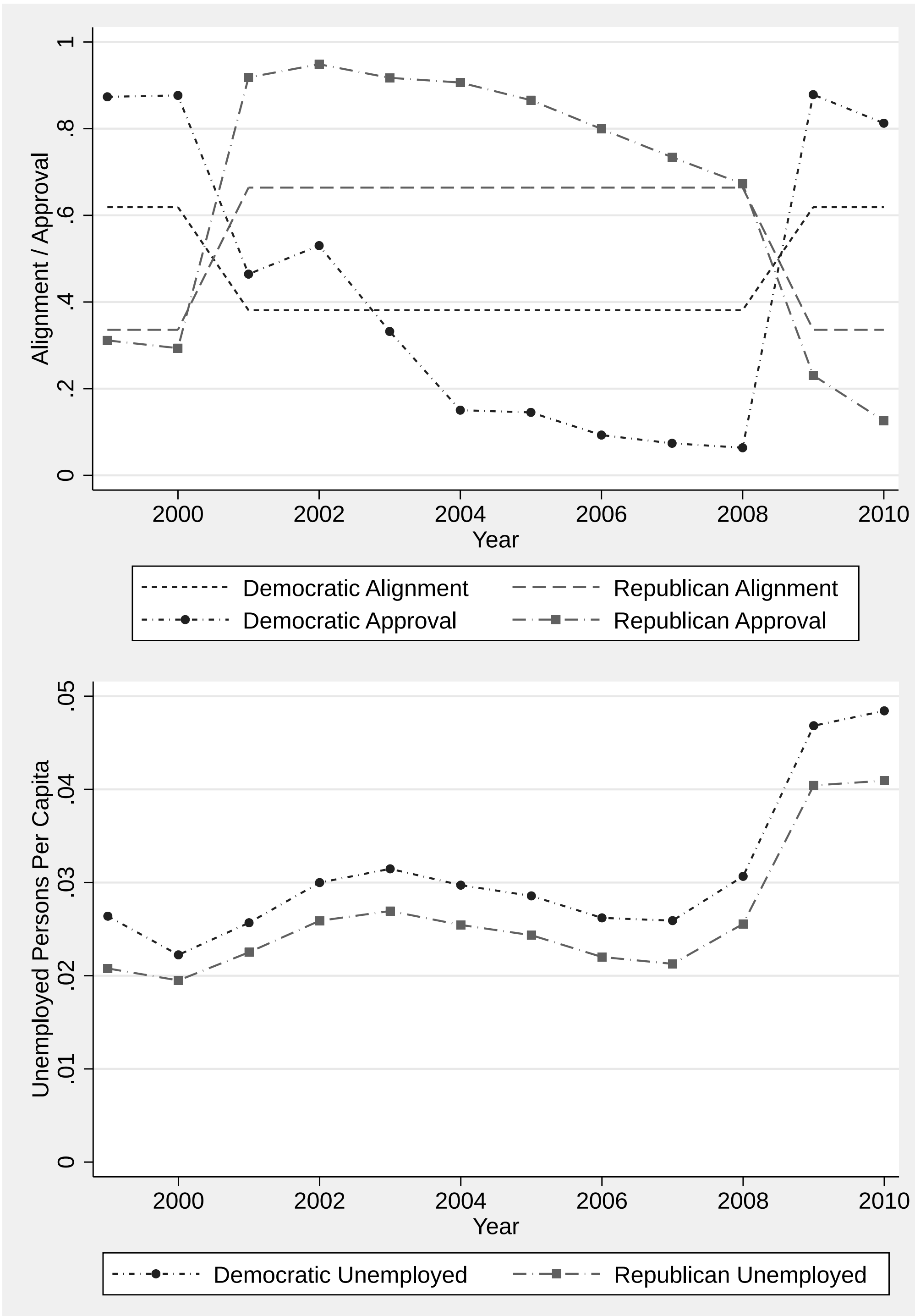
Notes: These statistics are from the Internal Revenue Service "Federal Tax Compliance Research: Tax Gap Estimates for Tax Years 2008-2010" (https://www.irs.gov/pub/irs-soi/p1415.pdf). The bars show the average annual underreporting tax gap by type of income, while the markers show the associated net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



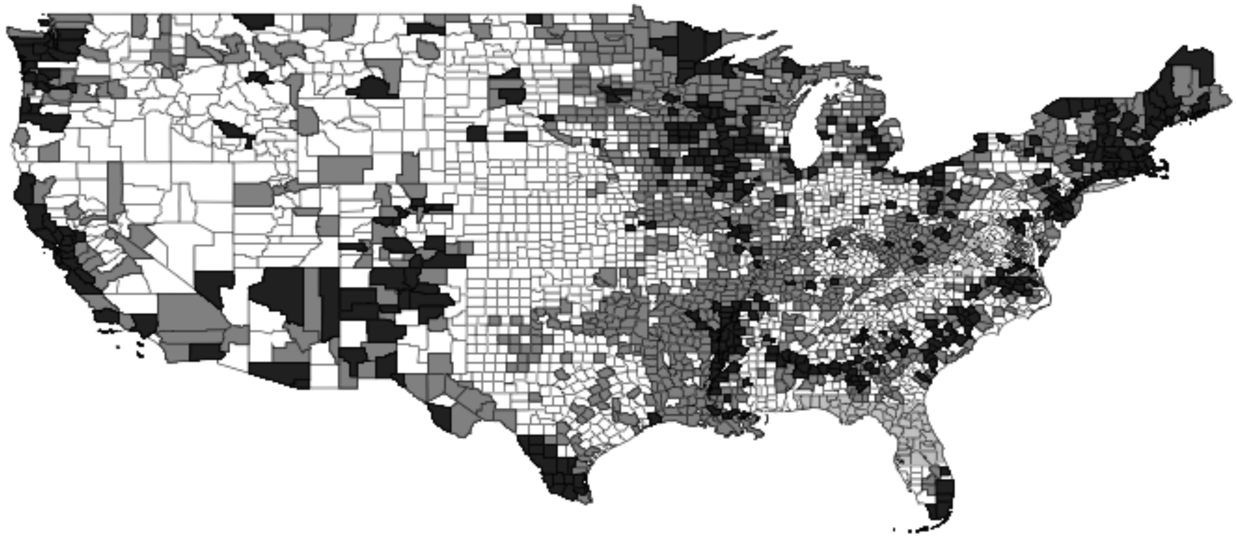
Notes: Each set of bars is estimated from a separate ordinary least squares regression for the dependent variable indicated, using data drawn from the 1972-2014 General Social Survey. The dependent variables are normalized to range from 0 to 1. All specifications include survey version-by-year fixed effects and a comprehensive set of respondent characteristics including an index for where the respondent falls on the partisan scale, ranging from strong Democrat (0) to strong Republican (1). The key independent variable of interest is party-alignment with the president, which is equal to this index under Republican administrations and 1 – this index under Democratic administrations. Reported here are the mean predicted value when party-alignment is set to 1 (aligned) and the mean predicted value when party-alignment is set to 0 (not aligned). More details on the sample, variables and regression results are provided in Appendix A.

Figure 4. 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. Presidential approval is based on Gallup's measure of national presidential approval averaged across the tax year, stratified by party. Shown in the bottom panel are the average numbers of unemployed persons per capita across counties of each type by year.

Figure 5. 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 nonpartisan 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.

Table 1a. Summary statistics for dependent variables, IRS aggregates for policy constant filers

	2000 and 2008 elections		
	D	R	Non-partisan
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 1b. Summary statistics for independent variables

	2000 and 2008 elections		
	D	R	Non-partisan
Partisanship measure			
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 ordinary dividends)	839	791	706
	(687)	(401)	(386)
Retirement income (1099R gross pension distributions; 1099SSA Social Security net payments)	5,185	5,280	5,223
	(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)
Housing market controls			
Mortgage-months serviced per capita	1.471	1.262	1.112
	(1.059)	(0.935)	(0.908)
Median home value ($2010)	159,184	112,481	107,632
	(142,945)	(52,112)	(57,808)
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. The annual number of mortgage-months serviced per capita is calculated from CoreLogic's Lender Processing Services Inc. (LEP) data. These include installment-type residential loans serviced by the largest servicers in the U.S. Since coverage (in terms of mortgage dollars) increases over our sample period, the counts are scaled by dividing by year-specific coverage rates. Real median home value is calculated by scaling the county value from the 2000 Census to account for nominal changes in county home prices since that year and then adjusting for the CPI. County nominal home price indices are estimated by taking population-weighted averages of the 3-digit zip code indices produced by the Federal Housing Finance Agency (FHFA) using a repeat sales methodology. The weights are based on the share of 2000 county populations residing in each 3-digit zip code. For the subset of large counties for which FHFA has produced county-level indices, the correlation between our estimates is 0.95.

Table 2. 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	-0.004	-0.001	0.086***	-0.029***	-0.048***	-0.077***	-0.128***	-0.072***
	(0.003)	(0.005)	(0.026)	(0.004)	(0.010)	(0.026)	(0.024)	(0.026)
More restrictive control set								
Omitting unemployment (U) x self-employment intensity	-0.005	0.006	0.090***	-0.028***	-0.020**	-0.015	-0.107***	-0.037
	(0.003)	(0.005)	(0.025)	(0.004)	(0.009)	(0.024)	(0.023)	(0.025)
More expansive control sets								
Adding U x predicted propensity to be partisan Democrat	-0.004	-0.000	0.076***	-0.027***	-0.063***	-0.097***	-0.137***	-0.085***
	(0.003)	(0.005)	(0.025)	(0.004)	(0.010)	(0.027)	(0.024)	(0.026)
Adding U x predicted propensity and U x avg. Dem. vote share	-0.004	-0.005	0.051**	-0.010*	-0.148***	-0.204***	-0.131***	-0.099***
	(0.003)	(0.006)	(0.024)	(0.005)	(0.013)	(0.033)	(0.029)	(0.030)
Adding housing market controls (H)	-0.004	-0.004	0.088***	-0.031***	-0.050***	-0.078***	-0.126***	-0.068***
	(0.003)	(0.005)	(0.026)	(0.004)	(0.010)	(0.026)	(0.024)	(0.026)
Adding H and H x predicted propensity to be partisan Democrat	-0.005	-0.004	0.095***	-0.029***	-0.040***	-0.068**	-0.136***	-0.076***
	(0.003)	(0.005)	(0.026)	(0.005)	(0.010)	(0.027)	(0.025)	(0.027)
Adding H, H x predicted propensity, and H x avg. Dem. vote share	-0.006*	-0.001	0.090***	-0.028***	-0.027**	-0.068**	-0.091***	-0.028
	(0.003)	(0.005)	(0.026)	(0.005)	(0.012)	(0.030)	(0.027)	(0.029)
Dependent variable mean (in levels)	14,067	2,929	1,872	0.083	0.015	0.001	0.003	0.002
Dependent variable standard deviation	5,249	1,441	1,561	0.032	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, 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 Democratic two-party vote share across the medium run. The housing market controls are the log of the number of mortgage-months serviced per capita and log real ($2010) median home value. *** p<0.01, ** p<0.05, * p<0.10

Table 3. 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	-0.004	-0.001	0.086***	-0.029***	-0.048***	-0.077***	-0.128***	-0.072***
	(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	-0.006	0.001	0.089***	-0.033***	-0.053***	-0.090***	-0.122***	-0.078***
	(0.004)	(0.005)	(0.029)	(0.005)	(0.011)	(0.030)	(0.028)	(0.029)
Binary alignment measures								
Indicator for party alignment, baseline partisanship status	-0.001	-0.002	0.022***	-0.007***	-0.010***	-0.016**	-0.035***	-0.018***
	(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***	-0.008***	-0.014***	-0.020**	-0.034***	-0.020**
	(0.001)	(0.001)	(0.008)	(0.001)	(0.003)	(0.009)	(0.008)	(0.008)
Continuous alignment x turnout								
Baseline alignment measure x avg. two-party turnout 1996 to 2008	-0.006	-0.000	0.127***	-0.055***	-0.093***	-0.164***	-0.224***	-0.110**
	(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	0.142***	-0.061***	-0.103***	-0.194***	-0.214***	-0.125**
	(0.007)	(0.011)	(0.057)	(0.011)	(0.022)	(0.063)	(0.059)	(0.061)
Presidential approval								
Party-specific presidential approval rating, baseline partisanship status	-0.001	-0.004*	0.043***	-0.013***	-0.023***	-0.028**	-0.070***	-0.040***
	(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***	-0.015***	-0.028***	-0.035**	-0.067***	-0.042***
	(0.002)	(0.003)	(0.014)	(0.002)	(0.005)	(0.015)	(0.013)	(0.014)

Notes: Each cell reports results from a separate regression. The top row reproduces results for the baseline specification from the top row of Table 2. 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 0.30 for the continuous alignment measures, 1 for the binary measures, 0.16 for the interactions with average turnout and 0.57 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. *** $p<0.01$, ** $p<0.05$, * $p<0.10$

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

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								
Alignment x 2000 election	0.000	-0.001	-0.041	0.009*	-0.089***	-0.210***	0.014	0.004
	(0.003)	(0.006)	(0.027)	(0.005)	(0.014)	(0.039)	(0.036)	(0.039)
Alignment x 2004 election	0.004*	-0.000	0.115***	-0.019**	-0.121***	-0.093**	-0.017	-0.023
	(0.002)	(0.006)	(0.027)	(0.008)	(0.015)	(0.039)	(0.042)	(0.049)
Alignment x 2008 election	-0.008	-0.003	0.218***	-0.070***	0.000	0.071**	-0.272***	-0.147***
	(0.006)	(0.009)	(0.046)	(0.007)	(0.014)	(0.035)	(0.030)	(0.034)
Adding U x predicted propensity to be partisan Democrat								
Alignment x 2000 election	0.000	-0.001	-0.039	0.009	-0.085***	-0.208***	0.018	0.009
	(0.003)	(0.006)	(0.027)	(0.005)	(0.014)	(0.039)	(0.036)	(0.039)
Alignment x 2004 election	0.004	0.001	0.108***	-0.018**	-0.132***	-0.107***	-0.029	-0.037
	(0.002)	(0.006)	(0.027)	(0.009)	(0.016)	(0.040)	(0.043)	(0.050)
Alignment x 2008 election	-0.009	-0.001	0.202***	-0.066***	-0.027*	0.037	-0.301***	-0.181***
	(0.006)	(0.009)	(0.047)	(0.007)	(0.015)	(0.037)	(0.032)	(0.035)
Adding H and H x predicted propensity to be partisan Democrat								
Alignment x 2000 election	-0.001	-0.002	-0.035	0.009	-0.080***	-0.205***	0.011	0.003
	(0.003)	(0.006)	(0.028)	(0.006)	(0.014)	(0.039)	(0.036)	(0.039)
Alignment x 2004 election	0.003	-0.003	0.111***	-0.024***	-0.098***	-0.074*	-0.022	-0.025
	(0.002)	(0.006)	(0.028)	(0.008)	(0.015)	(0.039)	(0.041)	(0.048)
Alignment x 2008 election	-0.009	-0.009	0.237***	-0.070***	0.003	0.079**	-0.286***	-0.157***
	(0.006)	(0.009)	(0.046)	(0.007)	(0.014)	(0.036)	(0.031)	(0.035)

Notes: This table replicates rows from Table 2 and the notes to Table 2 apply. The differences are that the window years surrounding the 2004 election (2003 and 2005) have been added to the sample, county x 2004 election fixed effects have been added to the control set (to account for the average level of county alignment in these years) and the baseline alignment measure is interacted with indicators for each of the three elections. For the 2004 election, alignment is coded as if there were a turnover from Democratic to Republican, though in fact a Republican was reelected. *** p<0.01, ** p<0.05, * p<0.10

Table 5. Estimates of the impact of alignment on proxies for tax compliance, by state income tax systems and politics

	Log per capita reported income			Log per capita number of returns				
Key controls	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	-0.001	-0.002	0.022***	-0.007***	-0.010***	-0.016**	-0.035***	-0.018***
	(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	-0.011***	-0.017***	-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*	-0.001	0.009	0.006***	0.010*	0.021	0.053***	0.051***
	(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	0.016**	-0.005***	-0.009***	-0.015	-0.044***	-0.030***
	(0.001)	(0.001)	(0.007)	(0.002)	(0.003)	(0.009)	(0.008)	(0.009)
Aligned only with governor	0.002	0.000	0.006	0.006	-0.043***	-0.013	-0.048**	-0.057**
	(0.002)	(0.004)	(0.020)	(0.006)	(0.013)	(0.025)	(0.020)	(0.023)
Aligned with both president and governor	0.000	-0.002	0.045**	-0.006	-0.062***	-0.031	-0.060***	-0.045**
	(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 2. 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 sample 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