Political Alignment, Attitudes Toward Government, and Tax Evasion

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We ask whether attitudes toward government play a causal role in the evasion of US personal income taxes. As turnover elections move voters in partisan counties into and out of alignment with the party of the president, we find with alignment (i) taxpayers report more easily evaded forms of income; (ii) suspect EITC claims decrease; and (iii) audits triggered and audits found to owe additional tax decrease. Coupled with evidence that alignment leads to more favorable views on taxation and spending, our results provide real world evidence that a positive outlook on government lowers tax evasion. (JEL D72, H24, H26, H31)

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 (1849, 1)

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 US 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 $381 billion on average across tax years 2011–2013.1 The vast majority of losses (roughly 70 percent or $271 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 Internal Revenue Service (IRS) estimates that individuals fail to report only 1 percent of the most visible income—income that is both withheld and third-party reported. However, taxpayers fail to report some 55 percent or $109 billion of the least visible income—income subject to no withholding and little to no third-party reporting—such as proprietor income.

1 See Figures 1 and 2 for more details and sources for the facts in this paragraph.
Failure to pay taxes impacts the efficiency, equity, and incidence of the tax system and alters the distribution of resources to and across economic activities. Given the widespread consequences of evasion, economists have a long history of studying the behavior. The classic model (e.g., Allingham and Sandmo 1972) characterizes tax evasion as a financial gamble that the agent undertakes if benefits exceed expected costs. In this framework, the impact of the marginal tax rate on evasion is ambiguous, 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).

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

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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 underreporting. The empirical relationship between the marginal tax rate and evasion is similarly nonrobust, with, for example, Clotfelter (1983) and Kleven et al. (2011) finding a positive relationship, and Feinstein (1991) finding a negative one.

3 See 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 (Gándelman and Hernández-Murillo 2014), only predicts a 14 percent compliance rate, far lower than even the 45 percent rate for the least visible income.
(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 US federal government also asserts that sentiments could have real consequences on tax collections; the 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 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), which is subject to high rates of overclaiming. In addition to taxpayers claiming ineligible children and misrepresenting their marital status, 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 particularly 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. Therefore our third grouping of evasion proxies are derived from frequencies of audits and 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. 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 elections. Given the time frame covered by our tax data, we focus on the

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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 (IRS 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.
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.

Overall, our results provide novel evidence for an attitudinal component to tax compliance. 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 the typical partisan 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 3 percent. We also show that suspect EITC claims and audits increase, which we interpret as further evidence of tax evasion. The magnitudes are not only statistically significant, but also economically significant. When the most partisan counties move into alignment, our proxies for evasion generally fall by about 0.1 standard deviations of the across county and year distributions.

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. 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, but not when looking at EITC claims. Second, we find that the evasion response is muted in states where the cost of evasion is higher because 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 president.

Our findings have several policy implications. For one, the sensitivity of reporting that we uncover for less visible income sources underscores the value of targeting auditing resources to returns with these types of income, as well as the potential for more extensive use of third-party reports to raise their visibility. More novel is the fact that we find suspect EITC claims often move in parallel with our other proxies, confirming that these are useful markers for evasion as well. The most obvious implication of the paper—that audits should be targeted along political grounds—is also obviously absurd, as it would directly contradict the IRS’s nonpartisan charge.

Instead of targeting people based on their attitudes toward government in order to improve tax compliance, what the government can do is attempt to change the attitudes themselves. Absent policy or institutional reforms or other real actions that increase taxpayer approval, this could be done through information campaigns. Americans are unclear about who bears the burden of taxes and how the government

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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.
spends their money. Typically, the IRS includes only limited information about the federal budget in the instructions to the individual income tax form. For example, in the 2019 instructions, there are two charts showing the revenues and outlay percentages by aggregate category (on page 103 of 108 pages) but no information about the distribution of the tax burden (IRS 2020). 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 are spent ineffectively overstate by a greater extent. Further, these perceptions are changeable. When Kaiser told poll respondents that the national government spends less than 1 percent of its budget on foreign aid, the fraction of respondents saying that too much was spent on aid fell by half, from 56 percent to 28 percent (Rutsch 2015). As noted in our literature review, single mailings on these topics produce mixed impacts on tax compliance. However, we speculate that clearly and repeatedly conveying information may change individuals’ perceptions about their tax burdens and alter their inclination to evade taxes.

The remainder of the paper proceeds as follows. In Section I, 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 II, and the results in Section III. In Section IV, we conclude.

I. Tax Morale and the Role of Political Alignment

A. 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 toward 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 more specifically.

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. (2010) 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., Steenbergen, McGraw, and Scholz 1992; Fortin, Lacroix, and Villeval 2007; Cummings et al. 2009).

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 are short-lived and do not change the allocation of revenues or political circumstances—on tax compliance have been mixed. While De Neve et al. (forthcoming) find that messages of reciprocity were effective in increasing Belgian 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. Among firm owners subject to the value-added tax in Uruguay (Bérgolo et al. 2017) and individuals subject to the property tax in Argentina (Castro and Scartascini 2015), the income tax in Minnesota (Blumenthal, Christian, and Slemrod 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. 2019) and from overdue personal income taxes in the United Kingdom (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 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

B. Linking Political Alignment to Tax Morale

Our proxy for tax morale with regard to 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

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

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 United Kingdom had an immediate negative impact on tax payments that due to changes to social norms persisted long after the tax was repealed.
understand that there are multiple players, including Congress, in macroeconomic conditions, and thus vote accordingly.\(^\text{11}\) 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 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 online 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.

\(^{11}\) 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).

II. Methodology and Data

A. 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. 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 inferring evasion is to identify suspect claims of the EITC. The EITC is subject to high rates of over-claiming, which may partly reflect taxpayer confusion but also likely reflects evasion given the relatively large size of the credit. Based on audit studies, the IRS estimates about one-third of credit payments reflect overpayments (Marcuss et al. 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 underreporting 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 EITC are suspect. Exploiting the random assignment of audits, they show that among EITC taxpayers with self-employment

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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 include information on nearly every line of the 1040 and most supporting schedules filed, as well as records of audits. See Online Appendix B for more details on the IRS variable creation.

14 The value of the EITC depends on the number of qualifying children. In 2019, values ranged 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 (IRS 2019b).
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, as markers for evasion we consider rates of claiming the EITC among all taxpayers and among the subset that is self-employed, as well as the propensity for the self-employed claimants to bunch near the minimum earnings level that qualifies for the maximum credit. Following Chetty, Friedman and Saez (2013), we identify these “sharp bunchers” as returns with dependents and nonzero Schedule C income that report net earnings within $500 of the minimum income required for the maximum credit.

Finally, we infer evasion using the frequency of audits. 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 audits, we look at the number of returns adjudicated via audit to owe additional tax.

For all our proxies, 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. 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 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.

B. Methodology

With these proxies for evasion in hand, we exploit presidential turnover elections to provide quasi-experimental variation in political alignment. Our focus is on partisan 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 shifts in tax morale.

We characterize counties based on 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

15 See online Appendix B for more details and online Appendix E (Table E3) for results that use the full, unrestricted sample and yield qualitatively similar results.
party of the president.\textsuperscript{16} 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.\textsuperscript{17} 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.

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 again. For our primary regression analyses, we employ a window sample bracketing these two elections. Specifically, we include the years 1999 and 2001 for the 2000 election and the years 2007 and 2009 for the 2008 election.\textsuperscript{18} 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 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 panel A 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 that 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 in Section IIID.

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:

$$Proxy_{cst} = \beta \times alignment_{cst} + X_{cst} \Omega + \alpha_c + \delta_{st} + \varepsilon_{cst},$$

\textsuperscript{16} 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.

\textsuperscript{17} 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 online Appendix Tables E5-E8.

\textsuperscript{18} 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.
where $\alpha$ is a vector of county fixed effects and $\delta$ 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 $\beta$ 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

**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.
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). A second channel that has been documented is federal spending targeted to counties on the basis of political alignment.

Figure 4 demonstrates this key challenge for our sample period where swings in alignment for Democratic and Republican counties (panel A) occur under contrasting economic environments (as demonstrated by the unemployment series in panel B). 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 amounts reported on information returns (wages from W2 forms and financial and retirement income and unemployment compensation from 1099 forms) and the shares of wages paid by different types of businesses (S-corporations, C-corporations and partnerships). Unemployment compensation paid out captures local unemployment intensity on extensive and intensive margins, and the wage shares control for the composition of business activity and possible shifting between personal and corporate tax bases. Finally, to allow for differential economic cyclicality of less visible income sources, we interact our measure of unemployment intensity with the pre-period share of residents self-employed 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 central economic differences between otherwise similar Republican 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 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.

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19 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.

20 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. 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.

21 We create the wage share variables by linking the W2 forms to various business tax returns by employer identification number. Online 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.
Our variety of dependent variables also addresses concerns that results may be driven by unobserved 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 tax 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 voter 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 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 operate through social multipliers.

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 partisans 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 online Appendix C for details). Further, to the extent that there are differential audit expectations, 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 greater perceived costs.

C. 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. Fifteen percent of counties are always majority Democratic, 48 percent are always majority Republican, and the remainder we classify as nonpartisan counties. 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. While many states

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22 County vote returns were purchased from http://uselectionatlas.org/ (Leip 2020). See online 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.

23 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.

24 Starting from an unbalanced panel of the 3,149 counties that ever existed from 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), and (v) combined with other areas for reporting by the Bureau of Economic Analysis (1996–2012a, b) (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.
Table 1 reports means and standard deviations for the dependent (panel A) and independent variables (panel B) by the partisan status of the county. In this table, all financial variables have been converted to real 2010 dollars per capita while all counts have been converted to per capita measures. 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.

It is important to note that all of the evasion proxies reported in Table 1 are highly skewed, even after we normalize by population. Thus, in the regression analyses, we transform these by taking the natural logarithm. To maintain comparability, we do the same for the independent variables that are shown in terms of real dollars per capita or counts per capita.

The skewness statistics are all well above 1 for the per capita versions of the dependent variables and well below 1 once the natural log is taken. Though log per capita is our preferred functional form, we reproduce our main estimates using level per capita versions in online Appendix Table E9.
Table 1—Summary Statistics

<table>
<thead>
<tr>
<th></th>
<th>2000 and 2008 elections</th>
<th>Democrat</th>
<th>Republican</th>
<th>Nonpartisan</th>
</tr>
</thead>
<tbody>
<tr>
<td><strong>Panel A. Dependent variables (IRS aggregates for policy constant filers)</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Reported income, per capita $2010</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Gross income less capital gains</td>
<td></td>
<td>20,575</td>
<td>18,186</td>
<td>17,457</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(9,246)</td>
<td>(6,152)</td>
<td>(5,818)</td>
</tr>
<tr>
<td>Information-reported and withheld (wages, salaries, and tips)</td>
<td></td>
<td>15,578</td>
<td>13,592</td>
<td>13,251</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(6,538)</td>
<td>(4,674)</td>
<td>(4,268)</td>
</tr>
<tr>
<td>Substantial information reporting (interest, dividend, and retirement income)</td>
<td></td>
<td>3,078</td>
<td>2,882</td>
<td>2,662</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1,783)</td>
<td>(1,312)</td>
<td>(1,211)</td>
</tr>
<tr>
<td>Little information reporting (Schedules C and E income)</td>
<td></td>
<td>1,831</td>
<td>1,886</td>
<td>1,520</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1,726)</td>
<td>(1,506)</td>
<td>(1,130)</td>
</tr>
<tr>
<td>Filing rates, per capita</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Filed an income tax return</td>
<td></td>
<td>0.398</td>
<td>0.382</td>
<td>0.376</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.056)</td>
<td>(0.057)</td>
<td>(0.052)</td>
</tr>
<tr>
<td>Claimed the EITC</td>
<td></td>
<td>0.096</td>
<td>0.077</td>
<td>0.086</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.048)</td>
<td>(0.025)</td>
<td>(0.029)</td>
</tr>
<tr>
<td>Filed a Schedule C and claimed the EITC</td>
<td></td>
<td>0.016</td>
<td>0.016</td>
<td>0.016</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.010)</td>
<td>(0.007)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>Filed a return exhibiting sharp bunching</td>
<td></td>
<td>0.001</td>
<td>0.001</td>
<td>0.001</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.002)</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Return was audited</td>
<td></td>
<td>0.004</td>
<td>0.002</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.003)</td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
<tr>
<td>Return was audited and found to owe taxes</td>
<td></td>
<td>0.003</td>
<td>0.002</td>
<td>0.002</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.003)</td>
<td>(0.001)</td>
<td>(0.002)</td>
</tr>
<tr>
<td><strong>Panel B. Independent variables</strong></td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Partisanship measure</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Average Democratic two-party vote share, 1996–2008</td>
<td></td>
<td>0.619</td>
<td>0.336</td>
<td>0.475</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.071)</td>
<td>(0.074)</td>
<td>(0.048)</td>
</tr>
<tr>
<td>Information return amounts, per capita $2010</td>
<td></td>
<td></td>
<td></td>
<td></td>
</tr>
<tr>
<td>Wages, tips and other compensation (W2 box 1)</td>
<td></td>
<td>15,752</td>
<td>13,778</td>
<td>13,417</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(6,508)</td>
<td>(4,665)</td>
<td>(4,253)</td>
</tr>
<tr>
<td>Share linked to S-corporation by employer id</td>
<td></td>
<td>0.151</td>
<td>0.160</td>
<td>0.158</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.045)</td>
<td>(0.050)</td>
<td>(0.047)</td>
</tr>
<tr>
<td>Share linked to C-corporation by employer id</td>
<td></td>
<td>0.209</td>
<td>0.205</td>
<td>0.205</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.074)</td>
<td>(0.067)</td>
<td>(0.069)</td>
</tr>
<tr>
<td>Share linked to partnership by employer id</td>
<td></td>
<td>0.049</td>
<td>0.051</td>
<td>0.047</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.027)</td>
<td>(0.029)</td>
<td>(0.028)</td>
</tr>
<tr>
<td>Financial income (1099INT taxable interest, 1099DIV ordinary dividends)</td>
<td></td>
<td>839</td>
<td>791</td>
<td>706</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(687)</td>
<td>(401)</td>
<td>(386)</td>
</tr>
<tr>
<td>Retirement income (1099R gross pension distributions, 1099SSA Social Security net payments)</td>
<td></td>
<td>5,185</td>
<td>5,280</td>
<td>5,223</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(1,695)</td>
<td>(1,640)</td>
<td>(1,435)</td>
</tr>
<tr>
<td>Unemployment compensation (1099G UI payments)</td>
<td></td>
<td>206</td>
<td>150</td>
<td>184</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(173)</td>
<td>(160)</td>
<td>(166)</td>
</tr>
<tr>
<td>Differential cyclicality</td>
<td>Self-employed, per capita in 1990</td>
<td></td>
<td>0.038</td>
<td>0.061</td>
</tr>
<tr>
<td></td>
<td></td>
<td>(0.020)</td>
<td>(0.035)</td>
<td>(0.024)</td>
</tr>
</tbody>
</table>

(continued)
III. Results

A. 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, standard error, and $R^2$ from the mean-deviated regressions on alignment from a different specification of equation (1)\(^{26}\). 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

\(^{26}\)We include the $R^2$ recognizing that coefficient stability across specifications is not sufficient to rule out omitted variable bias. Rather, following Oster (2019), which builds on Altonji, Elder, and Taber (2005), one needs to also evaluate movements in the $R^2$ relative to a theoretical maximum possible explained variation. We provide this input so that interested readers can construct Oster’s approximate bound after assuming a value for the theoretical maximum.
Table 2—Estimates of the Impact of Alignment on Proxies for Tax Compliance, Baseline Alignment Measure

<table>
<thead>
<tr>
<th>Control set</th>
<th>log per capita reported income</th>
<th>log per capita number of returns</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Wages and salaries (1)</td>
<td>Financial and retirement (2)</td>
</tr>
<tr>
<td>Baseline specification</td>
<td>−0.004 (0.003)</td>
<td>−0.001 (0.005)</td>
</tr>
<tr>
<td>Omitting unemployment × self-employment intensity</td>
<td>−0.004 (0.003)</td>
<td>0.006 (0.005)</td>
</tr>
<tr>
<td>Adding U × predicted propensity to be partisan Democrat</td>
<td>−0.004 (0.003)</td>
<td>−0.000 (0.005)</td>
</tr>
<tr>
<td>Adding U × predicted propensity and U × avg. Dem. vote share</td>
<td>−0.004 (0.003)</td>
<td>−0.005 (0.006)</td>
</tr>
<tr>
<td>Adding housing market controls (H)</td>
<td>−0.004 (0.003)</td>
<td>−0.004 (0.005)</td>
</tr>
<tr>
<td>Adding H and H × predicted propensity to be partisan Democrat</td>
<td>−0.005 (0.003)</td>
<td>−0.004 (0.005)</td>
</tr>
<tr>
<td>Adding H, H × predicted propensity, and H × avg. Dem. vote share</td>
<td>−0.006 (0.003)</td>
<td>−0.001 (0.005)</td>
</tr>
<tr>
<td>Dependent variable SD</td>
<td>0.337 (0.031)</td>
<td>0.417 (0.082)</td>
</tr>
<tr>
<td>Dependent variable mean (in levels)</td>
<td>14.067 (5.249)</td>
<td>2.929 (1.441)</td>
</tr>
</tbody>
</table>

Notes: Each cell reports results from a separate county fixed effects 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 and within $R^2s$ (from the mean-deviated regressions) in brackets. 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 in 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) nonfarm private employment, government employment, unemployment, and number of establishments, as well as number of housing permits 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.
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. 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 income increases (decreases) by a significant 0.086 log points or 13 percent of the dependent variable’s standard deviation. 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.29 By comparison, DeBacker et al. (2018) 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 on individual income tax returns accounts for nearly a third of the IRS estimate of the tax gap (IRS 2019a).

Our second type of evidence for a causal link between alignment and evasion is suspect EITC claims. Claiming the EITC unambiguously decreases tax liability. Further, there is the potential for the credit to be claimed erroneously because it is not possible to perfectly observe eligibility. For example, taxpayers may falsely claim to be unmarried or have resident children or may misreport self-employment income, as both gains and losses count toward earnings for the EITC. In the next three columns of Table 2, we explore two broader sets of EITC filers, as well as the rare sharp bunchers (where rates in our sample are about 1 per 1,000 residents).30

27 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.
28 When we examine the impact of alignment on Schedule C and Schedule 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.
29 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.
30 This is lower than the 2.1 percent 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.
The significant negative coefficients of $-0.029$, $-0.048$, and $-0.077$ on log per capita EITC claims, Schedule C and EITC claims, and sharp bunching returns represent movements of about 8–10 percent of a standard deviation of their respective dependent variables. Turning to how this plays out in counties, we find alignment decreases the rate of EITC claims by about 0.9 percent in the average partisan county. Moving into alignment also decreases the rates of filing both Schedule C and claiming the EITC by 1.4 percent. Finally, despite both the rarity of the event and the reduction in our sample size, 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 percent reduction in this behavior.

Our final two dependent variables are related to audits: the log per capita number of audits triggered and number adjudicated to owe money. Both decrease with alignment. The significant coefficient of $-0.128$ (column 7) indicates a relative change in audits that is double that of the tax gap and EITC proxies, at 22 percent of a standard deviation for a one-unit change in alignment. The coefficient of $-0.072$ (column 8) for audits found owing is more in line with the other evasion proxies, representing about 10 percent of a standard deviation. In other terms, residents of the average county that moves into alignment are nearly 4 percent less likely to submit returns that are audited. Moving out of alignment increases the frequency of submitting returns that are audited and found to owe by 2 percent, suggesting that audits that are marginal to alignment have a lower yield than the average audit.

### B. Robustness to Varying Economic Controls

In summary, the first row of Table 2 presents results from three types of tests (tax gap, EITC claims, 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. 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, likely correlated with alignment, according to its importance as a sector. And rather than the omitted variable biasing us away from zero, we see in row 2 of Table 2 that the omission of the...
interaction 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 cyclicity. We use non-IRS baseline eco-

33 Predictors include nonfarm private employment, government employment, unemployment, and number of
establishments, as well as number of housing permits and the share of establishments by industry (as detailed in
online Appendix Table E1). All are from 1990, and all but housing permits (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.

nomic variables to predict the propensity for a county to be partisan Democratic
as opposed to Republican. 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
analogous interaction using the county average vote share, which is the variable we
use to construct alignment each year. While some coefficients increase and others
decrease in magnitude, as demonstrated in row 4, all three types of measures still
yield significant evidence of a causal link between political alignment and evasion.

Our macroeconomic proxy based on unemployment may not fully capture income
dynamics, particularly during the Great Recession when housing price dynamics
were salient. Thus, in the remainder of Table 2, we explore the sensitivity of our
results to augmenting the specification to include measures of economic cyclicity
drawn from the housing market, namely the number of mortgage-months serviced
per capita and median home value. In row 5, 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 propen-
sity to be Democratic (row 6) and then additionally with the average Democratic
vote share (row 7). For both of these augmented interaction models, we find sig-
nificant 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
overcontrolling for differences across partisan counties that are noneconomic and
may be attitudinal.

We further explore robustness of our results to additional economic controls in
online Appendix Table E3. We demonstrate that we still find tax gap, EITC, and
audit evidence of the impact of moving out of alignment on evasion when in com-
parison to our baseline specification we: (i) allow for greater flexibility of inform-
ation return controls by interacting them with indicators for the second election;
(ii) include directly as controls time-varying versions of the non-IRS economics
variables used as predictors for county partisan status; and (iii) compare a coun-
try’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 across counties
through propensity score trimming and eliminating counties hit hard by the hous-
ing crisis, as shown in online Appendix Table E4. In other specifications in the
table, we exclude counties that are likely to have greater divergence between mea-
sured economic activity and resident incomes including those that are the location of
capital cities and those with large commuting flows. We demonstrate robustness to
including all counties, whether classified as partisan or not, by repeating all analyses
to this point with this expanded sample in online Appendix Tables E5–E8.

Finally, in online Appendix Table E9, we explore robustness to using level rather
than log per capita measures for our dependent variables, though recall that these
alternative measures exhibit skewness. While our tax gap and audit rate findings are
robust to this change, our suspect EITC findings, in particular the Schedule C and
EITC coefficient estimates are frequently insignificant across specifications.

C. 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 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 align-
ment as a binary variable that takes the value 1 for a county that has voted for the
current president’s party over the focal time period, either 1996 to 2008 (row 3)
or 1988 to 2008 (row 4). The 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 prob-
lems with this assumption. First, we do not know that the views about government

34 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 percent to
the 5 percent 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 approx-
imates 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 percent level, sharp bunching falls from the 1
to 10 percent level and Schedule C and EITC lose significance. The other three measures of evasion retain the same
significance levels.

35 As indicated in online 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.
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 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, panel A of 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 Gallup’s measure of national presidential approval (1993–2001, 2001–2009, 2009–2017) averaged across the tax year as our key independent variable, stratified by party. We assign the Democratic (Republican) approval measure to the Democratic (Republican) counties. Consistent with the binary alignment measure, approval abstracts from variation across 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.

D. Heterogeneity

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 panel A of 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 toward a particular president and secular trends. Given these limitations, we relegate event study style results to online 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. Instead, we interpret the results surrounding this election as reflecting increased intensity of alignment holding fixed the party of the incumbent president. 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 2004.36 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 2 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

36 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).
Table 4—Estimates of the Impact of Alignment on Proxies for Tax Compliance, by Election

<table>
<thead>
<tr>
<th>Control set</th>
<th>log per capita reported income</th>
<th>log per capita number of returns</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Wages and salaries</td>
<td>Financial and retirement</td>
</tr>
<tr>
<td></td>
<td>(1)</td>
<td>(2)</td>
</tr>
<tr>
<td>Baseline specification</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Alignment × 2000 election</td>
<td>0.000 (-0.001) -0.041</td>
<td>0.009 (-0.089) -0.210</td>
</tr>
<tr>
<td></td>
<td>(0.003) (0.006) (0.027)</td>
<td>(0.005) (0.014) (0.039)</td>
</tr>
<tr>
<td>Alignment × 2004 election</td>
<td>0.004 -0.000 0.115</td>
<td>-0.019 -0.121 -0.093 -0.017 -0.023</td>
</tr>
<tr>
<td></td>
<td>(0.002) (0.006) (0.027)</td>
<td>(0.008) (0.015) (0.039)</td>
</tr>
<tr>
<td>Alignment × 2008 election</td>
<td>-0.008 -0.003 0.218</td>
<td>-0.070 0.000 0.071 -0.272 -0.147</td>
</tr>
<tr>
<td></td>
<td>(0.006) (0.009) (0.046)</td>
<td>(0.007) (0.014) (0.035)</td>
</tr>
<tr>
<td>Adding U × predicted propensity to be partisan Democrat</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Alignment × 2000 election</td>
<td>0.000 -0.001 -0.039</td>
<td>0.009 -0.085 -0.205 0.018 0.009</td>
</tr>
<tr>
<td></td>
<td>(0.003) (0.006) (0.027)</td>
<td>(0.005) (0.014) (0.039)</td>
</tr>
<tr>
<td>Alignment × 2004 election</td>
<td>0.004 0.001 0.108</td>
<td>-0.018 -0.132 -0.107 -0.029 -0.037</td>
</tr>
<tr>
<td></td>
<td>(0.002) (0.006) (0.027)</td>
<td>(0.009) (0.016) (0.040)</td>
</tr>
<tr>
<td>Alignment × 2008 election</td>
<td>-0.009 -0.001 0.202</td>
<td>-0.066 -0.027 0.037 -0.301 -0.181</td>
</tr>
<tr>
<td></td>
<td>(0.006) (0.009) (0.047)</td>
<td>(0.007) (0.015) (0.037)</td>
</tr>
<tr>
<td>Adding H and H × predicted propensity to be partisan Democrat</td>
<td></td>
<td></td>
</tr>
<tr>
<td>Alignment × 2000 election</td>
<td>-0.001 -0.002 -0.035</td>
<td>0.009 -0.080 -0.205 0.011 0.003</td>
</tr>
<tr>
<td></td>
<td>(0.003) (0.006) (0.028)</td>
<td>(0.006) (0.014) (0.039)</td>
</tr>
<tr>
<td>Alignment × 2004 election</td>
<td>0.003 -0.003 0.111</td>
<td>-0.024 -0.098 -0.074 -0.022 -0.025</td>
</tr>
<tr>
<td></td>
<td>(0.002) (0.006) (0.028)</td>
<td>(0.008) (0.015) (0.039)</td>
</tr>
<tr>
<td>Alignment × 2008 election</td>
<td>-0.009 -0.009 0.237</td>
<td>-0.070 0.003 0.079 -0.286 -0.157</td>
</tr>
<tr>
<td></td>
<td>(0.006) (0.009) (0.046)</td>
<td>(0.007) (0.014) (0.036)</td>
</tr>
</tbody>
</table>

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 × 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.

Reducing evasion, while our tax gap and audit findings are not. In 2008 we find the reverse, robust evidence of alignment curbing evasion when looking at the tax gap and audit measures, while the EITC evidence is more mixed, less stable, and in the case of sharp bunching, significantly wrong-signed in two of three specifications.37 The differences across the elections may suggest that there is differential ease or knowledge of different forms of evasion across time and space. The results for the 2004 election are consistent with the change in (and perhaps greater salience of) presidential approval from before to after the election, resulting in significant changes in evasion as measured by both the tax gap and EITC outcomes.38

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.

37 The year-to-year patterns shown in online 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.
38 Given the smaller change in approval in 2004, we might expect the magnitudes of the coefficients in that row to be smaller than those in the other two rows if the contexts were otherwise equivalent.
Table 5—Estimates of the Impact of Alignment on Proxies for Tax Compliance, by State Income Tax Systems and Politics

<table>
<thead>
<tr>
<th>Key controls</th>
<th>log per capita reported income</th>
<th>log per capita number of returns</th>
</tr>
</thead>
<tbody>
<tr>
<td></td>
<td>Wages and salaries</td>
<td>Financial and retirement</td>
</tr>
<tr>
<td>Baseline indicator for binary alignment</td>
<td>$-0.001$</td>
<td>$-0.002$</td>
</tr>
<tr>
<td>(0.001)</td>
<td>(0.001)</td>
<td>(0.007)</td>
</tr>
<tr>
<td>State income tax piggybacking</td>
<td>$-0.003$</td>
<td>$-0.001$</td>
</tr>
<tr>
<td>Baseline indicator for binary alignment</td>
<td>(0.002)</td>
<td>(0.003)</td>
</tr>
<tr>
<td>Interaction with indicator for state tax system tied to reports on the federal return</td>
<td>$0.004$</td>
<td>$-0.001$</td>
</tr>
<tr>
<td>(0.002)</td>
<td>(0.005)</td>
<td>(0.016)</td>
</tr>
<tr>
<td>Dual alignment with president and governor</td>
<td>$-0.000$</td>
<td>$-0.002$</td>
</tr>
<tr>
<td>Aligned with president only</td>
<td>(0.001)</td>
<td>(0.001)</td>
</tr>
<tr>
<td>Aligned only with governor</td>
<td>$0.002$</td>
<td>$0.000$</td>
</tr>
<tr>
<td>(0.002)</td>
<td>(0.004)</td>
<td>(0.020)</td>
</tr>
<tr>
<td>Aligned with both president and governor</td>
<td>$0.000$</td>
<td>$-0.002$</td>
</tr>
<tr>
<td>(0.002)</td>
<td>(0.004)</td>
<td>(0.018)</td>
</tr>
</tbody>
</table>

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. (We culled this information from various issues of the Book of the States (Council of State Governments 2010–2012).) 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, data from Klarner (2013). 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.

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 1 percent level and in a fourth at the 10 percent level. These results are consistent with these ties increasing the costs of evasion and 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 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.\(^{39}\)

IV. Conclusion

While prior work finds that attitudes about government affect tax evasion in laboratory settings, we find real-world evidence consistent with taxpayers’ disapproval of government increasing evasion. We first provide evidence from national survey data that people’s attitudes toward 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 party alignment with the president following turnover elections to provide evidence from three types of proxies for evasion. The first proxy is based on the tax gap approach that relates reported income to income generated, attributing depressed rates of reporting for less visible income sources (such as Schedule C&E) to evasion. The second set are based on suspect EITC claims overall and among sole proprietors. The third set of proxies are derived from the frequencies at which audits are triggered and found owing. All are consistent with moving into alignment causally reducing tax evasion, with magnitudes of impacts for the most partisan counties on the order of 0.1 standard deviations. Heterogeneous effects further suggest a causal role for alignment in evasion. We find that responses are muted when the cost of evasion increases because federal income tax reports are direct inputs to state tax returns and that responses are magnified when the benefits of evasion increase because county residents are doubly unaligned with the president and governor.

We end with two suggestions for future research. First, does alignment impact taxpayer behaviors beyond evasion? One could explore the effect of alignment on avoidance, for example. Second, can the results of the current paper be used to shape policy aimed at reducing evasion? The next step is to identify interventions that are effective in shifting taxpayer attitudes in ways that carry through to improved tax compliance.

REFERENCES


\(^{39}\) Another type of heterogeneity that would be interesting to explore is by county party, however, we remind readers that our data use agreement precludes this. 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.
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