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INTERGOVERNMENTAL COOPERATION AND TAX ENFORCEMENT

Ugo Troiano

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ABSTRACT

Improving the efficiency of tax collection is important for development and fairness purposes. I study the Audit Exchange Information Agreements, which are agreements between the states and the U.S. federal government to exchange information about income tax audit plans and techniques, signed between the 1950s and the 1970s. Adopting an augmented difference-in-differences identification strategy, I show that the program increased state income tax revenues by about 15 percent. I show that mobility and the reported income do not appear to react to the policy, suggesting that the effects may be linked to higher quality auditing. The effects are stronger in places where there are more civic and social organizations, suggesting that tax compliance is higher when there is more cooperative gathering.

Ugo Troiano
Department of Economics
University of Michigan
611 Tappan Street, Lorch Hall 219
Ann Arbor, MI 48109
and NBER
troiano@umich.edu

1 Introduction

The ability to collect taxes in an efficient and fair way is one of the main determinants of state capacity and, in turn, economic growth (Besley and Persson (2013)).

In this short paper, I study the Audit Exchange Information Agreements, which are agreements between the states and the federal U.S. government to exchange information about income tax audit plans and techniques, signed between the 1950s and the 1970s, in a staggered fashion. Those agreements were voluntarily signed by both parties involved, and it is safe to assume, as I discuss below, that both parties had equal bargaining power in designing the agreement.

The main hypothesis that I am testing is whether and in which circumstances these agreements increased income tax revenues. There are some reasons for which this type of agreements may not work. The main reason is transaction costs. Transaction costs may limit the effectiveness of this agreement, especially given that the budget of the two institutions was not increased because of the agreements. Second, people may move as a response to the additional enforcement, leaving revenues unchanged, or even worsened.

At first, one may be tempted to simply compare states that adopted the policy, before and after the adoption of the policy. However, it should be noted that the timing of signing the agreements may not be as good as randomly assigned. States and federal government may be less reluctant to forego some of their jurisdiction in favor of another layer of government when facing budgetary needs, for instance. Additionally, there can be omitted variables creating bias in the estimates. The timing of the signing may be related to unobserved characteristics like the voters' taste for fiscal discipline. If this taste evolves in a non-random way, as is it plausible, one may think that the agreements are more likely to be signed when voters have a preference for fiscal discipline.

In order to alleviate these concerns, I adopt a difference-in-differences strategy. The idea is to compare the revenues from the income tax in states that sign the agreement versus states that do not sign the agreement, before and after the agreement. There are two main assumptions that need to be valid for this strategy to work. The first is that the outcomes of the treatment and the control group are on parallel trends before the treatment, and diverge only after the treatment. It is important to note that the outcomes do not need to be on similar levels before the treatment, it is enough to show a trend break after the treatment and accounting for the pre-existing differences in a proper way. The second assumption of the difference-in-

differences requires that there are no contemporaneous state-specific events to the agreements that affect differentially the treatment and control group that may in turn affect the income tax revenues.

The second assumption is more challenging to test, and requires either institutional work to make sure there were no other laws or policies or events contemporaneous to the signing and affecting differentially the treatment and control group, for every state, or requires augmenting the strategy with additional assumptions. This setting offers a interesting opportunity to rigorously apply the difference-in-differences strategy without the need of a formal randomized controlled trial. Below I show that, under the assumption that the federal government and the state have equal bargaining power in the agreement, a reasonable assumption, one could control for the interaction between federal budgets and state fixed effects and augmenting the standard difference-in-differences framework to identify the causal effect of interest.

I find that revenues from income taxes increased by about 15 percent. Mobility does not seem to react to the policy. Additionally, I investigate whether the future reported income and the future number of returns change as a response to the policy, and I find that they do not. The set of these results suggest that the main effects may be plausibly linked to higher quality auditing.

Then, I investigate whether the response was different in places characterized by higher cooperative gathering, as measured by the number of civic and social organizations per capita. I find that the increased tax revenues deriving from the policy are concentrated in places with more cooperative associations, suggesting that there is complementarity between norms of cooperation and tax enforcement.

Finally, I show that the results are robust to accounting for the introduction of state income tax withholding.

The results are consistent with the canonical tax evasion and tax enforcement models, Allingham and Sandmo (1972) and Slemrod and Yitzhaki (1987), where the audit exchange information agreements are interpreted as a pure reduction in enforcement costs – see the next paragraphs. If the interpretation is correct, the intervention causes an increase in tax revenues without affecting the total budget. The model is not provided because of its simplicity and its strong ties with the canonical model, but it can be provided upon request.

To the best of my knowledge, there are no other papers on intergovernmental audit exchange agreements between different layers of government. This paper naturally builds a

bridge among the literature on tax enforcement, the literature on the historical development of the U.S. tax enforcement policies, and the literature on fiscal federalism.

The theoretical literature on tax enforcement is summarized, among others, by Slemrod and Bakija (2008). The canonical model for analyzing tax enforcement problem derives from the economics of crime, Becker (1968), and has been adapted by Allingham and Sandmo (1972) and Slemrod and Yitzhaki (1987) for analyzing respectively tax evasion and tax enforcement problems. The main concerns with additional enforcement are that the richer population moves out of state, or that people work less as a result of the additional enforcement. This paper contributes to this literature by showing that cooperation among different layers of government can result in economically sizable increases in tax revenues, keeping constant the population in a state and the future reported income.

On the empirical tax enforcement side, the closest literature is the one about information disclosure. There are no papers about information disclosure of tax information among different layers of government specifically, but there is a rapidly growing literature on the information disclosure that tax agencies or researchers do to private citizens. Hasegawa et al. (2013) show that consumers and firms have a preference for having their income not publicly revealed. Slemrod, Thoresen and Bø (2012) look at a policy introduced in Norway in 2001 where income are publicly revealed and find a small average increase in reported business incomes after the program started in communities that previously had limited disclosure, consistent with public disclosure limiting tax evasion. Perez-Truglia and Troiano (2017) find evidence that publicly exposed delinquents are more likely to solve their tax debt when there is an increase in the salience of their delinquency status toward their neighbors. All the previous papers deal with information from tax agencies to citizens. Naritomi (2015) is an exception, she deals with the one-way tax reporting informational transmission from consumers to tax agencies, via consumption receipts.

For what matters the historical literature, Dincecco and Troiano (2015) find that the historical introduction of the income tax not only increased tax revenues, but it also affected the total size of government. Therefore, they interpret the introduction of the income tax as a broadening of the states' public goods production function.[1] Dušek (2006) finds that the historical introduction of state income tax withholding resulted in increased tax revenues and a change in the composition of the state budgets. I contribute to this literature by showing that when

[1] Another related paper is Casaburi and Troiano (2016) who show that when the national government identifies the buildings not registered in the cadastral maps, local governments are more likely to get re-elected thanks to the extra-enforcement and extra-public good provision that the government program generates.

there is an historical fiscal shock that can be interpreted as a pure reduction in enforcement costs, revenues from income tax increase without affecting the total budget.

To the best of my knowledge, there are no other papers on the two-way agreements between different layers of government within a country in the fiscal federalism literature. The closest sub-literature is the one about fiscal restraints, which are limits on the budget of local governments imposed by national governments on local governments. For surveys, see Poterba (1999), Wyplosz (2012). While some studies find that fiscal rules do indeed result in lower budget imbalances, others stress the reasons why they might not be effective (Alesina and Perotti, 1999). The recent empirical studies on the topic in developed countries show that fiscal rules do indeed result in increased fiscal discipline, see for instance, among others, Poterba (1994), Clemens and Miran (2012), Grembi, Nannicini and Troiano (2016).

Finally, by studying contracts between governments the paper contributes to the literature about international treaties between different countries. The great part of this literature focused on treaties to attract international investment in countries, and is therefore not relevant for this paper. The only exception I am aware of is Johannesen and Zucman (2014) who focus on information agreements among countries and how those affect deposits in tax heavens.

The paper proceeds as follows. In Section 2 I present the institutional framework and the data. In Section 3 the empirical strategy is described. In Section 4 our results are presented. In Section 5 two robustness tests are presented. In Section 6 I conclude.

2 Institutional Framework and Data

In this section I present the basic concepts to understand the intergovernmental relations in state income tax administration in the United States. After that, I discuss the dataset assembled for the analysis.

2.1 Audit Exchange Information Agreement Signing

Cooperative federalism in tax administration requires actions by both the national and the state legislative body plus joint effort by the IRS and the states' department of revenues.[2] Since the introduction of the income tax, Penniman (1980) claims that the cooperation among

[2]This section borrows heavily from Penniman (1980).

states and the federal government for the enforcement of the state income tax has steadily increased. It is interesting to note that even in earlier years, in the 1920s, federal individual, joint, partnership, estate and trust returns were freely available upon request of the state governments provided that "the request was for investigating issues arising from state income tax only". Penniman (1980) claims that in the 1930s all the income tax states except three had used that opportunity for tax enforcement purposes.

The first mention of formal agreements was made during a joint federal-state conference in the 1940s, where a formal exchange of information on audit plans and techniques was recommended. The first states that adopted such a formal signed agreement were North Carolina and Wisconsin during 1950. Colorado, Kentucky and Montana shortly followed during 1951 and 1952.

The U.S. Treasury's 1952 *Coordination Study* (Washington D.C.: Government Printing Office) described the program as follows:

> Under the procedure adopted for the two initial projects, the examining officers in the offices of collectors and revenues agents-in-charge prepare abstracts of audit information for each changed return showing a deficiency in tax.
>
> The abstracts are prepared in longhand by the examining officer at the time his report of examination is made and are attached to the face of the return. After the deficiencies have been listed for assessment, the abstract is detached and forwarded to the State tax authorities. The states procedure with respect of furnishing the abstracts to the federal government is similar to the Federal practice.[3]

After the 1950s, the technological developments helped lowering the costs of cooperative agreement, and the audit agreements spread so fast that in less than 20 years the great majority of the income tax states had signed an audit information agreement with the federal government.

From the previous two paragraphs the fact that these agreements are quite distinct from a hierarchical interaction should be clear to the reader. The agreements involve equal duties and responsibilities. Penniman (1980) shows qualitatively that state audit tax collections resulting from IRS revenue agent reports totaled more than 50 millions of dollars, while the IRS has never provided statistics on the value of its recoveries based on state audit information, because it has never consistently maintained such data.

[3]The last sentence is not quoted *verbatim*.

One of the main advantages of such agreements is dividing the work of auditing, lowering the costs of enforcement. For instance, the Colorado and Minnesota agreements in the 1950s "explicitly provided that the state would assume audit responsibility for the lower-income returns, whereas the IRS would audit only the larger-income returns in the state" (Penniman, 1980). Therefore, it is safe to assume that in those agreements both the state and the federal government had equal bargaining power. This assumption will be useful later on in the econometric strategy. The dates of agreements are summarized in Table 1.

2.2 Fiscal Data

Census data about state government finances are available online every two years from 1942 to 1950 and at the yearly level onward (US Department of Commerce, 2015). I merge this dataset with one including information about the total adjusted gross income (AGI) reported at the state level and the number of filed returns for income tax purposes, from Frank (2009), available every year since 1917. The result is an unbalanced panel that covers all 50 states and Washington DC between 1942 and 2008.

Table 2 displays the descriptive statistics for all of our main variables.

3 Empirical Strategy

In this section, I outline the approach to estimate the policy responses to the audit exchange information agreements. I implement a difference-in-differences approach based on the staggered introduction of the ability to raise state revenues by mutual cooperation between the state and the federal government.

The first specification is therefore:

$$Y_{it} = \beta_0 + \beta_1 Post_{is} + \phi_i + \phi_t + \epsilon_{ist} \tag{1}$$

The main dependent variable is the income tax revenue in state i and year t, in real terms. All of the dependent variables in the analysis are in logarithms. The dummy *Post* is equal to one after the introduction of the audit information exchange agreement in year s, which differs across states (see Table 1). The state and year fixed effects, ϕ_i and ϕ_t, control respectively for state and time invariant factors. The standard errors, ϵ_{ist}, are robust to heteroskedasticity

and clustered at the state level. The coefficient of interest β_1 captures the effect of introducing cooperation between the federal and the state government under plausible assumptions.

This methodology has two main assumptions. The first is that treated and control states have to be on parallel trends for the main outcomes of interest. If they were not on parallel trends, the coefficient β_1 may capture pre-existing differences in the evolution of the time-series. I verify this assumption when discussing the results. The second assumption is more complicated to test: absence of contemporaneous policy events that are systematically correlated with the treatment and affect differentially the treatment group and the control group. The rationale is clear: if there were such events, one would not be able to clearly ascribe the effect to the policy of interest. However, only if the policy were randomly assigned (in time, not only in space), one could be sure about this assumption.

In order to alleviate the concerns related to time, in the second specification I control for state-specific linear trends:

$$Y_{it} = \beta_0 + \beta_1 Post_{is} + \phi_i + \phi_t + \phi_{Li} + \epsilon_{ist} \tag{2}$$

The previous specification alleviates the concerns arising from the second assumption of the difference-in-differences method. Yet, there could still be non-linear events that affect differentially the treated and control states. If this happened, for instance because states sign the agreement when they are more in budgetary needs, the identifying assumptions would be violated. The design of the policy, which requires a two-way agreement and interaction between the state and the federal government offers a unique opportunity to make this method almost as good as random. Let's assume the following: (1) both the state and the federal government, if it was costless, would prefer more rather than less information about auditing income tax returns, (2) both the state and the federal government have equal bargaining power in the contractual arrangement of the exchange information agreements. If the two assumptions are satisfied, it is easy to see that if we had a way to control for the federal confounders, we would be able to bound (of 50 percent) the bias arising from the non random assignment (in time) of the policy. The way to do that is interacting the federal budget variables with state fixed effects as in the following specification:

$$Y_{it} = \beta_0 + \beta_1 Post_{is} + \phi_i + \phi_t + \phi_{Li} + \phi_i * FedSurpl + \phi_i * FedRev + \epsilon_{ist} \tag{3}$$

where FedSurpl is the Federal Surplus and FedRev are the Federal Revenues.

I focus on revenues from the income tax as the main outcome for a precise reason: as described in the institutional framework, it was the variable targeted by the policymakers when designing this policy.

In Subsection 4.1 we discuss whether the presence of civic and social organizations may correlate with the observed increase in the income tax revenues. The equation we estimate is the following

$$Y_{it} = \beta_0 + \beta_1 Post_{is} + \beta_2 Post_{is} * Org_i + \phi_i + \phi_t + \epsilon_{ist}, \tag{4}$$

where Org_i is the civic and social organizations in state i.

4 Main Results

In this section, I investigate the quasi-experimental consequences of signing the audit information exchange agreements on the revenues from the income tax. I present three set of specifications. The first specification is the equation (1), presented in the first panel of the following Tables, and it is a standard difference-in-differences specification. The second specification, equation (2), presented in the second column, adds to specification (1) the state-specific linear time trends, to check the robustness of the results to linear pre-trends. The third specification, equation (3), presented in the third column, adds both the federal surplus (or deficit) and the federal revenues interacted with state fixed effects. As discussed in the previous section, in the third specification the difference-in-differences approximates a randomized experiment.

The first outcome that I investigate is mobility. People do respond to the tax environment by moving out of the jurisdictions that increase taxes (Liebig, Puhani and Sousa-Poza, 2006, Moretti and Wilson, 2013, Kleven, Landais and Saez, 2013, Akgit, Baslandze and Stantcheva, 2015). If people moved as a response to the policy, one would need to worry about selection and composition of the adjustments. Therefore, I consider population as the first analyzed outcome, in Table 3. The coefficients are small and are never statistically different from zero.

Our main Table of interest is Table 4. In this Table I investigate whether the revenues from the income tax, the main outcome as intended by the policy-maker, respond to the information exchange agreements. The coefficients are always statistically different from zero. In the more credible specification, the revenues from income tax increase by about 15 percent.

Sometimes budget policies can have consequences on other outcomes that are not the

main ones of interest. For instance, the introduction of the income tax in the twentieth-century United States has affected the total size of the budget, likely because it changed the production function of producing public goods (Dincecco and Troiano, 2015). If the exchange information agreements just changed the cost of income tax enforcement, they should not change the total size of the budget, but just affect the main outcome of interest, the revenues from the income tax, under reasonable assumptions. This is indeed what one can see in Table 5, which show that total revenues stayed relatively stable after the intervention.

The Audit Exchange Information Agreement may have had also deterrence effect, other than just affecting the audited outcomes. This is what I investigate in Table 6 and Table 7, where I study whether the total reported future Adjusted Gross Income or the total number of Tax Returns changes as a response to the policy. The coefficients are almost always not statistically significant and are typically close to zero. The set of these results suggests that the audit exchange information policy affected the income tax revenue only through the intended effect, i.e. better auditing, even though data on audits that span so long back in time are not available, to the best of my knowledge.

4.1 Cooperative Gathering and Tax Enforcement

Social capital is typically defined as shared norms and beliefs of cooperation. The concept is quite broad, and often context-specific. While social capital has been for decades mainly studied by sociologists and political scientists, even economists are recently starting to care about social capital. Knack and Keefer (1997) show that social capital matters for economic growth. Slemrod (2001) reviews the literature about public finance and a specific aspect of social capital, trusting others.

In this setting, there are at least two ways through which social capital can influence the response of the income tax revenues to the cooperation agreements. My underlying assumption is that they go in the same direction. First, in an environment characterized by cooperative norms the transaction costs required for sharing audit strategies, that are often complicated algorithms, and previously confidential information, will be lower. With lower transaction costs, there will be more effective transmission of information.

Second, as the existing literature shows, in an environment characterized by cooperative norms, taxpayers will be more likely to respond cooperatively to increased enforcement without engaging in further avoidance or evasion behaviors. For instance, Andreoni, Erard and

Feinstein (1998) argue that standard crime models applied to tax evasion may fail to capture reality when citizens have social norms that affect the way through which they perceive and respond to government policy. Frey (1997) argues that increasing extrinsic motivation, for instance with additional enforcement, may crowd out intrinsic motivation –paying taxes because individuals feel is the right thing to do. Similary, in Cullis and Lewis (1997) individuals do not care just about consumption, but also about their compliance with the social norms that they share with their community. Falkinger (1995) argues that if tax equity strengthens the social norm against evasion, then evasion becomes more costly in terms of bad conscience (if not caught) or bad reputation (if caught). Perez-Truglia and Troiano (2017) find that social capital is negatively correlated with how many tax delinquents there are in a given area. Very relevant for this setting, Ponzetto and Troiano (2014) find that social capital increase the extent to which people communicate amongst themselves about government policies. Casaburi and Troiano (2016) find that when social capital is higher voters are more likely to reward the incumbent that is cracking down on buildings not registered in the tax base registry.

For the purposes of this study, the aspect of social capital that is more of interest is that of cooperative gathering, because of the nature of the agreements I am studying.

Finding a proxy for cooperation that can suit this setting is not an easy task. The standard "trust in strangers" variable does not capture well the type of cooperative norm that is required to sign an agreement between two layers of government that interact often even before the agreement, even during jointly organized conferences. Additionally, the standard trust measure has not been collected, to the best of my knowledge, at the state level, in any years overlapping the period of the policy changes of interest, that happened between the 1950s and the 1970s.

In order to investigate whether there is an interaction between the tax compliance response to the exchange information agreement and income tax revenues I decide to use the civic and social organizations (per 1000 inhabitants) collected over the 1970s and the 1980s by the FICA Record.[4]

In Table 8 one can see how, as expected, the policy response is higher in states characterized by higher norms of cooperation. It should also be noted that the norms of cooperation are not randomly assigned, therefore, the results of this table should not be interpreted as causal. This problem is common when analyzing data abou norms and beliefs, which are

[4]The effects when adopting the more recently collected trust as indicator are not statistically significant, and are available upon request.

typically not randomly assigned. I view this evidence as suggesting that tax enforcement has higher scope when cooperative gathering is higher.

5 Robustness Tests

In this section I investigate whether the results are robust to: (1) acconting for a major confounder, the introduction of state income tax withholding, that in some states happened very close in time to the signing of the audit information exchange, and (2) pre-trends.

5.1 The Introduction of State Income Tax Withholding

State income tax withholding has been introduced in the United States throughout the second part of the twentieth century (Dušek, 2006), typically after the introduction of the state income tax (Dincecco and Troiano, 2015).

As opposed to taxpayers voluntarily filing their tax returns at the end of the fiscal year, tax withholding makes evasion more difficult by deducting immediately taxes from workers' salaries or collecting it directly from the employers. Therefore, one could think of tax withholding and tax auditing as substitutes. The more the income is subject to tax withholding, *ceteris paribus*, the less likely is that an audit would detect evasion.

In Table 9, I investigate whether the effect for the main tax outcomes of interest is robust to the introduction of state income tax withholding. The specification I adopt is that of equation (3), which is the more credible and approximates a randomized controlled trial. It is remarkable that the coefficient of revenues from income taxes remains stable, even if by itself tax withholding is increasing income tax revenues by more than 20 percent.

5.2 Pre-trends

In Figure 1 I investigate whether the main effects are robust to dynamic considerations, or pre-trends are driving the results.

As one can see, for the main outcome of interest, income from tax revenues, there is not a clear pre-trend before the treatment, while shortly after the treatment all the coefficients are positive and statistically significant, strongly suggesting that the treatment has affected the main outcome around the time period of interest.

For all the remaining outcomes there does not seem to be nor a clear pre-trend, nor a clear effect, suggesting that it is unlikely that confounding factors in tax reporting or tax revenues are driving the results in income tax revenue collection.

6 Conclusion

Improving the efficiency of tax collection is a key issue for fostering economic development. In this short paper I discussed the role of audit information exchange agreement, which are intergovernmental agreements between the state and the federal US government to exchange information about audit plans and techniques. I find that, under plausible assumptions, signing the agreement causally increases the revenue from the income tax by about 15 percent. The effects appear to be stronger in places where there are more civic associations. The paper remained agnostic about which specific auditing practices worked and which ones did not, mainly because of the secrecy of the details of the intergovernmetal interaction I am studying. Future reseach should unbundle the details of the specific auditing practices that work, and those that do not work.

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Table 1: Years of the Information Exchange Agreement

State	Information Exchange Agreement
Alabama	1970
Alaska	1967
Arizona	1966
California	1961
Colorado	1952
Connecticut	1970
Delaware	1965
Florida	1963
Georgia	1968
Hawaii	1965
Idaho	1964
Illinois	1963
Indiana	1961
Iowa	1962
Kansas	1960
Kentucky	1951
Louisiana	1971
Maine	1964
Maryland	1963
Massachusetts	1963
Michigan	1965
Minnesota	1957
Mississippi	1966
Missouri	1962
Montana	1951
Nebraska	1963
New Hampshire	1964
New Jersey	1966
New York	1963
North Carolina	1950
North Dakota	1964

Table 1: Introduction of State-Level Exchange Information Agreement, Continued

State	Information Exchange Agreement
Ohio	1961
Oklahoma	1963
Oregon	1961
Pennsylvania	1965
Rhode Island	1970
South Carolina	1964
Tennessee	1963
Utah	1961
Vermont	1965
Virginia	1963
West Virginia	1962
Wisconsin	1950

Source: Penniman (1980).

Notes: Montana discontinued the agreement in 1955.

Table 2: Summary Statistics

	Mean	Std. Dev.	Min.	Max.	Obs.
General					
Population (1000s)	4128.80	4725.24	49.00	36580.00	3527
Post Information Agreement	0.59	0.49	0.00	1.00	3544
Post Withholding	0.54	0.50	0.00	1.00	3544
Federal Revenues	1279.41	657.35	167.80	2663.10	3187
Federal Surplus	−150.40	177.92	−536.20	296.40	3187
Adjusted Gross Income	424.03	1044.54	0.10	13820.00	3544
N. of Tax Returns	13.95	20.76	0.04	177.60	3544
Tot. Income Tax	23.76	53.50	0.00	675.95	3379

Notes: Income Tax Revenues are 100000$ calculated in (1982-1984) US dollars. Federal variables are calculated in billions 2009 US dollars. Adjusted Gross Income and N. of Tax Returns are in 100000.

Table 3: Effects of Information Agreement on Population

	(1) State Population	(2) State Population	(3) State Population
Post Information Agreement	-0.0591	0.0117	0.00460
	(0.0989)	(0.0099)	(0.0079)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	No	Yes	Yes
State Effects Interacted with Federal Surplus and Revenues	No	No	Yes
Observations	3187	3187	3187
R^2	0.973	0.999	0.999

Standard errors in parentheses
Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state introduces an individual incom
Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1902–2008.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors in parentheses. Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state adopts the audit information exchange agreement. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1942–2008. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4: log Effects of Information Agreement on Income Tax Revenues

	(1) Income Tax Revenues	(2) Income Tax Revenues	(3) Income Tax Revenues
Post Information Agreement	0.200***	0.184**	0.148**
	(0.0665)	(0.0688)	(0.0589)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	No	Yes	Yes
State Effects Interacted with Federal Surplus and Revenues	No	No	Yes
Observations	2747	2747	2747
R^2	0.959	0.976	0.982

Standard errors in parentheses
Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state introduces an individual incom
Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1902–2008.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors in parentheses. Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state adopts the audit information exchange agreement. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1942–2008. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5: log Effects of Information Agreement on Total Revenues

	(1) Total Revenues	(2) Total Revenues	(3) Total Revenues
Post Information Agreement	-0.0154	0.0103	0.0139
	(0.0756)	(0.0258)	(0.0227)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	No	Yes	Yes
State Effects Interacted with Federal Surplus and Revenues	No	No	Yes
Observations	3187	3187	3187
R^2	0.982	0.994	0.995

Standard errors in parentheses
Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state introduces an individual incom
Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1902–2008.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors in parentheses. Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state adopts the audit information exchange agreement. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1942–2008. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 6: log Effects of Information Agreement on Adjusted Gross Income

	(1) logAGI	(2) logAGI	(3) logAGI
Post Information Agreement	-0.0296	0.0357*	0.0155
	(0.0949)	(0.0186)	(0.0138)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	No	Yes	Yes
State Effects Interacted with Federal Surplus and Revenues	No	No	Yes
Observations	3181	3181	3181
R^2	0.987	0.998	0.999

Standard errors in parentheses
Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state introduces an individual incom
Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1902–2008.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors in parentheses. Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state adopts the audit information exchange agreement. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1942–2008. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 7: log Effects of Information Agreement on Tax Revenues

	(1) Number of Tax Returns	(2) Number of Tax Returns	(3) Number of Tax Returns
Post Information Agreement	-0.0341	0.0111	0.00473
	(0.0999)	(0.0131)	(0.0102)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	No	Yes	Yes
State Effects Interacted with Federal Surplus and Revenues	No	No	Yes
Observations	3181	3181	3181
R^2	0.968	0.997	0.998

Standard errors in parentheses
Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state introduces an individual incom
Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1902–2008.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors in parentheses. Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state adopts the audit information exchange agreement. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1942–2008. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 8: Effects of Information Agreement on Income Tax Revenues - Heterogeneity

	(1) Income Tax Revenues	(2) Income Tax Revenues	(3) Income Tax Revenues
Post Information Agreement	0.227	-0.0557	-0.0332
	(0.1722)	(0.1064)	(0.0891)
Post Information Interacted with Civic and Social Organizations	-0.206	1.448**	1.069*
	(0.9446)	(0.6526)	(0.5805)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	No	Yes	Yes
State Effects Interacted with Federal Surplus and Revenues	No	No	Yes
Observations	2691	2691	2691
R^2	0.960	0.976	0.982

Notes: Standard errors in parentheses. Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state adopts the audit information exchange agreement. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1942–2008. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

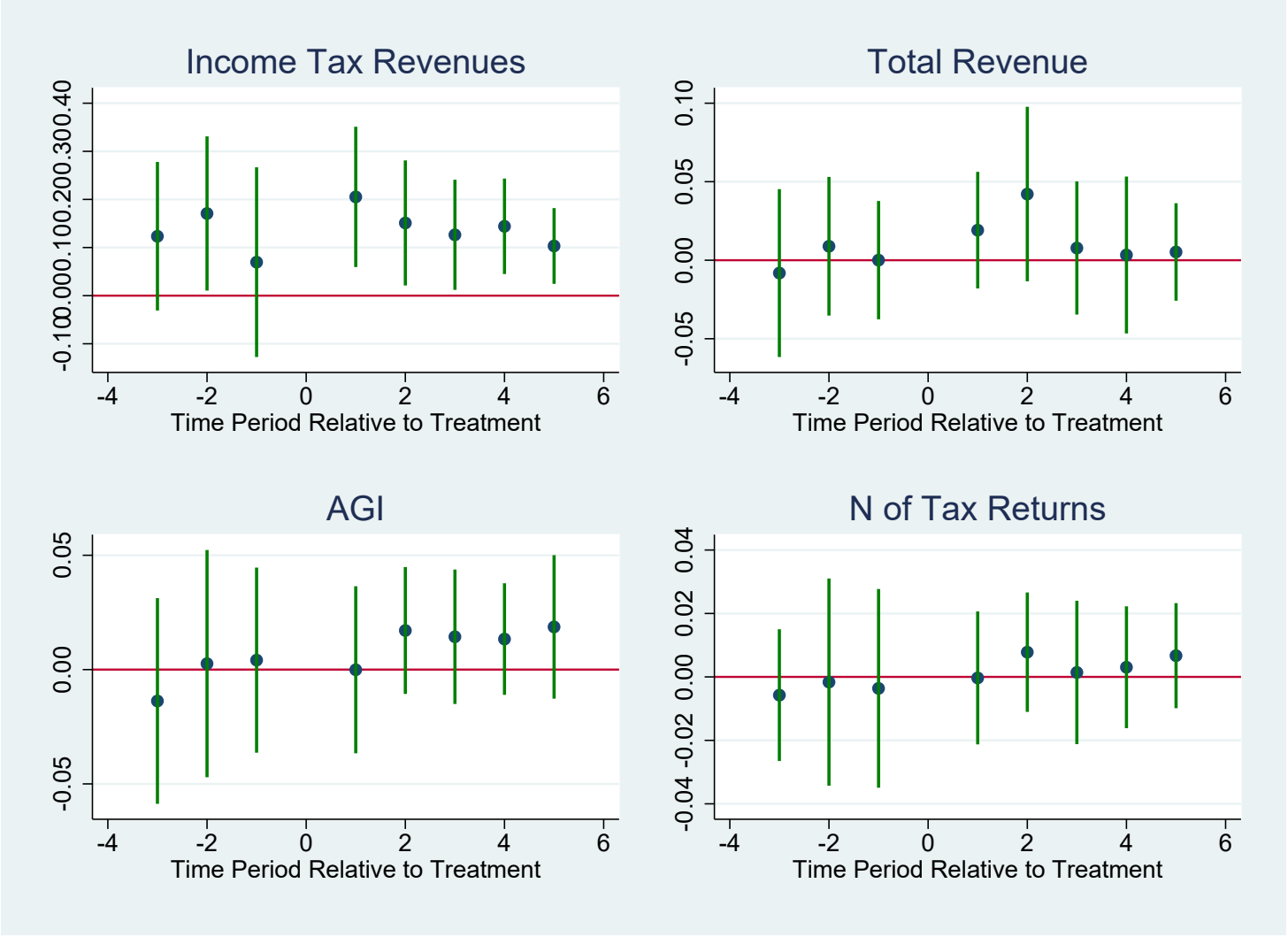
Table 9: log Effects of Information Agreement on Tax Revenues

	(1) Revenues from Income Tax	(2) Adjusted Gross Income	(3) Number of Tax Returns
Post Information Agreement	0.129**	0.0167	0.00615
	(0.0521)	(0.0139)	(0.0101)
Post Withholding	0.276***	-0.0115	-0.0136*
	(0.0559)	(0.0132)	(0.0077)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
State Effects Interacted with Federal Surplus and Revenues	Yes	Yes	Yes
Observations	2747	3181	3181
R^2	0.983	0.999	0.998

Standard errors in parentheses
Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state introduces an individual incom
Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1902–2008.
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Standard errors in parentheses. Estimates are obtained by OLS, using an indicator variable taking the value of 1 after a state adopts the audit information exchange agreement. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1942–2008. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Figure 1: Pretrends



Notes: The figures represent a pre-trends specification with lags and leads interacted with the main treatment of interest. Each period represent a two-year period. *AGI* stands for Adjusted Gross Income. *N of Tax Returns* stands for Number of Tax Returns.