Do Taxes Increase Economic Inequality? A Comparative Study Based on the State Income Tax*

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Abstract

I present new quasi-experimental evidence on the relationship between tax policies and the distribution of income. I focus on the twentieth century United States, and on the state income tax, since its inception. I study three major policy events that, as the existing literature shows, significantly raised the revenues from the income tax: the introduction of the state income tax, the introduction of tax withholding together with third-party reporting, and the intergovernmental agreements between the federal and state governments to coordinate tax auditing efforts. All the three policies were introduced in a staggered fashion and increased tax revenues, but had different fiscal consequences. Despite this, I find that income inequality raised after all the tax policy events. The result is robust to different measures of economic inequality and econometric specifications.

Keywords: Income inequality, tax policy, public economics, economic history.

JEL codes: D63, H23, N32.

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1 Introduction

The conventional wisdom is that taxes can help in reducing inequality and in redistributing income (Burman , 2015).[1] However, tax reforms are typically not randomly assigned in time and space, and little is known on whether and how taxes reduce inequality. This paper aims at contributing to fill this gap, and providing new quasi-experimental evidence about the relationship between taxes and inequality. I analyze three case studies in the twentieth-century United States, and in all three of them I find that taxes significantly increased economic inequality.

One of the main rationales for having taxes is to correct market failures, which include having an unequal distribution of income. Burman (2015) argues that the creation of the individual and corporate income taxes was largely motivated politically by "concerns about equity." Despite this, little is known about whether tax policies have a *causal* effect in reducing economic inequality.

The question is a hard one for at least three reasons. First, economic migration (Feldstein and Vaillant , 1994). People may just move out of the jurisdiction that is increasing taxes toward places with less taxation – and this is indeed what happened when US states introduced the income tax. Second, places that decide to increase taxes may be different than places with less taxes. For instance, their voters may care more about economic inequality, or may be more prone to fiscal discipline. Third, the timing of the tax policies reforms may not be as good as randomly assigned because the jurisdictions may decide to increase taxes when there are revenue needs. Additionally, reforming tax systems in order to reduce inequality may be related to political considerations and not be as good as randomly assigned (Leigh , 2005).

In this paper I consider three case studies taken from the United States history. All the case studies have in common to be about the state income tax.

The personal state income tax has steadily increased its importance as a revenue source for states, see Figure 1, despite the fact that taxes are losing their importance as revenue sources as opposed to transfers and other revenues, see Figure 2.

The three major reforms that I will analyze are: (1) the state income tax introduction, (2) the introduction of withholding, bundled with the introduction of third-party reporting, and (3) the intergovernmental agreement between the federal and the state governments for

[1] Feldstein and Vaillant (1994) argue that state taxes can not redistribute income if people migrate out of the jurisdiction as a response to the additional fiscal pressure.

coordinating auditing practices. The fiscal consequences of these reforms have been analyzed by three recent studies: Cassidy, Dincecco and Troiano (2017), Dušek and Bagchi (2017) and Troiano (2017), respectively. All of the studies find that the state revenues from the state income tax increased as a result of the policies.

However, for what matters the rest of the outcomes, the studies have different findings. For instance, while there has been economic migration following the introduction of the income tax, there not have been population responses following the other two policy events (Cassidy, Dincecco and Troiano, 2017, Dušek and Bagchi, 2017, Troiano , 2017). The introduction of the income tax was also accompanied by changes in other parts of the budget, such as expenditures in education, and even by an increase in educational outcomes, such as the number of public colleges in a given state (Cassidy, Dincecco and Troiano, 2017).

In this study I focus on the distributional aspect of these tax reforms. To the best of my knowledge, this is the first paper analyzing quasi-experimentally the distributional aspect of these tax reforms of the United States, and it complements existing papers about the relationship between inequality and taxation with analysis of data that do not suffer the limitation of few cross-sectional or time units, as it often happened in the past with studies about income inequality, as noted by Piketty and Saez (2003). Additionally, I complement existing studies (see for instance Duncan and Peter (2012), Paulus et al. (2009), Leigh (2005)) by focusing not on the consequences of tax rates on inequality, but on the effect of tax reforms on inequality.

Like the previous studies that focused on administrative data about inequality in the United States, it should be noted that I focus on pre-tax income inequality. While this is an important caveat to note, it should also be noted that only one of the three tax reforms that I study came as a bundle with increased tax rates. The other two tax reforms aimed at increasing tax compliance, without focusing on tax rates. Consistent with this point, Dušek and Bagchi (2017) find that the fact that the introduction of tax withholding increased tax revenues holds true controlling for tax rates.

In order to alleviate the aforementioned statistical concerns, I follow the methodology of the three previously cited studies in adopting a difference-in-differences identification strategy.[2] The main idea of the identification strategy is to compare economic inequality in the states that underwent the tax policy reforms and those that did not, before and after the policy change of interest.

[2]The differences between my study and theirs is in the fact that I focus on the distribution of income of these three reforms, and in the sample (mine is a balanced panel that goes from 1917 to 2015, while theirs is either unbalanced or covers a shorter time span).

The methodology has two main assumptions. The first one is that the treatment and control group have to be on parallel trends before the policy change. Absent this, one would not be able to ascribe the change in the series to the policy of interest, because of the confounding pre-existing difference. The second assumption is that there should not be other contemporaneous policy events affecting differentially the treatment and control group.

While checking whether the first assumption is verified is relatively straightforward, verifying the second assumption is trickier because it requires either institutional work to rule out contemporaneous policies, or augmenting the standard framework with additional assumptions. In this study I will verify both assumptions, sometimes with institutional work (this will be the case for the introduction of the income tax) or with additional investigation (for the introduction of withholding and the audit information exchange agreement).

My main finding is the following: each of the major studied state income tax reforms of the twentieth US increased the states' economic pre-tax inequality, regardless of the inequality measure used. For instance, in a difference-in-differences specification that controls also for state specific linear trends, introducing the income tax raised the percent of income held by the top 1 percent of the population by about 1 percentage point, 7.6 percent of its sample mean; introducing third party reporting and tax withholding raised the percent of income held by the top 1 percent of the population by 0.8 percentage points, about 6 percent of its sample mean; the audit information exchange agreement between the federal and the state governments raised the percent of income held by the top 1 percent of the population by 0.65 percentage points, which is about 5 percent of its sample mean.[3]

An important caveat that should be noted and that will be discussed in more detail later is that the inequality measures used in this paper derive from the IRS administrative data. While this has obvious advantages of data reliability, it comes with the cost that the data derive just from the filers of the income tax, which were relatively few before 1944. Therefore, the results of the introduction of the income tax, which is the only policy reform for which data before 1944 are relevant, should be interpreted keeping this important caveat in mind. It is however re-assuring that the results for the introduction of the income tax are qualitatively similar to those of the other two reforms. In the Appendix I present the results of the effects of the introduction of withholding and of the exchange information agreements on the inequality measures excluding the data before 1944.[4]

[3]I always control for state specific linear trends because the inequality measures are relatively predictable outcomes.

[4]I can not do a similar exercise for the introduction of the income tax given that the treatment happens before

This paper fits in the literature about the economic consequences of tax reforms for income. The literature about behavioral responses to taxation is reviewed by Saez, Slemrod and Giertz (2012). In a world without externalities, identifying the elasticity of taxable income with respect to tax reforms is sufficent to conduct welfare analaysis. Empirically, there is strong evidence that people respond to taxation, for instance Saez (2016) finds that the 2013 tax increase of the top marginal rate had a large negative effect on reported income at the top. People respond to taxation not only in terms of reported income but also labor supply, mobility, fertility and other important margins. Additionally, it is possible that not only the people that are statutorily targeted by a tax reform are the ones responding via the reported income. This paper contributes to this literature by showing that, the tax policy reforms of the twentieth century US raised economic inequality.

The paper contributes also to the scientific literature focused specifically on economic inequality, which in the last years has been extremely active. The authors in this literature are undertaking two directions. The first one is studying income inequality (Golding and Margo , 1992, Feenberg and Poterba , 1993, 2000, Frank , 2009, Atkinson, Piketty and Saez , 2011). The second one is studing wealth inequality (Saez and Zucman , 2016, Johannesen and Zucman, 2014, Alstadsaeter, Johannesen and Zucman , 2017).

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

2 Institutional Framework and Data

In this section I describe the institutional framework and the data used in the analysis. There are two sets of data. The first one is composed of the inequality measures used in the analysis, and the second one is about the tax policies I am focusing on. The variables are summarized in Table 1.

2.1 State Tax Policies

The setting of this study is the twentieth century United States. Throughout that century, three big events have happened related to the state income tax (Penniman , 1980): its in-

1944 for a great part of the states.

ception, the introduction of tax withholding (bundled with third-party reporting), and the intergovernmental audit exchange agreements between the federal and state governments. This sub-section borrows heavily from (Penniman , 1980). The timing of the policy reforms is summarized in Table 2.

The first modern income state tax was enacted by Wisconsin in 1911, "at the height of his political progressivism" (Penniman , 1980). Before 1920, Delaware, Massachussetts, Mississippi, Missouri, New York and North Dakota followed. Despite these early adopters, there is a substantial body of evidence, summarized by Penniman (1980), that there was a lot of political opposition to state income taxes. She writes:

> Yet, in a majority of the states, it took decades to win acceptance of the income tax as a policy choice and to develop individual and corporate income taxes somewhat near their potential for raising revenue.

The institutional work undertaken by Clara Penniman clearly shows that the specific *year* of adoption of the state income tax was not only due exclusively to considerations about revenue needs that instead could have been fulfilled through other means, such as fees and tariffs, but was also due to state-specific political considerations. Therefore, it is unlikely that there is a policy systematically correlated with the adoption of the income tax at the state level and that could bias the results. To further corroborate this point, Cassidy, Dincecco and Troiano (2017) consider for a number of states (the ones for which data could be collected) how close the legislature vote that introduced the state income tax was, and show that the revenue increase following the income tax introduction is not too different in states where the vote was close from those where the vote was not close.

Tax withholding was introduced together with third-party reporting in the United States (Dušek and Bagchi, 2017). The possibility of collecting taxes directly at the source seemed very appealing for policymakers, so as to avoid the work of identifying many non-filers, and, among the reasons officially cited for this reform, there was equity in tax administration (Penniman , 1980). Oregon was the first state to adopt withholding for wages and salaries (but not for other income) in 1948. By the early 1970s, almost all the income tax states had adopted withholding. Therefore withholding is not only an innovation on how to collect income, but it was partly also a tax enforcement technique, and a mean of "automatic tax budgeting" for the taxpayer. Penniman (1980) writes that the timing of adoption in each state varied with fiscal need, with the extent of personal exemptions and with state tax rates.

The third policy is the information exchange audit agreements. Those are voluntarily signed agreements between each state and the federal government, and that involve exchange of information on audit plans and techniques.[5] The first test of this policy was authorized in 1950 for the states of North Carolina and Wisconsin. What happened during the first period of the policy is that one group of agents for each governmental unit (one for the government, one for the state) prepared reports about the audited returns showing a deficiency in tax, and mutually exchanged these pieces of information about those returns. Later on, during the 1970s, cooperative auditing developed, also thanks to technological innovation, and the program arrived to the point that in some states the federal government and the states divided ex-ante the work, with the state having the task of auditing the smaller returns, while the federal government covering the bigger returns.

2.2 Inequality Measures

The baseline inequality measures that I use are the shares of income held by the top 10, 5, 1, 0.5, 0.1 and 0.01 percent of the filers. The data come from Frank et al. (2015). The advantage of these measures is that they have a clear interpretation. In the "Robustness tests" section, I will report the results for the rest of inequality measures (the Atkinson index, the Gini coefficient, the Relative Mean Deviation and the Theil index). The advantage of those measures, is assessing the inequality over the entire income distribution, as opposed to focus only on what happens at the top or at the very top of the income distribution.

All of these inequality measures are constructed from administrative data provided by the IRS to researchers, the *Statistics of Income*, by state-year and by size of the adjusted gross income in the individual tax returns. The pre-tax adjusted gross income in individual tax returns include wage and salaries, capital income and entrepreneurial income, and it does not include interest on state and local bonds, and transfer income from federal and state governments.

The main drawback of using inequality measures that include the full distribution of income with IRS data is that the data are truncated at the low-end of the income distribution because of the non filers. Therefore, I choose as baseline measures the shares. The problem of the non-filers is more pervasive before the 1944 (Piketty and Saez , 2003, Frank , 2014).

[5] On tax enforcement and the relationship between different layers of government see also Casaburi and Troiano (2016)

I report the results with four additional measures of income inequality. The Atkinson index is a social welfare function based measured of inequality and it is bounded between 0 and 1, with an inequality aversion parameter of 0.5, meaning that the function is more sensitive to what happens in the upper end of the income distribution. The Gini index varies between 0 and 1 and tends to be sensitive to what happens in the middle of the income distribution. The Relative Mean Deviation is bounded between 0 and 2, and represents the normalized average distance between each person's income and the mean income of the population. The Theil index is an unbounded measure of income inequality derived from statistical information theory and is defined as the maximum possible entropy of the data minus the observed entropy. All of the four measures are increasing in economic inequality.

Frank (2014) observes how over the last century the Theil index appears to closely follow the trend of the 1 percent of the income distribution, while the other three measures have an important difference with the shares of the income distribution: the decrease in inequality after the Great Depression and the Second World War is "not as precipitous."

3 Empirical Strategy

In this section, the empirical strategy is outlined. Following Cassidy, Dincecco and Troiano (2017), Dušek and Bagchi (2017) and Troiano (2017), I implement a difference-in-differences approach based on the staggered introduction of the tax reforms. My aforementioned contribution is analyzing quasi-experimentally the effect of these tax policies on the distribution of income. The baseline specification is therefore:

$$D_{it} = \beta_0 + \beta_1 Post_{it} + \phi_i + \phi_t + \phi_i * T_t + \epsilon_{it} \tag{1}$$

The dependent variables D are the inequality measures (the shares, the Atkinson index, Gini coefficient, Relative mean deviation and Theil index) in state i and year t. The dummy *Post* is equal to one after one of the policy reforms of interest. The state and year fixed effects, ϕ_i and ϕ_t, control respectively for place-specific and time-specific shocks, and $\phi_i * T_t$ control for state specific linear trends. The standard errors, ϵ_{it}, are robust to heteroskedasticity and clustered at the state level. The coefficient of interest β_1 captures the causal effect of the tax reform on the distribution of income under plausible assumptions.

I focus on the shares as inequality measures because they are less sensitive to the pre-

1944 non-filers problem. However, the other measures, after 1944, plausibly capture the full distribution of income, rather than just what happens at the very top. The dataset goes from 1917 to 2015.

One potential challenge to the identification is that the adoption of the policy changes may be correlated with past trends in the distribution of income, or other relevant variables such as fiscal policy. If this were the case, for instance because the policy reforms are undertaken when the revenue requirements are higher, the identifying assumptions would be violated. The plausibility of this important concern can be verifiyed in this setting. If the tax reforms are exogenous to past policies, one would expect that the pre-trends in policies in the treated and control states are parallel before the change, and diverge only *after* the introduction of the income tax. The assumptions are verified below.

I verify the pre-trend assumption by first estimating the following equation:

$$D_{it} = \beta_0 + \sum_{l=-k}^{k} \beta_l 1[t \in A_l^i] + \phi_i + \phi_t + \phi_i * T_t + \epsilon_{it} \tag{2}$$

where A_l^i denote the time period relative to the introduction of the policy of interest (for instance, if $k = 1$, A_1^i is the first period after the introduction of the policy of interest). I then plot the coefficients and the standard errors relative to the introduction of the policy period of interest (this is therefore a standard dynamic difference-in-differences specification).

Another challenge to the identification is the possibility that the timing of adoption of the policies may be *systematically* correlated with other policies. This is an important factor and the way to deal with it will vary from case to case. First, for the introduction of the income tax, Penniman (1980) argued institutionally that the reasons were largely idiosyncratic and political, and that states had other ways to raise revenues. Cassidy, Dincecco and Troiano (2017) conduct an additional robustness check. They look at how close the vote to introduce the income tax was, and they split the sample in places where the vote was close, and places where the vote was not close. They show that the increase in tax revenues is statistically similar in those place, providing suggestive evidence that Penniman's claims are right. For the other two policy reforms, I provide additional robustness tests, described below in the results section.

4 Main Results

In this section, I investigate quasi-experimentally the consequences of the tax reforms on the inequality measures. For each inequality measure, I present the baseline specification, a standard difference-in-differences with linear time trends. I present the effect of the three policy reforms in the following order: introduction of the income tax, introduction of withholding and audit information exchange agreement.

In Tables 3 to 8, I present the results on the main outcomes, the shares of income earned by top earners. The measures that are most significant, both statistically and economically, are those with a smaller fraction of rich people. For instance, after the introduction of the income tax and of withholding the fraction of income at the top 0.01 percent of people increases respectively of 0.277 and 0.415 out of a baseline mean of 1.91, both significant at the 1 percent level.

The fraction of income held by the top 0.5 percent of contributors increases by about 0.85 percentage points after the introduction of the income tax, by about 0.75 percentage points after the introduction of withholding, and by about 0.5 percentage points after the audit exchange information agreements, out of a baseline mean of 9.75.

The fraction of income held by the top 1 percent of contributors increases by about 1 percentage point after the introduction of the income tax, by about 0.8 percentage points after the introduction of withholding, and by about 0.65 percentage points after the audit exchange information agreements, out of a baseline mean of 13.1.

5 Robustness Tests

In this section I will cover the robustness tests for the main analysis. This section is divided into three parts. In the first part I show that my results are robust if I use alternative inequality measures. In the second part I discuss the assumption of absence of contemporaneous events affecting differentially the treatment and the control group. In the third part I present the pre-trends graphs for the main outcomes.

5.1 Additional Inequality Measures

In Table 9 one can see that the introduction of the income tax raised the Atkinson inequality index by 0.015, which is about 7 percent of the sample mean, statistically significant at the 1 percent level. Introducing the withhholding raised the Atkinson index by about 6 percent, statistically significant at the 5 percent level, and the audit exchange agreement raised it by about 4 percent, statistically significant at the 1 percent level. In Table 10 the Gini coefficient is presented. The income tax introduction raised the Gini coefficient by 0.014, which is about 3 percent, significant at the 5 percent level. The introduction of withholding raised the Gini coefficient by about 1 percent, but this is not significant at standard levels.

In Table 11 we move to consider the Relative Mean Deviation. The introduction of the income tax raised the Relative Mean Deviation by 0.027, which is about 3 percent. The magnitude of the effects for the withholding and the audit exchange are about half that size, and border line insignificant at standard levels.

In Table 12 we consider the Theil index, which is the most similar to the top 1 percent of the income distribution. All of the three reforms raised the Theil index in a statistically significant way, at least at the 5 percent level. The introduction of the income tax and of the withholding raised it by about 0.06, which is about one fourth of a standard deviation or one eight of the sample mean.

5.2 Contemporaneous events

One of the fundamental assumptions of the difference-in-differences is that there are no other contemporaneous events that affect differentially the treatment and control group. While the assumption is not testable, it is surely perfectly respected only in a randomized evaluation. However, I aim at persuading the reader that the concerns in this setting are limited. First, for what matters the introduction of the income tax, as already mentioned in the Institutional Framework section, there are institutional reasons to believe that there were no other events that regularly associated themselves with that policy and that affected differentially the treatment and control group. In order to alleviate even more those concerns, in Appendix Tables 18 and Table 19 I show that the results are not statistically different in the sub-sample where the votes that approved the individual income tax was not close. The main rationale of that test is the following: omitted variable bias is likely to be higher in states where the vote of approval of the state income tax was not close. Therefore, it is re-assuring that there are no

differences among those places and the rest of the places, alleviating the concerns of omitted variable bias, consistent with what Cassidy, Dincecco and Troiano (2017) find for the public finance variables with a similar empirical exercise.

Second, for what matters the introduction of withholding, that happened bundled with third party reporting, I show that the results are robust when controlling for the audit exchange agreements, which is the other major policy reforms approved at the same time and that relates to the state income tax. Therefore, I show in Table 13, Table 14 and Table 15 that the results are robust when this variable is included. The coefficients are statistically similar to those that control for the two reforms separately, although the statistical significance on the audit information exchange agreements gets reduced (or gets insignificant) in some of these specifications.

Third, for what matters the audit exchange agreements, I follow Troiano (2017) and instrument the timing of the adoption of the agreements with the interaction between state fixed effects and federal revenues. The main idea of the identification is that federal revenues will matter for the timing of the agreement (because one side of the two parties is the federal government), federal revenues-needs depend mostly on aggregate events and are exogenous to state specific considerations, and that changes in federal bargaining power will affect each state in a idiosyncratic way. In Table 16 and Table 17 I show that the results are robust to this exercise. The IV coefficients are larger (and more likely to be statistically significant) than the OLS coefficients. This, however, is relatively common (see, for example, the return on schooling literature, reviewed in Card (2001)) and can be explained by the fact that the use of an IV approach may reduce the downward attenuation bias due to measurement error or by the fact that in the set of states from which the IV estimates are identified-that is, states that sign an agreement because of federal revenue needs- the effect of the information agreements on inequality is greater than in the set of all states used for the OLS estimates.

5.3 Pre-trends

In Figures 3 to 8, I show that the pre-trends are not driving the results described in the previous Tables, and pre-trends are arguably parallel before the policy changes. From looking at the graphs, one can see how those variables don't exhibit statistically significant pre-trends, and there are reasonably significant jumps after the main treatments of interest. The timing of the response somewhat differs among the studied reforms. For what matters the introduction

of the income tax, inequality seems to decrease during the first three-four time periods, and it tends to increase afterwards. This, as I will note in the next subsection, will be potentially important to uncover the mechanisms behind the response. For what matters the other two reforms, the introduction of withholding and the audit exchange agreements, the pattern of the response is more standard, and it shows significant increase of inequality immediately after the policy change of interest.

In Figures 9, Figure 10 and Figure 11, I investigate whether the effects of the additional inequality measures are robust to dynamic considerations, or the pre-trends are driving the results. The pattern is similar to the one described in the other reforms.

5.4 Interpretation, Comparative Analysis and Potential Mechanisms

In this subsection I present my interpretation of the results and some of the potential mechanisms that may be at play, based on comparative analysis of the papers that analyzed the fiscal consequences of these reforms (Cassidy, Dincecco and Troiano, 2017, Dušek and Bagchi, 2017, Troiano , 2017). While I am not able to pin down the exact mechanisms explaining the results, comparative analysis allows me to be able to identify some of the potential mechanisms that may be at play and rule out some others.

The timing of the response of the inequality measures to the introduction of the income tax identified by the graphical evidence seems to suggest that inequality decreased immediately after the reform of the introduction of the income tax, and increased after some years to the introduction of the income tax. The timing of the response is remarkably consistent with the government expenditures responses (in particular, educational expenditures) and educational outcomes (in particular, number of four-year public colleges) identified by Cassidy, Dincecco and Troiano (2017). Therefore, one of the potential mechanisms at play for the result that the introduction of the income tax increases economic inequality is the role of government expenditures. While revenue policies may be progressive at first, government expenditures in tertiary education could have a regressive role. The fact that tertiary education was mainly undertaken by rich people at the time is consistent with this observation.

The timing of the response of the inequality measures to the introduction of withholding and to the information exchange agreements is similar, and immediate. Statistical significance began to show from the first time period after the reforms, hence, the mechanism could plausibly be related to the additional enforcement following those policies. If the rich people were

more sensitive to enforcement policies compared to the middle class or poor, additional enforcement would translate itself into higher income inequality (Alstadsaeter, Johannesen and Zucman , 2017).

Another explanation I can not fully rule out includes a set of adjustments in the labor market. The data and the setting do not allow me to disentangle if such adjustments take place on the side of labor demand or supply. It is however re-assuring that such labor market explanations are plausibly inconsistent with the observed timing of the inequality response to tax reforms, which took place either immediately (for what matters the introduction of withholding and audit exchange agreements) or in parallel with the educational expenditures responses (for what matters for the introduction of the income tax). Additionally, for the labor market explanation to be compelling, one would need to observe a comparative decrease of the richest contributors labor supply and/or out-mobility of the richest contributors. I do not observe any mobility response, nor any reduction in the total reported adjusted gross income following the information exchange agreements (Troiano , 2017).

There are two other explanations that receive less support from the data.

The first alternative explanation is economic migration (Feldstein and Vaillant , 1994). There is a growing body of empirical evidence according to which rich people move after economic shocks (Akgit, Baslandze and Stantcheva, 2015, Kleven, Landais and Saez , 2013, Liebig, Puhani and Sousa-Poza, 2006, Moretti and Wilson, 2013). If mobility were explaining these results, the econometrician would have found that people move not only after the introduction of the income tax, which is the only policy reform that affected mobility. All of the other policy reforms did not affect economic migration. Additionally, the rich are the people who are expected to move out of a jurisdiction that is making its tax system stronger: hence, this mechanism would reduce, rather than increase economic inequality.

The second alternative explanation is that those reforms have affected only reporting, by inducing a shift of reported income from personal to business (Gordon and Slemrod , 2000). I argue that this possibility can't explain the findings that people moved as a response to the introduction of the income tax, and that the same reform affected spending on education and access to colleges. Additionally, this mechanism would produce a decrease in income inequality, rather than an increase.

6 Conclusion

In this paper I presented new quasi-experimental evidence about the relationship between tax policies and income inequality in the United States throughout the twentieth century.

By exploiting the staggered introduction of three major policy reforms (the introduction of the income tax, the introduction of third-party reporting together with withholding, and the intergovernmental agreements between the states and the federal government to reduce evasion) and adopting a difference-in-difference identification strategy, I find that all the considered tax policy reforms raised economic inequality, instead of lowering it.

The results of this paper raise several avenues for future research. For instance, are there tax policies that can help reduce income inequality? How context-dependent are the results about this paper? Would the results be different if the unit of observations were countries rather than states? What was the response of post-tax income inequality to the reforms I studied? Future research should investigate these questions.

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Table 1: Summary Statistics

	Mean	Std. Dev.	Min.	Max.	Obs.
Inequality Measures					
Atkinson Index	0.20	0.06	0.05	0.51	4965
Gini Coefficient	0.49	0.08	0.23	0.75	4965
Relative Mean Deviation	0.68	0.12	0.31	1.22	4965
Theil Index	0.52	0.23	0.12	2.00	4965
Income Share Top 10 percent	37.43	6.44	18.02	69.54	4965
Income Share Top 5 percent	26.49	5.83	11.29	59.11	4965
Income Share Top 1 percent	13.10	4.58	3.76	46.18	4965
Income Share Top 0.5 percent	9.75	4.04	2.36	40.58	4965
Income Share Top 0.1 percent	4.98	2.92	0.76	27.59	4965
Income Share Top 0.01 percent	1.90	1.58	0.12	15.15	4965
Policy Changes					
Post Income Tax Introduction	0.69	0.46	0.00	1.00	4965
Post Withholding Introduction	0.44	0.50	0.00	1.00	4965
Post Information Exchange Agreement	0.48	0.50	0.00	1.00	4965

Notes: Sample covers years 1917–2015. .

Table 2: Policy Changes

	Income Tax Introduction	Withholding Introduction	Information Exchange Audit Agreement
Alabama	1933	1956	1970
Alaska	1959	1959	1967
Arizona	1933	1954	1966
Arkansas	1929	1956	1963
California	1935	1971	1961
Colorado	1937	1954	1952
Connecticut	1969		1970
Delaware	1917	1949	1965
Georgia	1929	1960	1968
Hawaii	1959	1959	1965
Idaho	1931	1955	1964
Illinois	1969	1969	1963
Indiana	1963	1963	1961
Iowa	1934	1966	1962
Kansas	1933	1966	1960
Kentucky	1936	1954	1951
Louisiana	1934	1961	1971
Maine	1969	1969	1964
Maryland	1937	1955	1963
Massachusetts	1916	1959	1963
Michigan	1967	1967	1965
Minnesota	1933	1961	1957
Mississippi	1912	1968	1966
Missouri	1917	1961	1962
Montana	1933	1955	1951
Nebraska	1967	1967	1963
New Jersey	1976	1976	1966
New Mexico	1933	1961	1963
New York	1919	1959	1963
North Carolina	1921	1959	1950
North Dakota	1919		1964
Ohio	1971	1971	1961
Oklahoma	1915	1961	1963
Oregon	1930	1948	1961
Pennsylvania	1971	1971	1965
Rhode Island	1971	1971	1970
South Carolina	1922	1959	1964
Tennessee	1931		1963
Utah	1931	1959	1961
Vermont	1931	1951	1965
Virginia	1961	1963	1963
West Virginia	1961	1961	1962
Wisconsin	1911	1962	1950

Table 3: Effects of Policy Changes on Inequality

	(1) Income Share Top 0.01 percent	(2) Income Share Top 0.01 percent	(3) Income Share Top 0.01 percent
Post Individual Income Tax	0.277***		
	(0.0954)		
Post Withholding		0.415***	
		(0.1546)	
Post Information Agreement			0.182
			(0.1532)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
Observations	4965	4965	4965
R^2	0.817	0.818	0.816

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 4: Effects of Policy Changes on Inequality

	(1) Income Share Top 0.1 percent	(2) Income Share Top 0.1 percent	(3) Income Share Top 0.1 percent
Post Individual Income Tax	0.536***		
	(0.1975)		
Post Withholding		0.660***	
		(0.2387)	
Post Information Agreement			0.342
			(0.2370)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
Observations	4965	4965	4965
R^2	0.852	0.852	0.851

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 5: Effects of Policy Changes on Inequality

	(1) Income Share Top 0.5 percent	(2) Income Share Top 0.5 percent	(3) Income Share Top 0.5 percent
Post Individual Income Tax	0.844**		
	(0.3249)		
Post Withholding		0.746**	
		(0.2988)	
Post Information Agreement			0.497*
			(0.2912)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
Observations	4965	4965	4965
R^2	0.863	0.862	0.861

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 6: Effects of Policy Changes on Inequality

	(1) Income Share Top 1 percent	(2) Income Share Top 1 percent	(3) Income Share Top 1 percent
Post Individual Income Tax	0.984**		
	(0.3927)		
Post Withholding		0.794**	
		(0.3518)	
Post Information Agreement			0.646**
			(0.3142)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
Observations	4965	4965	4965
R^2	0.867	0.866	0.866

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 7: Effects of Policy Changes on Inequality

	(1) Income Share Top 5 percent	(2) Income Share Top 5 percent	(3) Income Share Top 5 percent
Post Individual Income Tax	1.182*		
	(0.6221)		
Post Withholding		0.617	
		(0.4904)	
Post Information Agreement			0.500
			(0.4337)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
Observations	4965	4965	4965
R^2	0.846	0.845	0.844

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 8: Effects of Policy Changes on Inequality

	(1) Income Share Top 10 percent	(2) Income Share Top 10 percent	(3) Income Share Top 10 percent
Post Individual Income Tax	1.663*		
	(0.9218)		
Post Withholding		0.424	
		(0.7171)	
Post Information Agreement			0.467
			(0.6960)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
Observations	4965	4965	4965
R^2	0.801	0.798	0.798

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 9: Effects of Policy Changes on Inequality Measures

	(1) Atkinson Index	(2) Atkinson Index	(3) Atkinson Index
Post Income Tax Introduction	0.0149***		
	(0.0051)		
Post Withholding Introduction		0.0114**	
		(0.0044)	
Post Information Exchange Agreement			0.00821*
			(0.0041)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
Observations	4965	4965	4965
R^2	0.881	0.880	0.879

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 10: Effects of Policy Changes on Inequality Measures

	(1) Gini Coefficient	(2) Gini Coefficient	(3) Gini Coefficient
Post Income Tax Introduction	0.0143**		
	(0.0061)		
Post Withholding Introduction		0.00891*	
		(0.0052)	
Post Information Exchange Agreement			0.00534
			(0.0048)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
Observations	4965	4965	4965
R^2	0.907	0.906	0.906

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 11: Effects of Policy Changes on Inequality Measures

	(1) Relative Mean Deviation	(2) Relative Mean Deviation	(3) Relative Mean Deviation
Post Income Tax Introduction	0.0276**		
	(0.0110)		
Post Withholding Introduction		0.0155	
		(0.0104)	
Post Information Exchange Agreement			0.0134
			(0.0081)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
Observations	4965	4965	4965
R^2	0.883	0.881	0.881

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 12: Effects of Policy Changes on Inequality Measures

	(1) Theil Index	(2) Theil Index	(3) Theil Index
Post Income Tax Introduction	0.0596***		
	(0.0191)		
Post Withholding Introduction		0.0536***	
		(0.0165)	
Post Information Exchange Agreement			0.0358**
			(0.0168)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
Observations	4965	4965	4965
R^2	0.867	0.866	0.865

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 13: Effects of Policy Changes on Inequality Measures

	(1) Income Share Top 0.01 percent	(2) Income Share Top 0.1 percent
Post Withholding	0.398***	0.624***
	(0.1477)	(0.2286)
Post Information Agreement	0.0976	0.210
	(0.1399)	(0.2169)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	4965	4965
R^2	0.818	0.852

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 14: Effects of Policy Changes on Inequality Measures

	(1) Income Share Top 0.5 percent	(2) Income Share Top 1 percent
Post Withholding	0.686**	0.710**
	(0.2766)	(0.3289)
Post Information Agreement	0.352	0.496*
	(0.2550)	(0.2752)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	4965	4965
R^2	0.862	0.867

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 15: Effects of Policy Changes on Inequality Measures

	(1) Income Share Top 5 percent	(2) Income Share Top 10 percent
Post Withholding	0.552	0.358
	(0.4581)	(0.6748)
Post Information Agreement	0.383	0.391
	(0.3856)	(0.6400)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	4965	4965
R^2	0.845	0.798

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Table 16: Effects of Policy Changes on Inequality Measures

	(1) Income Share Top 0.01 percent	(2) Income Share Top 0.1 percent
Post Information Agreement	2.290**	3.621**
	(1.0769)	(1.5449)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	4965	4965
R^2	0.772	0.820

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by Instrumental Variable Estimation. *Post Information Agreement* is instrumented with the interaction between federal revenues and state fixed effects. F-statistic on the first-stage is 10.55. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

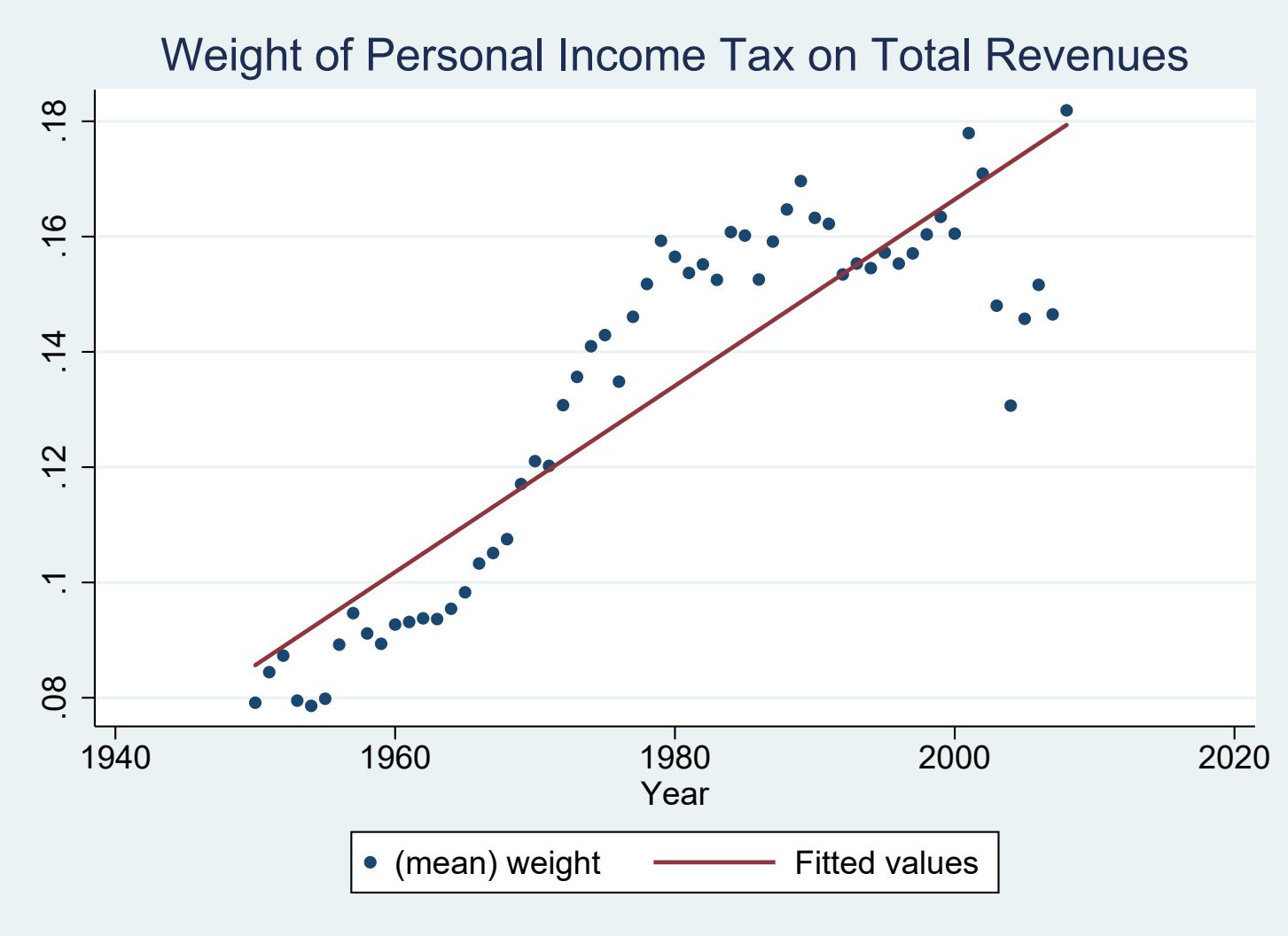
Table 17: Effects of Policy Changes on Inequality Measures

	(1) Income Share Top 0.5 percent	(2) Income Share Top 1 percent
Post Information Agreement	4.604**	5.138**
	(1.9786)	(2.1453)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	4965	4965
R^2	0.836	0.842

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

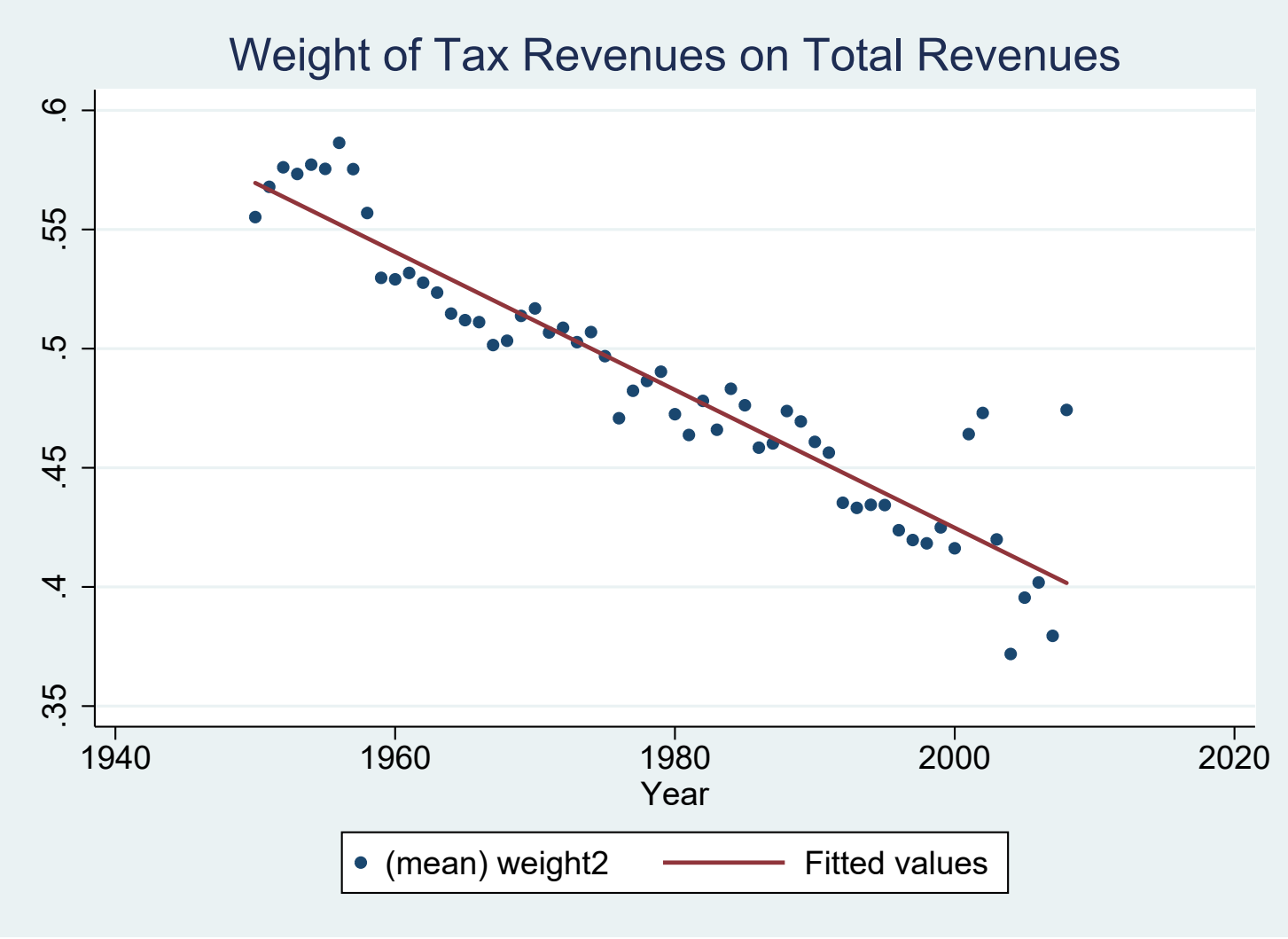
Notes: Estimates are obtained by Instrumental Variable Estimation. *Post Information Agreement* is instrumented with the interaction between federal revenues and state fixed effects. F-statistic on the first-stage is 10.55. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1917–2015.

Figure 1: The Importance of State Personal Income Tax Revenues for State Total Revenues



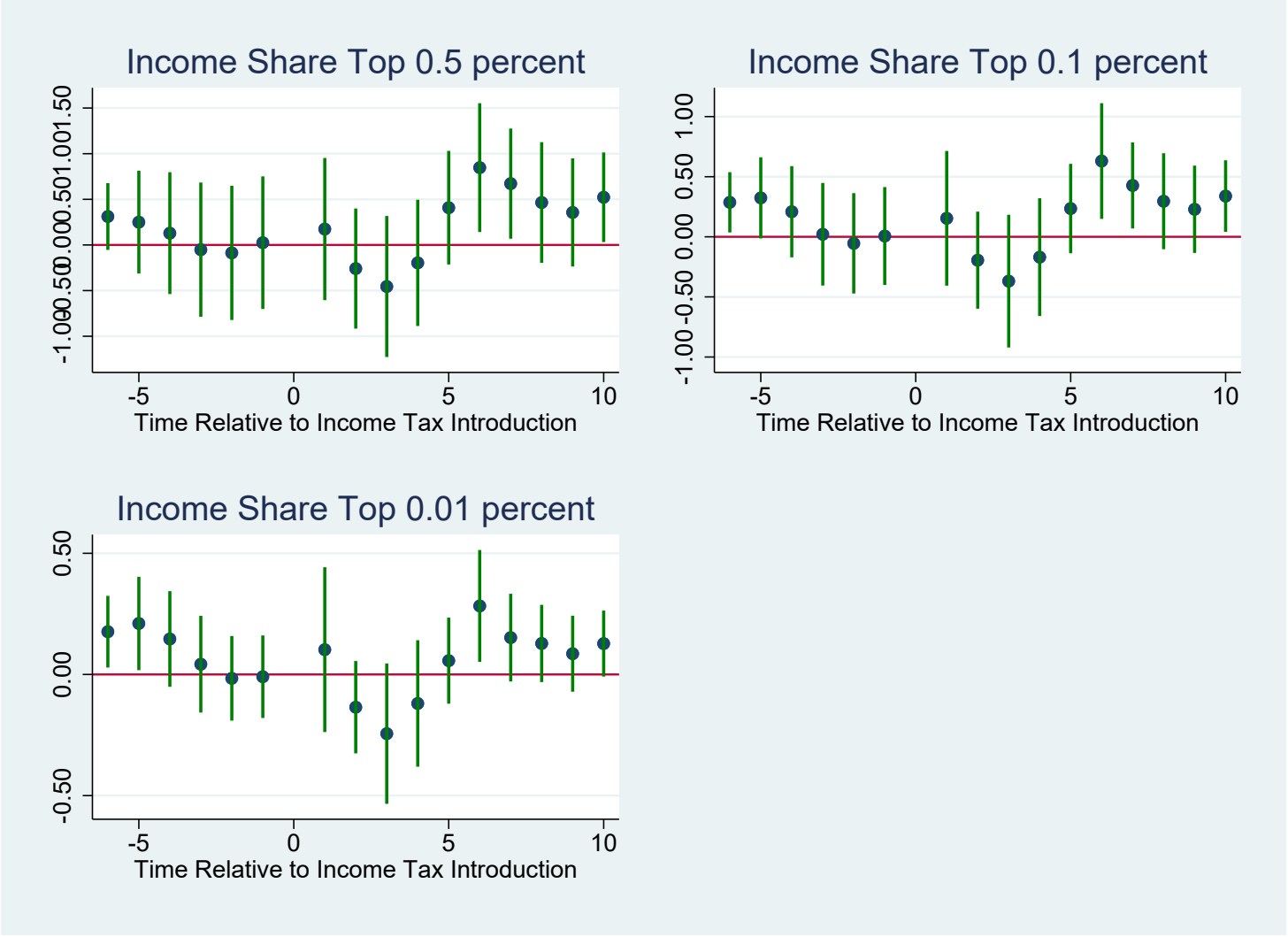
Notes: Sample covers years 1948–2015.

Figure 2: The Importance of Total State Tax Revenues for Total State Revenues



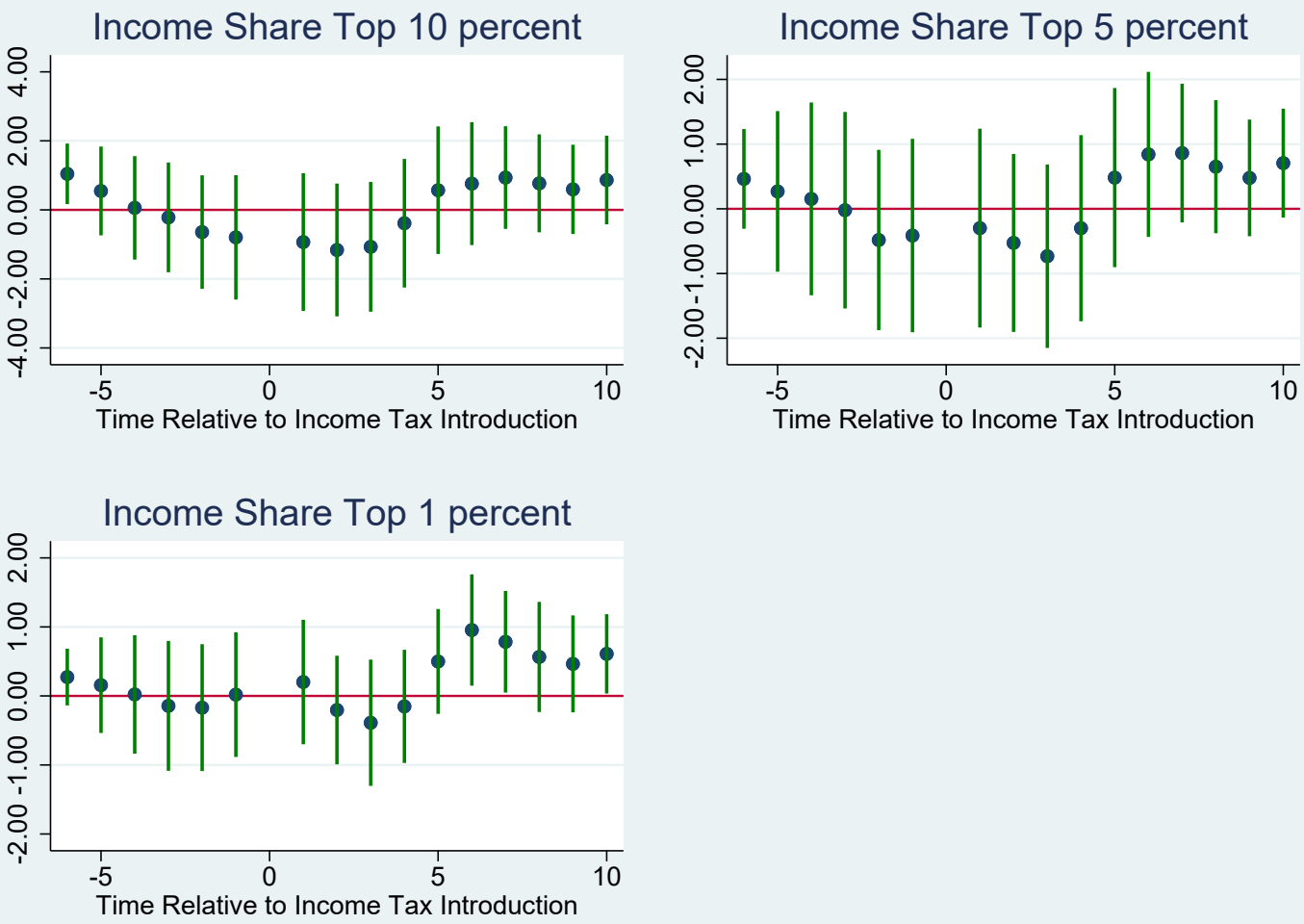
Notes: Sample covers years 1948–2015.

Figure 3: Inequality Pre-trends



Notes: Sample covers years 1917–2015. Pre-trends equation estimated as in equation (2).

Figure 4: Inequality Pre-trends



Notes: Sample covers years 1917–2015. Pre-trends equation estimated as in equation (2).

Figure 5: Inequality Pre-trends



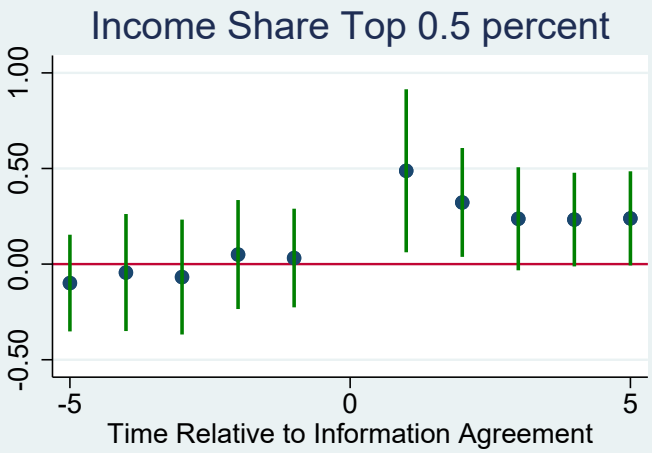
Notes: Sample covers years 1917–2015. Pre-trends equation estimated as in equation (2).

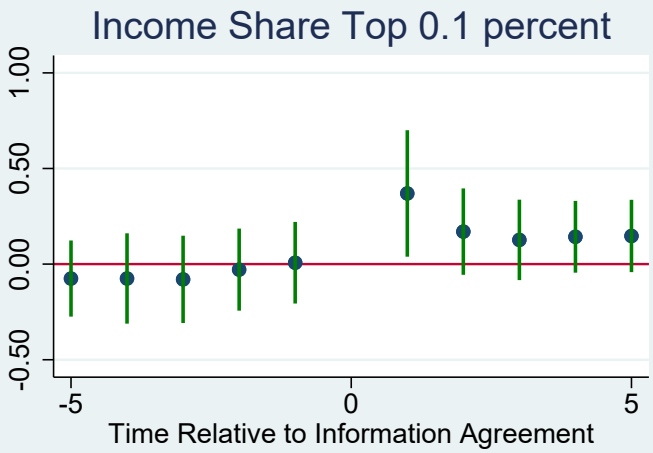
Figure 6: Inequality Pre-trends

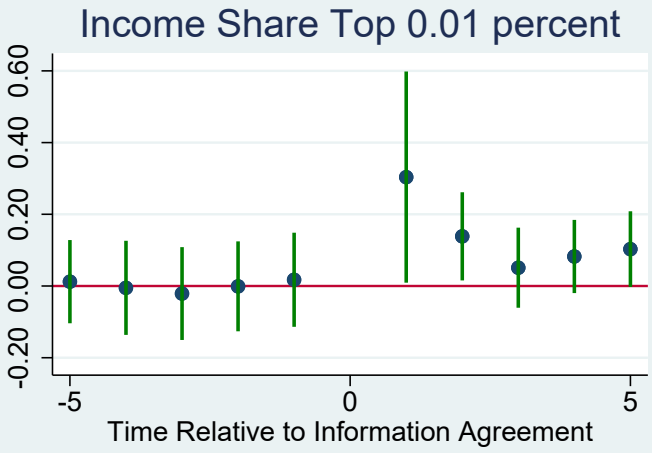


Notes: Sample covers years 1917–2015. Pre-trends equation estimated as in equation (2).

Figure 7: Inequality Pre-trends

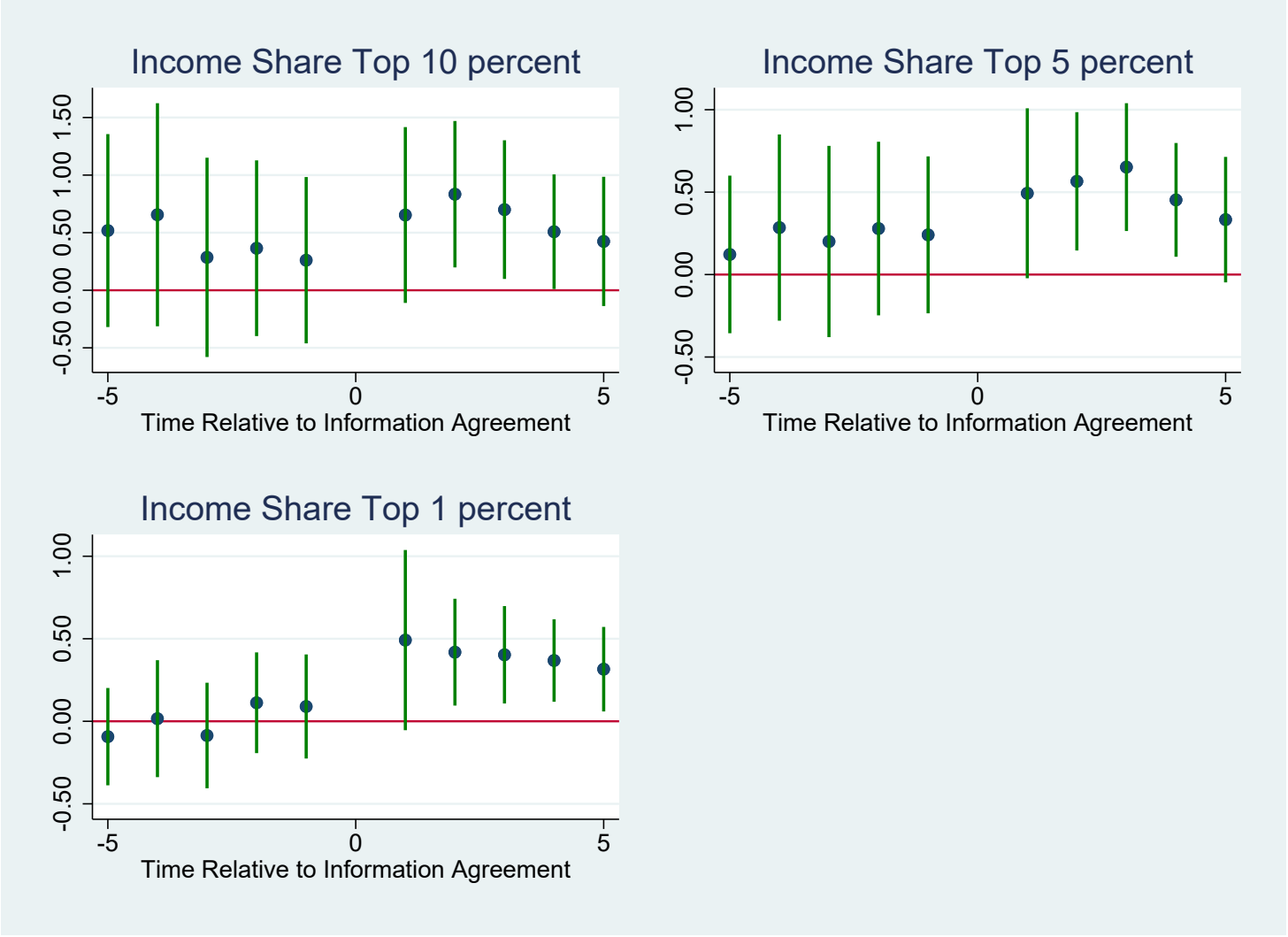






Notes: Sample covers years 1917–2015. Pre-trends equation estimated as in equation (2).

Figure 8: Inequality Pre-trends



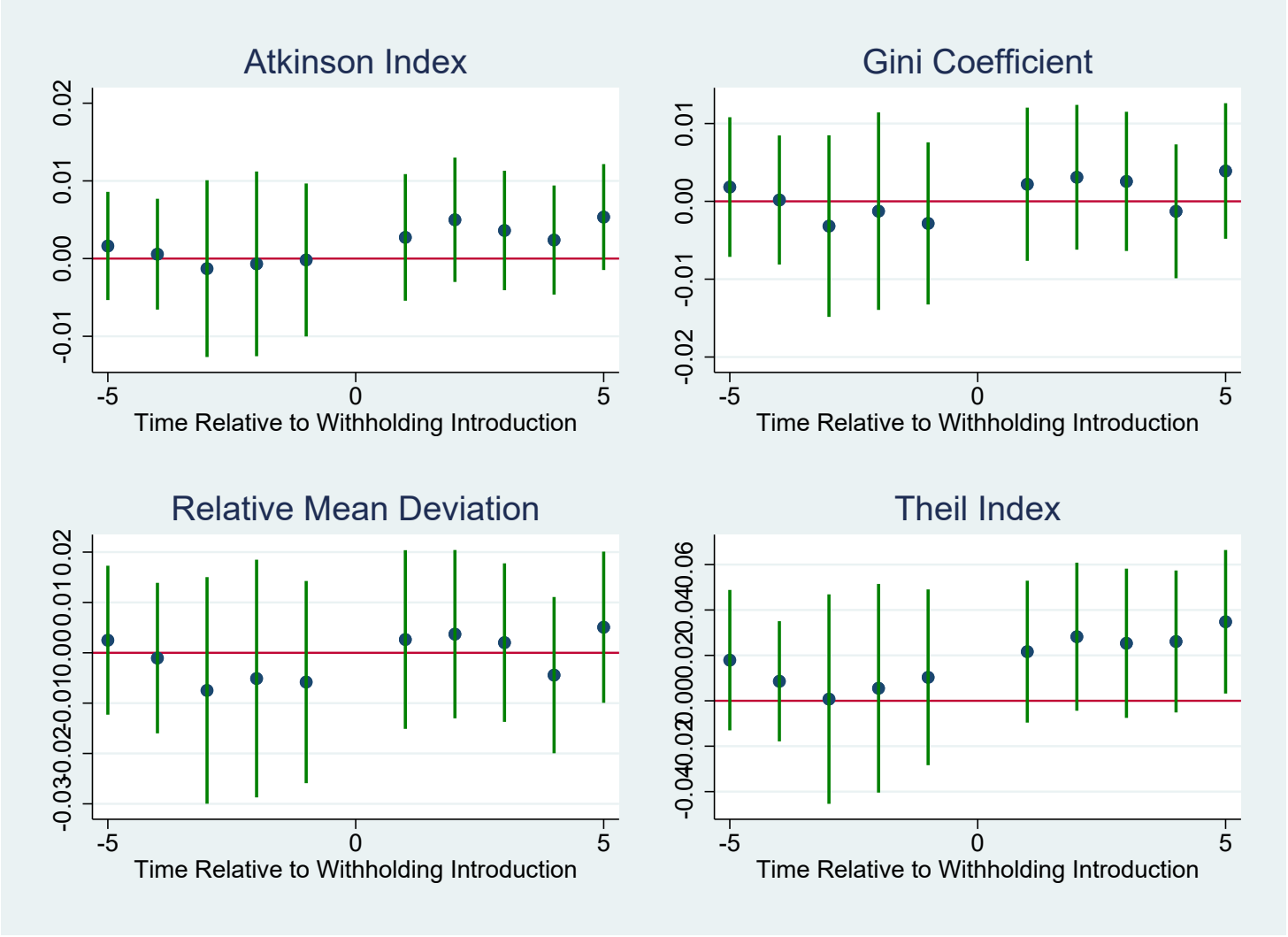
Notes: Sample covers years 1917–2015. Pre-trends equation estimated as in equation (2).

Figure 9: Inequality Pre-trends



Notes: Sample covers years 1917–2015. Pre-trends equation estimated as in equation (2).

Figure 10: Inequality Pre-trends



Notes: Sample covers years 1917–2015. Pre-trends equation estimated as in equation (2).

Figure 11: Inequality Pre-trends



Notes: Sample covers years 1917–2015. Pre-trends equation estimated as in equation (2).

Appendix

Table 18: Effects of Policy Changes on Inequality Measures

	(1) Income Share Top 0.01 percent	(2) Income Share Top 0.1 percent	(3) Income Share Top 0.5 percent
Post Individual Income Tax	0.190*	0.378	0.586
	(0.0982)	(0.2330)	(0.4195)
Post Ind. Inc. Tax X Not Close	0.208	0.380	0.620
	(0.1596)	(0.3555)	(0.6058)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
Observations	4965	4965	4965
R^2	0.817	0.852	0.863

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. *Post Ind. Inc. Tax X Not Close* is the interaction between Post Individual Income Tax and a dummy variable that is equal to 1 when the vote that introduced the income tax was not a close one. Sample covers years 1917–2015.

Table 19: Effects of Policy Changes on Inequality Measures

	(1) Income Share Top 1 percent	(2) Income Share Top 5 percent	(3) Income Share Top 10 percent
Post Individual Income Tax	0.671	0.857	1.347
	(0.5169)	(0.8481)	(1.3297)
Post Ind. Inc. Tax X Not Close	0.752	0.780	0.758
	(0.7401)	(1.1876)	(1.7109)
State Fixed Effects	Yes	Yes	Yes
Year Fixed Effects	Yes	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes	Yes
Observations	4965	4965	4965
R^2	0.868	0.846	0.801

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. *Post Ind. Inc. Tax X Not Close* is the interaction between Post Individual Income Tax and a dummy variable that is equal to 1 when the vote that introduced the income tax was not a close one. Sample covers years 1917–2015.

Table 20: Effects of Policy Changes on Inequality

	(1) Income Share Top 0.01 percent	(2) Income Share Top 0.01 percent
Post Withholding	0.336**	
	(0.1536)	
Post Information Agreement		0.172
		(0.1783)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	3593	3593
R^2	0.879	0.878

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1945–2015.

Table 21: Effects of Policy Changes on Inequality - Other Measures

	(1) Income Share Top 0.1 percent	(2) Income Share Top 0.1 percent
Post Withholding	0.561**	
	(0.2580)	
Post Information Agreement		0.235
		(0.2577)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	3593	3593
R^2	0.912	0.911

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1945–2015.

Table 22: Effects of Policy Changes on Inequality

	(1) Income Share Top 0.5 percent	(2) Income Share Top 0.5 percent
Post Withholding	0.688**	
	(0.2957)	
Post Information Agreement		0.248
		(0.2866)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	3593	3593
R^2	0.922	0.922

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1945–2015.

Table 23: Effects of Policy Changes on Inequality

	(1) Income Share Top 1 percent	(2) Income Share Top 1 percent
Post Withholding	0.762**	
	(0.3109)	
Post Information Agreement		0.312
		(0.3031)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	3593	3593
R^2	0.928	0.927

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1945–2015.

Table 24: Effects of Policy Changes on Inequality

	(1) Income Share Top 5 percent	(2) Income Share Top 5 percent
Post Withholding	0.740**	
	(0.3501)	
Post Information Agreement		0.173
		(0.3203)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	3593	3593
R^2	0.936	0.935

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1945–2015.

Table 25: Effects of Policy Changes on Inequality

	(1) Income Share Top 10 percent	(2) Income Share Top 10 percent
Post Withholding	0.580	
	(0.3482)	
Post Information Agreement		0.154
		(0.3834)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	3593	3593
R^2	0.929	0.928

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1945–2015.

Table 26: Effects of Policy Changes on Inequality Measures

	(1) Atkinson Index	(2) Atkinson Index
Post Withholding Introduction	0.00663***	
	(0.0024)	
Post Information Exchange Agreement		0.00339
		(0.0021)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	3642	3642
R^2	0.964	0.963

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1945–2015.

Table 27: Effects of Policy Changes on Inequality Measures

	(1) Gini Coefficient	(2) Gini Coefficient
Post Withholding Introduction	0.00157	
	(0.0030)	
Post Information Exchange Agreement		-0.00183
		(0.0030)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	3642	3642
R^2	0.953	0.953

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1945–2015.

Table 28: Effects of Policy Changes on Inequality Measures

	(1) Relative Mean Deviation	(2) Relative Mean Deviation
Post Withholding Introduction	0.00614	
	(0.0044)	
Post Information Exchange Agreement		0.000287
		(0.0039)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	3642	3642
R^2	0.964	0.964

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1945–2015.

Table 29: Effects of Policy Changes on Inequality Measures

	(1) Theil Index	(2) Theil Index
Post Withholding Introduction	0.0377***	
	(0.0131)	
Post Information Exchange Agreement		0.0225*
		(0.0120)
State Fixed Effects	Yes	Yes
Year Fixed Effects	Yes	Yes
State Effects Interacted with Linear Trend	Yes	Yes
Observations	3642	3642
R^2	0.954	0.954

Standard errors in parentheses
* $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Notes: Estimates are obtained by OLS. Heteroskedasticity-robust standard errors are clustered at the state level. Sample covers years 1945–2015.