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# Do Audits Deter or Provoke Future Tax Noncompliance? Evidence on Self-Employed Taxpayers

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## Abstract

This article employs unique tax administrative data and operational audit information, including risk scores used for audit selection, from a sample of approximately 7500 self-employed US taxpayers to investigate the effects of operational tax audits on future reporting behavior. Our estimates indicate that audits can have substantial deterrent or counter-deterrent effects, depending on the audit outcome. In the aggregate, taxable income is estimated to increase by roughly 15% 1 year after an operational audit. However, this figure masks substantial heterogeneity within the population. Among those taxpayers who receive an additional tax assessment, reported taxable income is estimated to be 64% higher in the first year after the audit (44% after 3 years) than it would have been in the absence of the audit. In contrast, among those taxpayers who do not receive an additional tax assessment, reported taxable income is estimated to be approximately 15% lower the year after the audit (21% 3 years later) than it would have been had the audit not taken place. Our results suggest that improved targeting of audits toward noncompliant taxpayers would not only yield more direct audit revenue but also pay dividends in terms of future tax collections. (JEL codes: H26, H30).

**Key words:** public administration, public finance, taxation, tax evasion, compliance

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## 1. Introduction

Administrative capacity to audit returns and detect tax noncompliance is limited. Therefore, tax agencies typically focus their scarce examination resources on returns that are deemed to have a high likelihood of irregularities or evasion (Andreoni et al. 1998). The US Internal Revenue Service (IRS), for instance, audits only about 1% of all self-employed individual income taxpayers annually. In Fiscal Year 2018, these audits resulted in almost

\$2 billion in recommended additional tax assessments, although not all of the recommended amount will ultimately be collected ([Internal Revenue Service 2019](#)). In addition to the direct revenue associated with the assessment of additional taxes, interest, and penalties, tax audits have the potential to generate (or diminish) revenue indirectly through deterrent effects. One such effect, ‘general deterrence’, is emphasized in the standard economic model of tax compliance ([Allingham and Sandmo 1972](#)). This term refers to the improvement in compliance within the general taxpayer population in response to an increase in the audit rate. The second effect, ‘specific deterrence’, relates to the impact of an audit on a taxpayer’s subsequent compliance behavior.

In this article, we empirically examine the specific deterrent effect of operational audit activities. Our findings draw on a rich administrative panel dataset that includes 2453 self-employed taxpayers (Schedule C filers) who were audited after filing their Tax Year 2007 (TY2007) returns as well as a comparison sample of 6922 Schedule C filers who were not audited. We employ statistical matching techniques to select unaudited controls from our comparison sample for each member of our audit sample. These controls permit us to develop a counterfactual estimate of the reports that the audited taxpayers would have made in future years had they not experienced an audit. The difference-in-differences-based estimation results provide robust evidence that audits have important short-term and medium-term revenue implications. In the aggregate, our estimates indicate that operational tax audits induce taxpayers to increase their reported taxable income by roughly 10% 1 year after the examination. Three years after the audit reported income remains modestly (2%) above pre-audit levels. Overall, then, the specific deterrent effect of audits on subsequent reporting behavior adds to the static gain from direct audit revenue.

However, this aggregate result belies a more nuanced picture. Some experimental research suggests that behavioral responses to tax audits may either amplify or undermine the immediate revenue gains associated with audits (for example, [Guala and Mittone 2005](#); [Mittone 2006](#)), and a recent field study finds that random tax audits reduce compliance among taxpayers who did not receive an additional tax assessment on audit ([Gemmel and Ratto 2012](#)). Building on this idea, we develop separate audit impact estimates for taxpayers who did (determined noncompliant) and did not (determined compliant) receive an additional tax assessment as a result of their examination.<sup>1</sup> To ensure credible estimates, we construct a separate set of unaudited controls for each of these two audit groups. We check that, for each group, the audited taxpayers and their matched controls show similar pretreatment trends in reported taxable income.

Among taxpayers who have received an additional tax assessment as a result of their audit, we find evidence of a massive pro-deterrent effect. Specifically, these taxpayers report approximately 64% more taxable income than their matched controls in the year following

1 The IRS may increase or reduce a taxpayers’ liability if the audit reveals that the initial assessment is incorrect. We classify audited taxpayers as having either a positive tax adjustment (determined noncompliant) or a non-positive tax adjustment (determined compliant) on the basis of the examiner-recommended tax change. Note that the group of determined compliant taxpayers includes not only truly compliant taxpayers but also taxpayers whose noncompliance was not discovered during the examination. Taxpayers who received an additional assessment (determined noncompliant) have the right to file an administrative appeal or petition the tax court. Only approximately 2% of audited taxpayers exercise this right.

the audit and around 44% more income 3 years after the audit. Conversely, we find that audits play a counter-deterrent role among those who have not experienced an additional tax assessment. We estimate that such taxpayers report approximately 15% less income the year after they were audited than their matched counterparts and around 21% less 3 years later. Finally, to investigate the robustness of our findings, we generate alternative difference-in-differences estimates based on a variety of different statistical matching techniques.

Our study differs from most of the empirical literature on the specific deterrent effect of tax audits in two key ways. First, our estimates are based on real-world operational audits, and not on random audits or laboratory experiments. Second, unlike most existing work, we account for the possibility that the specific deterrent effect depends on the outcome of the examination.

Prior studies have generally relied on an analysis of random audit results, either in a laboratory or field setting.<sup>2</sup> For example, [Kleven et al. \(2011\)](#) find that random audits lead to a significant increase in self-reported income among Danish taxpayers in the subsequent tax year. [Advani et al. \(2017\)](#) explore reporting changes over a longer time span and conclude that audits have an enduring positive impact on taxpayer reporting behavior. In their study, a sample of self-employed UK taxpayers exhibited a sustained increase in income reporting for at least 5 years following a random audit. [DeBacker et al. \(2018\)](#) also find evidence of a sustained pro-deterrent effect of audits. In their study, taxpayer reporting behavior improves for 3 years following a random audit before ultimately reverting.

In contrast to these prior studies, the focus of this article is on taxpayers selected through an ordinary operational audit process. In practice, the vast majority of taxpayer audits are operational in nature: they are targeted toward taxpayers with a relatively high likelihood of noncompliance. To identify tax returns with a high potential for evasion, the IRS employs insights from the National Research Program, a program of random tax audits conducted on a stratified sample of returns. The outcomes of these audits inform a ‘discriminant function’ (the ‘DIF score’) that quantifies the noncompliance risk of each tax return. A higher DIF score indicates a higher likelihood of additional assessments, and audits based upon the DIF score generate substantially larger tax assessments, on average, than random audits ([Alm and McKee 2004](#)).<sup>3</sup> Since high-risk taxpayers may respond differently to an examination than typical taxpayers, an analysis based on operational audit results is the preferred way to understand the actual impact of real-world enforcement programs.

Moreover, most existing studies have focused on the overall effect of audits on future compliance among those who have been subjected to an examination. However, psychological research indicates that audits may adversely impact subsequent reporting behavior among some groups of taxpayers (see for example, [Kirchler 2007](#)). The most prominent example of unintended consequences is the ‘bomb-crater’ effect ([Guala and Mittone 2005](#); [Mittone 2006](#), [Kastlunger et al. 2009](#)), often observed in public good games. Participants in laboratory experiments tend to reduce their contributions after being audited, either because they misperceive the probability of being audited a second time ([Mittone et al. 2017](#)) or due to loss–repair motivations ([Maciejovsky et al. 2007](#)). Overall, these laboratory

2 An exception is the study by [Erard \(1992\)](#), which examines subsequent compliance changes among a sample of taxpayers that had been subjected to ordinary operational audits by the IRS.

3 For a comprehensive overview of the audit selection process in the USA, see [Andreoni et al. \(1998\)](#).

findings suggest that behavioral responses to audits are not necessarily in line with the assumptions of the standard model of tax evasion (Allingham and Sandmo 1972).

Building on the findings of these experimental studies, Gemmel and Ratto (2012) examine data from an HM Revenue and Customs (HMRC) random audit program in the UK to explore whether the impact of audits on future taxpayer reporting behavior depends on the outcome of the examination. The authors find that randomly audited taxpayers who have been found to be noncompliant report more income on their subsequent tax return than those who were not audited. Conversely, randomly audited taxpayers who were found to be compliant show the opposite response. One explanation for this finding (which is favored by the authors) is that the outcome of the audit directly influences future reporting behavior. However, it is important to recognize that, while audited and unaudited taxpayers were randomly selected under the HMRC sampling design, the specific outcome of the examinations among those who were audited was not randomized. So, while the subsequent reporting behavior of the group of unaudited taxpayers is a reasonable counterfactual for how the overall group of audited respondents would have behaved in the absence of their examinations, it is not necessarily a good surrogate for a specific subgroup of audited taxpayers with a given examination outcome. Therefore, a potential alternative explanation for the authors' finding is that taxpayers who were found to be compliant differed in relevant ways from those who were found to be noncompliant, so that their subsequent reporting behavior would have differed even in the absence of the audits. In our analysis of the deterrent effect of operational tax audits, we follow Gemmel and Ratto in allowing for the possibility that future reporting behavior depends on the examination outcome. However, to avoid the ambiguities over the reasons underlying future reporting differences that are inherent in their study, we explicitly construct separate measures of counterfactual reporting behavior for each outcome group. Specifically, we develop separate sets of matched controls to estimate the counterfactual outcomes for taxpayers who did and did not receive an additional tax assessment as a result of their audit.

Further research is needed to understand why seemingly compliant taxpayers tend to make less compliant reports following an audit. Psychological research might offer some reference points. For example, Kirchler et al. (2008) argue that tax compliance results from a combination of effective enforcement and mutual trust between taxpayers and the authorities. While audits are crucial to enforce compliance among noncooperative taxpayers, a favorable climate between taxpayers and the tax authority likely promotes voluntarily compliance. Accordingly, if the nature or frequency of audits is perceived as disproportionate, audits might erode trust, crowd out the intrinsic motivation to comply, and thus, undermine the willingness to pay taxes in the future (Frey 2011).<sup>4</sup> Future work should thus explore taxpayers' perceptions of the audit experience and its effect on motivational processes.

Finally, it is important to recognize that an audit outcome is not a perfect measure of compliance. In particular, not all taxpayers who experience a 'no-change' audit are truly compliant, because audits do not always uncover tax evasion when it is present. Conversely, not all those who experience a tax adjustment during an audit are truly non-compliant; for instance, audit assessments are sometimes overruled upon appeal. Nor is the examination result necessarily a valid indicator of a taxpayer's intentions with regard to

4 Following this line of thought, Mendoza et al. (2017) show that high audit frequency is associated with high levels of perceived tax evasion.

tax compliance, because it is not uncommon for individuals to make unintentional tax reporting errors. While we address related concerns below, our findings need to be interpreted with caution.

The remainder of this article is organized as follows. Section 2 describes our methodology and data. Section 3 presents our estimation results, and Section 4 concludes.

## 2. Methodology

### 2.1. Empirical strategy

We rely on program evaluation techniques to quantify the impact of audits on future taxpayer reporting behavior. Specifically, we employ a matched difference-in-differences approach to compare the subsequent reporting behavior of audited taxpayers with that of several alternative matched control groups that have been constructed using different statistical matching techniques.

Denote the change in individual  $i$ 's reported income by  $\Delta Y_i^1$  if the taxpayer was audited and by  $\Delta Y_i^0$  if he was not audited. Each taxpayer is characterized by the variable  $D_i \in \{0, 1\}$  indicating treatment assignment. We will refer to audited taxpayers as members of the treatment group and to non-audited taxpayers as control group members. We seek to identify the average treatment effect on the treated (ATT), which is given by

$$\tau^{\text{ATT}} = E[\Delta Y^1 | D = 1] - E[\Delta Y^0 | D = 1]. \quad (1)$$

An estimate of the first term is readily available by drawing on the observed response among audited taxpayers. The second term is counterfactual, however. It represents the change in the reporting behavior of audited taxpayers, had they not been audited. Establishing a reliable counterfactual estimate is crucial, as quasi-experimental studies face the risk that unobserved confounding factors may play a role in determining whether an observation is assigned as a treatment or a control. Although the distribution of DIF scores is similar for the audited taxpayers in our sample and the comparison group, other factors beyond the DIF score play a role in IRS audit selection. Furthermore, in the case of returns flagged for possible audit specifically on the basis of their DIF score, these returns are reviewed by an experienced examiner known as a 'classifier' before a decision is made whether to go forward with an audit. It follows that the overall comparison sample is not sufficiently similar to the audit sample in all relevant ways that it can be relied upon to predict the counterfactual behavior of the latter group. Rather, it will be necessary to select a subset of the comparison sample that more closely matches the characteristics of the audited taxpayers with regard to the relevant set of covariates  $X$ .

A suitable matched control group is one that satisfies the following conditional independence assumption: treatment assignment  $D$  is independent of the outcome  $\Delta Y^0$  after conditioning on the set of confounding factors  $X \in \mathcal{R}^n$  (Heckman et al. 1998).<sup>5</sup> We thus require that audit selection is random within the group of taxpayers characterized by the same covariates  $X = x$ . To credibly satisfy this assumption, we select our control variables

5 More formally, conditional independence between  $D$  and  $\Delta Y^0$  implies  $E[E[\Delta Y^0 | X, D = 0] | D = 1] = E[\Delta Y^0 | D = 1]$ .

with a two-layered approach. First, we define three baseline covariate sets, of varying size, which include variables that likely affect treatment assignment and outcome simultaneously. We then reduce the dimensionality of the baseline sets with an automatic procedure to avoid over-parametrization. The first baseline set (Set I) is parsimoniously chosen. It includes the DIF score ventile, pretreatment profitability (defined as profit as a share of business income), and pretreatment total taxable income. We include the DIF score ventile in all covariate sets, since it is a key determinant of audit selection and likely based on various components of taxable income. By including pretreatment outcomes, we aim to avoid bias caused by ‘self-selection’ into treatment (Ashenfelter 1978). Finally, we expect that business profitability is also a key driver for enforcement activity. It is clearly correlated with our dependent variable.

In our second and third control variable sets, we extend our parsimonious baseline set to capture additional variables that are likely to influence the audit selection process. The second set (Set II) adds three variables reflecting the structure of income sources, business expenses and benefit schemes. The variables are distance measures, which build on an extensive set of indicator variables.<sup>6</sup> In the third set (Set III), we add the lag of all variables accounted for in the second set as well as interactions with their immediate pretreatment counterparts, resulting in a total of eighteen control variables. Finally, by using changes in reported income as our dependent variable, rather than levels, we aim to control for unobservable time-constant differences, which could bias the results of simple post-audit cross-sectional comparisons (Smith and Todd 2005).

The basic idea behind matching is to pair each member of the treatment group with a set of observationally similar control group members. With the confounding factors held constant, the difference between treated taxpayers and matched controls is a direct estimate of the treatment effect that does not rely on any parametric assumptions.

The most widely used metric to assess similarity between participants is the estimated individual-specific probability of treatment assignment, the propensity score. Rosenbaum and Rubin (1983) showed that the counterfactual outcome can thus be constructed as a simple weighted average, with weights reflecting the similarity across participants. Their results imply the following general formulation of ATT estimators

$$\hat{\tau}^{\text{ATT}} = \frac{1}{N^1} \sum_{i:D_i=1} [\Delta Y_i^1 - \hat{m}(p_i)], \quad (2)$$

where  $N^1$  is the number of audited taxpayers,  $p_i$  is the propensity score of individual  $i$ , and  $\hat{m}$  is an estimate of individual  $i$ 's change in reported income, had he not been audited. We employ four specifications of  $\hat{m}$ . The most common procedure to obtain individual-specific counterfactuals is to draw for each audited taxpayers a sample of  $k$  control group members, which are similar in terms of their propensity scores. The  $k$  nearest-neighbor estimator is defined as

6 Specifically, the income distance measure comprises information on whether the following income sources are present: dividends, interest, capital gains, Schedule E, Schedule F, wage, unemployment benefits, gross social security, taxable social security, individual retirement income, and pension/annuity income. The business expenditure measure is based on the following expenditure items: car/truck, depreciation, legal fees, travel, meal, wages, business-home use, and moving expenditures.

$$\hat{m}^{NN}(p_i) = \frac{1}{k} \sum_{j:D_j=0} \omega \Delta Y_j^0, \quad \text{where } \omega_j = \mathbb{1}\{|p_i - p_j| \leq \bar{d}(p_i)\}. \quad (3)$$

The function  $\bar{d}(p_i)$  defines a radius around individual  $i$  so that exactly  $k$  control group members lie inside the implied neighborhood, and  $\mathbb{1}\{\}$  is an indicator function. The choice of radius involves a trade-off between efficiency and bias. The more control group members are used to estimate the counterfactual, the higher becomes the probability of using bad matches with distant propensity scores. We employ a 3-nearest-neighbor matching procedure with replacement as a benchmark.

Heckman et al. (1997, 1998) advocate matching based on local polynomial regressions. Estimators in this class are more efficient because they construct weighted average counterfactuals based on all control group members. Our second specification is a simple kernel-matching estimator, defined by

$$\hat{m}^{PK}(p_i) = \frac{1}{K\left(\frac{p_i - \bar{p}}{h}\right)} \sum_{j:D_j=0} \Delta Y_j^0 K\left(\frac{p_i - p_j}{h}\right), \quad (4)$$

where  $K$  is the kernel function and  $h$  is a bandwidth parameter. We use the normal density to construct weights. Bias is reduced relative to nearest-neighbor matching as the weights are decreasing functions of dissimilarity, that is, the difference in propensity scores.

Local linear regressions display even increased optimality properties (Heckman et al. 1997) relative to kernel-based methods. However, the estimators tend to lead to rugged curves in regions of sparse or clustered data (Seifert and Gasser 1996). The local linear ridge estimator, proposed by Seifert and Gasser (2000), avoids the instability of local linear regressions by adding a small constant in the denominator. Our third estimator is defined by

$$\hat{m}^{LLR}(p_i) = \frac{T_0}{S_0} + \frac{T_1(p_i - \bar{p})}{S_2 + rh|p_i - \bar{p}|}, \quad (5)$$

where  $S_a = \sum_{j:D_j=0} (p_j - \bar{p})^a K\left(\frac{p_j - \bar{p}}{h}\right)$ ,  $T_a = \sum_{j:D_j=0} \Delta Y_j^0 (p_j - \bar{p})^a K\left(\frac{p_j - \bar{p}}{h}\right)$  and the conditional average propensity score is given by  $\bar{p} = \sum_{j:D_j=0} p_j K\left(\frac{p_j - \bar{p}}{h}\right)$ .

Matching estimators aim to reduce model dependence, inefficiency, and bias, by increasing the covariate balance between treated and controls. However, King et al. (2011) and King and Nielsen (2019) argue that propensity score matching, as commonly used, actually tends to increase the imbalance of covariates, thereby resulting in greater bias. In our fourth specification, we thus re-estimate the kernel-matching estimator, using the pairwise Mahalanobis distance (MHD).<sup>7</sup>

For each audited taxpayer in our data sample, each of our statistical matching approaches selects one or more unaudited taxpayers as matched controls. These matched controls are then used to predict how that audited taxpayer would have reported in future periods in the absence of the examination. In Section 3 below, we rely on our matched sample to explore whether the impact of an audit depends on the outcome of the examination.

7 The Mahalanobis distance is defined by  $d(X_i, X_j) = \sqrt{[(X_i - X_j)' S^{-1}(X_i - X_j)]}$ , where  $S$  is the covariance matrix of  $X$ .

To do so, we compare the future reporting behavior within a subgroup of audited taxpayers with a specific examination outcome (either a positive additional tax assessment or no additional tax assessment) against that observed among those members of our overall control group that have been specifically matched to the members of that audit outcome subgroup. In this way, we are able to rely on a separate tailored set of carefully matched controls for each audit outcome subgroup.

## 2.2. Data

Our initial tax return sample contains granular information on 6451 randomly selected self-employed taxpayers who experienced an audit of their TY2007 federal income tax return. These taxpayers had been assigned to any one of six different examination classes for that year, depending on their reported levels of gross business receipts and total positive income.<sup>8</sup> An initial comparison sample of 11,218 unaudited taxpayers was also drawn. The IRS assigns all taxpayers within a given examination class a risk score (known as the ‘DIF score’) based on the predicted likelihood that their return substantially understates the true tax liability. To ensure similar compliance risk characteristics among the two samples, the DIF score ventile cutoff values were computed for the members of the audit sample within a given examination class. These values were then used to stratify the population of unaudited taxpayers from that class. Roughly the same number of unaudited taxpayers was then randomly drawn from each stratum. This ensured that the distribution of DIF scores was approximately the same for the members of the audit (treatment) and non-audit (comparison) samples from that examination class.

To unambiguously distill the effect of one single audit, we have imposed some exclusion restrictions on our treatment and comparison samples. Members of the treatment sample are required to have had their TY2007 audit begin prior to filing their TY2008 return. With this one exception, however, no member of either sample is permitted to have experienced any audits of their TY2005–TY2009 returns. To increase the efficiency of our matching procedures, we have introduced a number of additional data requirements: taxpayers need to have filed a self-employment schedule (Schedule C) in both TY2006 and TY2007, and all of their returns from TY2005 through TY2009 must have been filed on time and in chronological order. Furthermore, we exclude taxpayers who have reported extreme incomes (from the top 2.5% and the bottom 2.5% of the income distribution). [Table 1](#) summarizes these data selection steps. Our final sample contains 9375 taxpayers, including 2453 treated taxpayers as well as a comparison group of 6922 taxpayers.

## Covariate balance

The central assumption for matching estimators is that treatment assignment is random conditional on a set of control variables. While there is no formal test, the assumption seems more likely to hold if relevant variables are similarly distributed among treatment and control group members. [Table 2](#) summarizes mean values for the main explanatory

8 These examination classes exclude self-employed taxpayers who filed a claim for the Earned Income Credit, a non-refundable credit available to low- and moderate-income taxpayers. The total positive income is computed by summing only the positive reported values for the following income sources (negative reported amounts are treated as zero): wages, interest, dividends, distributions, other income, Schedule C net profit, and Schedule F net profit.



**Table 1.** Sample description

Sample selection							
Step	Description	Comparison sample	% $\Delta$	Audit sample	% $\Delta$	Total	% $\Delta$
0	Initial sample	11,218	–	6451	–	17,699	–
1	Incomplete data or TY2007 audit of treatment group member began after filing of TY2008 return	9651	0.86	4251	0.66	13,902	0.79
2	Violation of non-audit restriction for TY2005–TY2009	9560	0.99	3768	0.89	13,328	0.76
3	Failure to file timely and chronologically	7278	0.76	2619	0.70	9897	0.74
4	Outlier	6922	0.95	2453	0.94	9375	0.95

variables in 2007 for taxpayers in the control group (Column 1), taxpayers in the determined noncompliant group and their matched counterparts (Columns 2 and 3), and taxpayers in the determined compliant group and their matched counterparts (Columns 4 and 5). We use four different matching procedures to determine weights and select taxpayers from the control group that resemble treated taxpayers as closely as possible. For simplicity, [Table 2](#) depicts the average of these four matched groups. Although the original comparison sample was selected in order to achieve a comparable distribution of DIF scores to that observed in the original audit sample, the mean DIF score ventile among control group members (9.79) in the final sample (after the data exclusion steps summarized in [Table 1](#)) is lower than that observed for either the members of the final determined compliant group (10.94) or the final determined noncompliant group (10.31). The mean DIF ventile scores among matched controls, however, are very close the respective values in the treatment group, both for determined compliant (10.23) and determined noncompliant taxpayers (10.86). Similarly, the balance between treated and control group members in terms of logarithmic taxable income, profitability and three distance measures capturing income indicators (income distance), business expense indicators (business distance) and other indicators (other distance) improves notably due to the matching procedure.

### 3. Empirical Results

#### 3.1. Aggregate effects

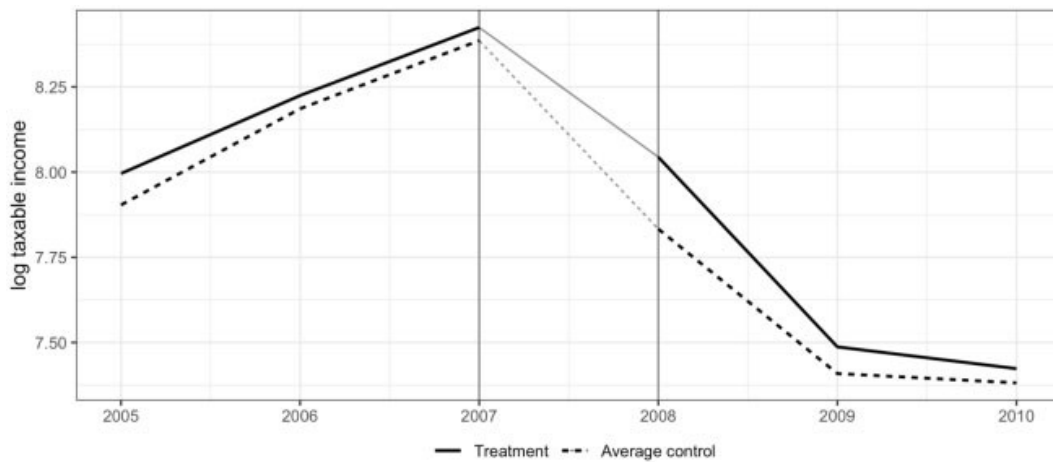
Difference-in-differences estimators are consistent if the ‘common trends’ assumption holds ([Meyer 1995](#)). Under this assumption, one would expect the reporting behavior of the treatment and control groups to evolve similarly prior to the treatment. [Figure 1](#) illustrates the estimated treatment effect for the overall audit sample without reference to specific audit outcomes. It shows the progression of the natural logarithm of reported taxable income for the treatment group of audited taxpayers (red) and the average matched counterfactual from TY2005 through TY2010.<sup>9</sup> The treatment group members had an audit commence

<sup>9</sup> The control groups were constructed based on the four alternative statistical matching methods described in Section 2.1. All of these methods have been applied using our parsimonious set of explanatory variables (Set I) described above in Section 2.1.

**Table 2.** Covariate balance

	Control group	Determined noncompliant		Determined compliant	
		Treatment	Matched Average	Treatment	Matched Average
DIF score	9.79	10.94	10.86	10.31	10.23
Log tax	7.92	8.63	8.58	7.93	7.89
Profitability	-672.47	-266.02	-228.80	-888.52	-893.94
Income distance	11.23	10.73	10.57	10.97	10.91
Business distance	7.90	7.69	7.65	8.69	8.59
Other distance	5.50	6.23	5.81	5.63	5.27

Note: Table depicts mean values of the main control variables in 2007.

**Figure 1** Aggregate effect of audits.

for their TY2007 return prior to filing their TY2008 return. The natural log of reported taxable income follows a similar rising trend within the treatment and control groups up through TY2007, as one would expect under the common trend assumption. While reported income in both groups declines substantially between TY2007 and TY2009, most likely due to the financial crisis, the trend is weaker for the treatment group, indicating a positive effect of audits on subsequent tax reporting compliance.

Table 3 reports estimation results for the overall sample. As discussed in Section 2.1, we have experimented with a parsimonious set of explanatory variables for our matching analysis (Set I) as well as a pair of increasingly more inclusive sets of covariates (Set II and Set III). The first column presents four independent estimates (based on our four alternative statistical matching methods) of the subsequent year reporting impact of audits. On average, we find that audited taxpayers report around 12% more income the year after they were audited, relative to the matched control samples. The average difference-in-differences estimator is significant at the 5% level, and there is little variation in the estimated treatment effects when using our most parsimonious set of controls (Set I). The smallest estimated impact is 9% (using nearest-neighbor matching) while the largest is 14% (using kernel matching based on the propensity score). The ratio of these two estimates is 1.6, suggesting modest model uncertainty.

**Table 3.** Estimated treatment effect 1 year and 3 years after the audit

Estimated ATT	1 year after the audit			3 years after the audit		
	I	II	III	I	II	III
Set of control variables	(1)	(2)	(3)	(4)	(5)	(6)
Nearest neighbor	0.087 (0.078)	0.136* (0.075)	0.159** (0.075)	-0.107 (0.095)	-0.073 (0.084)	-0.029 (0.091)
Kernel propensity	0.138** (0.058)	0.177*** (0.063)	0.168*** (0.057)	-0.062 (0.071)	-0.051 (0.074)	-0.019 (0.070)
Local ridge	0.129** (0.060)	0.181*** (0.065)	0.186*** (0.059)	-0.071 (0.073)	-0.057 (0.074)	-0.013 (0.072)
Kernel MHD	0.122** (0.056)	0.157** (0.062)	0.123** (0.062)	-0.059 (0.070)	-0.044 (0.073)	-0.071 (0.073)
Average	0.119** (0.059)	0.162*** (0.062)	0.159*** (0.058)	-0.075 (0.072)	-0.056 (0.070)	-0.033 (0.069)

Notes: \*, \*\*, and \*\*\* indicate significance at the 10, 5, and 1% levels, respectively. Bootstrapped standard errors are in parentheses. Control variables are described in Section 2.1.

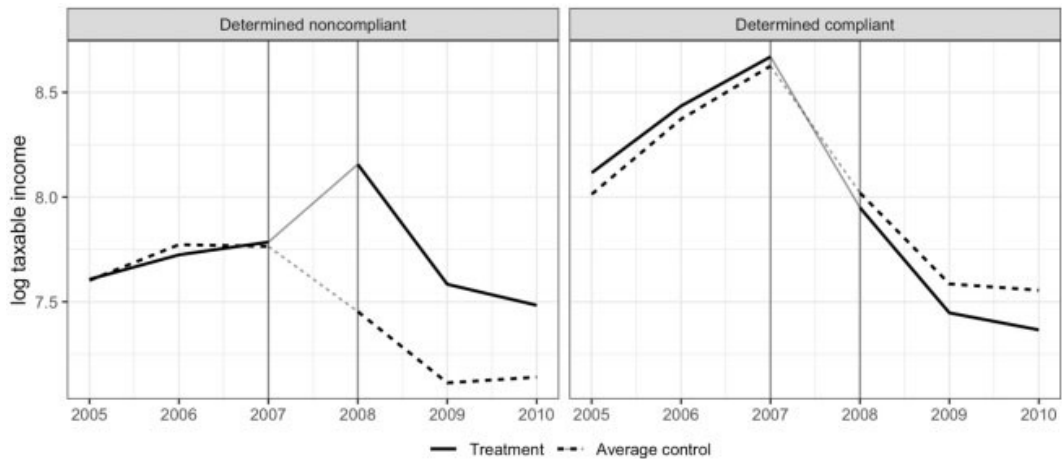
In the second and third columns, we present the results for our two more inclusive sets of covariates (Set II and Set III). The average estimated treatment effect increases to around 16% in these specifications and remains significant at the 5% level. As expected, nearest-neighbor matching is associated with somewhat less precise estimates than either local linear ridge matching or kernel matching. Overall, the estimates show less variation across the alternative statistical matching methods when using the more inclusive sets of covariates.

In Columns 4–6, we report estimated treatment effects 3 years after the audit. On average, audited taxpayers report around 3–8% less income than their matched counterparts 3 years after the audit, suggesting that the deterrent effect of audits diminishes over time. However, across all matching estimators and covariate sets, the effects are statistically insignificant.

### 3.2. Differential effects

Figure 2 provides a disaggregated depiction of estimated treatment effects by audit outcome. We again display the trend in the natural log of reported taxable income for the treatment group and four alternative control groups based on our alternative statistical matching methods. The left-hand panel illustrates the effect of audits that result in an additional tax assessment (determined noncompliant). It shows a common trend in reported income among the treatment and control groups prior to the audit. Reported taxable income increases very substantially for audited taxpayers who were determined noncompliant in the years following the audit, even as reported income declines among their matched controls. This is indicative of a strong pro-deterrent effect of audits among taxpayers who receive an additional tax assessment.

The right-hand panel shows the impact of audits on taxpayers who do not receive an additional tax assessment (determined compliant). Again, the trend in reported taxable income between the treatment and control groups is comparable before the audit. However, although treated taxpayers in this group report more income than their untreated



**Figure 2** Differential effects of audits.

counterparts prior to TY2007, they report less income in the years following the audit, which indicates that audits have a counter-deterrent effect among taxpayers who did not receive an additional assessment.

The two panels in [Figure 2](#) provide strong evidence that the behavioral response to an operational audit is highly dependent on the audit outcome. Compared with their matched counterparts, reported income increases substantially among audited taxpayers with a positive audit result (determined noncompliant), while it decreases among those who received a nonpositive assessment (determined noncompliant).

[Table 4](#) summarizes our main findings. The first column presents four independent estimates of the subsequent year reporting impact of audits that result in an additional tax assessment (determined noncompliant). On average, we find that that audited taxpayers in this group report around 64% more income the year after they were audited, relative to the matched control samples. The average difference-in-differences estimator is significant at the 5% level. There is little variation in the estimated treatment effects when using our most parsimonious set of controls (Set I). The smallest estimated impact is 59.1% (using kernel matching based on the MHD metric), while the largest is 62.6% (using kernel matching based on the propensity score), suggesting that audits have a massive pro-deterrent effect on noncompliance in the subsequent year among taxpayers who receive an additional tax assessment. The ratio of these two estimates is 1.06, suggesting relatively little model uncertainty.

In the second and third columns, we present the results for our two more inclusive sets of covariates (Sets II and III). The average estimated treatment effect on taxpayers who were determined noncompliant is largely unchanged and remains significant at the 5% level. As expected, nearest-neighbor matching is associated with somewhat less precise estimates than either kernel matching or local linear ridge matching. Overall, the estimates show relatively little variation across the alternative statistical matching methods and sets of covariates.

In Columns 4–6, we report treatment effects for operational audits that do not result in an additional tax assessment (determined compliant). Across all matching estimators and covariate sets, we find similarly large and statistically significant effects. On average, audited taxpayers who were determined compliant report around 15% less income than their

**Table 4.** Estimated treatment effect 1 year after the audit

Experimental group	Determined noncompliant			Determined compliant		
	I	II	III	I	II	III
Set of control variables	(1)	(2)	(3)	(4)	(5)	(6)
Nearest neighbor	0.609*** (0.142)	0.666*** (0.130)	0.604*** (0.142)	-0.154* (0.091)	-0.214** (0.094)	-0.153* (0.091)
Kernel propensity	0.626*** (0.111)	0.656*** (0.106)	0.650*** (0.111)	-0.140* (0.074)	-0.135* (0.074)	-0.149** (0.074)
Local ridge	0.622*** (0.112)	0.631*** (0.106)	0.631** (0.112)	-0.148** (0.075)	-0.140* (0.075)	-0.161** (0.075)
Kernel MHD	0.591*** (0.109)	0.664*** (0.098)	0.673*** (0.109)	-0.173** (0.085)	-0.125* (0.075)	-0.136 (0.085)
Average	0.612*** (0.104)	0.654*** (0.100)	0.639*** (0.104)	-0.154** (0.073)	-0.154** (0.074)	-0.150** (0.073)

Notes: \*, \*\*, and \*\*\* indicate significance at the 10, 5, and 1% levels, respectively. Bootstrapped standard errors are in parentheses. Control variables are described in Section 2.1.

matched counterparts 1 year after the audit, suggesting that audits have the potential to reduce subsequent compliance.

Table 5 reports estimated treatment effects for both groups, 3 years after the audit. The ratio of the largest estimated impact to the smallest is approximately 1.28 for taxpayers who were determined noncompliant and 1.26 for taxpayers who were determined compliant based on the parsimonious covariate set, suggesting slightly more uncertainty than our 1-year post-audit impact estimates. Consistent with those estimates, the estimated impact of an audit on reported taxable income remains positive and very substantial 3 years later (an increase of 44%) for those who received an additional tax assessment, while it remains negative and substantial (a decline of 21%) among those who received no additional tax assessment.

## 4. Conclusion

This study investigates the effect of operational tax audits on future compliance behavior. Overall, we find a moderate positive effect on reported taxable income. However, this result masks substantial heterogeneity among taxpayers who do and do not receive an additional tax assessment as a result of their examination. Within the former group, operational audits have a very substantial pro-deterrent effect on future noncompliance. In stark contrast, they have a large counter-deterrent effect among taxpayers who do not receive an additional tax assessment. While this result is in line with prior findings, our estimated income adjustments are substantially larger than those reported by Gemmel and Ratto (2012), particularly in the case of noncompliant taxpayers. Two aspects of our study design contribute to these differences. Gemmel and Ratto (2012) analyze the behavior of audited taxpayers who were randomly selected, while we focus on taxpayers who were selected based on their risk profile and who are thus more likely to receive an additional assessment. More importantly, Gemmel and Ratto (2012) use the same comparison group for both types of audit outcomes (determined noncompliant and determined compliant), while

**Table 5.** Estimated treatment effect 3 years after the audit

Experimental group	Determined noncompliant			Determined compliant		
	I	II	III	I	II	III
Set of control variables	(1)	(2)	(3)	(4)	(5)	(6)
Nearest neighbor	0.402** (0.166)	0.465*** (0.140)	0.506*** (0.166)	-0.241** (0.105)	-0.242** (0.108)	-0.220** (0.105)
Kernel propensity	0.421*** (0.123)	0.442*** (0.114)	0.450*** (0.123)	-0.201** (0.088)	-0.229** (0.088)	-0.204** (0.088)
Local ridge	0.418*** (0.123)	0.368*** (0.117)	0.420*** (0.123)	-0.209** (0.088)	-0.240*** (0.089)	-0.213** (0.088)
Kernel MHD	0.515*** (0.119)	0.421*** (0.106)	0.480*** (0.119)	-0.191** (0.091)	-0.161* (0.086)	-0.175* (0.091)
Average	0.439*** (0.120)	0.424*** (0.111)	0.464*** (0.120)	-0.211** (0.085)	-0.218** (0.087)	-0.203** (0.085)

Notes: \*, \*\*, and \*\*\* indicate significance at the 10, 5, and 1% levels, respectively. Bootstrapped standard errors are in parentheses. Control variables are described in Section 2.1.

this study relies on distinct and carefully matched controls for each of the two subgroups. This approach avoids the risk of downwardly biased results by taking into account that taxpayers who were found to be compliant might have differed in relevant ways from those who were found to be noncompliant, so that their subsequent reporting behavior would have differed even in the absence of the audits.<sup>10</sup>

What, then, drives the decline in reported income among audited taxpayers who were found to be compliant? There are several plausible explanations. First, the observed reduction in reported income might be attributable to dishonest taxpayers whose misreporting was not detected during the audit. Such taxpayers might infer that audits are ineffective and therefore choose to even more aggressively understate their income in subsequent years. Conversely, the effect might be driven by overly compliant taxpayers who correct their reporting behavior in response to an audit. Such taxpayers might learn that they have overpaid their taxes and thus report less income in subsequent years. However, as a majority of the audits had not closed before the start of the next filing season, many taxpayers would not have been certain what the outcome of the audit would be when filing their TY2008 return. Therefore, the above explanations seem to be better candidates for understanding the longer-term rather than immediate change in taxpayer reporting behavior. An alternative reason for the decline in reported income that began in TY2008 is that experiencing coercive enforcement activity reduces tax morale, erodes trust, and crowds out the intrinsic motivation to comply among honest taxpayers (Lederman 2018). Recent work explores this

10 Gemmell and Ratto (2012) over predict the average amount that noncompliant taxpayers would have reported in the absence of their audits, because the control group includes compliant taxpayers who do not reflect the behavior of noncompliant taxpayers in the absence of an audit. At the same time, the study underpredicts the average amount that compliant taxpayers would have reported in the absence of their audits, because the control group includes noncompliant taxpayers who do not reflect the behavior of compliant taxpayers in the absence of an audit. As a consequence, the difference-in-difference estimates are biased downward.

hypothesis (Erard et al. 2019). Finally, even if the audit experience does not affect tax morale, the examination process might lead currently compliant taxpayers to believe that the risk of a future examination is low given that no adjustments were made during the recent audit (bomb-crater effect). Based on the available data, we are unable to pinpoint which of these explanations prevails, and future work should aim to identify the behavioral drivers behind our results. For example, future studies should investigate taxpayers' perceptions of the audit experience and its effect on motivational processes. However, the observed reduction in compliance behavior suggests that there is scope for improving the efficiency of audits. On the one hand, improved targeting of noncompliant returns and an improved capacity to detect noncompliance would seem likely to improve deterrence among cheaters. On the other hand, a better understanding of the psychological impact of audits on compliant taxpayers may lead to revised examination approaches that mitigate the erosion of tax morale and maintain incentives to comply.

A central concern of any quasi-experimental study is that unobserved confounding factors may play a role in determining whether an observation is assigned as a treatment or a control. In our context, this concern is clearly justified. Ultimately, the choice of which returns to audit is at the discretion of experienced IRS examiners (classifiers). If the audit selection decision is driven in part by factors that we do not observe, but which are correlated with reported income, our estimated treatment effect may be biased. We aim at reducing the potential for such bias by accounting for a broad range of control variables, including the DIF score ventile and the prior reported values of income sources and offsets. Furthermore, given that propensity score matching does not impose a specific functional form regarding the influence of these variables on reported income, we are confident that we are able to account for most of the systematic differences between audited and unaudited taxpayers that are likely to be associated with future taxpayer reporting behavior.

An additional limitation of our analysis is that our sample period was subject to an unusually high level of economic volatility. Although both our treatment and control groups experienced the same shocks, which helps to mitigate the potential impact of these economic fluctuations, it would be useful in future work to replicate the analysis using a more stable sample period. It also would be constructive to explore the differential impact of alternative audit techniques (such as face-to-face vs. correspondence) or the differential response of low- and high-income taxpayers.

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