The Effect of Audits on Tax Compliance: Evidence from Italy

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Abstract

This paper studies the impact of audits on subsequent tax compliance on a panel of 528,540 individual Italian taxpayers observed for the 2007-2011 period. Comparing reporting behavior for audited subjects with a matched sample of non-audited taxpayers, we find that annual reported income increased on average by around 6 percent among audited taxpayers relative to untreated individuals. This result suggests that a target effect is in place. We also show that the positive effect of audits persists for a period of four years and that post-audit tax compliance is higher when taxpayers are audited more intensely. When we test for heterogeneous effects depending on the outcome of the audit, we find that the audit's target effect is in place only when evasion is detected (or it is likely to be detected). The results are robust to a variety of specifications and samples. Finally, we test for differentiated effects along the reported-income distribution and we find that the positive effect of audit is limited to low and middle reported-income taxpayers, while it turns negative at the highest deciles.

JEL Classification: H24, H26, C23

Keywords: Tax compliance, audit, tax enforcement

1. Introduction

This paper studies the impact of audits on subsequent tax compliance on a panel of 528,540 individual Italian taxpayers observed for the 2007-2011 period. Comparing reporting behavior for audited subjects with a matched sample of non-audited taxpayers, we find that annual reported income increased on average by around 6 percent among audited taxpayers relative to untreated individuals. This result suggests that a target effect is in place.

We also find evidence that the positive effect of audits persists for a period of four years and that post-audit tax compliance is higher when taxpayers are audited more intensely. The results are robust to a variety of specifications and samples.

Then, we test if audit's effect changes depending on its outcome. We find that the audit's target effect is in place only when evasion is detected (or it is likely to be detected). This implies that it is not the audit as such producing effects on individual behavior. Our results suggest that when taxpayers experience IRA's ability to detect evasion they possibly update their beliefs regarding such ability and, accordingly, they change behavior after audit because they think it is more likely that the IRA will detect non-compliance when auditing taxpayers.

Finally, we test for differentiated effects along the reported-income distribution, and we find that the positive effect of audits is actually limited to low and middle reported-income taxpayers, while it turns negative at the highest deciles. While what we observe in the data is reported income and not true income, this result might have redistributive implications.

This paper is part of a body of literature on the impact of audit on taxpayer behavior (among others, Slemrod *et al*. 2001, Kleven *et al*. 2011, DeBacker *et al*. 2015). Among these papers, ours is the first to analyze individual taxpayers' response to audits using a large administrative panel dataset.

The paper proceeds as follows. Section 2 describes the data and the tax reporting and auditing scheme in Italy. Section 3 describes the methods. Section 4 reports our main results on the impact of audit on subsequent tax behavior. Section 5 presents estimates of audit's effect by type of audit. Section 6 provides robustness checks. Section 7 analyzes post-audit tax compliance for taxpayers at different decile of the reported income distribution. Section 8 concludes.

2. Data, tax reporting and auditing scheme

Data

We analyze a panel of Italian taxpayers using data from the Tax Return Register (TRR) and the Italian Revenue Agency (IRA). The sample comprises the universe of self-employed and sole proprietorships with legal residence in three among the most populated Italian regions, namely Lombardy (located in the North), Lazio (located in the Centre) and Sicily (located in the South), accounting for around 30% of the whole Italian population. Overall, the sample includes 528,540 taxpayers observed for the 2007-2011 period, corresponding to 2,642,700 observations.

The group of taxpayers that we consider includes 'high opportunity' subjects (Slemrod et al., 2001) because we might expect that self-employment income, that is self-reported, is easier to evade than salary income, that is subject to third-party (employer) reporting.

The dataset contains detailed information on tax payments and reported income of both audited and non-audited taxpayers for each year and, for audited taxpayers, it includes data on the timing of audits. Information on a set of individual socio-demographic variables and on the main characteristics of taxpayers' economic activity (e.g. industry, number of employees, etc.) are included as well.

Our data have two limitations. First, they covers only 5 years. This implies that taxpayers that have not been audited in our sample period could have been audited just before (e.g., in 2006). Hence, it is possible that their behavior in the 2007-2011 period (especially in the first years of the period) is influenced by audits that we are not able to observe. We will perform a robustness check in order to address this limitation.

Second, our data contain only observations for taxpayers present continuously in the TRR for the whole 2007-2011 period, while we do not observe taxpayers who entered or left the register because they either started or closed their business (e.g. due to retirement or to closure caused by bankruptcy). Notice that, in any case, we should have drop the latter observations when analyzing dynamic audit's effect because taxpayers going out of business are likely different from surviving taxpayers and this would have biased our analysis of audit's long term effect (DeBacker *et al*., 2015).

To analyze the effects of enforcement actions on subsequent reporting behavior, we consider as outcome the total personal income from professional and firm activity as reported on taxpayers' tax return. Indeed, taxes on this type of income are highly subject to evasion through misreporting or underreporting income. However, given that the magnitude and the opportunity of evasion differs widely across types of income and deductions, we will use other outcomes in the robustness check section. Specifically, we will check if our results hold considering total before-tax income, taxable income and net tax. Moreover, in order to understand better the channels through which taxpayers adjust their behavior after an audit, we consider as outcome also total reported revenues.

Reporting and auditing mechanism

In Italy, individual taxpayers are required to pay taxes yearly on all personal incomes earned in each tax year. The tax year is based on the calendar year. Incomes earned in a given fiscal year have to be reported between July and November of the following calendar year. For instance, incomes earned between January 1st and December 31st of year *t* have to be reported between July and November of year *t+1*.

According to the IRA definition, a '*year t+1'* audit is an audit carried on between July 1st of year *t* and June 30th of year *t+1*. Figure 1 summarizes the reporting and auditing time-structure.

Given the above described mechanism, the first effect of a '*year t+1'* audit might be observed on reporting behavior relative to tax year *t*, whose incomes are declared between July and November of year *t+1* (after the closure of '*year t+1'* audit).

However, notice that in case the '*year t+1'* audit is carried on between July and November of year *t*, it could potentially affect also reporting behavior relative to tax year *t-1* (whose income is reported between July and November of year *t*). In view of this, we will check for the presence of audit's effect also on reported income of tax year *t-1*, although in this case the possibility to adjust tax behavior is limited (e.g. all invoices have already been issued) given that tax year *t-1* would be concluded at that time.

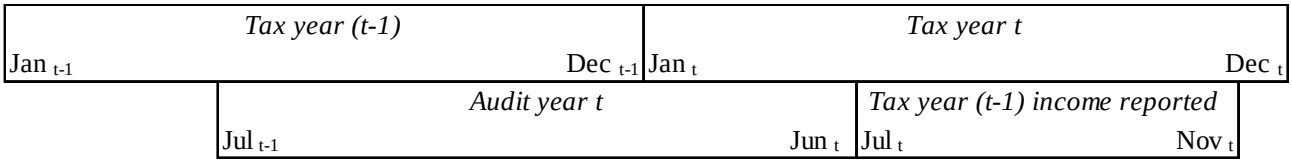

Fig. 1. Reporting and auditing time-structure

3. Empirical strategy

When estimating to what extent taxpayers adjust their tax compliance behavior in response to an audit, we have to consider that audits (the treatment) may be not completely randomly assigned. The IRA select specific taxpayers to be audited based on characteristics that are likely correlated with the outcome (reported income) but that we are not able to observe.

In general, it is likely that the IRA chooses subjects with a higher audit's expected net return. Selection can be based on taxpayer's income or industry, or the Agency can select taxpayers according to the distance between the amount of income reported in the tax return and the benchmark established by the same IRA.[1] In addition, we have to consider that the auditing policy concerning the specific characteristics of the taxpayers to be audited (e.g. in terms of industry or geographical location) is established at local level by the provincial IRA offices. Finally, we can expect that the selection is based also on unobserved variables. Overall, whatever the factors driving the selection process, this latter can bias correlations of audits and taxpayers' tax compliance.

The panel structure of our data, with information on both pre-treatment and post-treatment periods, enables identification of the causal effect of the audit tax-enforcement policy. Our identification strategy relies on difference-in-difference comparison with ex-ante matched samples of non-audited taxpayers. We compare changes in outcome between taxpayers who received the audit (the treated group) and similar (matched) taxpayers who were not subject to the audit (the control group). Indeed, if audited and non-audited taxpayers differ along both observable and unobservable characteristics, estimation that joins ex-ante matching on observable characteristics and fixed-effects to account for time invariant unobserved factors produces more reliable estimates than matching alone (Smith and Todd, 2005).

To match treated and control observations, we use the nearest-neighbor matching algorithm implemented by Abadie *et. al* (2004): for each taxpayer exposed to the treatment (i.e. audited), we identify the closest unaudited

[1] The IRA, through the *Studi di settore* (SDS) mechanism, computes for each taxpayer a presumptive income based on the estimated productivity of each input (principally labor and capital goods). Input productivity is estimated on a subset of taxpayers structurally similar to the taxpayer under consideration.

taxpayers based on gender, industry (classification based on 21 ATECO groups), province (i.e. the geographical level at which the auditing policy is established) and age deciles in 2007, which is the beginning of our period of analysis. The whole of these latter represent the control group. Given the large sample available, we could perform exact matching on the above variables to select control taxpayers. As a robustness check, we will provide results obtained on the full sample of untreated taxpayers as well.

Overall, in our sample we have 21,231 treated taxpayers that were audited in any of the 2007-2011 years. Considering that the matching algorithm can match the same untreated taxpayer to more than one audited taxpayer, after dropping unmatched observations the control group is composed of 464,525 distinct taxpayers.

Table 1

Summary statistics for audited and matched non-audited taxpayers, 2007

	Audited		Matched non-audited	
	Mean	Std dev	Mean	Std dev
Outcomes				
Personal income from professional and firm activity	42546	*98269*	38051	*81350*
Taxable income	41905	*109499*	34854	*92359*
Gross income	38001	*88018*	33806	*72838*
Total revenues	72407	*274731*	137745	*694260*
Net tax	13003	*46331*	10009	*39043*
Age	47.3	*11.510*	46.2	*11.246*
Male	79.77		79.62	
Industry share				
Agriculture, hunting and fishing	3.35		6.46	
Mining and quarrying	0.03		0.00	
Manufacturing	6.52		6.21	
Electricity, gas, steam and air conditioning supply	0.00		0.00	
Water supply; sewerage; waste management and remediation activities	0.16		0.02	
Construction	11.60		13.67	
Wholesale and retail trade; repair of motor vehicles and motorcycles	29.32		27.16	
Transporting and storage	4.61		2.50	
Accommodation and food service activities	7.58		3.94	
Information and communication	1.08		1.30	
Financial and insurance activities	1.71		0.91	
Real estate activities	1.35		0.79	
Professional, scientific and technical activities	20.62		26.19	
Administrative and support service activities	2.92		2.16	
Public adm and defense; compulsory social security	0.00		0.00	
Education	0.34		0.09	
Human health and social work activities	3.43		3.86	
Arts, entertainment and recreation	2.14		1.44	
Other services activities	3.23		3.30	
Activities of extraterritorial organizations and bodies	0.00		0.00	
Region				
Lombardy (North)	35.18		53.00	
Lazio (Centre)	34.82		26.54	
Sicily (South)	29.99		20.46	
Nr taxpayers	21,231		464,525	

Summary statistics for audited and matched non-audited groups are reported in Table 1 for 2007. The means for the two groups are very close as regards age and gender distribution. Audited taxpayers are relatively less concentrated in Lombardy, the northern region, than in both the Centre and Southern regions. Audits are relatively more frequent in specific industries. Considering the most represented industries, audits occur more often in the wholesale and retail, transport, accommodation and food service activities and manufacturing sectors.

The average pre-treatment income from professional and firm activity, the taxable income, the gross income and the average net tax paid is higher for audited taxpayers, suggesting that audits are actually not random. Specifically, it seems that the IRA tend to audit more often taxpayers with higher levels of reported income. DeBacker et al. (2015) show the same evidence on US corporations.

After selecting the control group according to the procedure described above, we explore the relationship between audits and reporting behavior. To assess the average impact of audits on subsequent tax compliance, we estimate the relative change in reported income before and after the audit comparing audited taxpayers and the matched control sample of non-audited taxpayers estimating the following equation:

$$Y_{itj} = \beta_1 TREATED_{i,t+1} \times Post_t + \alpha_i + \tau_t + \sigma_j + \gamma_1 X_{itj} + \varepsilon_{itj} \qquad [1]$$

Where Y_{itj} measures personal income as reported by taxpayer *i* in industry *j* in year *t* and $TREATED_{i,t+1}$ is a dummy equal to one for taxpayers *i* that has been audited in year *t+1* (the effect of an audit carried on in a given year starts from the previous year tax return, see Figure 1). The effect of audits on tax compliance is captured by β_1, the coefficient for the interaction term between treated taxpayers and the post-audit period ($Post_t$). The terms α_i, τ_t and σ_j are individual, year and industry fixed effects, respectively, and *X* is a vector of time-varying taxpayers' characteristics, including a set of dummies for the type of tax preparer and the reference income computed according to the *SDS* mechanism. ε is an error term.

The sample period is from 2007 to 2011. The inclusion of individual, year and industry fixed effect should ensure that our comparisons across treatment groups over time is not influenced by group specific characteristics.

The next step is estimating the dynamic effect of audits. More specifically, we want to assess if and how the audit's impact on reported income changes over time. To this purpose, we change slightly the previous specification and estimate the following equation:

$$Y_{itj} = \beta_2 TREATED_{i,t+1} \times Nr\_years_{it} + \alpha_i + \tau_t + \sigma_j + \gamma_2 X_{itj} + \varepsilon_{itj} \qquad [2]$$

where *Nr_years* is a continuous variable measuring the number of years elapsed since the audit potentially started producing its effect.

In order to account for potential non-linearity in the dynamic effect of audits, we also estimate a non-parametric version of equation [2] by substituting the continuous variable *Nr_years* with five dummy variables, one for each year after the audit commences exhibiting its impact.

A final specification allows the effect of audits to be different depending on the intensity of the treatment. We consider two measures of intensity. The first regards the number of audits that have been carried on. We consider as more intensely treated taxpayers those who receive multiple audits. In this case, we test for differentiated effects of subsequent audits.

In our data, taxpayers receive four audits at most (although potentially they could be audited up to five times). However, given that there are very few taxpayers audited three or four times (respectively, thirteen and one individual), we compare taxpayers audited once with those audited twice or more. In order to test whether tax compliance changes differently after the first and the following audits, we estimate the following equation:

$$Y_{itj} = \beta_I TREATED_{i,t+1} \times Post_{I,t} + \beta_{II} TREATED_{i,t+1} \times Post_{II,t} + \alpha_i + \tau_t + \sigma_j + \gamma_1 X_{itj} + \varepsilon_{itj} \qquad [3]$$

Where $Post_{I,t}$ and $Post_{II,t}$ are indicator variables for, respectively, post-first and post-subsequent audits periods. By comparing estimates of β_I and β_{II} we can assess whether and how tax compliance changes when the taxpayer receive multiple audits.

Finally, we consider a different measure of treatment intensity, namely the number of tax returns that are audited within a single audit episode. In order not to confound the '*number of audits*' with the '*number of audited tax returns*' effect, we restrict the analysis to treated taxpayers who have been audited once.[2] We compare the relative change in outcome among taxpayers for whom one, two, and three or more tax returns have been audited with respect to non-audited taxpayers estimating the following equation:

$$Y_{itj} = \sum_j \beta_j TREATED\_j_{i,t+1} \times Post_t + \alpha_i + \tau_t + \sigma_j + \gamma_1 X_{itj} + \varepsilon_{itj} \qquad [4]$$

Where *j*=1, 2 and 3 refers to the number of tax returns that are checked upon a single audit episode.

Notice that another measure of treatment intensity would be related to the monetary amount that the taxpayer is required to pay in addition to what he/she has already paid. However, our data does not contain such information.

4. Audit's effect on tax compliance

In this section, we report our main empirical findings presenting estimates of different audit's effects based on equations [1] to [4]. Although our sample-period is short and characterized by a very low inflation rate, we

[2] Although restricting the analysis to taxpayers audited once could bias results, the latter group accounts for the vast majority of the treated group (20,437 taxpayers, corresponding to 96.26% of audited taxpayers).

deflate all nominal values to 2011 euros. In all specifications, standard errors are clustered at taxpayer level to consider within period correlations in the error term.

4.1 Average audit's effect

Estimates of average audit's effect based on equation [1] are reported in Table 2. The first two columns show unconditional and conditional OLS results; column 3 and 4 shows difference-in-difference fixed-effect results. While unconditional OLS estimates are statistically non-significant, when adding controls a positive relationship between audit and reported income emerges.

When we use fixed effect estimator (columns 3 and 4), we find that the impact of audit is still positive, indicating that reported income increases after an audit: fixed effect estimates with controls for individual time-varying characteristics shows that annual reported income grows on average by around 6.8 percent among audited taxpayers relative to the change in reported income in untreated individuals.

The positive impact of audits suggests that a target effect is in place: when taxpayers experience an audit, they revise upward their perceived audit probability and, as a result, they increase their subsequent tax compliance.

Table 2
Average audit's effect

	OLS	Conditional OLS	FE	FE
VARIABLES				
Post audit	-0.0021	0.0989***	0.0598***	0.0679***
	(0.0078)	(0.0129)	(0.0052)	(0.0052)
Age		0.0474***		0.1035***
		(0.0008)		(0.0016)
Squared age		-0.0004***		-0.0013***
		(0.0000)		(0.0000)
Reference income		0.0000***		0.0000*
		(0.0000)		(0.0000)
Tax preparer 1		-0.0328***		-0.0074
		(0.0064)		(0.0067)
Tax preparer 2		-0.0407*		-0.0093
		(0.0239)		(0.0131)
Tax preparer 3		-0.0316***		-0.0139*
		(0.0088)		(0.0075)
Constant	10.1158***	8.0290***	9.7453***	7.8573***
	(0.0012)	(0.0360)	(0.0408)	(0.0554)
Individual time varying characteristics	NO	YES	NO	YES
Year FE	NO	YES	YES	YES
Industry FE	NO	YES	YES	YES
Individual FE	NO	NO	YES	YES
Observations	2,058,826	1,938,857	1,938,857	1,938,857
R-squared	0.0000	0.1792	0.0078	0.0182
Number of id			439,122	439,122

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

Notice that the fixed effect estimate of β_1 (6.8 percent) is considerably lower than the OLS value (9.9 percent), suggesting that treatment assignment is actually non-random and that OLS provides upward biased results. More specifically, the drop of β_1 suggests that taxpayers with higher reported income are more likely to be audited. When controlling for the non-randomness of treatment assignment through fixed effect estimator, the registered audit's effect decreases.

4.2 Dynamic audit's effect

The dynamic effect of audits is represented by β_2 in equation [2], the coefficient for the number of audits received. From now on, we present only difference-in-difference fixed effect results while omitting the OLS coefficients because we have verified that they are biased by non-random assignment of audits.

Table 3
Dynamic audit's effect

	(1)	(2)
VARIABLES		
Nr of year after the audit	0.0464***	
	(0.0036)	
I year effect		0.0514***
		(0.0055)
II year effect		0.1129***
		(0.0086)
III year effect		0.1107***
		(0.0183)
IV year effect		0.0952***
		(0.0337)
V year effect		0.0088
		(0.0984)
Constant	7.8603***	7.8616***
	(0.0554)	(0.0554)
Individual time varying characteristics	YES	YES
Year FE	YES	YES
Industry FE	YES	YES
Individual FE	YES	YES
Observations	1,938,857	1,938,857
R-squared	0.0183	0.0183
Number of id	439,122	439,122

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

The positive estimate for β_2 reported in column 1 of Table 3 indicates that the effect of audit is increasing over time (4.6 percent per year). Column 2 of Table 3 shows coefficients of the dummies for the different number of years for which the audit potentially produces its effect. In our sample period, we can observe effects for five tax years at most.

In the first year following an audit, taxpayers on average increase their reported income by around 5 percent more relative to non audited taxpayers. In the following three years, the audit effect is higher (around 10%), while the audit effect seems to disappear in the fifth year.

The lower first year effect is likely related to the temporal structure of the auditing mechanism (see Figure 1): year *t* audit may occur starting from July of tax year *t-1*, when half of the tax year has already passed. Put differently, the audit is conducted after at least half of the tax year *t* is completed. This limits the possibility to change behavior in response to the audit only to the second part of the fiscal year at most because, for the first part of the year, tax behavior with tax consequences had already been carried out. For the subsequent tax years, instead, audited taxpayers have more possibility to adjust their behavior.

Finally, the vanishing of audit's effect suggests that taxpayers stop considering themselves targets of audits after five years.

4.3 Intensity of treatment effect

Our final specifications in equations [3] and [4] allow testing how compliance responds to different intensity of treatment. Specifically, equation 3 allows for different effects of audits depending on the number of times that taxpayers have been audited. In this specification, we compare the change in outcomes between taxpayers who has never been audited with those who have been audited once and with those who have been audited twice or more.

Results in Table 4 shows that actually taxpayers increase more their reported income when they are more intensely treated: the relative increase in income is 6.7 percent for subjects audited once, while it is around 10 percent when they are audited twice or more.

Our next analysis shows how the reported income changes after an audit depending on the number of tax returns (i.e. the number of fiscal years) that are checked upon an audit. In this case, we limit our analysis to taxpayers audited once in order not to confound the two measures of treatment intensity.

Results in column 2 of Table 4 shows that, on average, the relative increase of reported income is by around 5 percent *per* tax return. In column 3, we compare the relative change in outcome among taxpayers for whom one, two and three or more tax returns have been audited with respect to non-audited taxpayers.

Results confirm that treatment intensity matters: the increase in outcome is around 6 percent when only one tax return is audited, while it is by around 9 and 14 percent, respectively, in case two or more than two tax returns are checked.

Table 4
Audit's effect by intensity of treatment

	(1)	(2)	(3)
VARIABLES			
Audited once	0.0672***		
	(0.0052)		
Audited twice or more	0.1005***		
	(0.0292)		
Nr of audited tax retunrs		0.0548***	
		(0.0042)	
1 audited tax return			0.0615***
			(0.0057)
2 audited tax returns			0.0924***
			(0.0140)
3 or more audited tax returns			0.1442***
			(0.0413)
Constant	7.8574***	7.8542***	7.8550***
	(0.0554)	(0.0554)	(0.0554)
Individual time varying characteristics	YES	YES	YES
Year FE	YES	YES	YES
Industry FE	YES	YES	YES
Individual FE	YES	YES	YES
Observations	1,938,857	1,935,851	1,935,851
R-squared	0.0182	0.0183	0.0183
Number of id	439,122	438,400	438,400

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

5. Robustness

5.1 The common trend assumption

To test whether other possible unidentified differences between treated and control taxpayers might affect how audited taxpayers respond to audits, we estimate equation [1] using data from the before-audit period of each audited taxpayer.

More specifically, we compare changes in outcomes between treated and control individuals in the periods before the audits (i.e. changes between *t-4* and *t-3*) to test for the common trend assumption. In Table 5, we compare different pre-treatment periods: 2007 *versus* 2008, 2008 *versus* 2009, 2007 *versus* 2008 and 2009 and 2007 and 2008 *versus* 2009. In every case, we exclude both the *t* and the *t-1* periods because we want to be sure to exclude even partially treated periods.

Whatever the before-treatment period that is considered, results show that the placebo post-audit variable is closed to zero and it is never statistically significant. The lack of a difference in these different earlier periods support our hypothesis that the relative change in reported income between audited and non-audited taxpayers from the pre- to the post-audit period actually reflects the impact of the audit.

Table 5
Test for the common trend assumption

Pre-treatment period	2007/2008	2008/2009	2007-2008/2009	2007/2008-2009
VARIABLES				
Placebo post audit	0.0051	0.0092	0.0051	-0.0067
	(0.0058)	(0.0073)	(0.0070)	(0.0068)
Constant	8.0229***	9.7672***	8.9178***	8.9135***
	(0.1372)	(0.1407)	(0.0840)	(0.0840)
Individual time varying characteristics	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES
Observations	807,744	803,030	1,192,942	1,192,942
R-squared	0.0177	0.0147	0.0197	0.0197
Number of id	424,956	419,732	424,882	424,882

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

5.2 Different outcomes

We check if our main results of Section 4 hold for different outcome variables. First, we consider gross before-tax income. In addition to income from professional and firm activity, it includes other sources of income like those subject to property tax, rental tax or land value tax. These latter should be less easily subject to evasion because the amount to be paid is computed on the basis of fixed parameters indicated in the real estate registry. Second, we test our main results using taxable income as outcome, after subtracting tax deductions (for instance compulsory social security contributions) from before-tax income. Finally, we use as outcome the value of the net tax. Table 6 shows that our main results do not change.

Two findings in Table 6 are worth emphasizing. First, when considering gross income, estimates are almost identical to results obtained when personal income from professional and firm activity is considered. The difference between the two latter outcomes is largely made of incomes subject to taxes that are computed using fixed parameters established by the law and levied on register values (e.g. house tax or land-value tax). Hence, although these incomes are self-reported and not subject to third-party reporting, they are less easy to evade than professional and firm incomes. Results in column 1 of Table 6 shows that the behavior adjustment after an audit comes entirely from more evadable professional and firm activity incomes. This result is in line with Kleven *et al.* (2011) that find that tax evasion rate is close to zero for income subject to third-party reporting, but substantial for self-reported income.

Second, we find a small but negative effect on total revenues. Given the overall positive effect, this result suggests that taxpayers tend to change more their reporting behavior relative to the cost component of their income than the revenues one.

Table 6
Audit's effect on different outcomes

	Gross income	Taxable income	Net tax	Total revenue
VARIABLES				
Post audit	0.0672***	0.0774***	0.0999***	-0.0099**
	(0.0049)	(0.0049)	(0.0071)	(0.0043)
Constant	8.0542***	9.0112***	7.9757***	9.5532***
	(0.0539)	(0.0497)	(0.0678)	(0.0609)
Individual time varying characteristics	YES	YES	YES	YES
Year FE	YES	YES	YES	YES
Industry FE	YES	YES	YES	YES
Individual FE	YES	YES	YES	YES
Observations	2,058,965	2,062,012	1,835,399	1,575,081
R-squared	0.0190	0.0077	0.0050	0.0340
Number of id	439,442	440,322	427,217	399,348

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

5.3 Additional robustness check

In this section, we perform additional robustness checks. First, given that we observe taxpayers only for the 2007-2011 period, we can't exclude that they have been audited slightly before the beginning of our period of observation, for instance in 2006. Given that we showed that audit's effect persist over time, we risk classifying as non-treated some taxpayers for whom previous audit's effect might be still be underway. Moreover, it is possible that we classify as first audit a second or successive audit.

In view of this, we replicate estimation restricting the sample to taxpayers audited first in 2012. In light of our previous results showing that audit's effect tend to vanish after five years, in this way we are quite confident that for the latter audited taxpayers there should be no effect of potential pre-2007 audits yet. Results are in the first column of Table 7 and they confirm a positive (first year) audit's effect, with a 4 percent increase in reported income among audited taxpayers relative to the change in reported income in untreated individuals.

Second, given the time structure of the auditing and reporting scheme described in Figure 1, it is possible that a '*year t+1 audit*' produces some effect also on reporting behavior relative to tax year *t-1* (although in this case the possibility to adjust tax behavior is partial given that tax year *t-1* is already concluded at the time of the audit). We replicate estimates considering tax year *t-1* as the first year on which the effect of a year *t+1* audit could show its effect. Indeed, results in column 2 confirms that a positive audit's effect is in place (+5.4 percent), although it is smaller as compared to when we consider *t* as the first year after audit (+6.8 percent, see Table 2, column 3).

Finally (column 3), we replicated estimates without matching, considering as control group the whole sample of untreated taxpayers. Also in this case the positive audit's effect is confirmed (+6.6 percent) and it is similar to our main results.

Table 7
Additional robustness check

	(1)[a]	(2)[b]	(3)[c]
VARIABLES			
Post audit	0.0405***	0.0549***	0.0662***
	(0.007)	(0.005)	(0.005)
Individual time varying characteristics	YES	YES	YES
Year FE	YES	YES	YES
Industry FE	YES	YES	YES
Individual FE	YES	YES	YES
Constant	7.8543***	7.8491***	7.847027
	(0.0557)	(0.0554)	(0.0519)
Observations	1,905,106	1,938,857	2,082,239
R-squared	0.0185	0.0182	0.0177
Number of id	431,205	439,122	471,993

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1
a. Estimates only on taxpayers audited first in 2012
b. Estimates when considering t-1 as first year after audit
c. Estimates on the whole control group (also unmatched)

6. Audit's effect by audit's outcome

In this section, we test if audit's effect changes depending on the kind of audit that is carried on. More specifically, we classify audits in three categories depending on their outcome.

The first category refers to audits for which the IRA have found a positive evasion that has been, at least to some extent, recognized by the taxpayer. This latter has then decided to pay immediately at least part of the taxes levied on the claimed additional taxable income and/or the sanction; we classify these audits as "*verified evasion*" audits.

The second category refers to audits for which the IRA did not find positive evasion and for which the IRA itself has interrupted the audit procedure either because the taxpayer has provided elements to justify his /her behavior or because the IRA has self-assessed the implausibility of its own claims. We classify these audits as "*no evasion*" audits.

Finally, there are audits whose outcome is uncertain, since the IRA has decided to pursue the procedure further but the taxpayer has not recognized the soundness of the Revenue Agency's claim; the majority of these cases are legal issues to be settled by the tax tribunals. We classify these audits as "*uncertain outcome*" audits.

Results on the audit's impact separately for the three kinds of audits are in Table 8. They show that in both the case of "*verified evasion*" audits and the case of "*uncertain outcome*" audits the effect is positive and significant, while we find no effect in the case on "*no evasion*" audits. This result implies that the audit's effect is positive conditional upon the audit not being surely ineffective on legal bases. Moreover, the fact that the audit's target effect is in place only when evasion is detected (or it is likely to be detected) implies that it is not

the audit as such producing effects on individual behavior. Our results suggest that when taxpayers experience IRA's ability to detect evasion, they possibly update their beliefs regarding such ability and, accordingly, they change behavior after audit because they think it is more likely that the IRA will detect non-compliance when auditing taxpayers.

Note also that the "*no evasion*" audits do not produce a 'fire back' effect: the audited taxpayer does not seemingly react by increasing tax evasion to offset the loss of time (and money) caused by the audit.

Table 8
Audit's effect by type of audit

	(1)
VARIABLES	
Verified evasion	0.0676***
	(0.0064)
No evasion	-0.0008
	(0.0161)
Uncertain evasion	0.0781***
	(0.0182)
Individual time varying characteristics	YES
Year FE	YES
Industry FE	YES
Individual FE	YES
Observations	1,924,269
Number of id	435,652
R-squared	0.0183

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

7. Audit's effect by level of reported income

In this section, we test whether audit's effect is different at different deciles of the reported income distribution. To this aim, we estimate equation [1] interacting the post-audit dummy with income deciles. The results are reported in Table 8 and illustrated in Figure 2.

They clearly show that audit's effect is higher at the lowest deciles. The effect decreases monotonically up to the fourth decile, where the audit's effect is around 6 percent. At the fifth and sixth decile, β_1 is not statistically significant, while at the eighth, ninth and tenth decile the effect turns negative and statistically significant.

In the same figure, we show estimates of equation [1] using income levels instead of log income as outcome. It can be observed that after an audit, on average, the increase of reported income is considerably higher at the lowest deciles: around 6.4 thousand euros more at the first decile and 10 thousand less at the highest decile.

These latter results suggest that the average positive audit's effect that we detected before is actually driven by low and middle reported-income taxpayers, while the effect is even negative at the highest deciles. Moreover, although what we observe in the data is reported income and not true income, this result might have redistributive implications.

Table 9
Audit's effect by income decile

	Log of reported income	Reported income
VARIABLES		
I decile	0.4322***	6,467.92***
	(0.0179)	(400.43)
II decile	0.1632***	4,782.95***
	(0.0146)	(284.23)
III decile	0.0822***	4,629.51***
	(0.0156)	(398.26)
IV decile	0.0601***	4,232.50***
	(0.0147)	(359.43)
V decile	0.0077	4,192.28***
	(0.0152)	(471.37)
VI decile	0.0171	3,665.59***
	(0.0141)	(487.47)
VII decile	-0.0315**	3,548.79***
	(0.0148)	(572.16)
VIII decile	-0.0582***	3,223.45***
	(0.0146)	(581.89)
IX decile	-0.0773***	1,947.36***
	(0.0156)	(710.55)
X decile	-0.1020***	-10,262.73***
	(0.0142)	(2,426.69)
Constant	7.8836***	-56,847.06***
	(0.0553)	(2,381.62)
Individual time varying characteristics	YES	YES
Year FE	YES	YES
Industry FE	YES	YES
Individual FE	YES	YES
Observations	1,938,857	1,938,857
R-squared	0.0195	0.02
Number of id	439,122	439,122

Robust standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

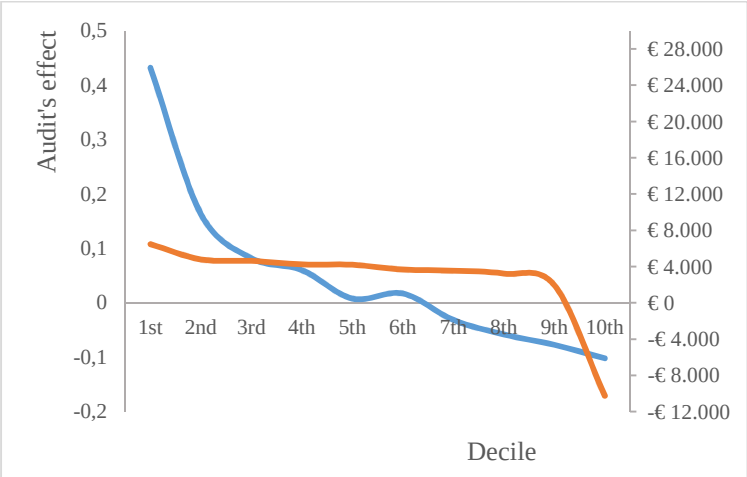

Fig. 2. Audit's effect by income decile

8. Conclusions

This paper studies the impact of audits on subsequent tax compliance. We find that, on average, annual reported income increased on average by around 6 percent among audited taxpayers relative to untreated individuals. Moreover, we show that the positive effect lasts for four years after the audit and that taxpayers increase more their reported income when they are more intensely treated. The positive impact of audits suggests that a target effect is in place.

When we test for heterogeneous effects depending on the outcome of the audit, we find that the audit's target effect is in place only when evasion is detected (or it is likely to be detected). This implies that it is not the audit as such producing effects on individual behavior, but it is the change in taxpayers' beliefs relative to IRA's ability to detect evasion that induces a higher post-audit tax compliance.

Finally, we test for differentiated effects along the reported-income distribution, and we find that the positive effect of audits is actually limited to low and middle-income taxpayers, while it turns negative at the highest deciles.

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