The effect of tax audits on Swedish small and medium-sized firms' compliance: evidence from random audits*

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Abstract

This paper studies the direct deterrent effect of random tax audits on Swedish small and medium-sized firms' subsequent compliance. Using rich administrative data from the Swedish Tax Agency, it is shown that the audits lead to an increase in compliance, measured as final tax paid, during at least three subsequent years. Both the magnitude and the dynamics of the effect vary considerably across different groups of firms. Among limited companies, the mechanical effect (i.e., the direct tax revenue from the audits) during the year of audit is not statistically significant, but is both statistically and economically significant during the years after the audit. For sole proprietorships, the result is the opposite: a statistically and economically significant effect during the audit year and no effects on compliance behaviour later on. To further study the mechanisms driving the effect, I investigate the roles of firm age at the time of audit and whether or not the firm had an auditor one year prior to the tax audit. The results show that the effect is driven by firms who were founded at least five years prior to being audited. Moreover, a separate analysis among limited companies reveals that the effect is found only among firms who employed an auditor prior to being randomly selected for a tax audit. Finally, a cost-revenue analysis shows that the tax revenue increase due to the audits on average notably exceeds the costs, consisting of estimates of the direct labor costs and an overhead.

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1 Introduction

This paper studies whether random audits have an impact on Swedish small and medium-sized firms subsequent compliance behaviour, contributing to a recent and growing literature on the direct deterrent effect of audits.[1] The study is related to a large body of research on the role of tax audits for compliance.

Tax audits are an important part of a tax authority's toolbox and can be useful in at least three ways. First, they can be used to estimate the tax gap, which is of great policy interest in many countries. A well-known and long-lasting audit program is the US Internal Revenue Service's Taxpayer Compliance Measurement Program (TCMP), which involved comprehensive random audits of taxpayers from 1968 to 1988. In 2001, the National Research Program (NRP) resumed the work of the TCMP (Dubin, 2012).

Apart from *measuring* the tax gap, tax audits can also play an important role in *affecting* it. Thus, second, a higher audit rate can improve compliance through an increase in the perceived probability of noncompliance detection. This theory, introduced by Allingham and Sandmo (1972), has found considerable empirical support. Initially, the predictions from the theory were confirmed in a series of laboratory experiments.[2] In addition, starting in the early 1990s, data from the TCMP were used in several observational studies to measure the impact of the audit rate on compliance, and gave further support to Allingham and Sandmo's theory prediction.[3] In more recent years, a growing number of field experiments have estimated a positive effect of a higher detection probability on tax compliance, by manipulating the perceived audit rate in various ways.[4]

Third and finally, there has been a surge of observational studies of the direct deterrent effect of an audit on the subsequent tax compliance. This recent literature is in part made possible by the increased availability of high-quality data from various administrative registers, as the research questions typically require longitudinal micro-level data. An early contribution was made by Gemmel and Ratto (2012) who used UK data from random audits to estimate the effect of being audited on the subsequent compliance. The study found no average effect. In a heterogeneity analysis, the subsequent compliance of

[1] I use the terminology in Alm et al. (2009), where the direct deterrent effect is contrasted to the indirect deterrent effect on individuals not actually audited.

[2] In an early experiment, Spicer och Thomas (1982) varied the audit probability and found a positive relationship between the audit rate and compliance. A series of lab experiments have since then replicated these initial results: Becker et al. (1987); Webley (1987); Alm et al. (1992b, 1992c, 1992d, 1995, 1999, 2017); Fortin et al. (2007); Cummings et al. (2009); Kastlunger et al. (2009); Calvet and Alm (2014); Soliman and Cullis (2014); Casagrande et al. (2015); Tandon och Kavita Rao (2017). The above mentioned experiments were conducted in various countries: US, UK, France, Canada, Germany, Spain, Egypt, and India.

[3] Dubin et al. (1990) used variation in the audit rate across US states to measure the relationship between detection risk and compliance, and found a strong positive relationship. Similar results using essentially the same data can be found in Tauchen et al. (1993), Plumley (1996) och Dubin (2007).

[4] See Slemrod et al. (2001), Kleven et al. (2011), Fellner et al. (2013) (albeit for TV-licence fees), Ortega and Sanguinetti (2013), Pomeranz (2015), Shimeles et al. (2017), Bott et al. (2017), Boning et al. (2018), Meiselmann (2018).

taxpayers who were found to be compliant was found to be reduced, while the opposite was observed for those who were found to be noncompliant. Beer et al. (2015) found similar results using data from the US.

A recent and closely related study to the present one is Advani et al. (2019), who estimate the direct deterrent effect by making use of random audits from the UK. Advani et al. find that audits raise reported tax liabilities during a period of at least five years after audit, and as documented in previous studies, that the change in compliance behaviour is driven by tax payers who were noncompliant. To study the mechanism behind the observed effect, Advani et al. make an extention of the model in Allingham and Sandmo (1972) and conclude that the observed dynamics of the effect are consistent with audits revealing information to the tax authority, which makes misreporting certain income sources easier to detect in the future.

The present study contributes to the literature in several ways. The main finding is a positive direct deterrence effect on compliance, measured as final tax paid, during at least three years after a tax audit. Estimating the effect separately for limited companies and sole proprietorships reveals substantial differences in the dynamics of the effect. For limited companies, the mechanical effect (i.e., the direct tax revenue from the audits) during the year of audit is not statistically significant, but is both statistically and economically significant during the years after the audit. For sole proprietorships, the result is the opposite: a statistically and economically significant effect during the audit year and no effects on compliance behaviour during the subsequent years. To further study the mechanisms driving the effect, I assess the role of when during its life cycle a firm is randomized into audit. The results clearly indicate that the direct deterrence effect is in practice found only among firms who were founded at least five years prior to being audited, or in other words, among reasonably well established firms. In addition, in a separate analysis among limited companies, I find that the effect of a tax audit on compliance is largely driven by firms who employed an auditor prior to being randomly selected for a tax audit by the Swedish Tax Agency. Finally, the study introduces a novelty to this field of research in comparing the cost of performing the audits to the achieved compliance improvement. To this end, I use unique data over how many work hours each audit took, along with the effect estimates. This simple cost vs. tax revenue analysis is performed under various scenarios and for different firm types. Under reasonable assumptions, the results show that the tax revenue increase resulting from the audits on average notably exceeds the cost, where the latter consists of the direct labour cost and an overhead.

The rest of the paper proceeds with a section describing the random audits, data, and the chosen method. The results from the estimation of the direct deterrence effect can be found in section 3. The cost vs. tax revenue analysis is presented in section 4, and section 5 concludes.

Table 1: Number of treated (T) and untreated (C) firms

	2014		2015		2016	
Stratum	T	C	T	C	T	C
Ltd. 1: wage sum missing	42	22,952	98	24,769	14	25,751
Ltd. 2: wage sum 1K-600K, non-complex	53	31,843	87	33,016	18	33,797
Ltd. 3: wage sum 1K-600K, complex	40	14,365	73	15,808	14	16,750
Ltd. 4: wage sum 600K-3M, non-complex	44	14,607	82	14,730	19	14,704
Ltd. 5: wage sum 600K-3M, complex	82	16,829	150	17,782	36	18,712
Ltd. 6: wage sum 3M-10M	70	9,568	121	9,981	35	10,361
Ltd. 7: wage sum >10M	37	3,040	79	3,147	22	3,300
SP 1: revenue 100K-600K, non-complex	30	25,893	30	26,165	30	25,274
SP 2: revenue 100K-600K, complex	39	21,225	31	21,399	40	20,903
SP 3: revenue 600K-3M, non-complex	17	8,093	20	8,121	18	8,238
SP 4: revenue 600K-3M, complex	52	17,828	52	17,900	58	17,671
SP 5: revenue >3M	17	2,580	23	2,680	24	2,785
Sum	523	188,823	846	195,498	328	198,246

2 Data and method

This study uses register data from the Swedish Tax Agency's data warehouse Informationslagret (IL), along with records of randomly audited firms and the corresponding target populations for the tax years 2014–2016. A description of the sampling procedure is given in section 2.1 below, followed by a description of the empirical specification in section 2.2. Finally, a balance test between audited and non-audited firms can be found in section 2.3.

2.1 The random tax audits

The audits used in the present study are part of the Tax Agency's random audit program among small and medium-sized firms. The random audit program has been in place since 2015 (for the tax year 2014) and its goal is to measure the tax gap, i.e., the difference between the total taxes owed and the taxes paid. Both limited companies (Ltd.) and sole proprietorships (SP) are subject to audit. The sampling is performed in two stages. In the first stage, each year, firms from counties located in the middle part of Sweden with an yearly turnover of over 100,000 SEK[5] at least two years in a row and a wage cost of at most 50,000,000 SEK are divided into twelve strata.[6] The strata are defined according to wage cost for limited companies and turnover for sole proprietorships. In the second stage, a certain number of firms from within each stratum are randomly drawn to be audited. As the audited firms (*treated*) are randomly chosen, the non-audited firms

[5]About 10,500 EUR according to the average rate during the data observation window.

[6]The following counties are part of the random audit program: Dalarna, Gävleborg, Stockholm, Uppsala, Värmland, Västmanland, and Örebro.

can be used as controls in the effect estimation. The identification assumption is further discussed in 2.2.

The audits from the random audit program used in this paper constitute only a minor part of the total audits performed by the Swedish Tax Agency. The lion's share of the audits performed by the Agency are risk-based and/or targeted at various groups of tax payers. For the data used in the present study, the number of treated (T) and controls (C) per year and stratum are shown in table 1. The total number of audited firms during the studied period is 1,697 and the tax year with the highest number of audits was 2015, when the number was approximately as high as the numbers for 2014 and 2016 together (846 audits in 2015 compared to 851 in 2013 and 2016 together). A larger number of audits have been performed among limited companies than among sole proprietorships, and the share of treated in the strata with the largest firms (Ltd. 5–7 and SP 4–5) is higher than in the rest of the strata. Initially, 1,886 firms were randomly drawn, but for various reasons, about 10% got no treatment whatsoever. In other words, no action was taken by the Swedish Tax Agency toward those firms, the firms were never contacted, and were thus not aware of being randomly selected. The reasons for the 10% drop are not well documented, but include time constraints on behalf of the auditors. As the initially randomly selected but non-audited firms are not treated in any way, and are also not aware of being randomly drawn, they are not part of the treated group in the present paper and are regarded as controls. In a robustness analysis in the results section, the randomly selected for audit but non-audited firms are instead regarded as treated along with the audited firms (intention-to-treat). In a second robustness test, they are dropped from the control group.

2.2 Method

As the assignment to being audited during year t is random, the average effect k years after the audit could potentially be estimated by the sample mean difference. However, as apparent from table 1, the sample sizes are relatively small, and in addition, there is access to longitudinal data. Therefore, I follow the previous literature (Gemmel and Ratto 2012; Advani et al. 2019) to take full advantage of the available data. The following empirical specification is used:

$$Y_{ikt} = \alpha_i + \delta_k + \theta_k D_i + \gamma_t + \varepsilon_{ikt}, \tag{1}$$

where Y_{ikt} is the outcome variable for firm i measured in calendar year t and k years since the random audit, α_i are firm fixed effects, $D_i \equiv 1[i \text{ has been audited}]$ and γ_t are calendar year fixed effects. As in Advani et al. (2019), k can be defined for firms in the control group because the random audit program makes it clear when the potential year of treatment is for the controls. The parameters of interest are θ_k which measure the average treatment effect on the treated of being audited k years after the audit. For statistical inference, standard errors clustered at the firm level are used.

The identification assumption is as in a standard difference-in-differences setting: θ_k can be estimated consistently under a parallel trends assumption. In other words, in the absence of audit, the trends in Y of the treated and untreated firms would have been the same. This identification assumption is stated in terms of a counterfactual and cannot be tested directly, but should be fulfilled in the present study due to the randomized treatment. Equation (1) provides a means of testing the identification assumption informally by estimating θ_k where we know that the effect should be zero, i.e., for $k < 0$. Such placebo effects are presented along with the direct deterrence effects for $k \geq 0$ throughout the results section later in the paper.

The empirical specification takes advantage of two valuable aspects of the data: that it is longitudinal and that there are several treatment waves. The firm fixed effects α_i control for potential time-constant differences in Y at the individual level, and thereby also across treated and controls, and can be estimated because the data is longitudinal. Also, because of the several treatment waves (i.e., audit waves in the years 2014, 2015 and 2016), potential calendar-year fixed effects can be controlled for with γ_t.

2.3 Data description

The purpose of this study is to estimate the average effect of a tax audit on the future tax compliance of the audited firm, to find the types of firms driving the effect, and to compare the costs for the audits with the achieved effect on compliance. Tax compliance is a broad concept that essentially covers all ways in which the taxpayer follows the current tax rules and regulations. Because tax laws are complicated and tax filing involves many steps, there are potentially many ways to measure compliance. In the present study, compliance is measured by *final tax paid*, a change in which summarizes the net result of many potential adjustments to filing behaviour following an audit. For limited companies, final tax is the corporate tax paid. A potential effect of an audit on a firm's corporate tax paid can be due to altered filed costs (e.g., wages), revenue (e.g., sold goods or services), depreciation, interest revenues or costs, various tax adjustments, etc. For sole proprietorship businesses, a change in final tax could be due to similar posts, but there could also be changes in the owner's private income, for instance from an employment. Although the tax audits used in this study are designed to target the sole proprietorship only, it cannot be ruled out that being audited can have an effect on the filing behaviour regarding private income, or even on actual behaviour outside the business (e.g., labour supply for individuals who have a sole proprietorship on a part-time basis). To sum up, changes in final tax paid as a result of an audit are used in the present study as a general measure of compliance for both limited companies and sole proprietorships, but the interpretation of the results can potentially differ depending on firm type. Both joint and separate results will be shown later on in the study.

The Swedish Tax Agency's registers contain comparable (over time) yearly data over final tax paid starting from the tax year 2013. Although a couple of more years of data could be added, there was a major shift in data storage routines in 2013 which makes data from before and after 2013, respectively, essentially incomparable. For this reason, data in this study covers the period 2013–2017. This means that the outcome variable on firm level is observed in the data at least one year prior to treatment (2013 for firms treated in 2014) and at least one year after (2017 for those treated in 2016). At most, the outcome is observed three years prior to treatment (for treated in 2016) and three years after (for treated in 2014).

As already mentioned, audit assignment is random within each stratum and year (2014–2016). This means that we should expect balance in the outcome variable prior to treatment, i.e., that there are no average differences between the final tax paid by treated (T) and controls (C) in $t-1$ for audits performed in t. However, because the sample sizes in each stratum are relatively small (see table 1), it cannot be ruled out that there are some imbalances in the sample. Table 2 shows averages for T and C as well as the average difference T-C, grouped by stratum and (potential) treatment year. The last column in table 2 contains p-values from a balance test. As might be expected, the null hypothesis of equal average final tax among treated and controls is occasionally rejected.[7] Given the small sample sizes, these rejections of the null do not necessarily indicate that something has gone wrong in the random treatment assignment. Also, as the effect estimation is not going to be done separately for the different strata, and because the statistical method described in section 2.2 allows for potential pre-treatment group differences, occasional imbalances in some strata should not pose any threat to the effect estimation. However, it could be problematic if it turns out that there are systematic imbalances between treated and controls. In this setting, a systematic imbalance means that there is not only an imbalance in a few strata, but also an average difference between T and C measured over strata and years. To ensure that this is not the case, a joint test for such systematic imbalance is performed below. Since the share of treated varies over strata and years, it is not suitable to perform a simple unweighted t-test. To this end, table 3 contains joint balance tests using weighted least squares (WLS). The table contains tests for all strata, separately for limited companies (Ltd.) and sole proprietorships (SP) and performed for all years 2014–2016 as well as separately per year. As apparent from the table, none of the estimates for T-C are significant. To summarize, the results shown in this section suggest that the randomization has worked well in practice and that the few imbalances observed in table 2 should not be considered a threat to the effect estimation.

[7] Given a significance level of 5%, $H_0 : T - C = 0$ is rejected in three out of twelve cases for 2014 and 2015, and in two out of twelve cases for 2016.

Table 2: Final tax $t-1$ for treated (T) and controls (C), 1000s SEK

Stratum	T	C	T-C	p-value
	2014			
Ltd. 1: wage sum missing	52.172	132.048	−79.876	0.007
Ltd. 2: wage sum 1K-600K, non-complex	50.939	48.631	2.308	0.796
Ltd. 3: wage sum 1K-600K, complex	30.239	42.393	−12.154	0.048
Ltd. 4: wage sum 600K-3M, non-complex	87.619	117.200	−29.581	0.146
Ltd. 5: wage sum 600K-3M, complex	229.369	135.559	93.809	0.170
Ltd. 6: wage sum 3M-10M	372.297	415.586	−43.290	0.500
Ltd. 7: wage sum >10M	1494.842	1454.836	40.006	0.929
SP 1: revenue 100K-600K, non-complex	100.827	115.029	−14.202	0.415
SP 2: revenue 100K-600K, complex	90.882	92.489	−1.608	0.867
SP 3: revenue 600K-3M, non-complex	202.044	228.874	−26.830	0.584
SP 4: revenue 600K-3M, complex	141.470	166.040	−24.570	0.129
SP 5: revenue >3M	156.916	346.612	−189.696	0.00000
	2015			
Ltd. 1: wage sum missing	135.649	136.760	−1.111	0.984
Ltd. 2: wage sum 1K-600K, non-complex	36.455	50.079	−13.624	0.039
Ltd. 3: wage sum 1K-600K, complex	39.232	46.001	−6.770	0.239
Ltd. 4: wage sum 600K-3M, non-complex	129.646	113.127	16.519	0.623
Ltd. 5: wage sum 600K-3M, complex	106.615	128.279	−21.664	0.075
Ltd. 6: wage sum 3M-10M	372.364	416.855	−44.491	0.376
Ltd. 7: wage sum >10M	1937.710	1524.568	413.142	0.210
SP 1: revenue 100K-600K, non-complex	93.199	112.956	−19.757	0.216
SP 2: revenue 100K-600K, complex	91.684	96.289	−4.605	0.708
SP 3: revenue 600K-3M, non-complex	157.382	223.637	−66.255	0.004
SP 4: revenue 600K-3M, complex	142.196	170.473	−28.277	0.059
SP 5: revenue >3M	229.980	424.309	−194.329	0.0004
	2016			
Ltd. 1: wage sum missing	50.692	141.202	−90.510	0.008
Ltd. 2: wage sum 1K-600K, non-complex	65.549	55.995	9.553	0.512
Ltd. 3: wage sum 1K-600K, complex	29.959	49.527	−19.567	0.105
Ltd. 4: wage sum 600K-3M, non-complex	163.800	128.054	35.746	0.427
Ltd. 5: wage sum 600K-3M, complex	142.168	137.259	4.909	0.876
Ltd. 6: wage sum 3M-10M	355.565	454.727	−99.161	0.119
Ltd. 7: wage sum >10M	1272.386	1835.287	−562.901	0.137
SP 1: revenue 100K-600K, non-complex	161.683	121.393	40.290	0.290
SP 2: revenue 100K-600K, complex	71.030	104.369	−33.340	0.001
SP 3: revenue 600K-3M, non-complex	280.704	240.335	40.369	0.519
SP 4: revenue 600K-3M, complex	193.578	187.405	6.173	0.876
SP 5: revenue >3M	339.512	454.601	−115.089	0.145

Note: The p-values are from t-tests for difference in means under the assumption that the group variances can differ (Welch–Satterthwaite t-test).

Table 3: Mean difference in final tax $t-1$ between treated (T) and controls (C), 1000s SEK

	All years	2014	2015	2016
		Ltd.		
C	166.168***	158.409***	159.561***	179.433***
	(2.302)	(3.515)	(2.614)	(5.196)
T-C	−6.741	2.180	−2.779	−10.590
	(31.128)	(49.829)	(26.833)	(115.470)
Observations	340,038	106,477	114,593	118,968
		SP		
C	147.865***	140.826***	144.755***	158.015***
	(1.605)	(2.621)	(2.583)	(3.107)
T-C	−15.826	−22.778	−27.179	−1.120
	(27.698)	(45.958)	(46.070)	(51.242)
Observations	228,555	75,375	77,225	75,955

Note: Results from WLS-regression where the weights correspond to the sample weights of each year and stratum.
*p<0,1; **p<0,05; ***p<0,01.

3 Results

This section first presents the baseline results from estimating equation (1) jointly for all firms and then separately by firm type (limited companies and sole proprietorships). This is followed by heterogeneity analyses where the roles of firm age and having an auditor for the effect size are studied.

3.1 Baseline results

The empirical specification in equation (1) allows estimating the average effect of an audit performed in year t on the firms' compliance in $t+1, t+2$, and $t+3$, as well as the direct effect during year t. The estimates for $t-2$ and $t-1$ can be viewed as placebo effects, in the sense that we should not expect any effects prior to treatment.

For ease of presentation, the baseline results are shown graphically in figure 1, and the corresponding detailed results can be found in table 10 in the appendix. The solid line shows the effect estimates for the complete set of firms, along with 95% confidence intervals. As expected, the placebo effects are not statistically significant, with point estimates being close to zero. Further, there is no statistically significant effect during the audit year, although the point estimate is relatively large in magnitude; as will be discussed shortly, there is significant heterogeneity depending on firm type. Looking

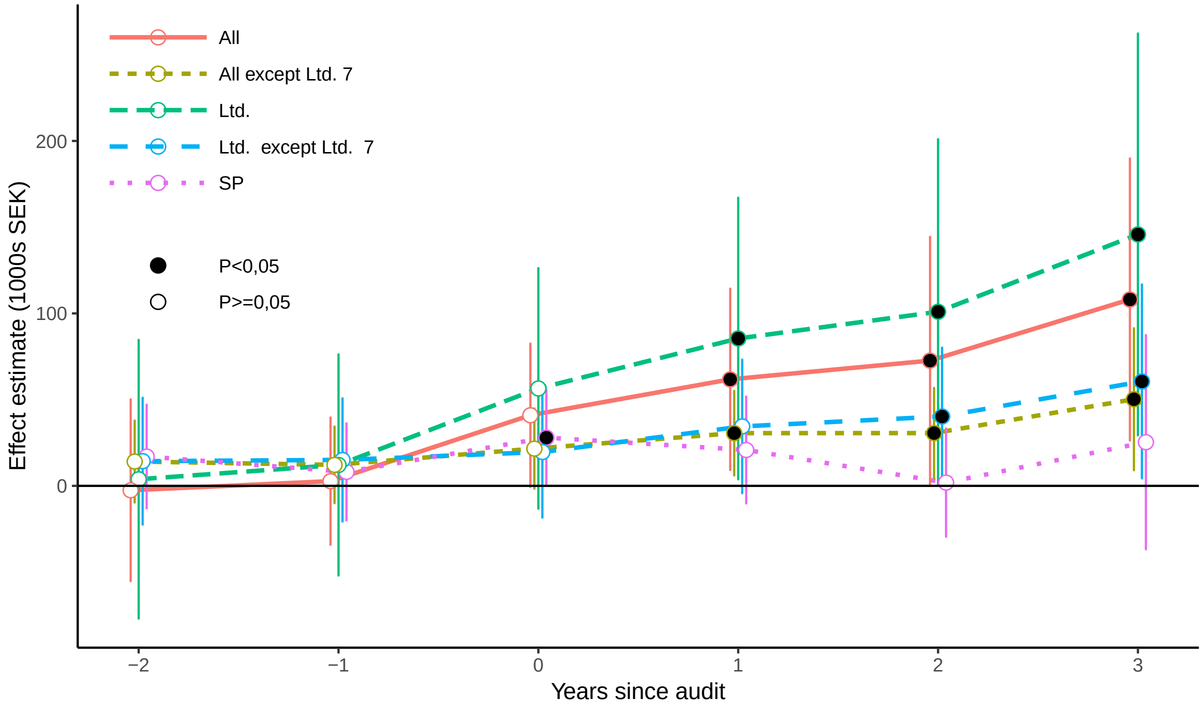

Figure 1: The effect of random audits on final tax (1000s SEK)

Note: Results from estimation of equation (1) using data from random tax audits performed in 2014, 2015 and 2016. The effect estimates k years since audit are given by $\hat{\theta}_k$ for $k = -2, -1$ (placebo estimates) och $k = 0, 1, 2, 3$ (effects of interest). The 95% confidence intervals are calculated using clustered standard errors. See table 10 for detailed results.

beyond the mechanical effect during the audit year, the estimates one, two and three years after audit are significant both in statistical and economic terms: for $t+1$ the effect is about 62 thousand SEK (62K SEK). This number can be compared to the average final tax measured during the audit year in the treated group which is 342K SEK, suggesting an effect of approximately 18% one year after audit. Table 11 contains the results from an attempt to estimate the percentage effect directly, where the inverse hyperbolic sine-transformation (IHS) has been applied to the outcome variable. The IHS has been used instead of a log transformation because the final tax can be zero. The estimates in table 11 are to be interpreted in terms of log points or percent but unfortunately, very few of the effect estimates in table 11 are statistically significant; it is well known that the difference-in-differences estimator, which is very closely related to the estimator in equation (1), is not invariant to monotone transformations of the outcome variable.[8] The point estimate from table 11 corresponding to the 18 percent figure calculated above is 12 log points or 13 percentage points,[9] which is not too far off but not statistically significant.

[8]See Meyer (1995) and the examples in section 3.2.4 in Lechner (2011).

[9]To translate log points to percentage points, I use the formula $\hat{pp}_k = 100 \times (e^{\hat{\theta}_k} - 1)$, where $\hat{\theta}_k$ is from table 11.

Continuing with the results for the complete set of firms (the solid line in figure 1), it is interesting to note that the effect estimates do not seem to decrease in magnitude two and three years after the audit, compared to one year after; if anything, there seems to be a slight increase. As equation 1 includes calendar-year fixed effects, this seeming effect increase is not driven by time trends. Interestingly, the dynamics of the effect qualitatively resemble the results found by Advani et al. (2019), which, as mentioned previously, is the study closest to the present one. Figure 3 in Advani et al. (2019) contains estimates of the effects of random audits on individual tax payers' total reported tax owed, a measure very similar to the final tax outcome used in the present paper, but for private taxpayers. Both in the present study and in Advani et al., the point estimate for the audit year is positive but not statistically significant. Further, the point estimates for years 1 and 2 are statistically significant and increase in magnitude, albeit more so in Advani et al. There is a difference in the effect dynamics for year 3: in Advani et al., there is a decrease in effect magnitude, while there appears to be an increase in figure 1 in the present paper. Some caution in interpreting the year 3-effect is perhaps warranted. Although the empirical specifications allows estimating this effect, in practice, only data variation from the first audit wave of 2014 is used for its identification. For this reason, in section 4 where the cost of performing the audits is related to the increased tax revenue due to their positive effect on compliance, only effects up to 2 years after audit are included.

As mentioned in the data section, in the baseline analysis, the randomly selected for audit but non-audited firms constituting 10% of the initially drawn random sample are not considered as treated. Since the Tax Agency has not contacted those firms and also has not treated them in any other way, they are a part of the control group in the present paper. An alternative would be to regard the randomly drawn but non-audited firms as treated in an intention-to-treat (ITT) specification. In a different setting, for instance a medical randomized controlled trial (RCT) or labor market program (LMP) evaluation, the ITT would have been a more natural choice. However, in both those cases, non-participation can be due to non-compliance (to participation) or attrition. For instance, in a medical RCT, some of the patients randomized to the treatment group may discontinue a started treatment. Likewise, eligible participants in a labor market training program may or may not comply to participation. In the present paper's setting, there is no way for a firm to choose not to participate in an audit, and once an audit is started it is also finished. Also, as mentioned above, unlike in the RCT and LMP examples, a non-audit is best regarded as a non-treatment since the non-audited are not aware of being randomly drawn. A problem could arise however if the selection of audited firms among the randomly drawn population is non-random and for instance based on the expected effect of an audit on compliance. There is no reason to believe that this or any other systematic selection mechanism is in place, bus as noted previously, this process is not well documented.

I have therefore performed a sensitivity analysis. Table 12 in the appendix presents the results from estimating an ITT-specification where the randomly selected non-audited firms are regarded as treated instead of controls (first column) and where they are altogether removed from the analysis (second column). Given the setting described above, we should expect a zero effect on the randomly drawn but non-audited firms. Consequently, the ITT should lead to somewhat lower effect estimates than the baseline, but the difference should be marginal as the non-audited firms are few compared to the audited. Comparing the first column in tables 12 and 10 reveals exactly that: the baseline effects for years 0, 1, 2 and 3 are 41K, 62K, 73K and 108K SEK, respectively, while the corresponding ITT-effects are 39K, 54K, 73K and 107K SEK. In addition, whether the randomly drawn but non-audited firms are included in the control group or dropped from the analysis is in practice inconsequential, as revealed by the almost identical results in the first column in table 10 and the second column in table 12.

3.2 Effect estimates by firm type

As firm size varies substantially over the different strata, it is of interest to investigate how the effect differs across strata. Before going further, it is worth mentioning that it is natural to expect the effect to vary by stratum, since the outcome final tax is measured in levels.[10] The question asked in this section is instead of a more qualitative type: is there evidence that the average effects shown in section 3.1 are driven by a certain type or size of firms? Due to the low number of treated, it is not feasible to estimate the effect separately by stratum. However, it is clear from the description in table 2 that the Ltd. 7-stratum is the one deviating most from the rest. For the group of treated, the average final tax for Ltd. 7 is between four and five times as large as the corresponding number for the stratum with the next-largest firms, Ltd. 6. It is therefore informative to remove Ltd. 7 from the estimation and see how the results change. When doing so, it is reasonable to expect the effect estimates to drop in magnitude. If this exercise also renders the point estimates statistically insignificant, it could be the case that the effect is entirely driven by the largest firms.

As figure 1 reveals, removing Ltd. 7 from the estimation results in approximately halved and still statistically significant effect estimates one, two, and three years after audit, and still no effects for other years. Thus, as expected, the Ltd. 7-stratum has a significant impact on the magnitude of the average effect, but the audits have a positive effect on compliance also for the rest of the strata. Figure 1 also shows that the audits have had the largest average effect among limited companies (the upper-most line), with a smaller effect magnitude when the Ltd. 7-stratum is removed.

[10]As mentioned previously, attempting to estimate the effect measured in log points unfortunately leads to bad precision; see the discussion in section 3.1.

Finally, the most important effect difference is when the limited companies are compared to sole proprietorships. First, the effect during the audit year is only statistically significant for sole proprietorships, although the point estimate for limited companies is relatively high and positive. Second, there are no statistically significant effects for sole proprietorships one, two and three years after audit, while those effects are both economically and statistically significant for limited companies. This might partly be a precision issue due to the relatively small sample size as the point estimates $\hat{\theta}_1$ and $\hat{\theta}_3$ are quite high. But precision is most likely not the whole reason as the point estimate $\hat{\theta}_2$ is close to zero for sole proprietorships.

Summing up, the results thus far suggest that the positive effects on compliance found one, two, and three years after audit are driven by a change in filing behaviour among limited companies. As expected, the effect magnitude is higher for the largest firms, but there is a positive effect for limited companies also when excluding the largest among them. For sole proprietorships, the only statistically significant effect is during the audit year; there is no support in data for an effect on compliance behaviour during the years after audit for those firms. To check whether these results could be partly driven by firm exit from the panel (bankruptcy or liquidation for other reasons), I have performed sensitivity analyses by enforcing the panel to be balanced using two alternative methods. As shown in tables 13 and 14 in the appendix, the results stand.

3.3 Newly founded versus older firms

The literature on tax compliance over the firm life cycle is scarce, and even less is known about the potential effect heterogeneity of audits with respect to firm age. Although it is reasonable to expect some heterogeneity, there is no strong theory giving precise predictions. On the one hand, according to the so called trust paradigm in the tax compliance literature, tax agencies could attempt reinforcing tax compliance as an ethical form of behaviour. This strategy could potentially work better when used toward newly founded firms, as it then introduces from the start the notion that paying taxes is the right thing to do (Alm, 2019). On the other hand, starting a company is in itself a complicated, time-demanding, and in some circumstances, surely chaotic enterprise. It is therefore not reasonable to expect all new firms to be able to find the time and resources needed to give the compliance decision its proper share of attention: the firm's survival is likely the owners' highest prioritized target during the first years of operation. Moreover, it is far from clear that an audit is the best way to achieve better compliance for a newly founded firm. Indeed, one of the few field experiments that does not find clear positive effects of audits on future compliance is the field experiment in Gangl et al. (2014), who focus on newly founded firms. Gangl et al. measure the effect of tax audit of the reports and payment liabilities on a monthly basis throughout newly founded firms' first year.

Table 4: Effect difference between firms founded less than 5 years ago and the rest

	All	All $\times 1[< 5$ years$]$	Ltd. 7 excluded	Ltd. 7 excluded $\times 1[< 5$ years$]$
$\hat{\theta}_{-2}$	-2.555	-2.395	14.118	13.472
	(26.844)	(26.907)	(12.076)	(12.099)
$\hat{\theta}_{-1}$	2.727	-0.137	12.168	16.031
	(18.790)	(21.461)	(11.293)	(11.604)
$\hat{\theta}_{0}$	40.955*	39.999*	21.599*	20.232*
	(21.150)	(20.702)	(11.819)	(11.811)
$\hat{\theta}_{1}$	61.754**	67.619**	30.563**	33.666**
	(26.781)	(31.456)	(12.472)	(13.088)
$\hat{\theta}_{2}$	72.620**	98.170**	30.602**	35.204**
	(36.582)	(44.195)	(13.318)	(14.507)
$\hat{\theta}_{3}$	108.085***	157.770***	50.232**	79.116***
	(41.679)	(49.222)	(20.958)	(23.063)
$\hat{\theta}_{0} \times 1[< 5$ years$]$		-10.511		19.977*
		(27.453)		(10.313)
$\hat{\theta}_{1} \times 1[< 5$ years$]$		-39.327		2.379
		(43.888)		(16.078)
$\hat{\theta}_{2} \times 1[< 5$ years$]$		-124.243**		-3.728
		(63.369)		(17.583)
$\hat{\theta}_{3} \times 1[< 5$ years$]$		-221.889***		-98.067**
		(78.563)		(40.185)
Observations	2,765,264	2,765,264	2,765,264	2,765,264
R^2	0.822	0.822	0.822	0.822
Adjusted R^2	0.805	0.805	0.805	0.805

Note: Results from estimation of equation (1) with interaction effects, using data from random tax audits performed in 2014, 2015 and 2016. The effect estimates k years after audit are given by θ_k for $k = -2, -1$ (placebo estimates) och $k = 0, 1, 2, 3$ (effects of interest). Standard errors clustered at firm level are presented within parentheses.
*p<0,1; **p<0,05; ***p<0,01.

The results suggest that the monthly tax audits lead to delayed tax payments. To sum up, it is theoretically not clear which effect heterogeneity with respect to firm age to expect, making this an interesting and policy relevant research question.

To this end, to study effect heterogeneity by firm age, the firms were first divided into two groups: those founded less than five years ago and the rest. As audit assignment is random, there should be balance on the shares of new and established firms, respectively, prior to the audit. A balance test by stratum and audit year is presented in table 15 in the appendix and as expected, there is good balance: the null hypothesis of equal means in the categorical variable $1[< 5$ years$]$ among treated and controls $t - 1$ is only rejected for one (out of 36) year-stratum group.

The results from the heterogeneity analysis are presented in table 4, where the effect of audit is interacted with the categorical variable $1[< 5 \text{ years}]$. For ease of comparison, the first column shows the baseline results for all firms. When the interaction term is included in the second column of table 4, the parameter estimates $\hat{\theta}_k$ now represent the results for the firms that are five years old or older. The estimates $\hat{\theta}_k \times 1[< 5 \text{ years}]$ are to be interpreted as the effect difference between firms that were founded less than five years ago and older firms; a negative sign means that the effect is lower for new firms. The second column in table 4 reveals two things. First, there is no empirical support for any significant heterogeneity during the audit year and the year after: The interaction terms for those years are not statistically significant and in addition, the point estimates $\hat{\theta}_0$ and $\hat{\theta}_1$ are quite similar.

Second, looking at the estimates for years two and three after the audit in the second column of the table, there does seem to be a quite substantial effect heterogeneity, revealed by the statistically and economically significant estimates of about -124K and -222K SEK two and three years after audit, respectively. These results clearly indicate that the effect of audit on the future compliance of newly founded firms is lower than for more well established firms, and that in essence, the effect for newly founded firms appears to be close to zero.[11]

As in section 3.2, it is informative to investigate whether the results stand also when the largest firms in the Ltd. 7-stratum are removed. The last column in table 4 therefore contains the results with interaction terms with firms in Ltd. 7 removed. Here, an interesting pattern appears. The effect estimate during the audit year is about 20K SEK and significant at the 10 percent level. The effect size then drops in magnitude and eventually turns negative and statistically significant three years after audit. Due to the relatively low precision, these results should be treated with some caution. Nonetheless, the findings do suggest that when the influence of the largest firms on the average effect is removed, the mechanical effect of audit during the audit year is higher in magnitude for new firms than for older ones. In the longer term, however, the behavioural effect of audit on compliance is on average lower for newly founded firms. These results raise an interesting hypothesis for future research: both the positive effect difference during the audit year and the negative effect difference further on could be due to new firms being inexperienced and therefore having difficulties both in complying (revealed by the positive estimate during the audit year) and changing their tax compliance behaviour later on (revealed by the negative estimate during the third year after audit, which in essence implies a zero effect).

[11]Due to the small sample sizes, estimating the effect separately for the two groups results in poor precision.

3.4 Limited companies with and without an auditor

Countries differ as to whether they have audit requirements for private companies and in Sweden there is currently a possibility for firms to opt out of having an auditor. In an overview of the research literature on the need for and value of private company audits, Vanstraelen and Schelleman (2017) conclude that there is much heterogeneity in the reasons driving audit demand in private companies and the value derived from having an auditor. The literature suggests that signaling the reliability of accounting information to stakeholders is one of the major aspects of the value of auditing. The audit can also reduce the likelihood of fraud by management by validating the effectiveness of how cash is transferred and monitored throughout the firm.

Until 2010, it was mandatory for all Swedish limited companies to have an auditor. A reform abolishing the audit obligation for some small limited companies came into force on November 1, 2010. According to the new law, if a company has exceeded two out of three specific thresholds for the past two consecutive years, it is subject to mandatory audit. The thresholds are the following:

1. Annual total sales of 3 million SEK
2. Annual total assets of 1.5 million SEK
3. Annual average of full-time employees of at least 50 employees.

Initially, the Government's forecast was that about 40% of the firms that could opt out would choose to do so, but in reality, the number turned out to be higher; about 60% of the firms that had the possibility had opted out by the end of 2015 (Riksrevisionen, 2017). Previous studies have looked at the impact of the audit obligation reform on compliance from different angles. Riksrevisionen (2017) finds some indications that the reform had a negative effect on tax compliance for firms that opted out, and may have facilitated economic crime and impaired the ability of authorities to discover such crime. Dong et al. (2019) instead focus on the firms that could opt out but chose to retain their auditor, and find that those firms had higher levels of tax avoidance relative to a matched sample of mandatory audit firms. According to Dong et al., these results could arise due to a shock to the competitive environment for audit firms which they respond to by taking steps to add additional value for their clients. Relaxing audit requirements could then benefit firms who retain their auditor, because they are able to obtain tax advisory services as an additional value from their auditor.

In the reminder of this subsection, interest is on the effect heterogeneity of a random tax audit on compliance among firms who had and had not opted out of having an auditor during the year prior to being randomly tax audited. The setup is much like the one in section 3.3, i.e., on estimating equation (1) with an interaction term involving

Table 5: Effect difference between limited companies which had an auditor $t-1$ and those who did not

	All Ltd.	All Ltd. $\times 1[< 5$ years$]$	Ltd. 7 excluded	Ltd. 7 excluded $\times 1[< 5$ years$]$
$\hat{\theta}_{-2}$	3.838	3.757	14.318	14.398
	(41.183)	(41.156)	(18.728)	(18.702)
$\hat{\theta}_{-1}$	12.126	4.186	15.046	13.513
	(32.655)	(35.998)	(18.182)	(18.965)
$\hat{\theta}_0$	56.485	56.473	19.623	19.809
	(35.549)	(35.503)	(19.368)	(19.361)
$\hat{\theta}_1$	85.459**	110.969**	34.468*	47.358**
	(41.609)	(50.029)	(19.711)	(21.336)
$\hat{\theta}_2$	100.961**	133.368**	40.235**	56.789**
	(51.004)	(62.996)	(20.327)	(22.202)
$\hat{\theta}_3$	145.795**	187.362***	60.555**	82.007**
	(59.401)	(71.784)	(28.646)	(34.430)
$\hat{\theta}_0 \times 1[$no auditor$]$		−33.900		−6.913
		(24.260)		(8.929)
$\hat{\theta}_1 \times 1[$no auditor$]$		−136.926**		−52.625***
		(57.452)		(14.323)
$\hat{\theta}_2 \times 1[$no auditor$]$		−166.156**		−66.583***
		(72.060)		(16.477)
$\hat{\theta}_3 \times 1[$no auditor$]$		−218.598***		−91.157***
		(72.917)		(31.108)
Observations	1,678,629	1,678,629	1,678,629	1,678,629
R^2	0.829	0.829	0.829	0.829
Adjusted R^2	0.813	0.813	0.813	0.813

Note: Results from estimation of equation (1) with interaction effects, using data from random tax audits performed in 2014, 2015 and 2016. The effect estimates k years after audit are given by θ_k for $k = -2, -1$ (placebo estimates) och $k = 0, 1, 2, 3$ (effects of interest). Standard errors clustered at firm level are presented within parentheses.
*p<0,1; **p<0,05; ***p<0,01.

a categorical variable, but this time $1[$no auditor$]$ measured at $t-1$. Interpreting the interaction effects is however slightly more complicated than in the case with new or old firms. It is reasonable to assume that the decision of whether to have an auditor could be endogenous for the compliance decision. This means that a potential effect difference between firms with and without an auditor can be interpreted in (at least) two ways: as a sign of a selection of certain types of firms into opting out of having an auditor, or as a measure of the direct effect of having an auditor on the firm's ability to adapt its tax compliance behaviour after a tax audit administered by the Tax Agency. There is no way of distinguishing between these two mechanisms in the present paper. However, as the Tax Agency has access to information on whether firms have an auditor or not, it is nevertheless possible to use the results from this section for maximizing the expected behavioural effect of an audit. The results here can therefore still be useful for policy, even though they do not provide us with a complete understanding of the mechanism driving the compliance effect heterogeneity.

As it was possible to opt out of having an auditor during the whole observation window of the present study (2013–2017), many firms did so. Table 16 in the appendix shows the share of firms that had an auditor during the year prior to the tax audit among treated and controls. As almost none of the sole proprietorships have an auditor, this section focuses on limited companies only. The shares increase by firm size, which is to be expected given the audit-obligation thresholds above. Moreover, there is balance on the share of firms with an auditor across all stratum-year combinations.

Table 5 contains the results with an interaction of the effect of audit with $1[<$ no auditor$]$. The parameter estimates $\hat{\theta}_k$ in the second column of table 5 represent the effects for firms that had an auditor prior to the tax audit. Further, the estimates $\hat{\theta}_k \times 1[<$ no auditor$]$ measure the effect differences between firms that did not have an auditor and those who did. A negative sign means that the effect is lower for firms without an auditor. Table 5 reveals a clear effect heterogeneity with respect to having or not having an auditor. Starting one year after the tax audit, the effect is much lower for firms that did not have an auditor and the effect difference increases in magnitude. The effect of being audited by the Tax Agency appears to be driven by firms that had an auditor at $t-1$ and there seems to be no effect for the other group.[12] This result holds qualitatively also when the largest firms (Ltd. 7) are excluded from the estimation, as the last column in table 5 reveals.

4 Audit costs related to compliance improvement

This section contains a simple comparison of the compliance improvement achieved through changes in tax behaviour resulting from the random audit program with the costs for running the program. The following question is asked: Assuming that compliance improvement is the only goal of the random audit program, is the return on the resources spent on the audits positive, and how positive is it? Both the cost and the revenue will be measured in monetary terms. Note that this is not a cost-benefit analysis attempting to measure the net welfare change due to the audits, but a much simpler exercise only taking into consideration the Tax Agency's goal of improving compliance in terms of collected tax revenue.

Starting with the costs, there is no direct measure of the wage cost of the audits.

[12] As in section 3.3, due to the small sample of treated, attempting to perform the estimation separately for the two groups results in very poor precision.

Table 6: Hours spent on audits

stratum	2014	2015	2016	all
Ltd. 1: wage sum missing	2,443	5,643	401	8,487
Ltd. 2: wage sum 1K-600K, non-complex	3,267	4,829	970	9,066
Ltd. 3: wage sum 1K-600K, complex	2,673	5,202	993	8,868
Ltd. 4: wage sum 600K-3M, non-complex	3,201	6,914	1,456	11,571
Ltd. 5: wage sum 600K-3M, complex	6,243	12,236	2,661	21,140
Ltd. 6: wage sum 3M-10M	5,731	10,819	3,048	19,598
Ltd. 7: wage sum >10M	3,597	8,077	2,247	13,921
Ltd.	27,155	53,720	11,776	92,651
SP 1: revenue 100K-600K, non-complex	1,307	1,345	1,495	4,147
SP 2: revenue 100K-600K, complex	2,097	1,513	1,848	5,458
SP 3: revenue 600K-3M, non-complex	930	897	918	2,745
SP 4: revenue 600K-3M, complex	3,128	3,526	3,232	9,886
SP 5: revenue >3M	1,018	2,319	1,732	5,069
SP	8,480	9,600	9,225	27,305
Ltd. and SP	35,635	63,320	21,001	119,956

Source: Own calculations using the Swedish Tax Agency's audit register.

However, the administrative registers contain actual hours spent on auditing each of the 1,697 firms used in the study. As table 6 shows, the total number of hours spent on the entire program during all three years was almost 120,000 hours, and the hours spend on sole proprietorships was about one third of those spent on limited companies. The average hours per audited firm are shown in table 7. The grand average is about 71 hours, which is slightly less than two workweeks. The average number of hours spent on auditing sole proprietorships is 57, and the number for limited companies is 76 or about 33% higher. To translate work hours into labour cost, two different monthly wage scenarios are used: 35,000 SEK and 45,000 SEK, respectively. Although I have no access to the wage distribution of auditors employed at the Swedish Tax Agency, these numbers should at least not underestimate the true wages. Table 8 contains calculations of the hourly labour cost for each of these scenarios using the terms in the current collective agreements covering state employees in Sweden. The hourly labour costs corresponding to the monthly wages 35,000 SEK and 45,000 SEK are found to be 300 and 386 SEK, respectively.

In addition to the direct labour cost for performing the audits, it is reasonable to include an overhead which, among other things, covers planning costs and travel. The only available number is from the Swedish National Financial Management Authority, who estimate that on average, overheads constitute about 50% of the total costs of Swedish government agencies (Ekonomistyrningsverket, 2005). This would imply a 100% overhead on the direct labour costs. As it is not entirely clear if it is sensible to include such a

Table 7: Average number of hours spent on audits

stratum	2014	2015	2016	all
Ltd. 1: wage sum missing	59.6	57.6	28.6	55.5
Ltd. 2: wage sum 1K-600K, non-complex	61.6	55.5	53.9	57.4
Ltd. 3: wage sum 1K-600K, complex	66.8	71.3	70.9	69.8
Ltd. 4: wage sum 600K-3M, non-complex	72.8	84.3	76.6	79.8
Ltd. 5: wage sum 600K-3M, complex	75.2	82.1	73.9	78.9
Ltd. 6: wage sum 3M-10M	81.9	89.4	87.1	86.7
Ltd. 7: wage sum >10M	97.2	101.0	102.1	100.2
Ltd.	73.8	77.9	74.5	76.2
SP 1: revenue 100K-600K, non-complex	43.6	44.8	49.8	46.1
SP 2: revenue 100K-600K, complex	53.8	48.8	46.2	49.6
SP 3: revenue 600K-3M, non-complex	58.1	44.9	51	50.8
SP 4: revenue 600K-3M, complex	62.6	67.8	55.7	61.8
SP 5: revenue >3M	59.9	100.8	72.2	79.2
SP	55.8	61.5	54.3	57.1
Ltd. and SP	68.5	74.8	64.0	70.8

Source: Own calculations using the Swedish Tax Agency's audit register.

Table 8: Yearly labour cost per employee

	35'	45'
Yearly wage for time worked (= monthly wage×11)	385,000	495,000
Vacation pay (= monthly wage×11×0.12)	46,200	59,400
Payroll tax	135,483	174,192
Insurance cost	21,560	27,720
Special payroll tax	4,707	6,052
Total labour cost	592,950	762,365
Work hours per year	1,975.50	1,975.50
Hourly labour cost	300.15	385.91

Source: Own calculations using information from the Swedish Agency for Government Employers (https://www.arbetsgivarverket.se/) and Government services for businesses (https://www.verksamt.se/).

high overhead, two scenarios are used in the results: 50% and 100% overhead.

Turning to the compliance improvement resulting from the audits, I use the results for limited companies and sole proprietorships, respectively (see table 10 in the appendix). The effect of audits on the final tax paid is a reasonable measure that sums up the achieved compliance improvement: it is simply the increase in the tax paid by the treated firms as

Table 9: Increased tax revenues minus audit costs (SEK millions)

		No overhead		50% overhead		100% overhead	
	typ	35'	45'	35'	45'	35'	45'
θ_0	all	−36.01	−46.29	−54.01	−69.44	−72.01	−92.58
	Ltd.	−27.81	−35.75	−41.71	−53.63	−55.62	−71.51
	SP	5.21	2.86	1.11	−2.40	−2.99	−7.67
$\theta_0 + \theta_1$	all	67.06	56.78	49.06	33.63	31.06	10.48
	Ltd.	73.89	65.94	59.98	48.06	46.08	30.19
	SP	5.21	2.86	1.11	−2.40	−2.99	−7.67
$\theta_0 + \theta_1 + \theta_2$	all	188.27	177.98	170.26	154.83	152.26	131.69
	Ltd.	194.03	186.08	180.13	168.21	166.22	150.33
	SP	5.21	2.86	1.11	−2.40	−2.99	−7.67

Note: Own calculations based on results in tables 6, 8, and 10.

a result of the audits. The total effect k years after audit is calculated as the product of the corresponding effect estimate $\hat{\theta}_k$ from table 10 and the number of treated. This figure is calculated separately for limited companies and sole proprietorships and then summed over $k = 0, 1, 2$. To be on the conservative side, the effect for $k = 3$ is not included for reasons already stated in section 3.1.[13] Also, only effect estimates that are statistically significant at the 5%-level are used, meaning that the results for sole proprietorships after the audit year and the results for limited companies during the audit year are not included.

The results from the cost-tax revenue analysis are presented in table 9. First, performing the analysis for all firms together when only the audit year is included, it is clear that the costs outweigh the tax revenues, irrespective of assumed monthly wage and level of overhead costs (see the first row in table 9 with figures ranging from -93 to -36 million SEK). However, taking into consideration also the achieved compliance improvement one year after the audit renders the results positive, irrespective of overhead and monthly wage (see the fourth row in table 9 with numbers ranging from 10 to 67 million SEK). Adding in addition the compliance improvement two years after the audit (seventh row in table 9) further improves the numbers.

Looking at the two firm types separately, the results for limited companies are negative if only the audit year is included, which is simply because the compliance improvement is zero as θ_0 is not statistically significant for limited companies. Including θ_1 leads to positive revenues minus costs, and when θ_2 is additionally included, the numbers are still improved. Finally, as only the estimate for θ_0 is statistically significant for sole proprietorships, the numbers for SP do not change row-wise and are positive assuming

[13] Although the empirical specification allows estimating θ_3, in practice, only data variation from the first audit wave of 2014 is used for its identification.

no overhead or 50% overhead and the low-wage scenario, and negative otherwise.

To sum up, a simple comparison of the increased tax revenues from the audits with the cost for performing them shows clear positive numbers when at least the behavioural tax compliance effect one year after the audit is included. This result is largely driven by the limited companies.

5 Conclusions

This study estimates the dynamics of the effect of random tax audits performed by the Swedish Tax Agency on the compliance of small and medium-sized firms. The main finding is that on average, an audit regarding a specific tax year leads to an increase in compliance during the following tax years. The effect, measured as the average impact on final tax paid, is about 62K SEK one year after the audit and about 73K SEK two years after.

There is considerable effect heterogeneity across firm type. For sole proprietorships, the only effect found to be statistically significant is the mechanical effect of audit during the audit year (i.e., the direct tax revenue collected). The average improved compliance due to changes in filing behaviour observed during the years following the audit is entirely driven by limited companies. Moreover, although the magnitude of the effect is larger for the largest firms, the effects during year one and two after the audit are still statistically significant (but smaller in magnitude) when the largest firms are removed from the sample.

Two more heterogeneity analyses are performed to further study the mechanism behind the observed audit impact. First, interacting the effect with firm age at the time of the tax audit reveals that the effect is mainly driven by firms older than five years; the compliance of those founded at most five years prior to the tax audit does not seem to be affected. Second, whether or not a firm had an auditor prior to being randomly assigned to a tax audit appears to be important for the impact of the audit on tax compliance: the effect appears to be driven by firms who had an auditor.

In an attempt to measure the return on used resources for performing the audits, the achieved compliance improvement is related to the cost, measured as the direct labour cost plus an overhead. The numbers show that, although the direct net result during the audit year is negative, the audits are clearly worthwhile in terms of improved compliance once the effects one and two years after audit are included. This result is entirely driven by compliance improvements among limited companies.

The results from this study stress the importance of evaluating audits not only by their direct effect in terms of collecting tax revenue during the audit year, but also by their impact on future compliance behaviour. In terms of policy, the results suggest that audits can make a larger overall positive impact on compliance if targeted to a larger extent at limited companies rather than sole proprietorships; at firms older than five years rather than newly established ones; and at firms who employed an auditor prior to being audited by the Tax Agency.

These results raise some interesting hypotheses for future research. The observation of a larger effect on the compliance of firms who have an auditor is not entirely in line with the result in Dong et al. (2019), who find that firms that could opt out but have chosen to retain their auditor had higher levels of tax avoidance relative to a matched sample of mandatory audit firms. The explanation offered by Dong et al. is that the auditors may offer additional tax counseling services to the firms that choose to have an auditor. This is certainly a plausible explanation, but in order for this theory to explain the findings in the present paper, these tax counseling services would have to be such that they *impair* the firms' baseline compliance (i.e., increase tax avoidance), at the same time as they *improve* the behavioural effect of audits on future compliance. Although not entirely implausible, such a theory is somewhat contradictory, and thus it cannot be excluded that some additional mechanism is driving the results. Finally, the result that audits do not seem to improve the compliance of newly founded firms is in line with at least one previous study (Gangl et al., 2014), but clearly, more research is needed to understand the mechanisms behind this finding.

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Appendix: Additional tables

Table 10: The effect of random audits on final tax (1000s SEK)

	All	SP	All except Ltd. 7	Ltd.	Ltd. except Ltd. 7
$\hat{\theta}_{-2}$	−2.555	17.010	14.118	3.838	14.318
	(26.844)	(15.299)	(12.076)	(41.183)	(18.728)
$\hat{\theta}_{-1}$	2.727	8.114	12.168	12.126	15.046
	(18.790)	(14.311)	(11.293)	(32.655)	(18.182)
$\hat{\theta}_{0}$	40.955*	27.979**	21.599*	56.485	19.623
	(21.150)	(14.003)	(11.819)	(35.549)	(19.368)
$\hat{\theta}_{1}$	61.754**	20.820	30.563**	85.459**	34.468*
	(26.781)	(15.766)	(12.472)	(41.609)	(19.711)
$\hat{\theta}_{2}$	72.620**	1.903	30.602**	100.961**	40.235**
	(36.582)	(15.992)	(13.318)	(51.004)	(20.327)
$\hat{\theta}_{3}$	108.085***	25.326	50.232**	145.795**	60.555**
	(41.679)	(31.651)	(20.958)	(59.401)	(28.646)
Firm FE	✓	✓	✓	✓	✓
Calendar year FE	✓	✓	✓	✓	✓
Observations	2,765,264	1,086,635	2,717,773	1,678,629	1,631,138
R^2	0.822	0.795	0.817	0.829	0.833
Adjusted R^2	0.805	0.774	0.799	0.813	0.817

Note: Results from estimation of equation (1) using data from random tax audits performed in 2014, 2015 and 2016. The effect estimates k years after audit are given by θ_k for $k = -2, -1$ (placebo estimates) och $k = 0, 1, 2, 3$ (effects of interest). Standard errors clustered at firm level are presented within parentheses. *p<0,1; **p<0,05; ***p<0,01.

Table 11: The effect of random audits on final tax (IHS transformation)

	All	SP	All except Ltd. 7	Ltd.	Ltd. except Ltd. 7
$\hat{\theta}_{-2}$	0.036	-0.021	0.052	0.085	0.132
	(0.071)	(0.069)	(0.076)	(0.134)	(0.158)
$\hat{\theta}_{-1}$	0.090	-0.007	0.107	0.155	0.206
	(0.072)	(0.068)	(0.077)	(0.137)	(0.160)
$\hat{\theta}_{0}$	0.096	0.146^{**}	0.117	0.101	0.148
	(0.073)	(0.067)	(0.079)	(0.139)	(0.163)
$\hat{\theta}_{1}$	0.122	0.073	0.139^{*}	0.177	0.223
	(0.075)	(0.071)	(0.080)	(0.140)	(0.164)
$\hat{\theta}_{2}$	0.124	-0.004	0.136	0.221	0.264
	(0.080)	(0.083)	(0.085)	(0.143)	(0.167)
$\hat{\theta}_{3}$	0.127	0.036	0.141	0.218	0.262
	(0.095)	(0.099)	(0.100)	(0.158)	(0.182)
Firm FE	✓	✓	✓	✓	✓
Calendar year FE	✓	✓	✓	✓	✓
Observations	2,764,589	1,085,960	2,717,098	1,678,629	1,631,138
R^2	0.803	0.756	0.796	0.789	0.775
Adjusted R^2	0.784	0.732	0.777	0.769	0.753

Note: Results from estimation of equation (1) using data from random tax audits performed in 2014, 2015 and 2016. The effect estimates k years after audit are given by θ_k for $k = -2, -1$ (placebo estimates) och $k = 0, 1, 2, 3$ (effects of interest). Standard errors clustered at firm level are presented within parentheses. Because the final tax can be zero, the Inverse Hyperbolic Sine-transformation (IHS) is used: $\ln(Y + (Y^2 + 1)^{1/2})$.

*p<0,1; **p<0,05; ***p<0,01.

Table 12: The effect of random audits on final tax (1000s SEK, ITT and adjusted control group)

	Intention-to-treat†	Baseline with adjusted C‡
$\hat{\theta}_{-2}$	-8.778	-2.502
	(24.393)	(26.844)
$\hat{\theta}_{-1}$	0.762	2.871
	(17.154)	(18.790)
$\hat{\theta}_{0}$	39.328^{**}	41.208^{*}
	(20.003)	(21.150)
$\hat{\theta}_{1}$	53.928^{**}	62.105^{**}
	(24.806)	(26.779)
$\hat{\theta}_{2}$	72.663^{**}	73.053^{**}
	(35.818)	(36.580)
$\hat{\theta}_{3}$	106.955^{***}	108.552^{***}
	(39.776)	(41.677)
Firm FE	✓	✓
Calendar year FE	✓	✓
Observations	2,764,577	2,763,702
R^2	0.823	0.823
Adjusted R^2	0.806	0.806

Note: Results from estimation of equation (1) using data from random tax audits performed in 2014, 2015 and 2016. The effect estimates k years after audit are given by θ_k for $k = -2, -1$ (placebo estimates) och $k = 0, 1, 2, 3$ (effects of interest). Standard errors clustered at firm level are presented within parentheses. *p<0,1; **p<0,05; ***p<0,01

† Randomly drawn but non-audited firms are regarded as treated.

‡ Randomly drawn but non-audited firms are removed from the control group.

Table 13: The effect of random audits on final tax (1000s SEK, balanced panel according to alternative A)

	All	SP	All except Ltd. 7	Ltd.	Ltd. except Ltd. 7
$\hat{\theta}_{-2}$	−2.422	17.111	13.813	4.353	13.708
	(28.489)	(16.521)	(12.547)	(42.482)	(18.924)
$\hat{\theta}_{-1}$	5.757	10.181	14.716	15.861	17.610
	(20.205)	(15.421)	(11.809)	(33.762)	(18.470)
$\hat{\theta}_0$	37.795*	27.890*	20.165	51.907	17.481
	(22.249)	(14.927)	(12.382)	(36.473)	(19.797)
$\hat{\theta}_1$	66.213**	22.914	30.033**	90.133**	32.281
	(28.140)	(16.794)	(12.987)	(43.114)	(20.080)
$\hat{\theta}_2$	80.790**	0.035	31.080**	111.432**	41.287**
	(37.432)	(16.807)	(13.871)	(51.951)	(20.756)
$\hat{\theta}_3$	109.340**	24.791	47.984**	147.458**	57.548**
	(42.726)	(32.177)	(21.440)	(60.890)	(29.178)
Firm FE	✓	✓	✓	✓	✓
Calendar year FE	✓	✓	✓	✓	✓
Observations	2,389,965	958,975	2,345,810	1,430,990	1,386,835
R^2	0,735	0,795	0,757	0,705	0,699
Adjusted R^2	0,714	0,778	0,737	0,682	0,675

Note: Results from estimation of equation (1) using data from random tax audits performed in 2014, 2015 and 2016. The effect estimates k years after audit are given by θ_k for $k = -2, -1$ (placebo estimates) och $k = 0, 1, 2, 3$ (effects of interest). Standard errors clustered at firm level are presented within parentheses. The panel has been balanced according to alternative A, i.e., by removing firms for which there is no complete data record for all years (2013–2017).
*p<0,1; **p<0,05; ***p<0,01.

Table 14: The effect of random audits on final tax (1000s SEK, balansed panel according to alternative B)

	All	SP	All except Ltd. 7	Ltd.	Ltd. except Ltd. 7
$\hat{\theta}_{-2}$	5.381	13.949	18.542*	17.231	26.586
	(25.510)	(14.040)	(10.968)	(37.506)	(17.004)
$\hat{\theta}_{-1}$	9.361	8.982	17.718*	24.202	27.829*
	(17.479)	(13.263)	(10.441)	(28.455)	(16.707)
$\hat{\theta}_0$	51.968**	29.823**	27.244**	73.093**	31.281*
	(20.324)	(13.042)	(11.156)	(32.183)	(18.177)
$\hat{\theta}_1$	57.501**	15.776	34.560***	83.741**	46.566**
	(26.046)	(14.731)	(12.289)	(39.214)	(19.614)
$\hat{\theta}_2$	68.449**	1.624	37.505***	97.645**	53.030***
	(34.737)	(14.946)	(12.519)	(47.585)	(19.164)
$\hat{\theta}_3$	105.710***	19.157	54.847***	148.018***	73.231***
	(37.628)	(27.581)	(19.197)	(53.783)	(26.759)
Firm FE	✓	✓	✓	✓	✓
Calendar year FE	✓	✓	✓	✓	✓
Observations	2,956,780	1,147,895	2,907,575	1,808,885	1,759,680
R^2	0,727	0,784	0,725	0,713	0,684
Adjusted R^2	0,703	0,764	0,700	0,688	0,656

Note: Results from estimation of equation (1) using data from random tax audits performed in 2014, 2015 and 2016. The effect estimates k years after audit are given by θ_k for $k = -2, -1$ (placebo estimates) och $k = 0, 1, 2, 3$ (effects of interest). Standard errors clustered at firm level are presented within parentheses. The panel has been balanced according to alternative B, i.e., by replacing missing firm-year observations with zeros.

*p<0,1; **p<0,05; ***p<0,01.

Table 15: Share of firms founded at most 5 years ago $t-1$ among treated (T) and controls (C)

Stratum	T	C	T-C	P-värde
	2014			
Ltd. 1: wage sum missing	0.515	0.397	0.118	0.191
Ltd. 2: wage sum 1K-600K, non-complex	0.365	0.382	-0.016	0.812
Ltd. 3: wage sum 1K-600K, complex	0.333	0.398	-0.065	0.403
Ltd. 4: wage sum 600K-3M, non-complex	0.341	0.277	0.064	0.380
Ltd. 5: wage sum 600K-3M, complex	0.247	0.243	0.004	0.932
Ltd. 6: wage sum 3M-10M	0.058	0.133	-0.075	0.011
Ltd. 7: wage sum >10M	0.054	0.079	-0.025	0.510
SP 1: revenue 100K-600K, non-complex	0.286	0.240	0.046	0.604
SP 2: revenue 100K-600K, complex	0.282	0.231	0.051	0.488
SP 3: revenue 600K-3M, non-complex	0.118	0.178	-0.060	0.467
SP 4: revenue 600K-3M, complex	0.173	0.169	0.004	0.936
SP 5: revenue >3M	0.059	0.108	-0.049	0.419
	2015			
Ltd. 1: wage sum missing	0.417	0.397	0.019	0.705
Ltd. 2: wage sum 1K-600K, non-complex	0.368	0.382	-0.014	0.791
Ltd. 3: wage sum 1K-600K, complex	0.361	0.403	-0.042	0.469
Ltd. 4: wage sum 600K-3M, non-complex	0.220	0.286	-0.067	0.151
Ltd. 5: wage sum 600K-3M, complex	0.221	0.247	-0.025	0.465
Ltd. 6: wage sum 3M-10M	0.058	0.131	-0.073	0.001
Ltd. 7: wage sum >10M	0.077	0.077	0.0003	0.992
SP 1: revenue 100K-600K, non-complex	0.133	0.226	-0.093	0.153
SP 2: revenue 100K-600K, complex	0.194	0.220	-0.027	0.713
SP 3: revenue 600K-3M, non-complex	0.100	0.168	-0.068	0.336
SP 4: revenue 600K-3M, complex	0.154	0.158	-0.004	0.935
SP 5: revenue >3M	0.174	0.108	0.066	0.421
	2016			
Ltd. 1: wage sum missing	0.455	0.358	0.097	0.552
Ltd. 2: wage sum 1K-600K, non-complex	0.412	0.331	0.081	0.520
Ltd. 3: wage sum 1K-600K, complex	0.357	0.344	0.014	0.920
Ltd. 4: wage sum 600K-3M, non-complex	0.278	0.247	0.031	0.779
Ltd. 5: wage sum 600K-3M, complex	0.314	0.217	0.097	0.231
Ltd. 6: wage sum 3M-10M	0.229	0.118	0.111	0.133
Ltd. 7: wage sum >10M	0.095	0.070	0.025	0.706
SP 1: revenue 100K-600K, non-complex	0.367	0.217	0.150	0.105
SP 2: revenue 100K-600K, complex	0.250	0.208	0.042	0.551
SP 3: revenue 600K-3M, non-complex	0.167	0.162	0.004	0.962
SP 4: revenue 600K-3M, complex	0.259	0.156	0.103	0.081
SP 5: revenue >3M	0.125	0.112	0.013	0.858

Note: The p-values are from t-tests for difference in means under the assumption that the group variances can differ (Welch–Satterthwaite t-test).

Table 16: Share of limited companies who had an auditor $t-1$ among treated (T) and controls (C)

Stratum	T	C	T-C	p-value
		2014		
Ltd. 1: wage sum missing	0.576	0.636	-0.060	0.498
Ltd. 2: wage sum 1K-600K, non-complex	0.558	0.504	0.054	0.444
Ltd. 3: wage sum 1K-600K, complex	0.487	0.538	-0.050	0.538
Ltd. 4: wage sum 600K-3M, non-complex	0.841	0.799	0.042	0.454
Ltd. 5: wage sum 600K-3M, complex	0.914	0.878	0.035	0.264
Ltd. 6: wage sum 3M-10M	1	0.989	0.011	–
Ltd. 7: wage sum >10M	1	0.997	0.003	–
		2015		
Ltd. 1: wage sum missing	0.531	0.552	-0.021	0.688
Ltd. 2: wage sum 1K-600K, non-complex	0.448	0.405	0.044	0.419
Ltd. 3: wage sum 1K-600K, complex	0.431	0.429	0.001	0.982
Ltd. 4: wage sum 600K-3M, non-complex	0.805	0.734	0.071	0.114
Ltd. 5: wage sum 600K-3M, complex	0.846	0.825	0.020	0.498
Ltd. 6: wage sum 3M-10M	0.983	0.984	-0.0004	0.970
Ltd. 7: wage sum >10M	1	0.997	0.003	–
		2016		
Ltd. 1: wage sum missing	0.545	0.509	0.036	0.822
Ltd. 2: wage sum 1K-600K, non-complex	0.353	0.337	0.016	0.896
Ltd. 3: wage sum 1K-600K, complex	0.429	0.367	0.062	0.661
Ltd. 4: wage sum 600K-3M, non-complex	0.667	0.673	-0.006	0.959
Ltd. 5: wage sum 600K-3M, complex	0.600	0.777	-0.177	0.043
Ltd. 6: wage sum 3M-10M	0.971	0.974	-0.002	0.933
Ltd. 7: wage sum >10M	0.952	0.997	-0.045	0.359

Note: The p-values are from t-tests for difference in means under the assumption that the group variances can differ (Welch–Satterthwaite t-test). The statistical test is not meaningful when the variable is constant in one of the groups, which is denoted by '–' in the table. Sole proprietors have been excluded since almost none of them has an accountant.