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The dynamic effects of tax audits

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The Dynamic Effects of Tax Audits

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Abstract

Understanding tax non-compliance and the effectiveness of strategies to tackle it is crucial for a modern tax authority. In this paper we study how and why audits impact reported tax in the years after audit – the dynamic effect – for individual income taxpayers. We exploit data from a random audit program covering almost 35,000 income tax self assessment returns in the UK. We show that audits raise reported tax liabilities for at least five years after audit, with the magnitude of the impact declining over time. In total this raises an additional £1,230 per audited individual in the five years after audit, 1.5 times the direct revenue raised from the audit. Looking by income source, we see that the magnitude of the initial impact is lower for income components which are third party reported, and the impact declines more quickly for components that are more volatile. We develop a model to allow us to distinguish different mechanisms that might explain the presence of dynamic effects, and show our findings can only be explained by audits providing improved information to the tax authority.

JEL codes: D04, H26, H83

Keywords: tax audits, tax revenue, tax reporting decisions, income tax, self assessment, HMRC

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1 Introduction

Central to the efficient functioning of a modern tax authority is the ability to assess and collect tax revenue owed by taxpayers in a timely and cost-effective manner. While the traditional economic analysis of taxation has focused on discussion of optimal *tax rates* across various *tax bases*, an important third arm of policy is *tax administration*: the assessment and collection of taxes owed. In the UK, around 6.5 per cent of tax liabilities go uncollected (HM Revenue and Customs, 2016); in cash terms this is equivalent to the value of corporation tax receipts. In the US this number is 16.3 per cent (Internal Revenue Service, 2016). Taxpayer audits are one important tool used by many tax authorities to improve compliance. Audits have a mechanical benefit in terms of the unpaid revenue they identify and recover. Historically, this is primarily what tax authorities such as the Internal Revenue Service (IRS) have focused on when selecting tax returns for further scrutiny (see Bloomquist, 2013). However, since most taxpayers pay taxes over many years, it is important to understand the extent of any ‘dynamic effects’ that may occur. Dynamic effects are changes in the behaviour of taxpayers in the years after they have been audited. Measuring these wider impacts of audits, and understanding why they occur, is crucial for determining audit strategy including the optimal extent and targeting of enforcement.

In this paper we study *how* and *why* audits affect reporting behaviour in subsequent years. Using administrative data from the UK, we show that audits have important dynamic effects: the aggregate additional tax paid in the five years after an audit is 1.5 times the direct revenue raised from an audit. The reason for this change in reporting is that audits provide the tax authority with information about a taxpayer’s income at the time of audit, making misreporting more difficult.

To estimate the dynamic effect we exploit a random audit programme run by the UK tax authority (HM Revenue and Customs, HMRC). This programme (unconditionally) randomly audits a subset of almost 35,000 individual taxpayers who are required to file ‘self assessment’ to report their income tax. This random auditing allows us to address the common concern that audits are typically targeted towards taxpayers believed to be non-compliant i.e. those who are underreporting their tax liability. Using data on the universe of UK self assessment taxpayers from 1998/99 to 2011/12, we construct a control group for each year of the programme of individuals who could have been selected for a random audit but were not, and study the difference in reporting behaviour over time.

To understand the mechanism driving the additional reported tax liabilities we extend the canonical model of tax evasion (Allingham and Sandmo, 1972; Kleven et al., 2011) to incorporate dynamics in the response to audit. This allows us to study the distinct predictions of different mechanisms that might drive changes in reporting: (1) changes in the perceived penalty for evasion; (2) changes in the perceived probability that evasion is detected; and (3) updates to the information held by the tax authority. Under the first mechanism, any impact should be permanent; while under the second and third it should decline over time. The second and third mechanisms are distinguished by their predictions across income sources: under the second any decline should be common to all income sources, whilst under the third less autocorrelated income sources should see faster declines.

Intuitively, an audit provides a snapshot measure of the current true level of income. For income sources which are relatively stable, this will also be a good predictor of revenue in the future, making it easier for the tax authority to detect later misreporting. For those which are more volatile, the predictive power of this snapshot will decline quickly over time, so it quickly becomes less useful in trying to detect misreports.¹

We demonstrate four main results. First, we show that there is significant non-compliance among individual self assessment taxpayers, both in the share of taxpayers who are found non-compliant and the share of tax that is misreported. More than one third of self-assessment taxpayers are found to be non-compliant, equal to 12 per cent of all income taxpayers. Second, we demonstrate that third party reporting is important in influencing levels of compliance, but only on the *intensive* margin of how much tax is misreported. Third party information does not predict whether a taxpayer is non-compliant, but conditional on non-compliance it is associated with lower magnitudes of misreporting. Third, we provide evidence of important *dynamic effects*, with the additional tax revenue over the five years post-audit equalling 1.5 times the direct revenue raised by audit. Finally, we show that the observed dynamics are consistent only with the information revelation mechanism discussed above.

This paper contributes to a small but growing literature on the dynamic effect of audits. Kleven et al. (2011) analyse the effect of a tax enforcement field experiment in Denmark. They show that audits increase self-reported, but not third party reported, income in the year after audit. We find similar effects of third party information on the amount of underreporting conditional on non-compliance, but in our context the choice of whether to be non-compliant at all appears to have additional determinants. Our analysis also extends up to eight years after the audits take place, allowing us to see that the increased reporting immediately after audit is only a transitory effect.

¹In fact an audit may not uncover all under-reporting. Instead these predictions will then hold relative to the revealed level of income, which may be lower than the true income.

In work conducted concurrently with this study, DeBacker et al. (2015) investigate the impact of random audits conducted by the US Internal Revenue Service (IRS) between 2006 and 2009 on subsequent taxpaying behaviour. They find remarkably similar quantitative results to us, with the cumulative impact in the five years subsequent to audit also around 1.5 times the direct effect.² In addition to providing complementary results to DeBacker et al. (2015), we develop a simple framework that allows us to capture different possible mechanisms that might explain the observed dynamics, as well as to pin down which mechanism can be supported by the data.

Finally, in work using laboratory experiments on students, Maciejovsky et al. (2007) and Kastlunger et al. (2009) find that compliance decreases immediately after an audit, suggestive of a ‘bomb-crater effect’.³ Choo et al. (2013) do similar experiments but with taxpayers rather than students, and do not find any evidence of a bomb-crater effect. We find that for taxpayers in a ‘real-world’ setting, a bomb-crater effect does not exist, at least in aggregate.

The remainder of the paper is organised as follows. Section 2 outlines a model of tax evasion with dynamics in the response to audits. Section 3 describes the policy context and data sources, while Section 4 sets out the method we use. The results are presented in Section 5. Section 6 concludes.

2 Simple Model of Tax Evasion and Audit Response

We consider an extended version of the model by Kleven et al. (2011), which is itself an extension of the model by Allingham and Sandmo (1972). Taxpayers are risk-neutral, and choose how much tax to evade.⁴ The probability of detection is endogenous and is increasing in the difference between reported and true income. The key idea underlying this assumption is that other contextual information the tax authority might have, such as where a taxpayer lives, provides some information on the taxpayer’s income, so that reports further from the truth are more likely to be noticed.⁵

²Earlier studies for the US found some limited evidence for such dynamic effects, but only studied one or two years after audit (Long and Schwartz, 1987; Erard, 1992; Tauchen et al., 1993).

³The ‘bomb-crater effect’ refers to the idea that individuals might perceive the risk of being audited to fall immediately after an audit. The name originates from preference of World War I soldiers to hide out in bomb craters, believing that it was unlikely that a bomb would strike exactly the same place again (see Mittone, 2006). A competing explanation for the decline in reported tax following an audit is the mechanism of loss repair: experiencing an audit may make taxpayers “want to evade more in the future in an attempt to ‘get back’ at the tax agency” (see Andreoni et al., 1998, p. 844).

⁴Relaxing the assumption of risk-neutrality would reduce evasion. It could also introduce a positive correlation between the level of income and evasion, assuming no outside wealth and decreasing absolute risk aversion. This is inconsistent with what we see empirically in our data. Instead we see that there is little variation across the income distribution in the probability of evasion (extensive margin) or the amount of tax evaded in cash terms (intensive margin). The latter result means the *share* of income evaded is falling across the income distribution. See Figure A1 for details.

⁵Other papers using this assumption include Allingham and Sandmo (1972), Yitzhaki (1987), Slemrod and Yitzhaki (2002), Sandmo (2005), and Kleven et al. (2011).

The key innovation of our model, relative to the literature, is to note that incomes from some sources, such as pension annuity income, are very stable over time, while other sources, such as self-employment income for a sole trader, are much more volatile. By first extending the model of Kleven et al. (2011) to multiple time periods, and then allowing for differential volatility of income sources we are able to distinguish different possible mechanisms for *why* audits are observed to have long term outcomes. The importance of volatility is that an audit provides a snapshot measure of the current true level of income. For income sources which are relatively stable, this will also be a good predictor of revenue in the future. For those which are more volatile, the predictive power of this snapshot will decline quickly over time. As we discuss below, different mechanisms make distinct predictions about the dynamics of any audit impact *split by volatility of income source*.

Consider a taxpayer with true income \tilde{y}_s in year s . This income can be decomposed into three parts: (i) a third party reported component, \tilde{y}_s^{TPR} ; (ii) a self-reported permanent component, \tilde{y}^{perm} ; and (iii) a self-reported stochastic component, $\tilde{y}_s^{\text{stoch}}$. The distinction between third-party reported and self-reported income can explain why evasion rates appear much lower than would be expected given the empirical probability of audit (Kleven et al., 2011), which is itself an upper bound on the probability evasion is detected. We distinguish within self-reported income sources those which are fixed over time, and those which are time-varying. This is a simplification for expositional purposes, but our main results can be generalised to having multiple self-reported income sources with varying degrees of autocorrelation, as we will have in our empirical setting.

In year s the taxpayer reports an income of y_s , so evasion is $e_s := \tilde{y}_s - y_s$. Evasion is detected with probability $p(e_s)$, which is increasing in the level of evasion i.e. $p'(e_s) > 0$. This is a composite of both the probability of audit and of the audit successfully detecting evasion.

When evasion is detected, the taxpayer must pay the evaded tax and an additional penalty. The tax is proportional to income, at rate τ , and the penalty is proportional to the tax evaded, with penalty rate θ . The taxpayer's problem is therefore to choose an evasion rate e_s to maximise expected net-of-tax income:⁶

$$[1 - p(e_s)] [(1 - \tau)\tilde{y}_s + \tau e_s] + p(e_s) [(1 - \tau)\tilde{y}_s - \theta \tau e_s] \quad (1)$$

⁶Here we present the taxpayer's problem as a static decision, independent across periods. This simplifies exposition and is equivalent to assuming that the tax authority can neither audit old tax returns when it selects a taxpayer for audit, nor condition future audit probabilities on the outcomes of past audits. The latter assumption is an accurate description of our empirical context.

Differentiating with respect to evasion, e_s , gives the first order necessary condition for an interior optimum:

$$[p(e_s) + p'(e_s)e_s](1 - \theta) = 1 \quad (2)$$

Analysis of this problem is straightforward, and the literature studying this (from Allingham and Sandmo (1972) onwards) has focused on comparative static predictions with respect to the probability that evasion is detected, the penalty for evasion, and the marginal tax rate.

In this context the main innovation of Kleven et al. (2011) is to note that attempts to evade tax on third party reported income are highly unlikely to succeed. This is because matching of tax returns and third party reports will detect any discrepancies, and auditing these returns will uncover the evasion. On the other hand, evasion of self-reported income is much less likely to be noticed. To capture this intuition, the authors first note that a taxpayer who wishes to evade should evade first on the sources of income which are relatively less likely to be detected, before switching to the more easily detected. This means that any evasion up to $\tilde{y}_s^{\text{self}} = \tilde{y}_s^{\text{perm}} + \tilde{y}_s^{\text{stoch}}$ should be of self-reported income, and only after this will taxpayers evade by misreporting \tilde{y}_s^{TPR} . Given this optimal structure for any evasion, the probability of detecting evasion is low for $e_s < \tilde{y}_s^{\text{self}}$, and then increases quickly once e_s reaches $\tilde{y}_s^{\text{self}}$. Finally, given these detection probabilities, the optimal strategy for a taxpayer is to evade some amount less than $\tilde{y}_s^{\text{self}}$.

Our insight builds directly on these ideas. Consider an individual who is audited (for the first time) in year t . Until the time of audit his optimal reporting strategy was identical to that described above: he evaded some amount less than the total amount of self-reported income he received, and none of the misreporting related to third party reported income. Being audited may change his reporting for some combination of the following three reasons: (1) changes in the perceived penalty for evasion; (2) changes in the perceived probability that evasion is detected; and (3) updates to the information held by the tax authority.

In the first of these mechanisms, being audited ($D = 1$) changes the perceived penalty i.e. beliefs about the penalty depend on audit status, $\theta(D)$, and vary with audit status, $\theta(1) \neq \theta(0)$. If this belief is revised upwards, so $\theta(1) > \theta(0)$, then the cost of evasion increases and evasion falls; if it is revised downwards then the opposite occurs. Note that this does not require any particular assumptions on either the initial beliefs or whether updating is rational. It is simply a statement of the direction in which these beliefs about θ change. Absent any policy changes which shift beliefs about the penalty rate, this change is permanent so any change in behaviour will also be permanent.

The second mechanism supposes instead that the perceived probability that evasion is detected, $p(e_s, h)$, is now different because the taxpayer has been audited, and varies with how many years it has been since audit, $h = s - t$.⁷ If he believes he is initially being monitored more carefully than before, so $p(e, 1) \geq p(e, 0)$, this leads to higher compliance immediately after the audit. Alternatively if he believes he is now unlikely to be audited for some time, the so-called ‘bomb crater effect’ (Mittone, 2006; Maciejovsky et al., 2007; and Kastlunger et al., 2009), then $p(e, 1) \leq p(e, 0)$, and compliance initially falls. As time since audit increases, the (perceived) effect of having just been audited wears off, so that beliefs about the probability of evasion being detected converge back towards baseline i.e. $|p(e, h + 1) - p(e, 0)| \leq |p(e, h) - p(e, 0)|$ for $h > 0$.⁸ Again this does not impose anything on where the perceived audit probability comes from, only on how it is updated. The implication is that as the perceived probability of audit converges back to its initial level, the initial impact on reporting behaviour will decline back to baseline. This convergence will be common across incomes from all sources since the probability is common across them all.

The final mechanism by which audits might affect reporting is that they differentially change the ability to hide certain sources of income. Performing an audit provides the tax authority with more accurate information on a taxpayer’s income at a point in time. In subsequent years, information from the audit will make evasion of less variable income sources easier to detect. To operationalise this, recall our earlier distinction between the permanent and stochastic components of self-reported income, $\{\tilde{y}^{\text{perm}}, \tilde{y}_{t+h}^{\text{stoch}}\}$. Once the tax authority performs audits a taxpayer, \tilde{y}^{perm} for that taxpayer is observed. After this, evasion of the permanent component of income is easily detected, analogous to the case with third party reported income. Hence taxpayers should now evade by misreporting $\tilde{y}_{t+h}^{\text{stoch}}$ before any misreporting of \tilde{y}^{perm} (or $\tilde{y}_{t+h}^{\text{TPR}}$).

Also, if this information is the mechanism by which misreporting can be uncovered, then as the amount of the information about stochastic income is reduced over time, misreporting becomes easier. In particular, information about past income is useful because the tax authority can compare reported income in some period h to their expectation of income given the past observation in the audit year: $\nu_{t,h}^{\text{stoch}} := y_{t+h}^{\text{stoch}} - \mathbb{E}[\tilde{y}_{t+h}^{\text{stoch}} | \tilde{y}_t^{\text{stoch}}]$. Such deviations will be more informative if past incomes are a good predictor of current income i.e the autocorrelation of stochastic income,

⁷For a taxpayer that has not yet been audited, h will be negative and $p(e_s, h)$ will take the same value for all $h < 0$.

⁸Note that, given the risk-neutrality assumption, *permanent* shifts in the level of $p(e, h)$ when $h > 0$ versus $h \leq 0$ are observationally equivalent to a shift in θ . So any permanent shift in perceived audit probability, $p(e, h) - p(e, 0)$ as $h \rightarrow \infty$ is observationally equivalent to some shift in the perceived penalty $\theta(1) - \theta(0)$. This equivalence would breakdown with risk aversion, which would allow separate testing of these hypotheses.

$\rho(h, \text{stoch}) = \text{Corr}(y_{t+h}^{\text{stoch}}, y_t^{\text{stoch}})$, is high.⁹ As h increases, $\rho(h, \text{stoch})$ falls, so the ‘amount of information’ about current stochastic income is lower and so misreporting becomes easier. The case of permanent income is the natural limit of this case, where $\rho(h, \text{perm}) = 1 \forall h$. This also makes it clear that if multiple sources of stochastic income were available, misreporting should increase more quickly for those sources which have a lower autocorrelation. Hence under this mechanism, the prediction is that the initial impact on reporting behaviour will decline back to baseline, and this decline will be more rapid for income sources which have a lower autocorrelation.

3 Context and Data

3.1 The UK self assessment tax collection and enforcement system

In this paper, we focus on individuals who file an income tax self assessment return in the UK. In 2015 this comprised around 11 million individuals, over one-third of all individual taxpayers in the UK. Income tax is the largest of all UK taxes, contributing 25.0 per cent of total government receipts in 2014-15. Most sources of income are subject to income tax, including earnings, retirement pensions, income from property, interest on deposits in bank accounts, dividends, and some welfare benefits. Income tax is levied on an individual basis and operates through a system of allowances and bands. Each individual has a personal allowance, which is deducted from total income. The remainder—taxable income—is then subject to a progressive schedule of tax rates. Table 1 shows the share of individuals in our sample reporting non-zero values for each component of income. When we later study income components separately, we focus on those where at least five per cent of the population report non-zero values.

Unlike the US, not all taxpayers have to file a tax return in the UK: for the tax year 2014-15, around 11 million individuals filed a return (around 31 million individuals were liable for income tax). Individuals required to submit a tax return tend to be individuals with forms of income not subject to withholding or for whom the tax system struggles to calculate and withhold the right amount of tax. It includes self-employed individuals, those with incomes over £100,000, company directors, landlords, and many pensioners.

Since incomes covered by self assessment tend to be harder to verify, there is a significant risk of non-compliance. As a result, HM Revenue and Customs (HMRC, the UK tax authority) carries out audits each year to deter non-compliance and recover lost revenue. HMRC runs two types of

⁹In principle what is needed is that the absolute value of this autocorrelation is high, but in practice such autocorrelations are observed to be non-negative.

audit, ‘targeted’ and ‘random’. Targeted audits are based on perceived risks of non-compliance. Random audits are unconditionally random from the population, and are used to ensure that all self assessment taxpayers face a positive probability of being audited, as well as to collect statistical information about the scale of non-compliance and predictors of non-compliance that can be used to implement targeting.

The timeline for the audit process is as follows. The tax year ends on 5 April. Shortly after the end of the tax year, HMRC issues a ‘notice to file’ to taxpayers who they believe need to submit a tax return. This is based on information that HMRC held shortly before the end of the tax year. Random audit cases are provisionally selected from the population of individuals issued with a notice to file. The deadline by which taxpayers must submit their tax return is 31 January the following calendar year (e.g. 31 January 2008 for the 2006/07 tax year). Once returns have been submitted, HMRC deselects some random audit cases (e.g. due to severe illness or death of the taxpayer). At the same time, targeted audits are selected on the basis of the information provided in self assessment returns and other intelligence. Random audits are selected before targeted audits, and individuals cannot be selected for a targeted audit in the same tax year as a random audit. The list of taxpayers to be audited is passed on to local compliance teams who carry out the audits. Audits must be opened within a year of the 31 January filing deadline, or a year from the actual date of filing for returns filed late.¹⁰ Taxpayers subject to an audit are informed when it is opened but they are not told whether it is a random or targeted audit. Approximately one third of taxpayers on the list passed on to local compliance teams end up not being audited, largely due to resource constraints.

3.2 Data

We exploit data on income tax self assessment random audits together with information on income tax returns. This combines a number of different HMRC datasets, linked together on the basis of encrypted taxpayer reference number and tax year.

Audit records for tax years 1998/99 to 2008/09 come from CQI (Compliance Quality Initiative), an HMRC operational dataset that records audits made into income tax self assessment and corporation tax self assessment returns. It does not include audits by the HMRC’s Large Business Service, Special Investigations or Employer Compliance Reviews. It includes operational information about the audits, such as start and end dates and audit outcomes (whether non-compliance was found, and the size of any correction, penalties and interest).

¹⁰For tax returns relating to 2007/08 or later, audits had to be opened within a year of the date when the return was filed.

We track individuals before and after the audit using information from tax returns for the years 1998/99 to 2011/12. This comes from two datasets: SA302 and Valid View. The SA302 dataset contains information that is sent out to taxpayers summarising their income and tax liability (the SA302 tax calculation form). It is derived from self assessment returns, which have been put through a tax calculation process. It contains information about total income and tax liability as well as a breakdown into different income sources (employment earnings, self employment profits, pensions, property income etc). For all of these variables, we uprate to 2012 using the Consumer Prices Index (CPI) and trim the top 1 per cent to avoid outliers having an undue impact on the results. We supplement these variables with information from Valid View, a dataset that draws directly from individuals' tax returns. For our purposes, Valid View provides demographics and filing information (e.g. filing date). Note that we cannot identify actual compliance behaviour subsequent to the audit: the number of random audit taxpayers that are re-audited is far too small for it to be possible to focus just on them.

One limitation is that a control group of non-selected individuals was not constructed at the time of selection for audit. We therefore draw control individuals from the pool of individuals who actually filed a tax return (i.e. those who appear in SA302). This creates some differences in the filing history between those selected for audit and those who we deem as controls. To understand the source of this difference, consider random audits for the 2006/07 tax year. These were selected on the basis of information held by HMRC in around February 2007, i.e. just before the end of the relevant tax year, and almost a year before the January 2008 filing deadline. At this point, HMRC had a near-complete list of taxpayers who would file a 2006/07 return, but a small number of taxpayers were missing. Those missing tended to be taxpayers newer to self-assessment including a number of first-time filers. As a result, the cases HMRC selected for a random audit tended to have been in self-assessment for slightly longer than the average among those who ended up filing a 2006/07 tax return. As a result, in our empirical strategy it is necessary for us to control for the length of time each taxpayer has been in self-assessment. More details – including tests to demonstrate this ensures samples are balanced – are given in Section 4 below.

3.3 Audit descriptives

Figure 1 shows the number of individuals per year who face an income tax random audit over the period 1998/99 to 2008/09.¹¹ On average over the period the probabilities of being audited are 0.04 per cent (four in 10,000) for random audits and 2.8 per cent for targeted audits.

¹¹There are also a small number of partnerships and trusts that are audited, but we exclude these from our analysis.

Table A3 provides some summary statistics for lags in, and durations of, the audit process among random audit cases. As described above, up to and including the 2006/07 return, HMRC had to begin an audit within 12 months of the 31 January filing deadline; since then, HMRC has had to begin an audit within 12 months of the filing date. The average lag between when the tax return was filed and when the random audit was started is 8.9 months, but 10 per cent have a lag of 14 months or more. The average duration of audits is 5.3 months, but 10 per cent experience a duration of 13 months or more. Taken together, this means that the average time between when a return is filed and when the audit is concluded is 14.3 months but there is a long tail for whom the experience is much more drawn out: for 10 per cent it is almost two years or more. This means that individuals will generally have filed at least one subsequent tax return before the outcome of the audit is clear and some will have filed two tax returns. This will be relevant for interpreting the results in Section 5.

4 Empirical Approach

4.1 Parameters of interest

To understand how audits affect future tax receipts we want to estimate the change in tax paid in the years subsequent to an audit that is caused by the audit. To understand the mechanisms we want to estimate a similar causal impact but on reported income, by income source.

In the case of tax paid (analogously for income sources), let $Y_{i,t+h}(1)$ denote the tax that would be reported by individual i , for tax year $t+h$, if he were audited in year t . Let $Y_{i,t+h}(0)$ be the tax he would report for tax year $t+h$ if he were not audited in year t . Then $\beta_{i,t,h} = Y_{i,t+h}(1) - Y_{i,t+h}(0)$ is the causal impact on the reporting of individual i if audited in year t , h years after the audit. Our interest is in estimating the average impact of an audit, across individuals and across audited tax years, separately by the number of years since audit:

$$\beta_h = \mathbb{E}_t [\mathbb{E}_i [\beta_{i,t,h}]] \quad \forall h \tag{3}$$

To estimate this we make use of the ‘random audits programme’ run by the tax authority (HMRC). This programme, described above, selects for audit a random sample of taxpayers from the pool of taxpayers known to be required to file for a given tax year. One can therefore compare those selected for audit with others who were not selected but who could have been.

In practice the random audit programme is successful only in *implementing* audits for a little more than two-thirds of those selected. This is largely explained by the high workload faced by the compliance teams implementing audits.¹² Although compliance officers are informed that random audit cases are considered high priority, they often receive more cases than they can complete and so some end up not being worked, though implementation is significantly higher than among targeted audits.

A small fraction of the control group (around two per cent) is also audited. This occurs because the random audits are selected before targeted audits, and no explicit control group is constructed for the programme to ‘hold out’ some individuals from targeting. Hence those selected for a random audit are being compared to a ‘business as usual’ group, rather than a pure control group. This will tend to reduce the estimated impacts, since individuals in the control group who are most likely to be non-compliant are actually audited.

In the empirical work to follow, we focus on the local average treatment effect (LATE), instrumenting receipt of audit with selection for random audit.¹³ This can be interpreted as the average effect of implementing an audit on the type of individuals who receive an audit only when selected for a random audit. This is the relevant number for a tax authority thinking about expanding the size of the random audit programme and the number of auditors. It gives the average impact h years after audit for a random additional audit case that might be worked, against which the cost of the audit would be compared.

It is not clear whether this should overestimate or underestimate individual-level non-compliance. Table A1 shows that audits are opened more quickly for those within the random audit group who were found to be more non-compliant, so it is possible that the remaining unworked cases had even lower misreporting.¹⁴ On the other hand, since individuals in the control group may still be selected for a targeted audit, the impact from many of the highest-yielding cases will not be included.

4.2 Estimation

Our sample consists of individuals who were randomly audited in some year between 1999 and 2009, and individuals who could have been audited in those same years but were not. For every individual selected for audit in a given tax year, we draw six control individuals from the population of those

¹²A very small share are also ‘deselected’ for audit after being assigned to the random audit group, for example because the taxpayer has died.

¹³In practice this scales up the ‘intention to treat parameter’ – the mean difference between those selected for audit and those who could have been but were not – by 1.5, since the difference in share audited between the groups is two-thirds.

¹⁴Note that an alternative explanation might be that auditors had less time to work on cases opened later, and hence revenues raised were lower. We do not attempt to distinguish these possibilities.

who could have been audited in the same tax year.¹⁵ If all audits occurred in a single tax year, we would ideally estimate the following specification:

$$Y_{ih} = \sum_h \beta_{0,h} \eta_h + \sum_h \beta_{1,h} \eta_h D_i + \varepsilon_{ih} \quad (4)$$

where Y_{ih} is the outcome for individual i , h years after audit (where h may be negative: this indicates years before audit); η_h are indicators for being h years after audit; and D_i is an indicator for whether the individual is audited. The error term, ε_{ih} , is clustered at the individual level. As noted above audit status, D_i , should be instrumented by selection in to audit, Z_i . Then $\beta_{1,h}$ is the impact of the audit on the outcome variable h years after audit, measured as the difference in the mean outcome for those actually audited and those who could have been audited.

Since we actually have audits across a number of different tax years, we can control additionally for year-specific effects. Adding a subscript s for the current calendar year gives the following specification:

$$Y_{ih_s} = \sum_h \beta_{0,h} \eta_h + \sum_h \beta_{1,h} \eta_h D_i + \sum_s \gamma_s T_s + \varepsilon_{ih_s} \quad (5)$$

where T_s is an indicator for the observation being observed in tax year s .

One limitation of our data is a slight mismatch between our treated and control samples in terms of their probability of filing in previous years, for reasons relating to the audit timeline as described in Subsection 3.2. This can be seen in Table A2 which studies sample balance between five and one years before audit, for income and tax totals, income components and individual characteristics. Overall balancing statistics suggest that the samples are fairly well balanced: the p-value of the likelihood-ratio test of the joint insignificance of all the regressors is 0.181, while the mean and median absolute standardised percentage bias across all outcomes of interest are low at 2.4 per cent and 1.7 per cent respectively.¹⁶ However, the likelihood of being in the sample in previous years ('survival') differs between our treatment and control groups. We therefore also include controls, $\{S_{i,-1}, \dots, S_{i,-4}\}$, for whether the individual was in the data in each of the four years before audit. Table 2 shows that once we condition on past survival the sample does appear balanced. We therefore

¹⁵In principle the entire population of taxpayers who could have been audited could have been used. However, since the data could be accessed only in a secure facility at the tax office, computational constraints limited the sample size that could be used.

¹⁶The standardised percentage bias is the difference in the sample means between treated and control groups as a percentage of the square root of the average of the sample variances in the treated and control groups (see Rosenbaum and Rubin, 1985). Rubin's B and R statistics are also well within reasonable thresholds to consider the samples to be balanced, at 10.8 and 0.983 respectively. Rubin's B is the absolute standardized difference of the means of the linear index of the propensity score in the treated and control group. Rubin's R is the ratio of treated to control variances of the propensity score index. Rubin (2001) recommends that B be less than 25 and that R be between 0.5 and 2 for the samples to be considered sufficiently balanced.

estimate the following specification:

$$Y_{ihs} = \sum_h \beta_{0,h} \eta_h + \sum_h \beta_{1,h} \eta_h D_i + \sum_s \gamma_s T_s + \sum_{s=-4}^{-1} \delta_s S_{i,s} + \varepsilon_{ihs} \quad (6)$$

instrumenting D_i by Z_i , as discussed previously. $\beta_{1,h} \forall h$ are the parameters of interest, measuring the audit impact.

5 Short and Long Run Effects of Audits

In this section we demonstrate four main results. First, we show that there is significant non-compliance among individual self assessment taxpayers, both in the share of taxpayers who are found non-compliant and the share of tax that is misreported. Second, we demonstrate that third party reporting is important in influencing levels of compliance, but only on the *intensive* margin of how much tax is misreported. Third, we provide evidence of important *dynamic effects*, with the additional tax revenue over the five years post-audit equalling 1.5 times the direct revenue raised by audit. Finally, we show that the observed dynamics are consistent only with the information revelation mechanism discussed in Section 2.

5.1 Evidence of non-compliance

We begin by studying the direct results of random audits, using data on 34,630 random audits of on individual self assessment taxpayers from 1998/99 to 2008/09. Table 3 summarises the outcomes of these random audits. More than half of all returns are found to be correct, and 11 per cent are found to be incorrect but with no underpayment of tax, but 36 per cent are ‘non-compliant’ i.e. incorrect and have a tax underpayment.¹⁷ Whilst this is a much higher rate of non-compliance than has been found in other developed country contexts, it should be noted that the self assessment tax population is a selected subset of all taxpayers. In particular, it covers those for whom simple withholding of income at source is not sufficient to collect the correct tax. This may be either because some income cannot be withheld e.g. property or self-employed income, or because there are interactions between various taxes and benefits, such as the withdrawal from a husband’s income of child benefit claimed by his wife if his income is high enough. Despite this, since self assessment

¹⁷Incorrect with no underpayment includes those who paid the correct amount of tax, but in reporting incomes and deductions had for example claimed a legitimate deduction but filed it under the wrong deduction line, for example claiming mortgage interest relief on a rented property but including it under legal fees rather than loan interest. It also includes some cases where actual overpayments of tax were made.

taxpayers make up a third of all UK taxpayers, this implies an overall non-compliance rate of at least 12 per cent among all taxpayers.

Turning to the intensive margin, the average additional tax owed among the non-compliant is £2,314. Since just over a third of random audits find evidence of non-compliance, the average return from an audit is then £826. However, the distribution is heavily skewed: 60 per cent of non-compliant individuals owe additional tax of £1,000 or less, whilst four per cent owe more than £10,000. In terms of total revenue, those owing £1,000 or less make up only 9 per cent of the underreported revenue; the four per cent owing more than £10,000 collectively owe more than 42 per cent of the revenue.

5.2 Third party reporting

Understanding the role of third party reporting has been an important advance in the recent literature: this makes it much easier for a tax authority to detect misreporting by a taxpayer, since tax returns can be automatically checked against third party reports. Existing evidence suggests that third party reporting is important in explaining the relatively high levels of observed compliance (Kleven et al., 2011).

In our context such effects are also visible, but they are seen only on the intensive margin (Figure 2). Figure 2 orders income sources from no third party information on the left to fully third party reported on the right.¹⁸ The left panel of Figure 2 shows that third party reporting is not a good predictor of *whether* a taxpayer is non-compliant (the extensive margin). However, as seen in the right panel, conditional on being non-compliant, misreporting is clearly greater when no third party income is available.

To compare these results to Kleven et al. (2011), they find 14.9 per cent of self-employment income is under-reported but only 2.3 per cent of dividend income and 1.1 per cent of employment income, not conditioning on compliance status. The comparable numbers from our context are 26.1 per cent, 5.2 per cent, and 3.5 per cent. Under-reporting therefore seems to be a greater problem in the UK context than in Denmark, whether third party information is available or not. Similar results apply when looking at non-compliance status: in Denmark the non-compliant share are 44.9, 2.2, and 2.6 per cent for self-employment, dividend, and employment income respectively, compared with 58.7, 10.5, and 28.5 per cent respectively.¹⁹

¹⁸See Table A4 for more details on the level of third party reporting by income source in the UK.

¹⁹One point to note is that in our data we cannot distinguish between non-compliance on the income itself versus on deductions claimed against that income source. Since deductions may be harder to get third party reports for, non-compliance here is likely to be higher than on the income itself. However, these figures remain much larger than the share who are non-compliant on deductions in Kleven et al. (2011).

The extensive margin results imply that there are some other important differences between individuals receiving different types of income, potentially suggesting some effect of ‘tax morale’ – the intrinsic disutility from misreporting (Luttmer and Singhal, 2014). Although our later results across income sources will focus on the the dynamics of any impact compared to its peak, so are not directly affected by this, this pattern will motivate our study of within person effects to ensure that the results are not driven by cross-sectional differences between taxpayers.²⁰

5.3 Dynamic effects

Beyond the direct effects of the audit, described above, we also see clear evidence of *dynamic* effects. Comparing individuals who were randomly selected for audit with individuals who could have been (but were not) selected, those selected for audit on average report higher incomes in the years after audit. Figure 3 shows the estimated impact on those who were actually audited (i.e. the LATE).²¹ The impact of an audit peaks two years after the tax year for which the audit is conducted: this is consistent with the fact that many audits are not started until after the following year’s tax return has already been submitted.²² Reported income among audited taxpayers is significantly greater than among non-audited taxpayers for five years after the audit, and the point estimate appears to decline relatively smoothly, getting close to zero by the eighth tax year after the audited year.

Figure 3 shows that a very similar pattern holds for the impact on *tax revenue* reported: again there is a clear dynamic effect, peaking two years after the audited year and declining to zero by year eight, though not significantly different from zero by year five. From this we can estimate how much revenue audits raise on average by changing the behaviour of audited individuals. Over the five (eight) years after the audited year, the dynamic effects bring in an additional £1,230 (£1,530). This is 1.5 (1.8) times the direct effect of audit. This highlight the policy importance of studying such dynamic effects: when determining the optimal audit strategy, the revenue raising effects of audits would be grossly understated without considering the impact on future behaviour, which would typically imply too few audits taking place.

²⁰Appendix Appendix B provides more information on how non-compliance relates to individual characteristics.

²¹The difference in the share audited between the treated and control group is around 66 percentage points, so the LATE is around 1.5 times the intention to treat estimate.

²²In our sample, almost a quarter of audits are not opened for more than 12 months from the date of filing (see Table A3). Additionally, there can be some lag between the tax authority ‘taking up’ a case for audit and notification being received by the taxpayer. If taxpayers each consistently file at the same time every year, this implies at least one quarter would have filed without knowledge of the audit. More than half will have filed without knowing the result of the audit (Table A3).

5.4 Understanding the mechanism

To understand *why* audits appear to change reporting behaviour, recall the possible mechanisms and their implications, as discussed in Section 2. First, receiving an audit may change the perceived penalty from non-compliance being detected. Second, being audited may change the perceived probability of being audited again (and evasion being detected) in subsequent years; the probability may change positively or negatively. Third, audits may provide additional information on current income to the tax authority, which taxpayers realise affects their ability to misreport without being detected in the future.

The first explanation should lead to a permanent change in reporting behaviour. Results from Subsection 5.3 therefore already rule out this explanation: from Figures 3 and 4 it can be seen that the impact on reported income and tax is declining over time. The second and third explanations are both consistent with reported income returning back to baseline over some years after audit. However, they are distinguished by their predictions by income source: the second mechanism implies that income from all sources should decline at the same rate, while the third mechanism implies that the decline should be more rapid for sources with a lower autocorrelation.

Table 4 show the autocorrelation in income by income source. From this we see that pension income is highly autocorrelated, since it will typically be an annuity and therefore fixed over time; property income is slightly more volatile, since rents may vary more; and at the other extreme self-employment and dividend income are considerably more volatile. Figure 5 shows how the impact of an audit changes over time for the different components of income, in each case relative to the peak impact for that income source. The dynamic effects for each source are normalised by the peak impact to allow a comparison of how reporting changes over time. From this it can be seen that the pattern for rates of decline is exactly in line with the table of autocorrelations: more highly autocorrelated variables decline less quickly. This is exactly as predicted by the third mechanism, and is inconsistent with mechanism two.

6 Conclusion

This paper investigated the dynamic effects of audits on income reported in subsequent tax returns. Understanding these effects is important both from the perspective of quantifying the returns to the tax authority from an audit, and for assessing the mechanisms by which audits might influence taxpayer behaviour. To answer this question we exploited a random audit program run by the UK tax authority (HMRC) under which an average of around 3,300 individuals are audited each year.

We used data on audits over the period 1998/99 to 2008/09 and tracked responses on tax returns between 1998/99 and 2011/12.

We established four main results. First, we showed that there is significant non-compliance among individual self assessment taxpayers, both in the share of taxpayers who are found non-compliant and the share of tax that is misreported. Second, we demonstrated that in our context third party reporting is important in influencing levels of compliance, but only on the *intensive* margin of how much tax is misreported. Third, we provided evidence of important *dynamic effects*, with the additional tax revenue over the five years post-audit equalling 1.5 times the direct revenue raised by audit. Finally, we showed that the observed dynamics are consistent only with audits revealing information to the tax authority, which makes misreporting certain income sources easier to detect for a period after the audit.

Our results have three main policy implications. First, taking dynamic effects into account substantially increases the estimated revenue impact of audits. The direct effect of an audit is (on average) £830, whilst the cumulative dynamic effect over the subsequent five years is £1230, 1.5 times the direct effect. This suggests that the optimal audit rate should be increased relative to the situation where there are no dynamic effects. Second, the variation in dynamic effects observed across different income components alters the way in which targeted audits should be targeted: audits should focus more on individuals reporting types of income with the largest dynamic effects. For example, the peak annual impact on reported self-employment income for each self-employed individual is over £1,000, higher than other components. This suggests focusing more on individuals reporting self-employment income. Likewise, although the maximum annual impact on pension income is lower, it is persistent, so there may be more incentive to target individuals reporting pension of income. Third, there are implications for setting optimal re-auditing strategies. Impacts for reported self-employment income and dividend income die away after about four years, so it might make sense to revisit these individuals after this sort of horizon. In contrast, the impact on reported pension income seems to persist for at least eight years, implying that there is less need to re-audit these individuals so soon.

Our findings also highlight the importance of further study of the indirect effect of tax compliance audits. One natural direction for further work would be to understand how the dynamic effects differ in the context of targeted audits, which are focused on individuals deemed likely to be non-compliant. A second avenue for exploration is the spillover effect of audits: does auditing taxpayers change the behaviour of other taxpayers with whom they interact. Better understanding these effects is crucial in determining optimal audit policy.

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Tables and Figures

Table 1: Share of Taxpayers with Each Source of Income

| Income component | Proportion |
|------------------------|------------|
| Employment | .309 |
| Self employment | .240 |
| Interest and dividends | .420 |
| Pensions | .193 |
| Property | .087 |
| Foreign | .031 |
| Trusts and estates | .007 |
| Share schemes | .001 |
| Other | .020 |

Notes: Annual averages for tax years 1998/99 to 2008/09.

Source: Authors' calculations based on HMRC administrative datasets.

Table 2: Sample Balance, Conditioning on Filing History

| Years after audit | | -5 | -4 | -3 | -2 | -1 |
|------------------------|------------|--------|--------|--------|--------|--------|
| Characteristics | | | | | | |
| Female | Mean | .274 | .276 | .278 | .282 | .287 |
| | Difference | -.005 | -.006 | -.005 | -.006 | -.005 |
| | p-value | .236 | .212 | .292 | .234 | .338 |
| Age | Mean | 49.2 | 49.3 | 49.3 | 49.4 | 49.5 |
| | Difference | .0 | .0 | .1 | .1 | .1 |
| | p-value | .472 | .600 | .188 | .170 | .110 |
| In London or SE | Mean | .333 | .334 | .335 | .333 | .331 |
| | Difference | -.003 | .001* | .003 | .002 | .002 |
| | p-value | .159 | .026 | .015 | .317 | .190 |
| Has tax agent | Mean | .628 | .614 | .603 | .589 | .573 |
| | Difference | -.003 | -.001 | -.001 | .002 | .002 |
| | p-value | .522 | .500 | .376 | .675 | .606 |
| Income and tax totals | | | | | | |
| Total taxable income | Mean | 35,075 | 34,670 | 34,030 | 32,912 | 31,755 |
| | Difference | -2 | 35 | -163 | 71 | 56 |
| | p-value | .979 | .469 | .012 | .280 | .439 |
| Total tax | Mean | 9,646 | 9,539 | 9,321 | 8,979 | 8,635 |
| | Difference | 14 | 12 | -40 | 12 | 15 |
| | p-value | .982 | .288 | .061 | .261 | .887 |
| Income components | | | | | | |
| Employment | Mean | 22,508 | 22,534 | 22,266 | 21,708 | 21,145 |
| | Difference | 11 | -57 | -98 | 112* | 43* |
| | p-value | .758 | .023 | .152 | .049 | .05 |
| Self employment | Mean | 6,546 | 6,379 | 6,200 | 5,950 | 5,581 |
| | Difference | 56 | 38 | -49 | -18 | 29 |
| | p-value | .298 | .435 | .033 | .161 | .684 |
| Interest and dividends | Mean | 4,007 | 3,905 | 3,895 | 3,759 | 3,645 |
| | Difference | -26 | 16 | -27 | 7 | 4 |
| | p-value | .667 | .189 | .235 | .958 | .086 |
| Pensions | Mean | 3,493 | 3,542 | 3,561 | 3,562 | 3,531 |
| | Difference | -23 | -23 | -3 | 4 | 22 |
| | p-value | .806 | .482 | .681 | .463 | .523 |
| Property | Mean | 869 | 844 | 811 | 769 | 726 |
| | Difference | -5 | -6 | 0 | 6 | 0 |
| | p-value | .282 | .209 | .525 | .072 | .518 |
| Foreign | Mean | 194 | 193 | 194 | 181 | 169 |
| | Difference | 1 | -1 | 5 | -4 | 0 |
| | p-value | .627 | .137 | .526 | .117 | .925 |
| Trusts and estates | Mean | 150 | 145 | 145 | 131 | 123 |
| | Difference | 17 | -1 | -6 | 4 | 3 |
| | p-value | .153 | .963 | .204 | .686 | .824 |
| Share schemes | Mean | 91 | 104 | 68 | 62 | 55 |
| | Difference | 8* | -8 | -1 | 9 | 1 |
| | p-value | .019 | .711 | .783 | .683 | .243 |
| Other | Mean | 80 | 75 | 76 | 73 | 71 |
| | Difference | 3 | -3 | 0 | 1 | 2 |
| | p-value | .618 | .184 | .228 | .984 | .194 |

Notes: ‘Mean’ is the mean outcome in the control (not selected for audit) group across all years. ‘Difference’ is the coefficient on the treatment dummy in a regression of the outcome on a treatment dummy and dummies for whether the taxpayer filed taxes in each of the four years before audit (or placebo audit for controls). Treatment dummy equals 1 if taxpayer was selected by HMRC for a random audit. p-values are derived from an F-test that coefficients on interactions between treatment and tax year dummies are all zero in a regression of the outcome of interest on tax year dummies, interactions between treatment and tax year dummies, and dummies for whether the taxpayer filed taxes in each of the four years before audit (or placebo audit for controls). This is a stronger test than just testing the coefficient on treatment not interacted. Tests for all outcomes other than ‘survives’ are conditional on survives = 1. Monetary values are in 2012 prices. Standard errors are clustered by taxpayer. * $p < .05$, ** $p < .01$, *** $p < .001$. **Source:** Authors’ calculations based on HMRC administrative datasets.

Table 3: Random Audit Outcomes

| | Mean | Std. dev. |
|---|--------|-----------|
| Proportion of audited returns deemed | | |
| Correct | .532 | .499 |
| Incorrect but no underpayment | .111 | .314 |
| Incorrect with underpayment (non-compliant) | .357 | .479 |
| Mean additional tax if non-compliant (£) | 2,314 | 7,758 |
| Distribution of additional tax if non-compliant | | |
| Share £1-100 | .116 | .320 |
| Share £101-1,000 | .483 | .500 |
| Share £1,001-10,000 | .361 | .480 |
| Share £10,001+ | .039 | .194 |
| Observations | 34,630 | |

Notes: Annual averages for tax years 1998/99 to 2008/09. Includes all individuals with a completed random audit.
Source: Authors' calculations based on HMRC administrative datasets.

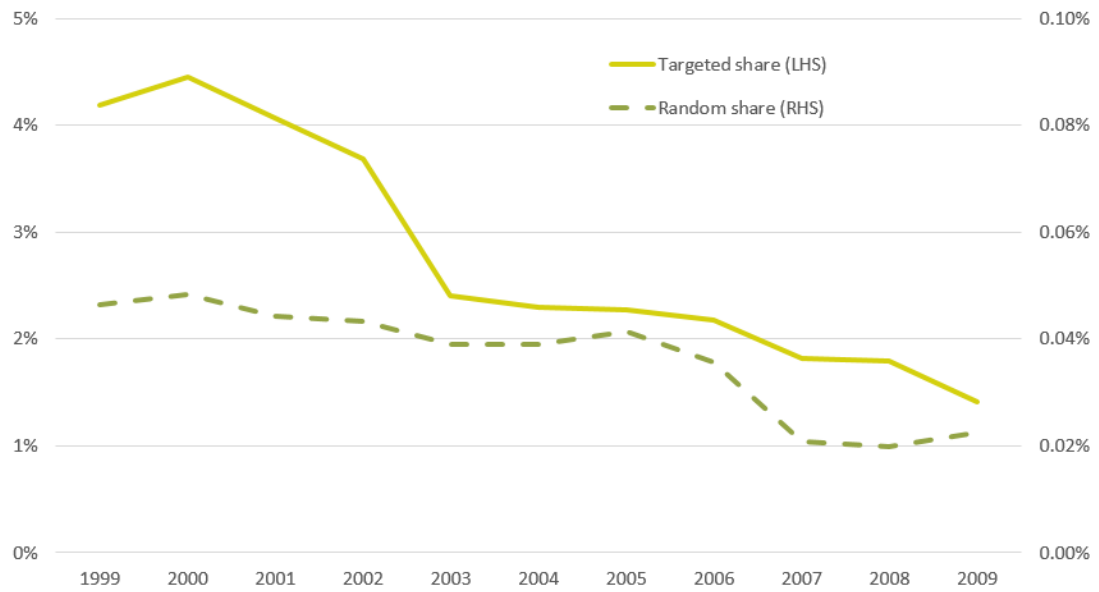
Table 4: Autocorrelation by Income Source

| | Corr(t,t-1) | Corr(t,t-2) | Corr(t,t-3) |
|------------------------|-------------|-------------|-------------|
| Pension income | .946 | .904 | .864 |
| Property income | .896 | .836 | .790 |
| Employment income | .862 | .769 | .690 |
| Interest income | .835 | .722 | .640 |
| Self-employment income | .832 | .728 | .644 |
| Dividend income | .813 | .723 | .657 |
| Sample size | 4,506,548 | 4,506,548 | 4,506,548 |

Notes: Annual averages for years 1998/99 to 2011/12.

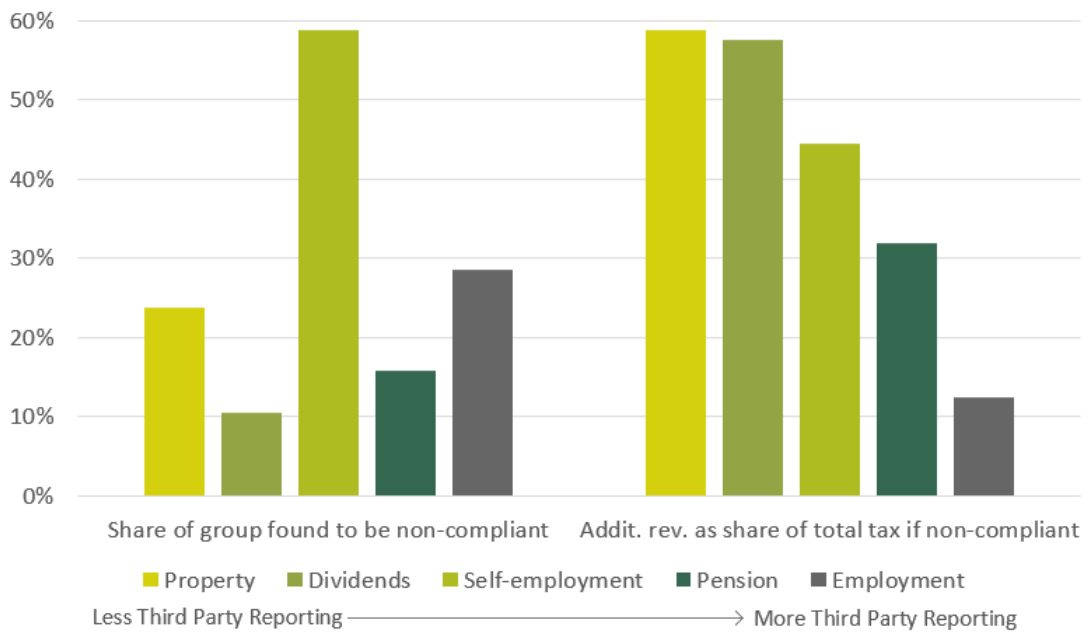
Source: Calculations based on HMRC administrative datasets.

Figure 1: Change in the probability of audit over time



Notes: Constructed using data on individuals who received an audit of their self assessment tax return for a tax year between 1998/1999 and 2008/2009, and the full sample of self assessment returns for the same period.
Source: Calculations based on HMRC administrative datasets.

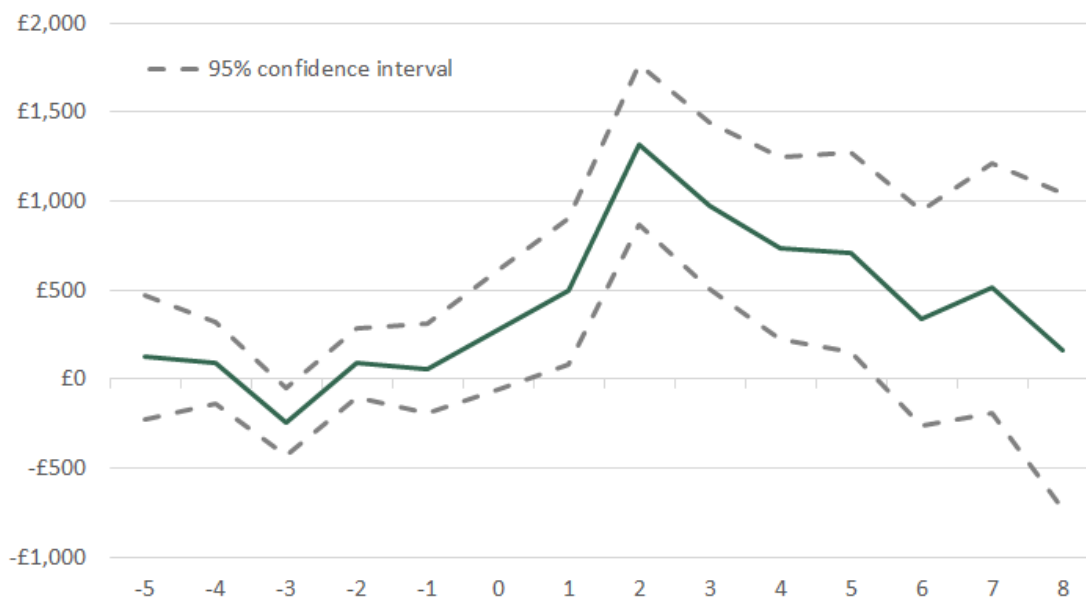
Figure 2: Third party information doesn't explain the extensive margin of non-compliance, but does explain the intensive margin



Notes: Constructed using data on individuals who received a random audit of their self assessment tax return for a tax year between 1998/1999 and 2008/2009. Income sources are ordered from least third party reporting on the left, to most third party reporting on the right. See Table A4 for details. ‘Share of group found non-compliant’ is the share of individual taxpayers receiving income from only that income source, plus potentially bank interest, who are found to owe additional tax when audited. ‘Additional revenue as a share of total tax if non-compliant’ is the additional tax owed divided by total tax owed, averaged across individual taxpayers who owed a positive amount i.e. who were non-compliant.

Source: Calculations based on HMRC administrative datasets.

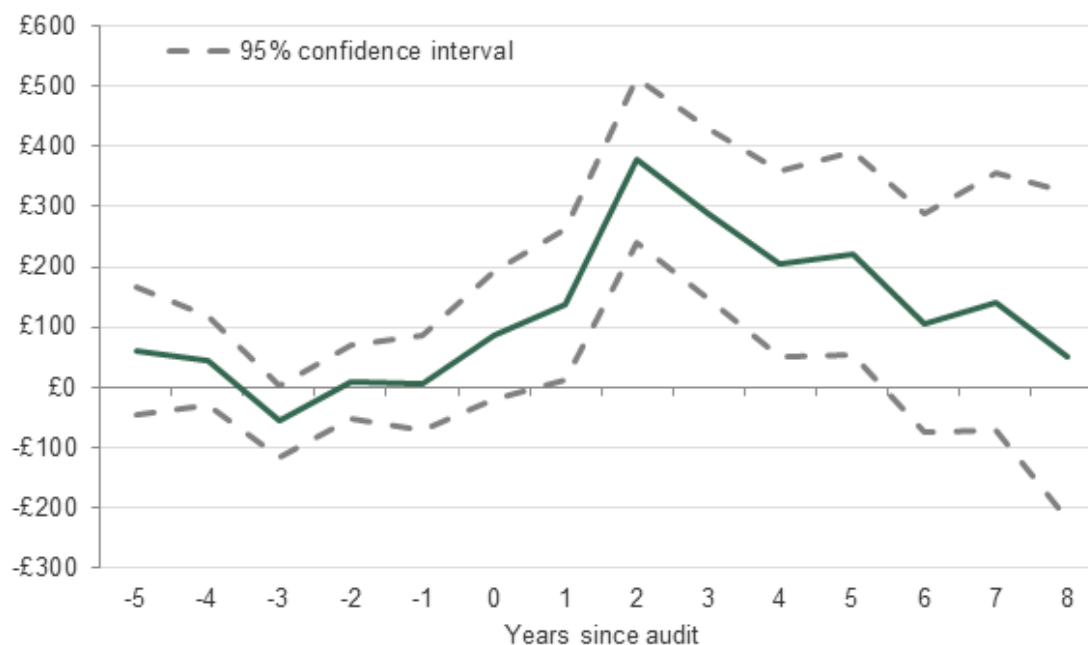
Figure 3: Dynamic effect of audits on total reported income



Notes: Sample includes individuals selected for a random audit between 1998/99 and 2008/09, and control individuals who could have been selected in the same years but were not. The solid line plots the point estimate for the difference in average ‘total reported income’ (income from all sources) between individuals who were and weren’t audited, for different numbers of years after the audit. This comes from a regression of total reported income on dummies for years since audit (or placebo audit for controls), dummies for years since audit (or placebo audit for controls) interacted with treatment status, tax year dummies, and dummies for whether the taxpayer filed a return in each of the four years before audit, with audit status instrumented by selection for audit. Standard errors are clustered at the individual level.

Source: Calculations based on HMRC administrative datasets.

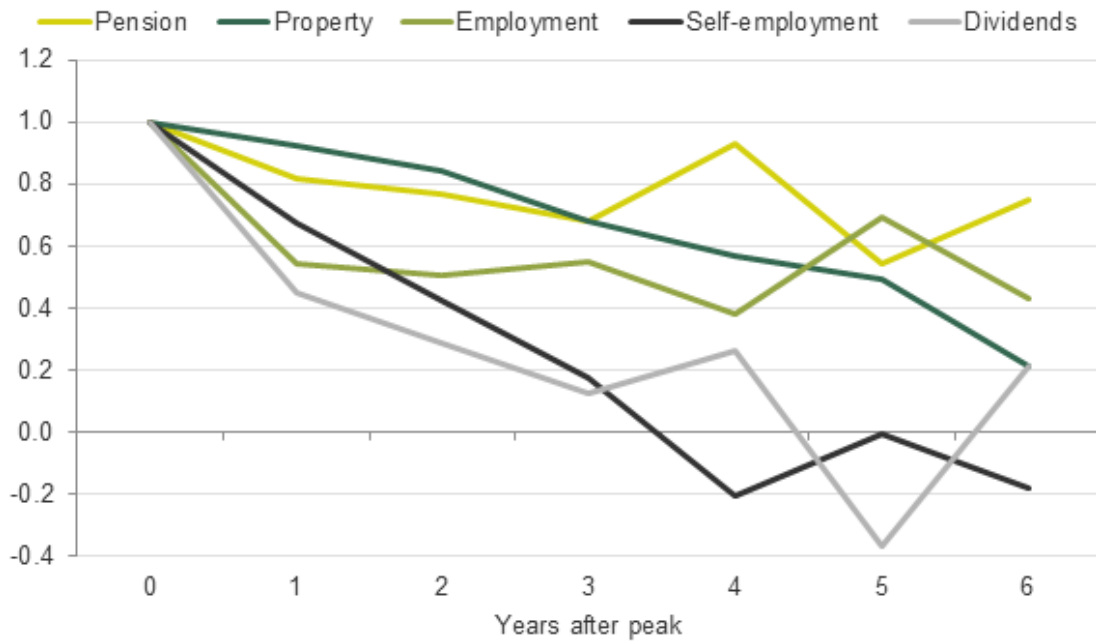
Figure 4: Dynamic effect of audits on total reported tax owed



Notes: Sample includes individuals selected for a random audit between 1998/99 and 2008/09, and control individuals who could have been selected in the same years but were not. The solid line plots the point estimate for the difference in average 'total reported tax' between individuals who were and weren't audited, for different numbers of years after the audit. This comes from a regression of total reported tax on dummies for years since audit (or placebo audit for controls), dummies for years since audit (or placebo audit for controls) interacted with treatment status, tax year dummies, and dummies for whether the taxpayer filed a return in each of the four years before audit, with audit status instrumented by selection for audit. Standard errors are clustered at the individual level.

Source: Calculations based on HMRC administrative datasets.

Figure 5: Relative dynamics by income source: more volatile sources of income see faster declines



Notes: Sample includes individuals selected for a random audit between 1998/99 and 2008/09, and control individuals who could have been selected in the same years but were not. Each line plots the point estimate for the difference in the average of a particular component of income between individuals who were and weren't audited, for different numbers of years after the audit. This comes from a regression of each income component on dummies for years since audit (or placebo audit for controls), dummies for years since audit (or placebo audit for controls) interacted with treatment status, tax year dummies, and dummies for whether the taxpayer filed a return in each of the four years before audit, with audit status instrumented by selection for audit. Standard errors are clustered at the individual level.

Source: Calculations based on HMRC administrative datasets.

Appendices

Appendix A Additional Tables and Figures

Table A1: Cases opened quickly uncover more underreported tax

| Lag between filing and audit start (months) | Mean underpaid tax (£) | Standard Error (£) | N |
|--|---------------------------|-----------------------|-------|
| ≤2 | 1460 | 249 | 614 |
| 3 | 1120 | 145 | 921 |
| 4 | 953 | 86 | 1,338 |
| 5 | 882 | 97 | 1,369 |
| 6 | 1040 | 100 | 1,409 |
| 7 | 896 | 78 | 1,454 |
| 8 | 834 | 76 | 1,564 |
| 9 | 923 | 96 | 1,591 |
| 10 | 784 | 86 | 1,906 |
| 11 | 784 | 99 | 2,286 |
| 12 | 600 | 73 | 1,101 |
| 13-21 | 693 | 129 | 3,006 |

Notes: Annual averages for tax years 1998/99 to 2008/09. Includes all individuals with a completed random audit. Audits opened within two months of filing are grouped together to satisfy disclosivity restrictions.

Source: Authors' calculations based on HMRC administrative datasets.

Table A2: Sample balance

| Years after audit | | -5 | -4 | -3 | -2 | -1 |
|------------------------|------------|---------|---------|---------|---------|---------|
| Characteristics | | | | | | |
| Female | Mean | .274 | .276 | .278 | .282 | .287 |
| | Difference | -.006 | -.004 | -.002 | -.001 | -.002 |
| | p-value | .221 | .359 | .606 | .627 | .863 |
| Age | Mean | 49.2 | 49.3 | 49.3 | 49.4 | 49.5 |
| | Difference | .2 | .3 | .3 | .2 | .2 |
| | p-value | .756 | .586 | .390 | .610 | .057 |
| In London or SE | Mean | .333 | .334 | .335 | .333 | .331 |
| | Difference | -.006 | .001* | .004* | .002 | .002 |
| | p-value | .177 | .025 | .011 | .281 | .152 |
| Has tax agent | Mean | .628 | .614 | .603 | .589 | .573 |
| | Difference | .000 | .002 | .001 | .003 | .005 |
| | p-value | .547 | .508 | .405 | .396 | .412 |
| Survives | Mean | .624 | .669 | .728 | .803 | .892 |
| | Difference | .032*** | .039*** | .047*** | .050*** | .050*** |
| | p-value | .000 | .000 | .000 | .000 | .000 |
| Income and tax totals | | | | | | |
| Total taxable income | Mean | 35,075 | 34,670 | 34,030 | 32,912 | 31,755 |
| | Difference | 881 | 492 | 403* | 1,051* | 1,095* |
| | p-value | .374 | .157 | .028 | .012 | .012 |
| Total tax | Mean | 9,646 | 9,539 | 9,321 | 8,979 | 8,635 |
| | Difference | 260 | 63 | 82 | 310 | 337* |
| | p-value | .539 | .303 | .055 | .064 | .027 |
| Income components | | | | | | |
| Employment | Mean | 22,508 | 22,534 | 22,266 | 21,708 | 21,145 |
| | Difference | -31 | -136 | 180* | 909** | 721* |
| | p-value | .162 | .371 | .028 | .006 | .027 |
| Self employment | Mean | 6,546 | 6,379 | 6,200 | 5,950 | 5,581 |
| | Difference | 356 | 328 | 173 | 99 | 200* |
| | p-value | .151 | .174 | .311 | .106 | .025 |
| Interest and dividends | Mean | 4,007 | 3,905 | 3,895 | 3,759 | 3,645 |
| | Difference | -36 | 208 | 18 | 63 | 112 |
| | p-value | .767 | .432 | .700 | .578 | .580 |
| Pensions | Mean | 3,493 | 3,542 | 3,561 | 3,562 | 3,531 |
| | Difference | 176 | 168 | 128 | 148 | 159 |
| | p-value | .425 | .478 | .642 | .307 | .327 |
| Property | Mean | 869 | 844 | 811 | 769 | 726 |
| | Difference | 18 | -2 | 37 | 47 | 31 |
| | p-value | .813 | .952 | .576 | .498 | .134 |
| Foreign | Mean | 194 | 193 | 194 | 181 | 169 |
| | Difference | 23 | -1 | -5 | -6 | 1 |
| | p-value | .759 | .240 | .956 | .317 | .766 |
| Trusts and estates | Mean | 150 | 145 | 145 | 131 | 123 |
| | Difference | 46 | 17 | 8 | 19 | 8 |
| | p-value | .245 | .367 | .290 | .125 | .367 |
| Share schemes | Mean | 91 | 104 | 68 | 62 | 55 |
| | Difference | 17* | 24 | 22** | 11 | -6** |
| | p-value | .043 | .062 | .004 | .132 | .008 |
| Other | Mean | 80 | 75 | 76 | 73 | 71 |
| | Difference | -1 | -4 | 0 | 5 | 6 |
| | p-value | .747 | .675 | .645 | .796 | .167 |

Notes: ‘Mean’ is the mean outcome in the control (not selected for audit) group across all years. ‘Difference’ is the coefficient on the treatment dummy in a regression of the outcome on a treatment dummy. Treatment dummy equals 1 if taxpayer was selected by HMRC for a random audit. p-values are derived from an F-test that coefficients on interactions between treatment and tax year dummies are all zero in a regression of the outcome of interest on tax year dummies and interactions between treatment and tax year dummies. This is a stronger test than just testing the coefficient on treatment not interacted. Tests for all outcomes other than ‘survives’ are conditional on survives = 1. Monetary values are in 2012 prices. Standard errors are clustered by taxpayer. * $p < .05$, ** $p < .01$, *** $p < .001$. **Source:** Authors’ calculations based on HMRC administrative datasets.

Table A3: Random audit lags and durations

| | Mean | Std. dev. | Median | 75th | 90th |
|----------------------------------|------|-----------|--------|------|------|
| Lag to audit start (months) | 8.9 | 4.0 | 9 | 11 | 14 |
| Audit duration (months) | 5.3 | 6.6 | 3 | 7 | 13 |
| Total time to audit end (months) | 14.3 | 7.3 | 13 | 17 | 23 |

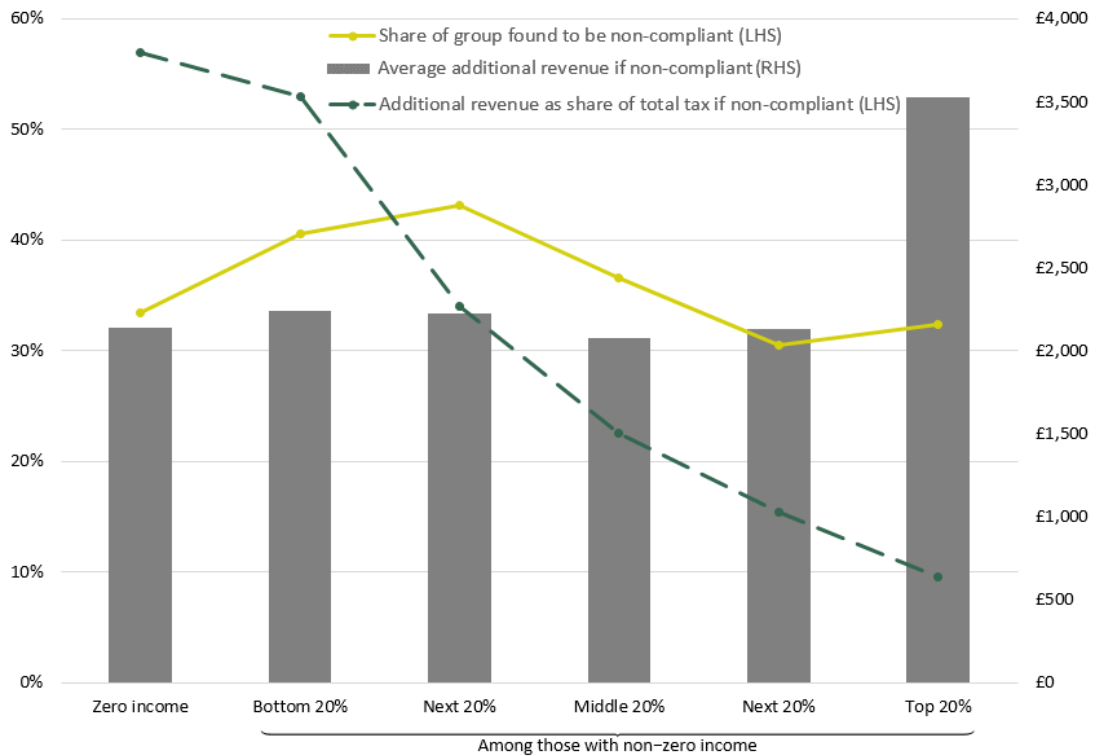
Notes: Annual averages for tax years 1998/99 to 2008/09. Includes all individuals with a completed random audit.
Source: Authors' calculations based on HMRC administrative datasets.

Table A4: Third party reporting arrangements in the UK

| Income component | Degree of third-party reporting |
|------------------|---|
| Employment | Yes through the employer |
| Self employment | No unless an entertainer, sportsman, or contractor in the construction industry |
| Interest | Yes through bank |
| Dividends | No |
| Pensions | Yes through pension provider |
| Property | No |

Source: Personal communication with Tracey Bowler, Tax Law Review Committee.

Figure A1: The share of income which is misreported is falling across the income distribution



Notes: Constructed using data on individuals who received a random audit of their self assessment tax return for a tax year between 1998/1999 and 2008/2009. Income grouping is done based on previous year's reported income. 16.2% of individuals report having zero income in the previous year. The remaining individuals are divided into five equal sized bins based on their previous income: quintiles conditional on reporting non-zero income. 'Share of group found to be non-compliant' is the share of individual taxpayers receiving income from only that income source, plus potentially bank interest, who are found to owe additional tax when audited. 'Average additional revenue if non-compliant' is the average total tax in 2012 £ that was not reported among those individuals for whom some tax was not reported (the non-compliant). 'Additional revenue as a share of total tax if non-compliant' is the additional tax owed divided by total tax owed, averaged across individual taxpayers who were non-compliant.

Appendix B Evidence on Heterogeneity in Audit Impact

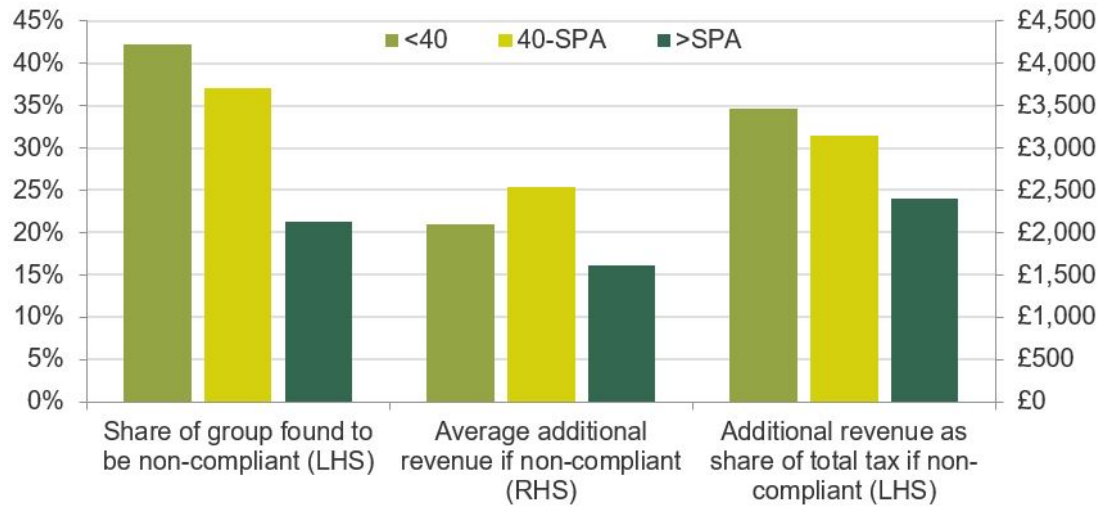
In this section we describe the heterogeneity in the direct impact of audits.

As seen in Table 3, 36 per cent of taxpayers are found to be non-compliant i.e. to have under-reported tax due. Among these taxpayers, 32 per cent of the tax they owe was not reported, around £2,320 in cash terms (2012 prices). Figures 2 and A1 already provide evidence on how these results look across different income sources and across the distribution of reported income.

Figures B1 and B2 show how non-compliance varies by age and sex. These results are consistent with Kleven et al. (2011), who find that being older and being female are negatively associated with evasion. However, while they find that only the result on sex is statistically significant, these differences are all significant in our case. Table B1 shows that these results also hold conditional on additional predictors of non-compliance, including age, sex, income source, industry, and region. Note that these are not to be interpreted causally, since many of these characteristics are choices for the taxpayer. Nevertheless, the associations are interesting to understand, and are relevant for current targeting of audits, though of course using them for targeting is subject to the Lucas critique.

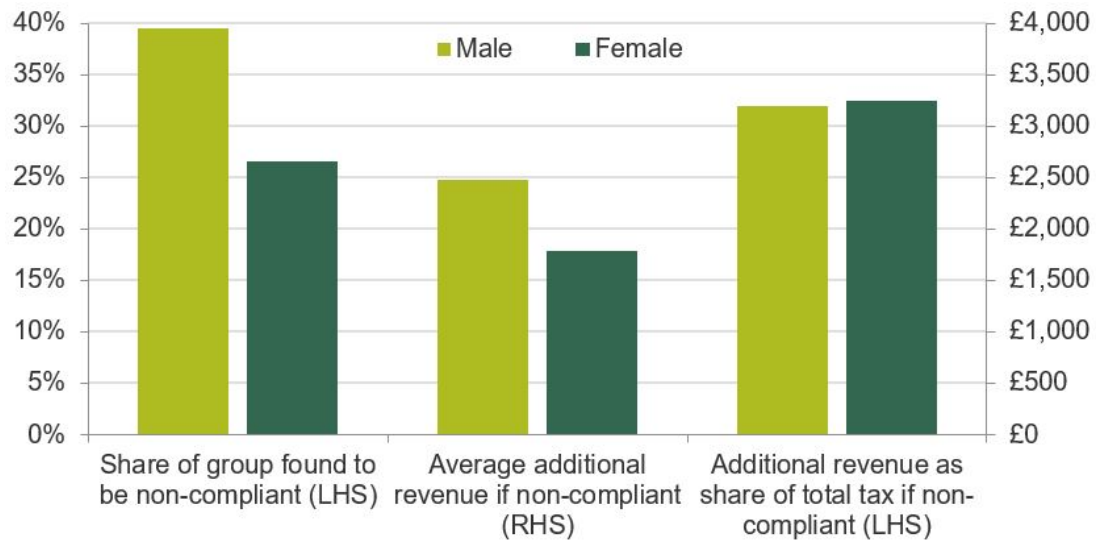
In contrast to Kleven et al. (2011), being located in the capital city is not associated with lower evasion. Breaking this down further, the share of Londoners found to be non-compliant is higher than in the other nations of the UK (where England is calculated excluding London) except Northern Ireland (see B3). Again these differences are all statistically significant, and hold when other characteristics are controlled for.

Figure B1: Non-compliance by age



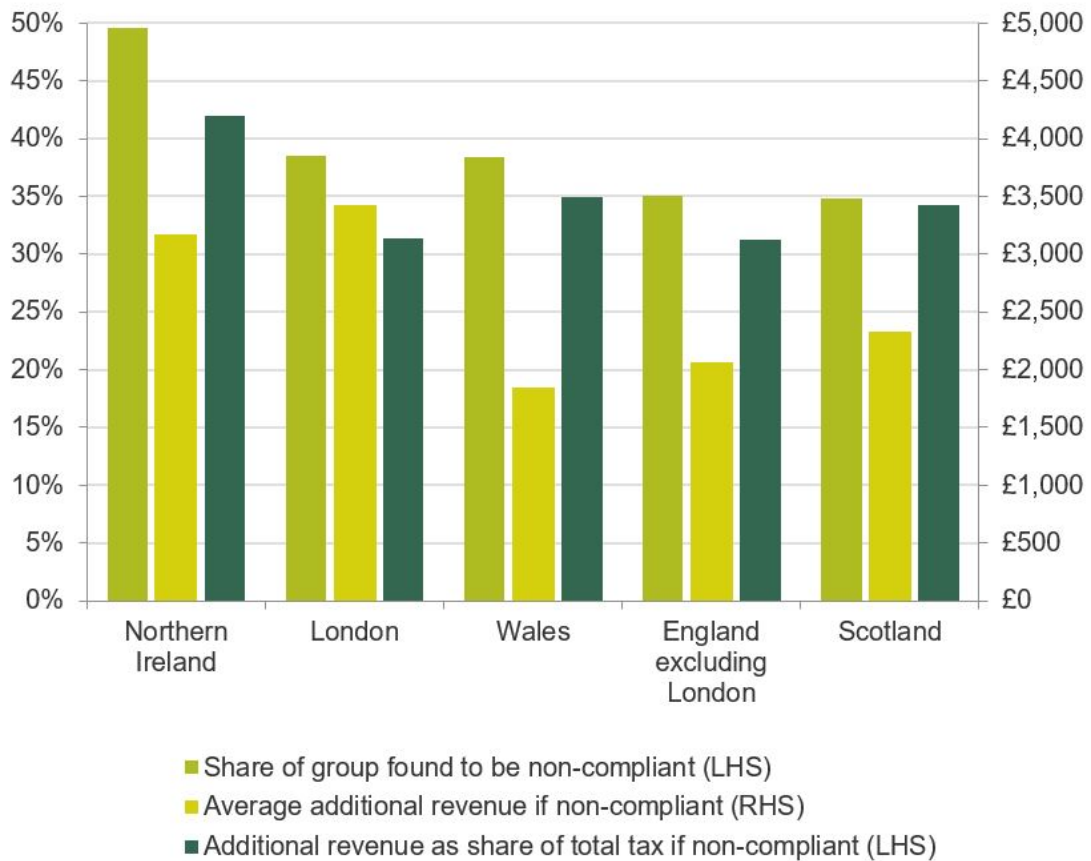
Notes: Constructed using data on individuals who received a random audit of their self assessment tax return for a tax year between 1998/1999 and 2008/2009. SPA is the state pension age, which in the UK was 65 for men and 60 for women over this period. 'Share of group found to be non-compliant' is the share of individual taxpayers receiving income from only that income source, plus potentially bank interest, who are found to owe additional tax when audited. 'Average additional revenue if non-compliant' is the average total tax in 2012 £ that was not reported among those individuals for whom some tax was not reported (the non-compliant). 'Additional revenue as a share of total tax if non-compliant' is the additional tax owed divided by total tax owed, averaged across individual taxpayers who were non-compliant.

Figure B2: Non-compliance by sex



Notes: Constructed using data on individuals who received a random audit of their self assessment tax return for a tax year between 1998/1999 and 2008/2009. 'Share of group found to be non-compliant' is the share of individual taxpayers receiving income from only that income source, plus potentially bank interest, who are found to owe additional tax when audited. 'Average additional revenue if non-compliant' is the average total tax in 2012 £ that was not reported among those individuals for whom some tax was not reported (the non-compliant). 'Additional revenue as a share of total tax if non-compliant' is the additional tax owed divided by total tax owed, averaged across individual taxpayers who were non-compliant.

Figure B3: Non-compliance by region



Notes: Constructed using data on individuals who received a random audit of their self assessment tax return for a tax year between 1998/1999 and 2008/2009. Region is either London or the nation of the UK (where London is excluded for England) in which the taxpayer's home address is located. 'Share of group found to be non-compliant' is the share of individual taxpayers receiving income from only that income source, plus potentially bank interest, who are found to owe additional tax when audited. 'Average additional revenue if non-compliant' is the average total tax in 2012 £ that was not reported among those individuals for whom some tax was not reported (the non-compliant). 'Additional revenue as a share of total tax if non-compliant' is the additional tax owed divided by total tax owed, averaged across individual taxpayers who were non-compliant.

Table B1: Predictors of non-compliance

Dependent Variable: Taxpayer is found non-compliant

Sample: Self-assessment taxpayers subject to random audit on tax returns for 1999-2009

Standard errors (in parentheses) clustered at individual level

| | (1) | (2) | (3) |
|----------------------------|---------------------|--------------------|--------------------|
| Female | -.417*** (.031) | -.417*** (.031) | -.425*** (.032) |
| Age | -.013*** (.001) | -.014*** (.001) | -.014*** (.001) |
| Age ² | | -.000*** (.000) | -.000*** (.000) |
| Has self-employment income | .831*** (.051) | .818*** (.051) | .787*** (.051) |
| Has employment income | -.152*** (.033) | -.176*** (.033) | -.213*** (.034) |
| Has property income | .368*** (.040) | .362*** (.040) | .384*** (.040) |
| Has pension income | -.161*** (.039) | -.081 (.042) | -.100* (.043) |
| Manufacturing | -.085 (.119) | -.108 (.119) | -.063 (.122) |
| Construction | .263** (.094) | .258** (.094) | .297** (.097) |
| Wholesale | .073 (.106) | .052 (.106) | .081 (.108) |
| Hospitality | .468** (.145) | .448** (.145) | .484** (.148) |
| Transport | .372*** (.111) | .345** (.112) | .383*** (.114) |
| Legal | -.179 (.134) | -.194 (.134) | -.150 (.137) |
| Financial | -.060 (.098) | -.077 (.098) | -.030 (.101) |
| Education | -.069 (.111) | -.085 (.111) | -.028 (.113) |
| Other industry | -.139 (.100) | -.147 (.100) | -.100 (.102) |
| Industry not recorded | -.428*** (.098) | -.430*** (.098) | -.363*** (.100) |
| Bottom income quintile | .350*** (.045) | .338*** (.045) | .344*** (.046) |
| Second income quintile | .469*** (.045) | .456*** (.045) | .454*** (.045) |
| Third income quintile | .426*** (.046) | .414*** (.046) | .396*** (.047) |
| Fourth income quintile | .509*** (.049) | .488*** (.049) | .461*** (.050) |
| Top income quintile | .653*** (.049) | .627*** (.050) | .605*** (.051) |
| Constant | -1.009*** (.103) | -.929*** (.104) | -.951*** (.107) |
| Regional FE | No | No | Yes |
| Observations | 30,252 | 30,252 | 29,323 |

Notes: *** denotes significance at .1%, ** at 1%, and * at 5% level. The sample comprises all self-assessment taxpayers who were subject to a random audit on a tax return for tax years 1998/99 to 2008/09. Observations are at the individual taxpayer level. The outcome measures whether the taxpayer was found to have under-reported tax due for the tax year in which they were audited. Table shows coefficients from a logistic regression of the outcome on demographics, income sources, industry, and income quintile based on reported income level for the previous year. Income quintile is calculated based on the previous years' reported income, among those reporting positive income the previous year. The excluded group is those declaring no income in the previous year. The excluded industry category is agriculture. Column (2) includes a square term in age, to allow for non-linearity in this effect. Column (3) additionally allows for regional level fixed effects. Regions are London, England excluding London, Scotland, Wales, and Northern Ireland.