Unwilling or Unable to Cheat? Evidence from a Tax Audit Experiment in Denmark*

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Abstract

This paper analyzes a tax enforcement field experiment in Denmark. In the base year, a stratified and representative sample of over 40,000 individual income tax filers was selected for the experiment. Half of the tax filers were randomly selected to be thoroughly audited, while the rest were deliberately not audited. The following year, threat-of-audit letters were randomly assigned and sent to tax filers in both groups. We present three main empirical findings. First, using baseline audit data, we find that the tax evasion rate is close to zero for income subject to third-party reporting, but substantial for self-reported income. Since most income is subject to third-party reporting, the overall evasion rate is modest. Second, using quasi-experimental variation created by large kinks in the income tax schedule, we find that marginal tax rates have a positive impact on tax evasion for self-reported income, but that this effect is small in comparison to legal avoidance and behavioral responses. Third, using the randomization of enforcement, we find that prior audits and threat-of-audit letters have significant effects on self-reported income, but no effect on third-party reported income. All these empirical results can be explained by extending the standard model of (rational) tax evasion to allow for the key distinction between self-reported and third-party reported income.

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1 Introduction

An extensive literature has studied tax evasion and tax enforcement from both the theoretical and empirical perspective. The theoretical literature builds on the Allingham and Sandmo (1972) model in which taxpayers report income to the tax authorities to maximize expected utility taking into account a probability of audit and a penalty for cheating. Under low audit probabilities and low penalties, the expected return to evasion is high and the model predicts substantial noncompliance. This prediction is in stark contrast with the observation that compliance levels are high in modern tax systems despite low audit rates and fairly modest penalties.[1] This suggests that the standard economic model misses important aspects of the real-world reporting environment. In particular, many have argued that observed compliance levels can only be explained by psychological or cultural aspects of tax compliance such as social norms, tax morale, patriotism, guilt and shame (e.g., Andreoni et al., 1998). In other words, taxpayers, despite being able to cheat, are unwilling to do so for non-economic reasons.

While psychology and culture may be important in the decision to evade taxes, the standard economic model deviates from the real world in another potentially important aspect: it focuses on a situation with pure self-reporting. By contrast, all advanced economies make extensive use of third-party information reporting whereby institutions such as employers, banks, investment funds and pension funds report taxable income earned by individuals (employees or clients) directly to the government. Under third-party reporting, the observed audit rate is a poor proxy for the probability of detection faced by a taxpayer contemplating to engage in tax evasion, because systematic matching of information reports to income tax returns will uncover any discrepancy between the two (Sandmo, 2005; Slemrod, 2007). Thus, taxpayers with only third-party reported income may be unable to cheat on their taxes. Indeed, the US Taxpayer Compliance Measurement Program (TCMP) has documented that aggregate compliance is much higher for income categories with substantial information reporting than for income categories with little or no information reporting (Internal Revenue Service, 1996, 2006).

In this study, we first extend the standard economic model of tax evasion to account for the fact that the probability of detection is endogenous to the type of income being underreported

[1] For example, Andreoni et al. (1998) conclude at the end of their influential survey that "the most significant discrepancy that has been documented between the standard economic model of compliance and real-world compliance behavior is that the theoretical model greatly over-predicts noncompliance."

(third-party reported versus self-reported income). The model predicts that evasion will be very low for third-party reported income, but substantial for self-reported income. It also predicts that the effects of tax enforcement (audits, penalties) and tax policy (marginal tax rates) on evasion will be larger for self-reported income than for third-party reported income. Second, we provide a comprehensive empirical test of these predictions based on a large field experiment carried out in collaboration with the Danish tax collection agency (SKAT). The experiment imposes different audit regimes on randomly selected taxpayers, and has been designed to provide evidence on the size of evasion as well as the response of evasion to tax enforcement and tax rates under different information environments (third-party reporting versus self-reporting). Unlike previous work such as the US TCMP studies, our data allow us to distinguish precisely between income items subject to third-party reporting and income items subject to self-reporting for each individual in the sample, and to measure treatment effects on those two forms of income separately.

The experiment was implemented on a stratified random sample of about 42,800 individual taxpayers during the filing and auditing seasons of 2007 and 2008. In the first stage, taxpayers were randomly selected for *unannounced* audits of tax returns filed in 2007. These audits were comprehensive and any detected misreporting was corrected and penalized according to Danish law. The selected taxpayers were not aware that the audits were part of a special study. For taxpayers not selected for these audits, tax returns were not examined under any circumstances. In the second stage, employees in both the audit and no-audit groups were randomly selected for *pre-announced* audits of tax returns filed in 2008. One group of taxpayers received a letter telling them that their return would certainly be audited, another group received a letter telling them that half of everyone in their group would be audited, while a third group received no letter. The second stage therefore provides exogenous variation in the probability of being audited. The empirical analysis is divided into three main parts.

The first part studies the *anatomy of tax compliance* using the baseline audit data. While the overall tax evasion uncovered by audits constitutes a modest share of total income, there is considerable variation in tax evasion rates across income items depending on the information environment. The tax evasion rate for third-party reported income is close to zero, whereas the tax evasion rate for self-reported income is substantial. Across different taxpayers, we find that individuals who earn mostly self-reported income and display substantial noncompliance overall

still do not underreport their third-party reported income, while individuals who earn mostly third-party reported income and display very little noncompliance overall often fully evade taxes on their self-reported income. These findings are consistent with the theoretical model and suggest that the high degree of compliance is driven by the widespread use of information reporting rather than an intrinsic aversion to cheating. We also study the impact of social and cultural variables on compliance. Although some of these variables are correlated with tax evasion, their impact is very small in comparison to variables that capture information and incentives, namely the presence and size of self-reported income or losses. Taken together, our findings suggest that tax evasion is low, not because taxpayers are *unwilling* to cheat, but because they are *unable* to cheat successfully due to the widespread use of third-party reporting.

The second part estimates the *effect of the marginal tax rate on evasion* using quasi-experimental variation in tax rates created by large and salient kinks in the nonlinear income tax schedule. The effect of marginal tax rates on evasion is theoretically ambiguous, and existing empirical results have been very sensitive to specification due to data and identification problems. As showed by Saez (2010), the compensated elasticity of reported income with respect to the marginal tax rate can be identified from bunching around kinks in progressive tax schedules. Unlike existing bunching studies, our data allows us to compare bunching in pre-audit and post-audit incomes in order to separately identify compensated elasticities of illegal evasion versus legal avoidance. We find that evasion elasticities for self-reported income are positive but small relative to the total elasticity. This implies that marginal tax rates have only modest effects on tax evasion that are dwarfed by the third-party reporting effects obtained in part one.

The third part studies the *effect of tax enforcement on evasion* using the randomization of audits and audit threats. First, we estimate the effect of audits on future reported income by comparing the audit and no-audit groups in the following year. Past audits may affect reported income by changing the perceived probability of detection. Consistent with our theoretical model, we find that audits have a strong positive impact on reported income in the following year, with the effect driven entirely by self-reported income. Second, we estimate the effect of the probability of audit on reported income by comparing the threat-of-audit letter and no-letter groups. Because taxpayers received the letters shortly after receiving a pre-populated return containing third-party information, we focus on the effect of letters on self-reported adjustments to the pre-populated return. Consistent with the predictions of the model, we find that audit

threats have a positive impact on self-reported income, and that the effects are stronger for the 100% threat than for the 50% threat.

Our paper contributes to a large body of empirical work studying the size and determinants of tax evasion, including the effect of tax rates, prior audits, audit probabilities, penalties, and socio-economic variables.[2] Most of the literature relies on observational and non-experimental data, which is associated with important measurement and identification problems, or on laboratory experiments that do not capture central aspects of the real-world reporting environment such as the presence of third-party reporting. An important exception in the literature is Slemrod et al. (2001), who analyze the effects of threat-of-audit letters in a small field experiment in Minnesota, and upon which the last part of our analysis is built.

The paper is organized as follows. Section 2 presents an economic model of tax evasion with third-party reporting. Section 3 describes the context, experimental design, and data. Section 4 analyzes the anatomy of tax compliance. Section 5 estimates the effect of the marginal tax rate on evasion. Section 6 estimates the effects of tax enforcement on evasion. Section 7 concludes.

2 A Simple Economic Model of Tax Evasion

We consider a version of the Allingham-Sandmo (henceforth AS) model with risk neutral taxpayers and an endogenous audit probability that depends on reported income.[3] The basic model is similar to models that have been considered in the literature, but we will present the condition determining tax evasion in a different manner in order to demonstrate that a high degree of tax compliance is potentially consistent with a low audit probability and a low, or even zero, penalty for evasion. We then introduce third-party reporting into the model and discuss its implications for the structure of the (endogenous) audit probability and tax compliance behavior. Notice that the assumption of risk neutrality, besides simplifying the analysis, makes our case harder because risk-neutral taxpayers are more inclined to evade taxes than risk-averse taxpayers.

We consider a taxpayer with true income $\bar{y}$, reported income y, and undeclared income $e \equiv \bar{y} - y$. Let p be the probability that the government detects undeclared income. We can think of the detection probability as a product of the probability of audit and the probability of

[2] Andreoni et al. (1998) and Slemrod and Yitzhaki (2002) provide extensive surveys. An earlier version of this paper (Kleven et al., 2010) also provides a more thorough review of the literature.

[3] A number of previous studies have considered an endogenous audit probability, including the original paper by Allingham and Sandmo (1972), Yitzhaki (1987), Slemrod and Yitzhaki (2002), and Sandmo (2005).

detection conditional on audit.[4] The distinction between these two probabilities will be implicit in the model, but becomes relevant in the interpretation of the empirical findings from the randomized experiment. We assume that the probability of detection is an increasing function of undeclared income, $p = p(e)$ where $p'(e) > 0$. That is, the more the individual evades, the more likely is the tax administration to suspect underreporting and carry out an audit.

When evasion is detected, the taxpayer is forced to pay the evaded tax plus a penalty. The tax is proportional to income with rate τ, and the penalty is proportional to the evaded tax and given by θ. The risk-neutral taxpayer maximizes expected net-of-tax income, i.e.

$$u = (1 - p(e)) \cdot [\bar{y}(1 - \tau) + \tau e] + p(e) \cdot [\bar{y}(1 - \tau) - \theta \tau e]. \tag{1}$$

An interior optimum for e satisfies the first-order condition $du/de = 0$, which can be written as

$$[p(e) + p'(e) \cdot e](1 + \theta) = 1. \tag{2}$$

The second-order condition to this problem puts a restriction on the second-order derivative of $p(e)$.[5] We may define the elasticity of the detection probability with respect to evasion as $\varepsilon \equiv p'(e) e/p \geq 0$. The first-order condition determining tax evasion can then be written as

$$p(e) \cdot (1 + \theta) \cdot (1 + \varepsilon(e)) = 1. \tag{3}$$

The right-hand side of this condition is the marginal benefit of an extra dollar of evasion, while the left-hand side is the expected marginal cost of an extra dollar of evasion. Under $\varepsilon = 0$ as in the standard model with fixed p, the expected marginal cost equals the probability of detection p times the evaded tax plus penalty, $1+\theta$. The presence of the elasticity ε in the formula reflects that the taxpayer by evading an extra dollar incurs a higher probability of detection on all the infra-marginal units of evasion. Interestingly, this simple model is consistent with less than full tax evasion even under a zero penalty, $\theta = 0$. In this case, partial evasion may be better than full evasion because it involves a lower probability of being detected and having to pay the full statutory tax (but no penalty).

The comparative statics of such a model have been analyzed in the literature (e.g., Yitzhaki, 1987). A higher penalty and a positive shift of the detection probability are both associated

[4]For expositional simplicity, we make the assumption that a tax audit either uncovers everything or nothing; there is no middle ground where tax evasion is partially uncovered.

[5]The second-order condition is given by $-2p'(e) - p''(e) \cdot e < 0$. A sufficient condition for this to hold is that $p(.)$ is convex so that $p''(e) > 0$.

with lower tax evasion. Moreover, as can be seen directly from (3), the marginal tax rate has no impact on tax evasion. This result relies on the assumptions of risk-neutrality, linear taxation, and a linear penalty in evaded tax. In particular, the combination of a linear penalty and linear taxation implies that the substitution effect of the marginal tax rate is zero, while risk-neutrality implies that the income effect is also zero. Under a nonlinear penalty, the marginal tax rate will have a non-zero substitution effect with the sign of the effect depending on the second-order derivative of the fine. Moreover, in a nonlinear tax system, an increase in the marginal tax rate for a constant total tax liability can have a positive substitution effect on evasion, although this is true only under an endogenous audit probability and the result depends on the second-order derivative of the audit probability. In general, the substitution effect of the marginal tax rate on evasion is theoretically ambiguous and its sign is an open empirical question.

The strongest critique of the economic model of tax evasion centers on its predictions of the level of noncompliance. In our model, the taxpayer should increase evasion as long as the left-hand side of equation (3) is below one. The fact that the observed p and θ are close to zero is often argued to imply that it is privately optimal for taxpayers to increase evasion and that they are therefore complying too much from the perspective of the economic model. This reasoning ignores the role of $\varepsilon(e)$, and this is particularly important in a tax system using third-party information reporting. As we will now argue, the presence of third party reporting puts a specific structure on the functions $p(e)$ and $\varepsilon(e)$.

Third-party reporting can be embedded in the model in the following way. Let true income be given by $\bar{y} = \bar{y}_t + \bar{y}_s$, where $\bar{y}_t$ is subject to third-party reporting (wages and salaries, interest income, mortgage payments, etc.) and $\bar{y}_s$ is self-reported (self-employment income, various deductions, etc.). For third-party reported income, assuming there is no collusion between the taxpayer and the third party, the probability of detection is close to one as systematic matching of tax returns and information reports will uncover any evasion.[6] By contrast, the detection probability for self-reported income is very low because there is no smoking gun for tax evasion and tax administrations have limited resources to carry out blind audits.

Based on these observations, it is natural to assume that the probability of detection $p(e)$ is very low for $e < \bar{y}_s$, very high for $e > \bar{y}_s$, and increases rapidly around $e = \bar{y}_s$. Notice that these properties rely on a specific sequence of underdeclaration: as tax evasion goes from 0 to

[6]Kleven, Kreiner, and Saez (2009) study the issue of collusion and third-party reporting in detail, and demonstrate that collusion cannot be sustained in large formal firms even with low audit rates and penalties.

$\bar{y}$, the taxpayer first evades taxes on income items with a low detection probability and then evades taxes on items with a high detection probability. Given that the tax rate and penalty are the same across different income items, this is the optimal sequence for the taxpayer. This implies that the detection probability has an S-shape like the one shown in Figure 1, where $p(e)$ is initially very close to zero and then decreases rapidly towards one around the threshold $\bar{y}_s$.[7]

In this model, the taxpayer's optimum will be at a point to the left of $\bar{y}_s$ as shown in the figure. At this equilibrium, $p(e)$ is much lower than $\frac{1}{1+\theta}$, but the elasticity $\varepsilon(e)$ is very high as evasion is close to the level where third-party reporting starts. The taxpayer almost fully underdeclares self-reported income, while fully declaring third-party reported income.

It is useful to briefly consider heterogeneous taxpayers as this will play a role in the empirical analysis. There is heterogeneity in the share of income that is third-party reported depending on self-employment, job type, wealth composition, etc. Hence, the threshold at $\bar{y}_s$ in Figure 1 varies across taxpayers for a given $\bar{y}$. While the arguments above imply that tax evasion should always be close to $\bar{y}_s$, in practice, taxpayers who derive most of their income in self-reported form cannot easily evade all their self-reported income. This is because total reported income after tax needs to be roughly consistent with consumption and change in wealth, which can be partially ascertained by the government using information from financial institutions, credit cards records, etc. This can be seen as additional third-party information that can be obtained by the tax authorities if total disposable income appears unrealistically low.[8] This information matters for those with mostly self-reported income (e.g., self-employed individuals), but not for those with mostly third-party income (e.g., wage earners with small additional amounts of self-reported income). This leads to the prediction that those with little self-reported income should almost fully evade self-reported income, while those with substantial self-reported income should evade less as a share of self-reported income (but evade more in total).

Besides these predictions about the level of tax evasion across different income items and taxpayers, the model also predicts that the deterrence effect of enforcement will depend on the

[7] A microfoundation of the S-shape in the figure would allow for many income items, some of which are third-party reported and some of which are self-reported. In general, let there be N third-party reported items with true incomes $\bar{y}_t^1, ..., \bar{y}_t^N$, and let there be M self-reported items with true incomes $\bar{y}_s^1, ..., \bar{y}_s^M$. The N third-party reported items have higher detection probabilities than the M self-reported items, but there is heterogeneity in the probability across items in each group. As argued above, an optimizing taxpayer choosing total tax evasion e will underdeclare income items sequentially such that the detection probability is increasing in total evasion. In this case, it is natural to assume that the detection probability has a shape like the one shown in Figure 1.

[8] As we describe in section 3, tax audits do indeed compare disposable reported income to estimates of consumption and wealth changes using information from banks and other financial institutions.

information environment. The deterrence effect for self-reported income should be significant and consistent with the standard comparative statics discussed above, whereas there should be no effect on third-party income.

In the following sections, we present a comprehensive test of the model predictions with respect to compliance levels and deterrence effects under different information environments.

3 Context, Experimental Design, and Data

3.1 The Danish Income Tax and Enforcement System

The Danish income tax system is described in Table 1. Panel A describes the different tax bases and Panel B describes the tax rate structure. The system combines national and local taxes that are enforced and administered in an integrated system. Labor income first faces a national payroll tax imposed at a flat rate of 8%. This tax is deducted when computing all other taxes, so that the effective labor income tax equals the payroll tax plus 92% of the other taxes. The national income tax is a progressive three-bracket system imposed on a tax base equal to personal income (labor income, transfers, pensions, and other adjustments) plus net capital income (if it is positive) with marginal tax rates equal to 5.5%, 11.5%, and 26.5%. The local income tax is imposed on taxable income (personal income plus net capital income minus deductions) above a standard exemption at a flat rate that varies by municipality and is equal to 32.6% on average.[9] Finally, at the national level, stock income (dividends and capital gains) is taxed separately by a progressive two-bracket system with rates equal to 28% and 43%.

About 88% of the Danish population is liable to pay income tax, and all tax liable individuals are required to file a return.[10] Income tax filing occurs in the Spring of year $t+1$ for income earned in year t. By the end of January in year $t+1$, SKAT will have received most information reports from third parties. Based on the third-party reports, SKAT constructs pre-populated tax returns that are sent to taxpayers in mid-March. Other than third-party information, the pre-populated return may contain additional hard information that SKAT possesses such as an estimated commuting allowance based on knowledge of the taxpayer's residence and work

[9]There is a ceiling on the combined local and national marginal tax rate of 59%. This ceiling is binding in the average municipality as $32.6\% + 26.5\% = 59.1\%$. Hence, in the average municipality, the top marginal tax rate on labor income (including the payroll tax) is equal to $8\% + 0.92 \cdot 59\% = 62.3\%$. This is among the highest marginal tax rates in the world.

[10]The group of citizens who are not tax liable and therefore not required to file a return consists mostly of children under the age of 16 who have not received any taxable income over the year.

addresses.[11] Upon receiving the pre-populated return, the taxpayer has the option of making adjustments and submit a final return before May 1.[12] This filing system implies that, for most tax filers, the difference between income items on the final return and the pre-populated return is a measure of item-by-item self-reported income.

After each tax return has been filed, audit flags are generated based on the characteristics of the return. Audit flags do not involve any randomness, but are a deterministic function of the computerized tax information available to SKAT. Flagged returns are looked at by a tax examiner, who decides whether or not to instigate an audit based on the severity of flags, local knowledge, and resources. The audit-flag rate for the entire population of individual tax filers is 4.2%. Audits may generate adjustments to the final return and a tax correction. In the case of underreporting, the taxpayer has the option of paying taxes owed immediately or postponing the payment at an interest. If the underreporting is seen as deliberate cheating, a fine may be imposed. In practice, fines are rare because it is difficult to draw the line between honest mistakes and deliberate fraud. An audit may alternatively find overreporting, in which case excess taxes are repaid with interest.

3.2 Experimental Design

The experiment is based on a stratified random sample of 25,020 employees and 17,764 self-employed.[13] The sample of employees was stratified by tax return complexity, with an over-sampling of filers with high-complexity returns.[14] The experimental treatments and their timing are shown in Figure 2. The experiment was implemented by SKAT in two stages during the filing and auditing seasons of 2007 and 2008. In the first stage, taxpayers were randomly assigned to a 0% audit group or a 100% audit group. In the 0% audit group, taxpayers were never audited even when the characteristics of the return would normally have triggered an audit. In the 100% audit group, all taxpayers were subject to *unannounced* tax audits of tax returns filed in 2007 (for 2006 income), meaning that taxpayers were unaware at the time of filing that they

[11] Since Denmark introduced pre-populated returns as the first country in the world in 1988, this policy has been introduced in several other European and South-American countries.

[12] New returns can be submitted by phone, internet or mail. The taxpayer may keep filing new returns all the way up to the deadline, only the last return counts. If no adjustments are made, the pre-populated return counts as the final return.

[13] The "employee" category includes transfer recipients such as retired and unemployed individuals, and would therefore be more accurately described as "not self-employed".

[14] An additional stratification ensured that the same number of taxpayers was selected from each of the regional tax collection agencies located around the country.

had been selected for an audit.[15]

The tax audits in the 100% audit group were comprehensive and examined every item on the tax return using various verification procedures. Some items were checked by matching the return to administrative register data (e.g., deductions for paid alimony can be matched to received alimony of the ex-spouse, commuting deductions can be verified from information about the residence and work addresses). Other items required SKAT to request supporting documentation from the taxpayer, including self-reported deductions that cannot be double-checked in administrative registers and capital gains/losses from stock based on self-reported buying and selling prices. For some items such as taxable fringe benefits not third-party reported, SKAT would sometimes match self-reported income with the accounting books of the employer. Finally, in addition to these item-by-item verification procedures, SKAT compared disposable reported income to estimates of consumption and the change in wealth over the tax year, drawing on information from financial institutions, credit cards, etc. In the case of detected misreporting, the tax liability was corrected and a penalty possibly imposed depending on the nature of the error and as appropriate according to Danish law. Importantly, audited taxpayers were not told that the audits were part of a special study. The cost of implementing the experimental audits equaled 21% of SKAT's total annual audit resources.

Despite the large amount of resources spent on these audits, they are unlikely to uncover all tax evasion for all taxpayers and our results therefore provide lower bounds on total evasion.[16] The same issue arises in the TCMP studies, which blow up detected tax evasion by a multiplier of 3.28 to arrive at the official US tax evasion estimates. Unfortunately, this multiplier is large and has a very large measurement error, so that total evasion rates are at best rough approximations.[17] In this study, we therefore focus solely on *detectable* tax evasion.

The first stage of the experiment is used for two purposes. First, audit data for the 100%

[15]The actual audit rate in the 100% audit group was slightly lower than 100%, because some tax returns were impossible to audit due to special circumstances (individuals dying, disappearing, leaving the country, filing with substantial delay, etc.). The actual audit rates were 98.7% for employees and 92% for self-employed individuals. All of our estimates are based on the full 100%-audit sample, so that we are measuring intent-to-treat effects rather than treatment effects. We prefer to present intent-to-treat effects rather than treatment effects (which would be obtained by running a 2SLS regression on actual audit and using intend-to-audit group as an instrument), because the impossibility of auditing some returns reflects relevant real-world limitations.

[16]Income that is likely to go undetected include labor income from the informal economy, in-kind exchanges among professionals, foreign income from jurisdictions with bank secrecy laws, and some fringe benefits not subject to third-party reporting.

[17]The multiplier of 3.28 is based on a TCMP direct survey of taxpayers from 1976 (See Internal Revenue Service, 1996, for details). Obviously, such self-reported levels of tax evasion are likely to be very noisy.

audit group is used to study the anatomy of compliance in the baseline. We also combine baseline audit data with quasi-experimental variation in marginal tax rates to study the effect of tax policy on compliance. Second, the random assignment of taxpayers to the 100% and 0% audit groups is used to estimate the causal effect of audits on future reporting behavior.

In the second stage, individuals in both the 0% and 100% audit groups were randomly selected for *pre-announced* tax audits of tax returns filed in 2008 (for 2007 income). This part of the experiment was implemented only for the employees since it was administratively infeasible for SKAT to include the self-employed. The pre-announcements were made by official letters from SKAT sent to taxpayers one month prior to the filing deadline on May 1, 2008.[18] A third of the employees in each group received a letter telling them that their return would certainly be audited, another third received a letter telling them that half of everyone in their group would be audited, and the final third received no letter. The second stage therefore creates exogenous variation in the probability of being audited, conditional on having been audited in the first stage or not. The audit probability is 100% for the first group, 50% for the second group, and equal to the current perceived probability in the third group.

The wording of the threat-of-audit letters was designed to make the message simple and salient. The wording of the 100% letter (50% letter, respectively) was the following: "As part of the effort to ensure a more effective and fair tax collection, SKAT has selected a group of taxpayers—including you—for a special investigation. For (half the) taxpayers in this group, the upcoming tax return for 2007 will be subject to a special tax audit after May 1, 2008. Hence, (there is a probability of 50% that) your return for 2007 will be closely investigated. If errors or omissions are found, you will be contacted by SKAT." Both types of letter included an additional paragraph saying that "As always, you have the possibility of changing or adding items on your return until May 1, 2008. This possibility applies even if you have already made adjustments to your return at this point."

After returns had been filed in 2008, SKAT audited all taxpayers in the 100%-letter group and half of all taxpayers (selected randomly) in the 50%-letter group. However, to save on resources, these audits were much less rigorous than the first round of audits in 2007. Hence,

[18]Recall that pre-populated returns are created around mid-March after which taxpayers can file their tax return. When the pre-announcement letters were delivered, 17% of those taxpayers had already filed a new return. However, as explained in the previous section, taxpayers are allowed to change their returns all the way up to the deadline, only the final report is considered by tax examiners.

we do not show results from the actual audits in 2008, but focus instead on the variation in audit probabilities created by the letters.

Let us briefly consider the possibility of spillover effects between treatments and controls. For several reasons, this is not likely to be a central issue here. First, there was no media coverage of the experiment and therefore no general public awareness about it. Second, audited taxpayers were not aware that the audits were part of an experiment, only letter recipients were aware of an experimental treatment. Third, information about income tax filing and auditing is strictly private, and hence spillovers can arise only if a treated individual voluntarily decides to reveal this information to others. This limits the issue primarily to close relatives such as spouses. Given a sample of 42,784 individuals spread across a country of about 5.5 million people, there is bound to be very few close family members in the sample. The potential importance of spillover effects within families can actually be checked by linking individuals in the sample to their spouses and cohabitating partners. We have carried out robustness checks where we drop all individuals in the sample whose partner is also in the sample (456 observations, or .107% of the sample). Dropping these observations has no impact on any of the empirical results.[19] We therefore conclude that spillover effects is not a key concern for this experiment.

3.3 Data

The data are obtained from SKAT's Business Object Database, which contains all information available to SKAT for each taxpayer. This includes all income items from the third-party reports and the pre-populated, filed, and audited tax returns for each year and taxpayer. For the 2007 and 2008 filing seasons (2006 and 2007 incomes, respectively), we extract item-by-item income data from the third-party information reports (**I**), pre-populated return (**P**), filed return (**F**), and after-audit return (**A**). We also extract information about audit flags (described above) and historical audit adjustments. Finally, the database contains a number of socio-economic variables such as age, gender, marital status, church membership, home ownership, residence, and characteristics of the taxpayer's employer (sector, number of employees).

[19] The sub-sample where both spouses are present in the experiment is too small to reliably estimate spillovers.

4 The Anatomy of Tax Compliance

4.1 Overall Compliance

This section analyzes data from the baseline audits of tax returns filed in 2007 for incomes earned in 2006 in the 100% audit group. Table 2 presents audit statistics for total reported income in Panel A, and for third-party and self-reported income separately in Panel B. Starting with total net income and total tax liability in the top rows of the table, statistics are then presented by specific income categories in lower rows. For each income category, Panel A shows pre-audit income (column (1)), total audit adjustment (column (2)), audit adjustment due to underreporting (column (3)), and audit adjustment due to overreporting (column (4)). Each column shows average amounts in Danish kroner as well as percent of tax filers with non-zero amounts, and standard errors are displayed in parentheses. All statistics are calculated using population weights to reflect averages in the full population of tax filers in Denmark.

Average net income before audits is 206,038 kroner (about $40,000), and average tax liability is 69,940 kroner, corresponding to an average tax rate of 34%. The most important income component is personal income, which includes earnings, transfers, pensions, and various adjustments.[20] Personal income is reported by 95% of tax filers and the average amount is close to total net income as the other components about cancel out on average. Capital income is negative on average mainly due to mortgage interest payments. It is equal to about -5% of total net income and is reported by 94% of tax filers.[21] Deductions also represent about -5% of net income, but only 60% of tax filers claim deductions. Stock income constitutes less than 3% of net income and is reported by 22% of tax filers. Self-employment income is about 5% of net income and is reported by 8% of tax filers.

Each income category is itself a sum of several line items on the tax return. A given line item is either always positive (such as interest income received) or always negative (such as mortgage interest payments). As we shall see, the distinction between positive line items and negative line items matters for separately measuring underreporting of third-party and self-reported income. We therefore split total net income into "positive income" and "negative income" defined as the sum totals of all the positive and negative items, respectively.

[20] See Table 1 for a detailed definition. In all tables, the personal income variable includes only earnings of employees, while earnings of the self-employed are reported separately as part of self-employment income.

[21] Non-zero capital income is extremely common as most taxpayers have either negative capital income from various loans or positive capital income from bank interest (most Danish bank accounts pay interest).

Column (2) shows that the adjustment amounts are positive for all categories, implying that taxpayers do indeed evade taxes.[22] These adjustments are strongly statistically significant in all cases. Total detectable tax evasion can be measured by the adjustment of net income and is equal to 4,532 kroner (about $900), corresponding to about 2.2% of net income. The tax lost through detectable tax evasion is 1,980 kroner, or 2.8% of total tax liability.[23] Considering the positive and negative income items separately, the evasion rate is 1.6% for positive income and 1.9% for negative income (in absolute value). Hence, overall tax evasion appears to be very small in Denmark despite the high marginal tax rates described in the previous section. However, the low evasion rates overall mask substantial heterogeneity across different income components, with evasion rates equal to 1.1% for personal income, 2.3% for capital income (in absolute value), 1.6% for deductions (in absolute value), 4.6% for stock income, and 14.9% for self-employment income. We explore the reasons for this heterogeneity below.

We may also consider evasion rates measured by the share of taxpayers evading (i.e., percent in columns (2)/(1)). The overall evasion rate measured by the share of taxpayers having their net income adjusted is equal to 10.7%. For each income component separately, we have evasion rates of 2.6% for personal income, 2.2% for capital income, 5.7% of deductions, 4.2% for stock income, and 44.9% for self-employment income. These evasion rates are generally larger than for amounts, but follow the same qualitative pattern of heterogeneity.

The audit adjustments discussed so far reflect a combination of upward adjustments (underreporting) and downward adjustments (overreporting), which are reported separately in columns (3) and (4). We see that underreporting takes place in all income categories, and that the detected underreporting is always strongly significant. The heterogeneity across income categories follows the same pattern as for the total adjustment. The amounts of overreporting are always small but still statistically significant. The small amount of overreporting most likely reflects honest mistakes resulting from a complex tax code and the associated transaction costs of filing a tax return correctly.

[22]For negative items (such as mortgage interest payments included in capital income), a positive adjustment means that the absolute value of the mortgage interest payment was reduced. We use this convention so that upward adjustments always mean higher net income and hence a higher tax liability.

[23]Estimated underreporting from the 1992 TCMP study for the U.S. individual income tax is 13.2% of total tax liability (Internal Revenue Service, 1996). However, as discussed above, this estimate is obtained by applying a multiplier of 3.28 to detected underreporting. Hence, detected evasion in the U.S. is about 4%, higher than the 2.8% we find for Denmark but not overwhelmingly so.

4.2 Self-Reported versus Third-Party Reported Income

Each income category in Table 2 consists of some items that are self-reported and other items that are third-party reported. But the prevalence of information reporting varies substantially across income categories, with substantial third-party reporting for personal income at one end of the spectrum and very little third-party reporting for self-employment income at the other end. The results described above therefore suggest that evasion rates are higher when there is little third-party reporting, consistent with the findings of the TCMP studies in the United States. A key advantage of our data is that it allows an exact breakdown of income into third-party reported income and self-reported income for each income category and taxpayer, enabling a more rigorous analysis of the role of third-party reporting for tax compliance. We consider this breakdown in Panel B of Table 2, which displays third-party income (column (5)), underreporting of third-party income (column (6)), self-reported income (column (7)), and underreporting of self-reported income (column (8)).

Columns (5) and (7) show that the use of third-party reporting is very pervasive in Denmark. Third-party reporting covers 95% of total net income while self-reporting is responsible for only 5%. The share of third-party reporting in positive income is 92% and its share in negative income is 74%. While the widespread use of information reporting indicates that detection probabilities are very high on average, there is considerable heterogeneity across income components. For personal income, third-party reporting corresponds to more than 100% of total income as self-reported income includes both positive and negative adjustments and is negative on average. Capital income reported by third parties is negative on average due to interest payments on debt, and is more than 100% of total negative capital income as self-reported capital income is positive. For the remaining components, the share of third-party reporting is 62% for deductions, 67% for stock income, and 11% for self-employment income. The fact that third-party reporting is not strictly zero for self-employed individuals is useful, because it allows an exploration of the separate implications of information environment versus self-employment.[24]

We split total tax evasion into underreporting of self-reported income and underreporting of third-party reported income. As mentioned above, we observe line-by-line income amounts in the information report (**I**), the filed tax return (**F**), and the audit-adjusted return (**A**). Each

[24] An example of third-party reporting for self-employed individuals would be an independent contractor working for a firm (but not as a formal employee) which reports the contractor's compensation directly to the government.

report consists of line items that are either always positive (as in the case of earnings) or always negative (as in the case of deductions and losses). Consider first the always-positive line items. We can say that there is underreporting of third-party income if the individual reports less on the return than what is obtained from third-party reports and there is a subsequent upward audit adjustment. Formally, if we have $\mathbf{F} < \mathbf{A} < \mathbf{I}$, then third-party cheating is equal to $\mathbf{A} - \mathbf{F}$. If we have $\mathbf{F} < \mathbf{I} \leq \mathbf{A}$, then third-party cheating is equal to $\mathbf{I} - \mathbf{F}$. In all other cases (i.e., if either $\mathbf{A} \leq \mathbf{F} < \mathbf{I}$ or $\mathbf{F} \geq \mathbf{I}$), third-party cheating is zero. Given this procedure, we measure underreporting of self-reported income as the residual difference between total underreporting and third-party underreporting.

Consider next the always-negative line items such as losses and deductions. If the taxpayer reports larger losses or deductions (in absolute value) than what is obtained from third-party reports and is then denied part or all of those extra losses in the audit, this may reflect either self-reported losses that are unjustified or manipulation of third-party reported losses. Our prior methodology does not allow us to separate between the two. However, closer examination of the data shows that negative income items are either (a) exclusively third-party reported items with no self-reported component or (b) have a significant self-reported income component. For negative items (a), under-reporting has to be of the third-party category. It is reasonable to assume, consistent with our theoretical model, that for items (b) with a significant self-reported income component, under-reporting is always in the self-reported category (as detection probability is expected to be much lower for self-reported changes). We classify under-reporting for negative items into self-reported and third-party components using this alternative methodology.

We find a very strong variation in tax evasion depending on the information environment. For third-party reported income, the evasion rate is always extremely small: it is equal to 0.23% for total positive income, 0.35% for total negative income, and always below 1% across all the different categories. Interestingly, the evasion rate for self-employment income conditional on third-party reporting is only 0.33%, suggesting that overall tax evasion among the self-employed is large because of the information environment and not because of, for example, different preferences among those choosing self-employment (such as attitudes towards risk and cheating). By contrast, tax evasion for self-reported income is substantial: the evasion rate is 17.1% for total positive income, 7.5% for total negative income, 5.4% for capital income, 13.6% for stock income, and 17.7% for self-employment income. The evasion rate for self-employment

income is not particularly high compared to the other forms of income once we condition on self-reporting. For total self-reported net income, the tax evasion rate is equal to 41.6%. Because self-reported net income consists of positive amounts and negative amounts that just about cancel on average (self-reported net income is quite small), measuring tax evasion as a share of self-reported net income may give an exaggerated representation of the evasion rate. Note however that these estimates capture only detectable evasion and are therefore lower bounds on true evasion, particularly for self-reported income where traceable evidence is often limited.

The model presented earlier predicts that each taxpayer substantially underdeclares self-reported income while fully declaring third-party income. We can think of this as a "within-person" prediction. The cross-sectional evidence on evasion rates for third-party and self-reported income is consistent with this within-person prediction, but could also reflect a pattern where those with mostly self-reported income are large evaders and underdeclare any type of income, whereas those with mostly third-party income are non-evaders. In this case, big evaders would display substantial evasion even for third-party income, while non-evaders would report truthfully even for self-reported income. To explore this alternative hypothesis, we first point out two pieces of evidence in Table 2 that go against it. First, the evidence for self-employment income discussed above shows that self-employed individuals are major evaders overall, but do not underdeclare third-party income. Second, the population shares shows that, among those who are found to evade taxes, only a small fraction underdeclare third-party income.

Figure 3 provides direct within-person evidence. Panel A depicts the distribution of the ratio of evaded income to self-reported income among those who evade. Income is defined as the sum of all positive items, so that self-reported income is always positive. The large spike around a ratio of one shows that, among evaders, the most common strategy is to evade all self-reported income. The figure also shows that almost no taxpayers evade more than their self-reported income. Panel B plots the fraction of taxpayers evading and the fraction of income evaded against the fraction of income that is self-reported. The fraction of income evaded is shown for both total (positive) income and third-party (positive) income. Three findings in the figure support the within-person prediction of the model. First, the probability of evading jumps up immediately once the taxpayer has some income that is self-reported (although it never exceeds 40%). Second, the share of total income evaded is increasing in the share of income that is self-reported, whereas the share of third-party income evaded is always very close to zero. This

shows that taxpayers with more self-reported income evade more, but always declare third-party income fully. Third, the share of total income evaded is very close to the 45-degree line as long as self-reported income is less than 20% of total income, and then starts to fall below the 45-degree line. This shows that those with relatively little self-reported income evade *more* as a share of self-reported income than those with relatively high self-reported income, which goes directly against the alternative hypothesis above. This finding is consistent with the model in section 2 where we argued that those with a large share of income in self-reported form cannot evade all their self-reported income, because total disposable income cannot fall too far below the sum of consumption and the change in wealth without triggering an investigation. Although information about consumption and wealth is not automatically third-party reported, it can be (partially) obtained from third parties at the discretion of tax authorities.

To summarize these results, tax evasion is very low overall but substantial once we zoom in on purely self-reported income. This reflects an underlying pattern where each taxpayer fully declares third-party reported income (where detection probabilities are very high) and at the same time substantially underreports self-reported income (where detection probabilities are low). This is consistent with our model and suggests that overall tax compliance is high, not because taxpayers are *unwilling* to cheat, but because they are *unable* to cheat successfully due to the widespread use of third-party reporting.

4.3 Social versus Information Factors

To explore the role of social, economic, and information factors in determining evasion, Table 3 reports the results of OLS regressions of a dummy for underreporting net-income on a number of dummy covariates, using the full-audit group and population weights. Panel A (columns (1)-(4)) considers a basic set of explanatory variables, while Panel B (columns (5)-(8)) considers a richer set of variables. Column (1) includes only social variables: gender, marital status, church membership, geographical location (dummy for living in the capital Copenhagen), and age (dummy for being older than 45). The table shows that being female, a church member, living in the capital, and older than 45 are negatively associated with evasion, while being married is positively associated with evasion. However, among these social variables, only gender is statistically significant. Column (2) adds three socio-economic variables: home ownership, firm size (a dummy for working in a firm with less than 10 employees), and industrial sector (a dummy

for working in the "informal sector" defined as agriculture, forestry, fishing, construction, and real estate).[25] Being a homeowner, working in a small firm, and working in the informal sector are all positively and significantly associated with evasion.

Column (3) considers information-related tax return factors, in particular the presence and size of self-reported income: a dummy for having non-zero self-reported income, a dummy for having self-reported income above 20,000 kroner, and a dummy for having self-reported income below -10,000 kroner. We also include a dummy for having been flagged by the automated audit selection system (see Section 3), because audit flags are to a large extent a (complex) function of self-reported income. The results show very strong effects of all these information-related variables. Column (4) brings all the variables together in order to study their relative importance. The results show that by far the strongest predictors of evasion are the variables capturing self-reported income. The effect of firm size is also fairly strong, whereas the effect of "informal sector" disappears.[26] As for the social variables, their effects remain small and all but female gender and marital status are statistically insignificant. Note that the coefficient on marital status actually changes sign.

It is illuminating to consider the adjusted R-squares across the different specifications. The specification including only self-reported income variables explains about 16.1% of the variation, while the specification with only socio-economic factors explains just about 2.5%. Adding socio-economic variables to the specification with tax return variables has almost no effect on the R-square. This provides suggestive evidence that information, and specifically the presence and size of income that is difficult to trace, is the key aspect of the compliance decision.

In Panel B, we investigate whether these findings are robust to including a much richer set of explanatory variables. Besides the basic variables described above, we include 6 location dummies (for the 6 main regions of Denmark), 4 age-group dummies, 5 firm-size dummies, 22 industry dummies, 6 income-group dummies, dummies for having non-zero income in different categories, and a dummy for having experienced an audit adjustment in the past two years. The conclusions are the same as above: the effects of social variables are small and mostly insignificant, whereas variables capturing information (presence and size of self-reported income, self-employment, audit flags, and prior audit adjustments) have very strong effects. This

[25]The informal sector classification is meant to capture industries that are generally prone to informal activities.

[26]The fact that firm size remains significant suggests that collusion between taxpayers and third parties may be important in small firms, a finding which is consistent with the theoretical results of Kleven et al. (2009).

confirms the conclusion that information and traceability are central to the compliance decision.

5 The Effect of the Marginal Tax Rate on Evasion

The effect of marginal tax rates on tax evasion is a central parameter for tax policy design. As discussed earlier, the effect of the marginal tax rate on tax evasion is theoretically ambiguous, not just because of income effects, but because the substitution effect can be either positive or negative depending on the structure of penalties, taxes, and detection probabilities. In this section, we sign the substitution effect by presenting evidence on the compensated elasticity of tax evasion with respect to the marginal tax rate. Earlier studies of this parameter have been based on US TCMP data and observational variation in marginal tax rates across taxpayers and over time (Clotfelter, 1983; Feinstein, 1991). The results have been very sensitive to the empirical specification, due to the lack of exogenous variation in tax rates. We therefore follow a different approach using quasi-experimental variation created by the discontinuity in marginal tax rates around large and salient kinks in the Danish tax schedule.

As described in section 3 and Table 1, the Danish tax system consists of two separate piecewise linear schedules: a three-bracket income tax and a two-bracket stock income tax. The most significant kinks are created by the top-bracket threshold in the income tax (where the marginal tax jumps from 49% to 62%) and the bracket threshold in the stock income tax (where the marginal tax jump from 28% to 43%). Economic theory predicts that taxpayers will respond to such jumps in marginal tax rates by bunching at the kink points. Saez (2010) shows that such bunching can be used to identify the compensated elasticity of reported income with respect to the net-of-tax rate. This strategy has been pursued on Danish data by Chetty et al. (2009), who find evidence of substantial bunching around the top kink in the income tax system. We also consider the top kink in the income tax, focusing on individuals with self-employment income where evasion is substantial and a significant response is therefore more likely. Moreover, we consider the kink in the stock income tax, since this kink is also large and much of stock income is self-reported and therefore prone to evasion. Our key contribution to the existing literature is that the combination of pre-audit and post-audit data allows us to separately identify elasticities of illegal evasion and legal avoidance, as opposed to only the overall elasticity of reported income.

Figure 4 plots empirical distributions of taxable income (excluding stock-income) in Panel A

and stock income in Panel B around the major cutoffs in the income tax and stock income tax schedules. Panel A shows the distributions of pre-audit taxable income (full black curve) and post-audit taxable income (dashed grey curve) for the self-employed in 2006 around the top kink at 318,700 kroner (vertical line). The figure groups individuals into 3000 kroner bins and plots the number of taxpayers in each bin. Like Chetty et al. (2009), we find substantial bunching in pre-audit incomes around the kink, with almost 5 times as many taxpayers in the bin including the kink as in the surrounding bins. This provides clear evidence of an overall taxable income response to taxation, which may reflect evasion, avoidance, or real responses. To uncover the evasion response to marginal tax rates, we turn to the distribution of post-audit income. Here we continue to see bunching, but less than for pre-audit income. This suggests that bunching is achieved partly by under-declaring income, which is consistent with an evasion response to the marginal tax rate. The post-audit bunching reflects real and avoidance responses purged of the (detectable) evasion response.[27]

As shown in Panel B, we find even stronger evidence of bunching around the kink point in the stock income tax schedule (at 88,600 kroner), with about 10 times as many taxpayers in the bin around the kink as in the surrounding bins. However, we see essentially no difference between the pre-audit and post-audit distributions, suggesting that the bunching effect reflects solely avoidance and not (detectable) evasion.

Table 4 uses the bunching evidence to estimate elasticities of tax evasion and tax avoidance for self-employment income (Panel A) and stock income (Panel B). The first row in each panel shows the fraction of individuals bunching (defined as having an income within 1,500 kroner of the kink) among individuals within 40,000 kroner of the kink. The second row in each panel shows compensated elasticities based on comparing the actual distribution to a counterfactual distribution estimated by excluding observations in a band around the kink (Saez, 2010). The difference between the actual and counterfactual distributions gives an estimate of excess mass around the kink point, which can be compared to the size of the jump in the net-of-tax rate in order to infer the elasticity. The identifying assumption is that, in the absence of the discontinuous jump in tax rates, there would have been no spike in the density distribution at the kink.

[27]The post-audit bunching is a lower bound on real and avoidance responses, because individuals who respond to tax rates both along the avoidance/real margin and the evasion margin and bunch at the kink point (before audits) will be displaced from the kink by the audit. Hence, the difference in bunching between pre-audit and post-audit incomes is an upper bound on the evasion response to marginal tax rates.

The estimated elasticity of pre-audit taxable income for the self-employed is equal to 0.16, while the elasticity of post-audit taxable income equals 0.085. The difference between the two is the compensated evasion elasticity with respect to the net-of-tax rate and is equal to 0.076. All of these estimates are strongly significant. For stock income, the pre-audit elasticity is 2.24 and strongly significant, while the post-audit elasticity is equal to 2.00. This implies an elasticity of evasion equal to 0.25, but this elasticity is not statistically significant. The last column of the table explores the robustness to the bandwidth around the kink used to estimate the elasticities. We find that the estimates are not very sensitive to bandwidth, which is because the bunching in the Danish tax data is very sharp.

To summarize these results, the marginal tax rate has at most a small positive substitution effect on tax evasion for individuals with substantial self-reported income. Estimated evasion responses are smaller than avoidance responses, although this decomposition could be biased by the presence of undetected evasion that the method attributes to avoidance. The combination of large evasion rates for self-reported income (as documented in the previous section) and small evasion effects of the marginal tax rate is not incompatible with the model in section 2. Importantly, the combined results of this and the previous section suggest that information reporting is much more important than low marginal tax rates to achieve enforcement.

6 The Effects of Tax Enforcement on Evasion

6.1 Randomization Test

In this section, we consider the effects of audits and threat-of-audit letters on subsequent reporting. We start by running a randomization test to verify that the treatment and control groups are indeed ex-ante identical in both experiments. Table A1 in the appendix shows the results of the audit randomization (0% vs. 100% audit group) in Panel A, letter randomization (letter vs. no-letter group) in Panel B, and within-letter randomization (50% vs. 100% letter group) in Panel C. The table shows mean income and percent of taxpayers with non-zero income in different categories, the percent filing a return the following year in 2008, and a number of socio-economic characteristics. Unlike the baseline compliance study, statistics are not reported using population weights to match the full Danish population, but reflects instead the composition in the stratified random sample on which the experiments are based. We use sample weights as this increases slightly the power of our results.

For the audit randomization, income statistics are based on the tax returns filed in 2007, i.e. right before the baseline audits were implemented. We see that the differences between the 0% and 100% audit groups are always very small and never statistically significant at the 5% level, showing that the randomization was indeed successful. Importantly, the fraction filing returns the following year in 2008 is also statistically identical across the two groups (97.08% and 96.94% respectively). We have also verified that *conditional* on filing a 2008 return, there are no statistically significant differences across the 0% and 100% audit groups. This absence of selective attrition is critical as our analysis of prior audits effects is based on 2008 returns.

For the letter and within-letter randomizations, statistics are based on the pre-populated tax returns in 2008, i.e. right before the letter experiment was implemented.[28] Among the 39 differences we show, only two (capital income and fraction married in the letter vs. no-letter groups) are borderline significant at the 5% level. Because we are looking at so many statistics, it is not surprising that a small fraction (actually 2/39=5.1%) is (borderline) significant at the 5% level. Hence, we conclude that the letter randomization was also successful.

6.2 The Effect of Audits on Future Reporting

Let us first consider the effect of audits on future reporting in the context of the economic model in section 2. In that model, reported income depends on the perceived probability of detection when engaging in tax evasion. Because audits are rare events for a taxpayer, they are likely to provide new information and therefore lead to a change in the perceived detection probability. We may think of the detection probability as a product of two probabilities: the probability of audit and the probability of detection conditional on audit. Audits may have an effect through both channels. One would expect the effect on the perceived audit probability to be positive. The effect on the perceived probability of detection conditional on audit is ambiguous, because the taxpayer may learn that the tax administration is either more or less effective at uncovering evasion than expected. In practice however, audited taxpayers are contacted only if tax inspectors upon examining the return believe that hidden income or unjustified deductions

[28] More precisely, the statistics are based on the last version of the return before the letters were sent out. As the letters were distributed shortly after the pre-populated returns were created, the last return for most taxpayers was indeed the pre-populated return. However, a small fraction of taxpayers (about 17%) had already made self-reported adjustments to their returns in the short time window between pre-populated returns and letters (recall that taxpayers can repeatedly correct their returns at any time before the May 1st deadline). To minimize noise, we consider the effect of letters on adjustments to the latest return for each taxpayer at the time of receiving the letter, and hence the randomization test is based on this tax return concept.

can potentially be uncovered. Hence, taxpayers are typically only aware of being audited when tax inspectors are successful. This means that the probability of detection conditional on audit is likely to increase as a result of experiencing an audit. Therefore, the model predicts an increase in reported income. In particular, self-reported income should increase, but not third-party reported income where the detection probability is already close to one.

The few previous studies of the effect of audits on future reporting have not found significant results. These studies have considered either TCMP audits (Long and Schwartz, 1987) or ordinary audits (Erard, 1992). The problem with TCMP audits is that taxpayers are aware that selection is random and that the audit is part of a special study. The problem with using ordinary audits is that selection is endogenous and it is very difficult to control for the ensuing selection bias in a convincing way. Our data contain more compelling variation based on randomized audit treatments where participants are not aware of the randomization.

As the experimental audits were implemented on tax returns filed in 2007, we estimate the effects of audits on subsequent reporting by comparing changes in filed income from 2007 to 2008 (income earned in 2006 and 2007, respectively) in the 0% and 100% audit groups. Table 5 shows the results for the full sample in Panel A and the sample limited to those receiving no threat-of-audit letter in Panel B.[29] Each panel shows amounts of income change at the top and the probability of income increase at the bottom. Income changes have been trimmed at -200,000 and +200,000 kroner to get rid of extreme observations that make estimates imprecise. This trimming affects less than 2% of the observations on average.

To provide a benchmark, column (1) shows actual detected evasion in the baseline audits, i.e. the average amount of detected underreporting at the top of each panel and the fraction of taxpayers found underreporting at the bottom. Actual detected evasion can be seen as the *mechanical* effect of a tax audit, whereas the effect on subsequent income reporting is the *behavioral* (deterrence) effect of the change in perceived detection probability.

We show the estimated deterrence effect of audits on total reported income in column (2), self-reported income in column (3), and third-party reported income in columns (4). Column (5) shows the ratio of column (2) to (1) obtained as an IV-regression of the income change

[29]The threat-of-audit letter treatment (analyzed in the next section) is orthogonal to the audit treatment, and both panels therefore show causal effects of audits. But the full sample may produce different results than the no-letter sample either because of cross-effects between the two treatments or because the no-letter sample contains a higher share of self-employed individuals as the letter experiment excluded the self-employed.

(amount and income-increase dummy, respectively) on the baseline audit adjustment (amount and upward-adjustment dummy, respectively), using as an instrument the 100% audit-group dummy. For amounts, this can be interpreted as the causal effect of an additional dollar of audit adjustment on total reported income the following year, assuming that audits that do not lead to any adjustment have no behavioral effect. For probabilities, it gives the causal effect of experiencing an upward audit adjustment on the probability of increasing reported income.

Table 5 shows that audits have a positive deterrence effect on tax evasion. For the full sample, the effect on total net income is 2557 kroner, or 30.1 cents per additional kroner of audit adjustment. The effect on tax liability is 1375 kroner, corresponding to 41.7 cents per dollar of audit adjustment. These estimates are strongly significant. The effects on the probabilities of increasing total income and tax liability are qualitatively similar, but these estimates are only marginally significant at the 5% level. We find that experiencing an audit adjustment raises the probabilities of increasing reported income and tax liability the following year by about 1 percentage point, or 5% of the baseline probability.

According to the model in section 2, the deterrence effects should be driven entirely by self-reported income as there is no room for additional deterrence for third-party reported income. The breakdown of the total estimated effect into the separate effects on self-reported income and third-party income confirms this prediction. For third-party reported income, the estimated effects are close to zero and statistically insignificant. For self-reported income, the effect on the reported amount equals 2331 kroner, or 91% of the total effect. The effect on the probability of increasing self-reported income is 2.1 percentage point, more twice as large as the total effect, and this estimate is now strongly significant.

Considering the no-letter sample in Panel B, we find that the qualitative effects are the same as for the full sample. Moreover, the quantitative magnitudes do not change by much, and in fact the estimated deterrence effects for the no-letter sample are not significantly different from the full-sample estimates at the 5% level.

To conclude, the overall deterrence effect of audits is positive but quite modest. The effect of audits on total net income corresponds to only about 1% of income. But this effect is driven entirely by purely self-reported income and constitutes a substantial fraction of self-reported income. Hence, when the information environment is such that taxpayers are able to cheat, they display substantial underreporting (section 4) and respond to increased enforcement by

substantially reducing underreporting (this section).[30] The overall deterrence effect of increased enforcement is therefore modest because of the widespread use of third-party information reporting where detection probabilities are close to one initially. These results are consistent with the economic model in section 2.

6.3 The Effect of Threat-of-Audit Letters

We now turn to the effect of the threat-of-audit letters, which provide exogenous variation in the probability of audit. As described above, the letters announce audit probabilities of either 50% or 100% to randomly selected taxpayers in the full-audit and no-audit groups. When interpreting the results, it is important to keep in mind that the probability of audit is not the same as the probability of detection, the parameter that ultimately determines tax compliance according to theory. The variation in the audit probability creates variation in the detection probability, with the size of the variation depending on the probability of detection conditional on audit. This conditional detection probability is unobservable, but is likely to be small for self-reported income where tax inspectors have little hard information to guide them. Hence, while the audit probabilities in the letter experiment are very high, the detection probabilities are much more modest and the magnitude of the estimates should be seen in this light.

To study the effects of the threat-of-audit letters, we consider the sample of employees (as the letter randomization did not include self-employed individuals), who filed tax returns in both 2007 and 2008 and have an address on record so that they could be reached by post. Because taxpayers received the threat-of-audit letters shortly after receiving the pre-populated return (**P**-event) and about one month prior to the filing deadline (**F**-event) in 2008, we focus on the effect of letters on the difference between the **P**- and **F**-events in 2008 (for incomes earned in 2007). These are self-reported adjustments to the pre-populated return (see Section 6.1 above for exact details). As this pre-populated return includes all third-party information available to

[30]The size of the audit effect on self-reported income can be gauged by comparing it to the effect of the marginal tax rate. We can do this for the self-employed for whom we estimate the elasticity of evasion with respect to the marginal tax rate. For this subsample, average income is 298,200 kroner and the audit effect on next-year income is 4,083 kroner (obtained as in Table 5, conditioning on self-employment). The evasion elasticity with respect to the marginal tax rate equals 0.076 (Table 4) and the average marginal tax rate for the self-employed is 45%. Denoting the elasticity by e, we have $\log(z+\Delta z)/z = e \cdot \log(1-t-\Delta t)/(1-t)$, where z is income and t is the marginal tax rate. Using the numbers $z = 298200$, $\Delta z = 4083$, $e = .076$, and $t = 45\%$, the formula implies $\Delta t = -10.8\%$. That is, it takes a 10.8 percentage-point cut in the marginal tax rate (on a given taxpayer in a given year) to reduce evasion by as much as one prior-year audit of that taxpayer. This shows that prior-audit effects on self-reported income are very large compared to the tax-rate effect.

the government, the estimates should be interpreted as effects on self-reported income.

Table 6 shows results for amounts of income adjustment in Panel A and probabilities of income adjustment in Panel B. To reduce noise from extreme observations, adjustment amounts have been capped at 10,000 kroner, which affects less than 2% of observations. The first column in the table establishes a baseline by showing the amounts and probabilities of self-reported adjustments to the pre-populated return among those who did not receive a letter. Columns (2)-(4) then show the effect of receiving any letter (50% or 100% letter) for the full sample of employees (including both the 0% and 100% audit groups). Column (2) displays the effect on total adjustments, while columns (3) and (4) split the total effect into upward and downward adjustments. As an adjustment is either upward or downward, column (2) is the sum of columns (3) and (4). The following three findings emerge.

First, there is a positive effect of letters on the amounts and probabilities of self-reported adjustments to income and tax liability. For total net income, the amount goes up significantly by 94 kroner as a result of receiving a letter. As the baseline adjustment is -497 kroner, the letter effect corresponds to an increase of 19% of the initial adjustment in absolute value. The probability of adjustment increases by 1.63 percentage points from a base of 13.37%, corresponding to an increase of 12.2%, and this estimate is strongly significant. The effects are roughly similar for total tax paid. Second, the effect of letters on adjustments reflect almost exclusively *upward* adjustments, and the effect on upward adjustments is always strongly significant. This is of course consistent with the economic model in section 2: letters increase the perceived probability of detection and therefore deter taxpayers from underreporting. Third, the effect of letters on *downward* adjustment is always close to zero and never statistically significant.

The following columns split the sample by 100% audit and 0% audit in the baseline year. This allows us to explore the presence of cross-effects between the letter and audit treatments. The broad conclusion from these estimates is that letter effects are roughly the same in the 0% and 100% audit groups. In particular, the effects on upward adjustments are almost exactly the same in the two groups. For downward adjustments, the effects on amounts are close to zero and insignificant in both groups. The effects on probabilities of downward adjustment display larger differences between the two groups, but are always statistically insignificant. Hence, there does not appear to be important cross-effects between the two treatments.

Finally, columns (11) and (12) explore the differential impact of 50% and 100% letters.

Column (11) shows the difference in upward adjustments between the 50%-letter and the no-letter groups, while column (12) shows the difference in upward adjustments between the 100%-letter and 50%-letter groups. We see a significant difference in the effects of the two types of letters, and the direction of the difference is consistent with the economic model in section 2. For both amounts and probabilities, the differential impact of the 100% letter over the 50% letter tends to be roughly similar to the impact of the 50% letter over no letter, implying that a 100% audit probability has about twice the effect of a 50% audit probability.

We may summarize the results in this section as follows. Consistent with the model in section 2, audit threats have a significant positive effect on self-reported income, and the effect of 100% audit threats is significantly larger than the effect of 50% audit threats. However, the quantitative magnitudes of the letter effects are modest compared to the effects of actual audits in the previous section, which suggests that audit-threat letters create less variation in the perceived probability of detection than actual audit experiences. A key difference between the two treatments is that audit-threat letters change the probability of audit without affecting the probability of detection conditional on audit, whereas actual audits are likely to raise the probability of detection conditional on audit as discussed earlier. If conditional detection probabilities are low for self-reported income, threat-of-audit letters will have a relatively small effect. An additional possibility is that taxpayers pay less attention to letter threats than to actual audit experiences. For these reasons, analyzing actual audits may be a more powerful way of understanding the deterrence effect of enforcement than sending out letters.

7 Conclusion

The economics literature on tax evasion follows on the seminal work of Allingham and Sandmo (1972), who considers a situation where a taxpayer decides how much income to self-report facing a probability of detection and a penalty for cheating. Micro-simulations as well as laboratory experiments show that, at realistic levels of detection probabilities and penalties, an AS-type setting predicts much less compliance than we observe in practice, at least in developed countries. This suggests that the AS-model misses important aspects of the real-world reporting environment, and a number of different generalizations have been proposed and analyzed in the literature. In particular, several authors have argued that observed compliance levels can only be explained by accounting for psychological or cultural aspects of the reporting decision.

While we do not deny the importance of psychological and cultural aspects in the decision to evade taxes, the evidence presented in this paper points to a more classic information story. In particular, we show that the key distinction in the taxpayer's reporting decision is whether income is subject to third-party reporting or if it is solely self-reported. Augmenting the AS-model with third-party reporting can account for most of our empirical findings.

For self-reported income, our empirical results fit remarkably well with the basic AS-model: tax evasion is substantial and responds negatively to an increase in the perceived probability of detection coming from either a prior audit or a threat-of-audit letter. Interestingly, evidence from bunching at kink points shows that the elasticity of tax evasion with respect to the marginal tax rate is very low, which suggests that rigorous tax enforcement is a much more effective tool to combat evasion than cutting marginal tax rates.

For third-party reported income, tax evasion is extremely modest and does not respond to the perceived probability of detection, because this probability is already very high. This shows that third-party reporting is a very effective enforcement device. Given that audits are very costly and eliminates only a part of tax evasion, enforcement resources may be better spent on expanding third-party reporting than on audits of self-reported income.[31] This also suggests that more work is needed in building a tax enforcement theory that centers on third-party reporting by firms, as recently explored by Kleven et al. (2009).

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Figure 1: Probability of Detection under Third-Party Reporting

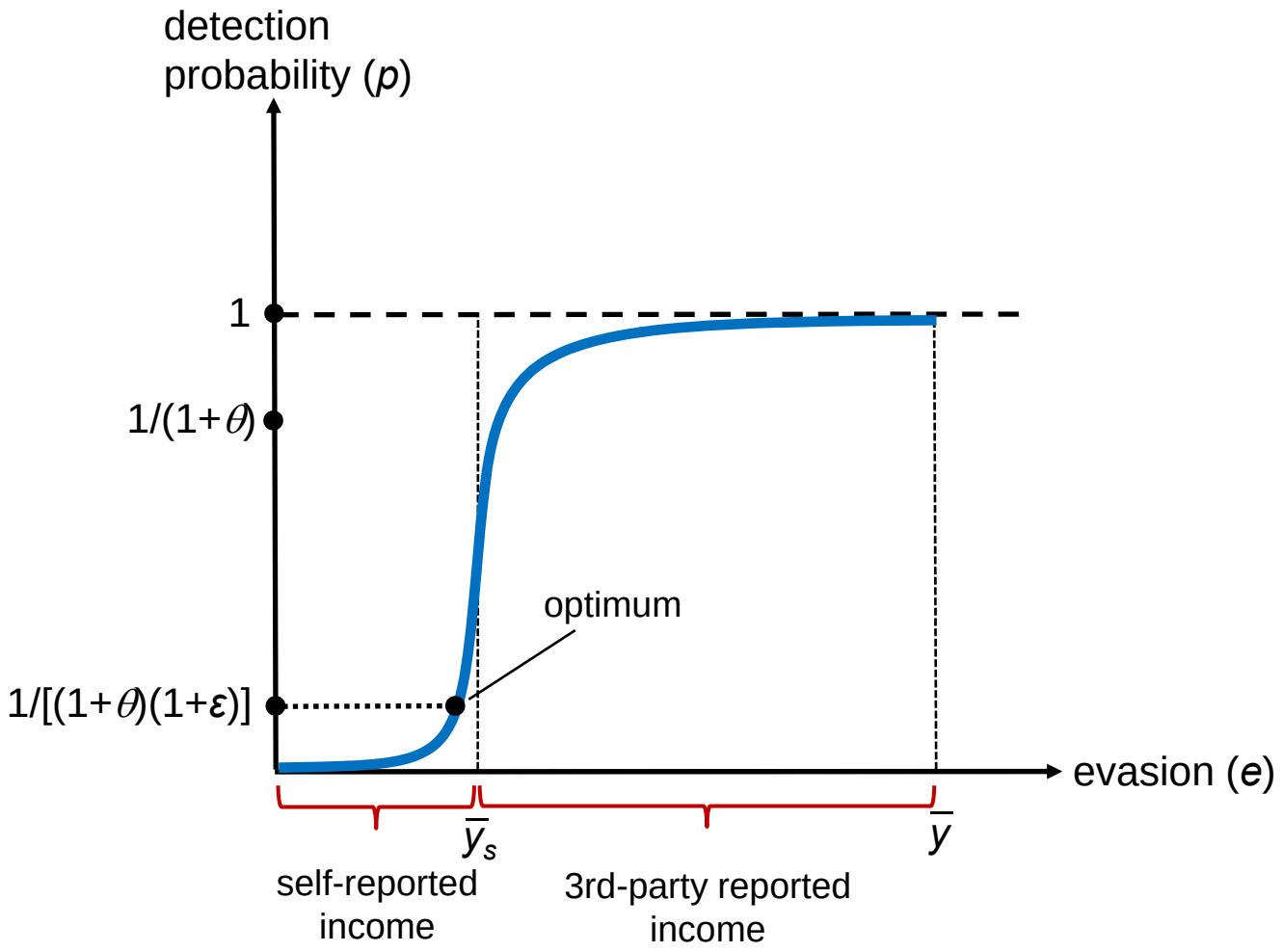
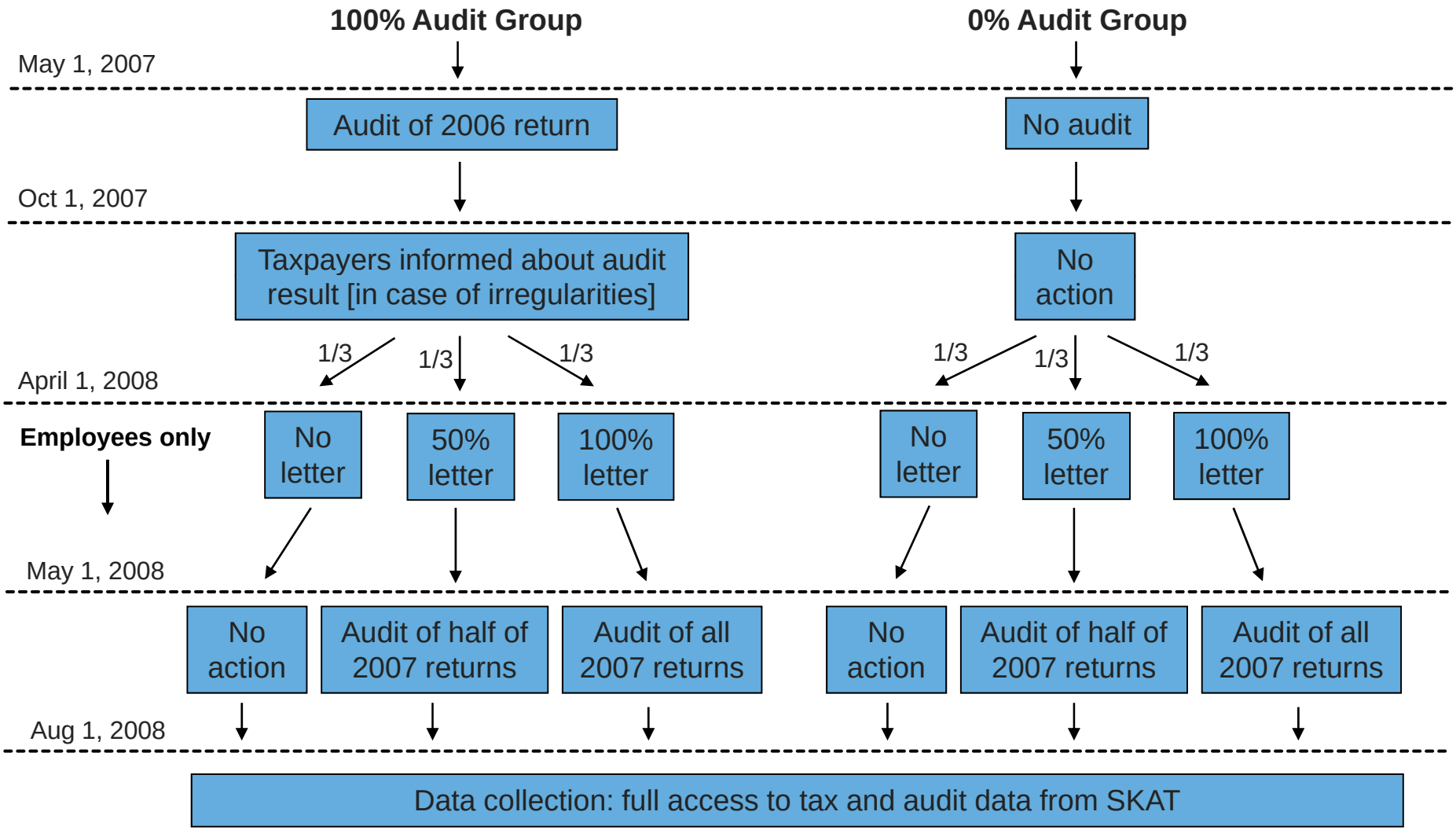


Figure 2. Overview of Experimental Design



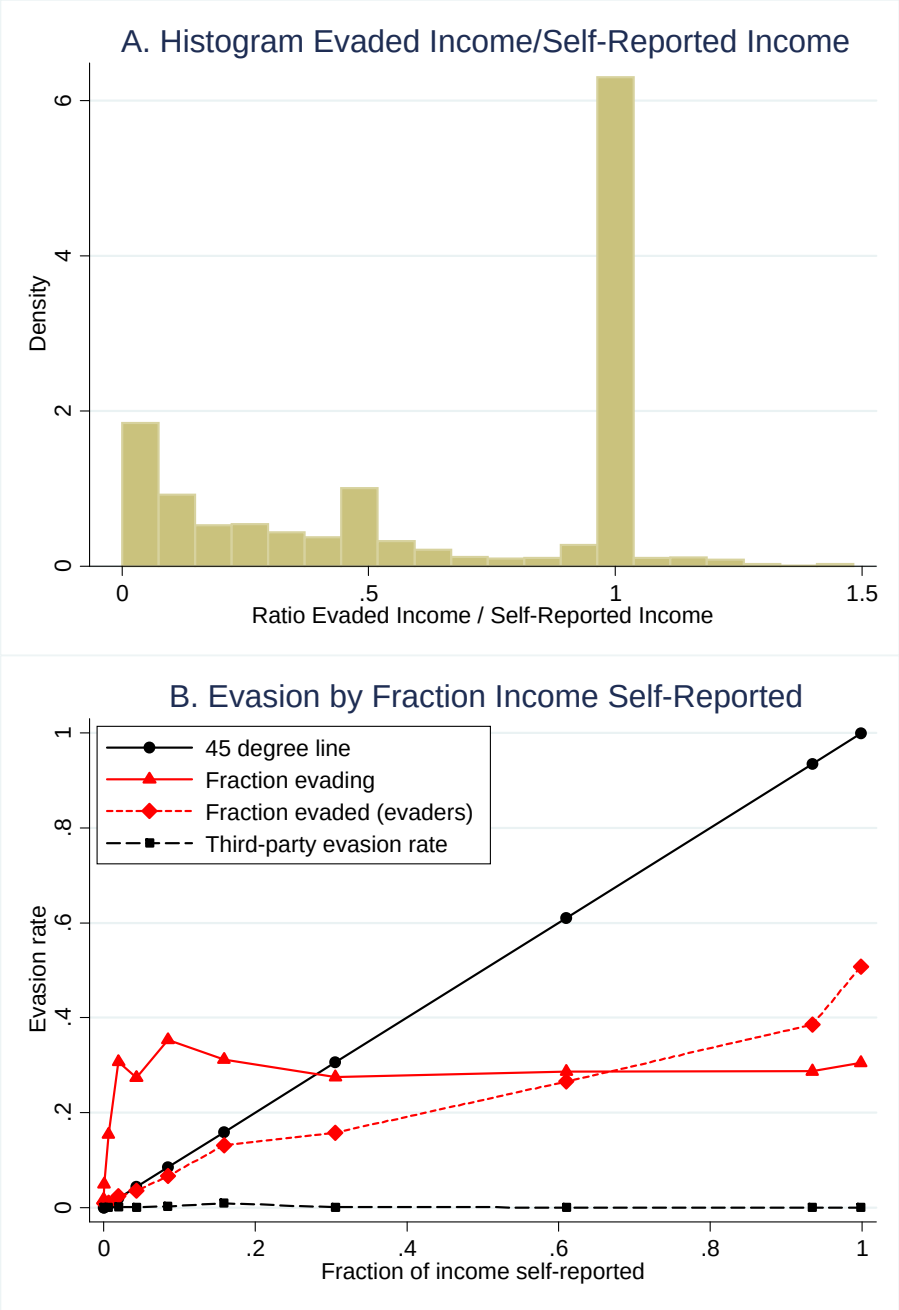


Figure 3. Anatomy of Tax Evasion

Panel A displays the density of the ratio of evaded income to self-reported income (after audit adjustment) among those with a positive tax evasion, using the 100% audit group and population weights. Income is defined as the sum of all positive items (so that self-reported income is always positive). Panel A shows that, among evaders, the most common is to evade all self-reported income. About 70% of taxpayers with positive self-reported income do not have any adjustment and are not represented on panel A.

Panel B displays the fraction evading and the fraction evaded (conditional on evading) by deciles of fraction of income self-reported (after audit adjustment and adding as one category those with no self-reported income). Panel B also displays the fraction of third-party income evaded (unconditional). Income is defined as positive income.

In both panels, the sample is limited to those with positive income above 38,500 kroner, the tax liability threshold (see Table 1).

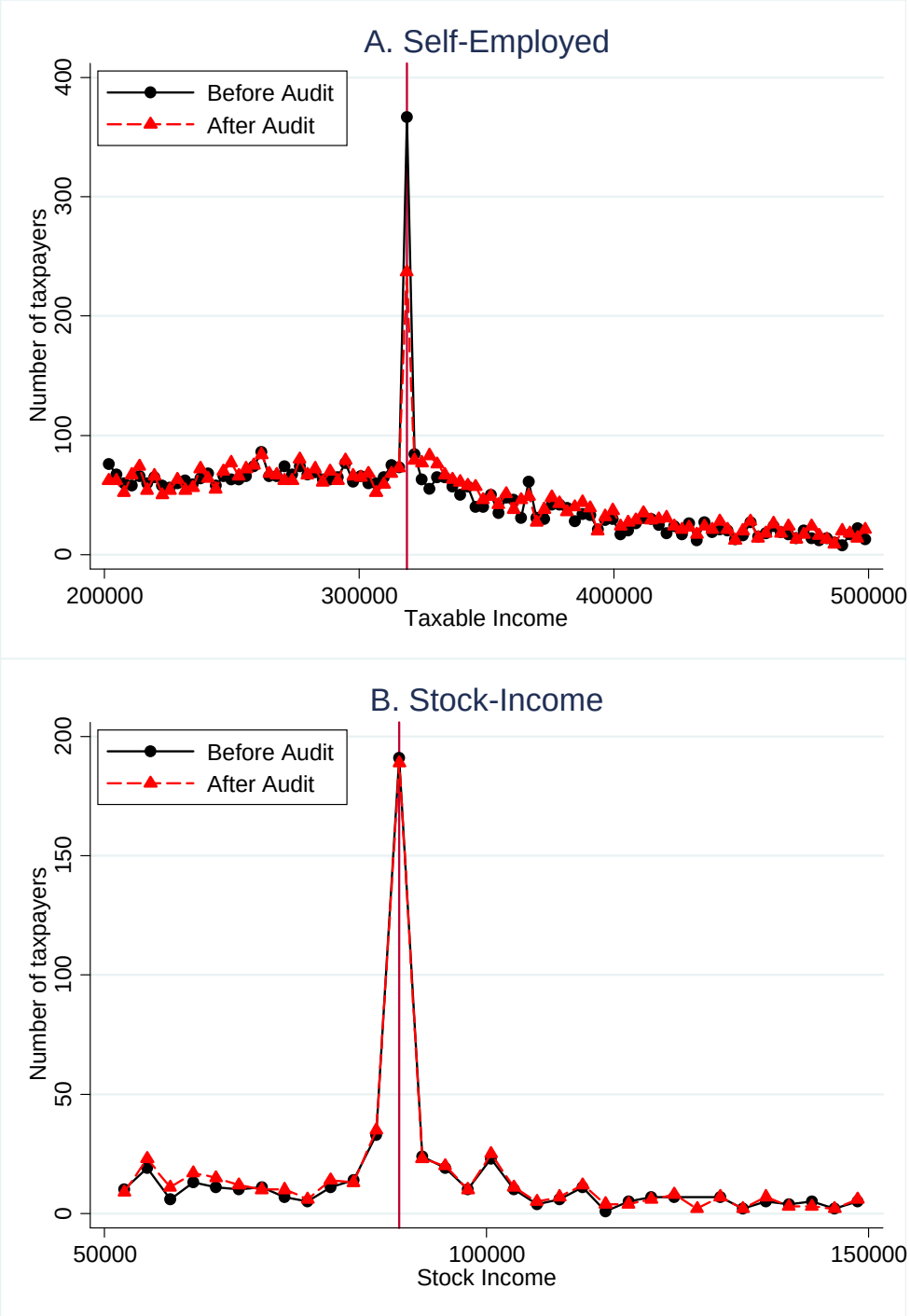


Figure 4. Density Distributions around Kink Points

The figure displays number of taxpayers (by 3000DKK bins) for taxable income for the self-employed (Panel A) and stock income (Panel B). In both panels, we report the series for incomes before audits (in black) and incomes after audits (in red) for the 100% audit group. The vertical line denotes the kink point where marginal tax rates jump. The jump is from 49% to 62% in panel A (top taxable income bracket) and from 28% to 43% in Panel B (top stock income bracket). For married filers, the stock income tax is assessed jointly, and the bracket threshold in the figure is the one applying to such joint filers. For single filers, the bracket threshold is half as large at 44,300 kroner. We have aligned single and married filers in the figure by multiplying the stock income of singles by two.

Table 1. Danish Individual Income Tax in 2006

A. Income Concepts

Income Concept	Definition
(1) Labor Income	Salary, wages, honoraria, fees, bonuses, fringe benefits, business earnings
(2) Personal Income	Labor Income (1) + social transfers, grants, awards, gifts, received alimony - payroll tax, and certain pension contributions.
(3) Capital Income	Interest income, rental income, business capital income - interest on debt (mortgage, bank loans, credit cards, student loans)
(4) Deductions	Commuting costs, union fees, unemployment contributions, other work related expenditures, charitable contributions, alimony paid
(5) Taxable Income	= Personal income (2) + Capital income (3) - Deductions (4)
(6) Stock Income	Dividends and realized capital gains from corporate stock

B. Tax Rates and Tax Bases

Tax Type	Tax Base	Bracket (DKK)	Tax Rate
(1) Payroll Tax	Labor Income	All income	8.0%
(2) National Income Tax	Personal Income + max(Capital Income,0)	38,500-265,500 265,500-318,700 318,700-	5.5% 11.5% 26.5% [1]
(3) Regional Income Tax	Taxable Income	38,500-	32.6% [2]
(4) Stock Income Tax	Stock Income	0-44,400 44,400-	28.0% 43.0%

Notes: All amounts in Danish kroner: US $1 = 5.2 DKK as of January 2010.

[1]: the top rate is reduced so that the combined national and regional income top marginal tax rate never exceeds 59%. The top marginal tax rate on labor income including the payroll tax is therefore .08+.92*.59=62.3%.

[2]: The regional tax includes municipal and county taxes in 2006. The rate shown is the average across all municipalities, and includes the optional church tax equal to 0.7%.

[3]: The national and regional income taxes are based on individual income (not family income). The stock income tax is based on family income with brackets twice as large for married tax filers than those reported in the table.

Table 2. Audit Adjustments Decomposition

		A. Total income reported				B. Third-party vs. self-reported income			
		Pre-audit Income	Audit adjustment	Under-reporting	Over-reporting	Third-party income	Third-party under-reporting	Self-reported income	Self-reported under-reporting
		(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Net Income and Total Tax									
Net Income	Amounts	206,038	4,532	4,796	-264	195,969	612	10,069	4,183
		(2,159)	(494)	(493)	(31)	(1,798)	(77)	(1,380)	(486)
	% non zero	98.38	10.74	8.58	2.16	98.57	2.31	38.18	7.39
		(0.09)	(0.22)	(0.20)	(0.10)	(0.08)	(0.11)	(0.35)	(0.19)
Total Tax	Amounts	69,940	1,980	2,071	-91				
		(1,142)	(236)	(235)	(11)				
	% non zero	90.76	10.59	8.41	2.18				
		(0.21)	(0.22)	(0.20)	(0.10)				
Positive and Negative Income									
Positive Income	Amounts	243,984	3,776	3,943	-167	223,882	516	20,102	3,427
		(2,511)	(485)	(485)	(27)	(1,860)	(76)	(1,693)	(478)
	% non zero	98.24	5.80	4.78	1.02	98.15	1.60	19.53	3.41
		(0.09)	(0.17)	(0.15)	(0.07)	(0.10)	(0.09)	(0.28)	(0.13)
Negative Income	Amounts	-37,946	756	853	-97	-27,913	97	-10,033	756
		(1,014)	(71)	(69)	(14)	(406)	(12)	(862)	(68)
	% non zero	79.09	6.45	5.13	1.32	78.21	0.75	29.49	4.99
		(0.29)	(0.18)	(0.16)	(0.08)	(0.29)	(0.06)	(0.33)	(0.16)
Income Components									
Personal Income	Amounts	210,178	2,327	2,398	-71	211,244	463	-1,066	1,936
		(1,481)	(399)	(399)	(11)	(1,385)	(74)	(548)	(392)
	% non zero	95.22	2.49	1.99	0.50	95.20	1.30	11.95	0.82
		(0.15)	(0.11)	(0.10)	(0.05)	(0.15)	(0.08)	(0.23)	(0.06)
Capital Income	Amounts	-11,075	254	286	-32	-14,556	98	3,481	188
		(340)	(49)	(49)	(6)	(602)	(11)	(542)	(47)
	% non zero	93.93	2.10	1.69	0.41	94.91	0.79	12.29	1.28
		(0.17)	(0.10)	(0.09)	(0.05)	(0.16)	(0.06)	(0.23)	(0.08)
Deductions	Amounts	-9,098	148	197	-49	-5,666	18	-3,432	179
		(104)	(17)	(15)	(7)	(48)	(3)	(85)	(15)
	% non zero	60.07	3.45	2.56	0.89	57.61	0.31	22.60	2.49
		(0.35)	(0.13)	(0.11)	(0.07)	(0.35)	(0.04)	(0.30)	(0.11)
Stock Income	Amounts	5,635	259	281	-22	3,783	30	1,852	251
		(1,405)	(45)	(45)	(8)	(976)	(12)	(943)	(43)
	% non zero	22.47	0.95	0.80	0.15	22.44	0.07	2.45	0.75
		(0.30)	(0.07)	(0.06)	(0.03)	(0.30)	(0.02)	(0.11)	(0.06)
Self-Employment	Amounts	10,398	1,544	1,633	-89	1,164	4	9,234	1,630
		(812)	(280)	(279)	(26)	(177)	(2)	(816)	(279)
	% non zero	7.63	3.43	3.02	0.41	1.40	0.04	7.66	3.00
		(0.19)	(0.13)	(0.12)	(0.05)	(0.08)	(0.01)	(0.19)	(0.12)

Notes: All amounts are in Danish Kroner (US $1 = DKK 5.2 as of 1/2010) and negative amounts (such as deductions) are reported in negative. Column (1) reports pre-audit amounts and the percent of filers with non-zero pre-audit amounts. Column (2) displays the net audit adjustment (and percent with non-zero net audit adjustment), column (3) displays under-reporting in the audit adjustment defined as upward audit adjustments increasing tax liability (and percent with under-reporting), column (4) displays over-reporting in the audit adjustment defined as downward audit adjustments decreasing tax liability (and percent with over-reporting). Note that (3)+(4)=(2). Column (5) displays third-party income (and percent with non-zero third-party income), column (6) displays third-party income under-reporting defined as upward audit adjustments in case where third party income is higher than final reported income for positive income items (and percent with third-party income under-reporting), column (7) displays self-reported income defined as total reported income minus third-party reported income (and percent with non-zero self-reported income), column (8) displays self-reported income under-reporting defined as all upward audit adjustments

Top panel reports net income (sum of all positive income components minus all negative income components and other deductions) and total tax. Middle panel reports positive income (sum of all positive income components) and negative income (sum of all negative income components and deductions). Bottom panel displays various income components. Personal income is earnings, pensions, alimony, minus some retirement contributions. Capital income is interest income, returns on bonds, net rents, minus all interest payments. Deductions include work related expenses, union fees, charitable contributions, alimony paid, and various smaller items. Stock income includes dividends and realized capital gains on stocks. Self-employment income is net profits from unincorporated businesses. Net income is personal income, capital income, stock income, self-employment income, minus deductions.

All estimates are population weighted and based solely on the 100% audit group (19,680 observations). Standard errors are reported in parenthesis.

Table 3. Probability of Under-reporting: Socio-economics vs. Tax Return Factors

Coefficients are in percent	A. Basic Variables					B. Detailed Variables				
		Social factors	Socio-economic factors	Tax return factors	All factors		Social factors	Socio-economic factors	Tax return factors	All factors
		(1)	(2)	(3)	(4)		(5)	(6)	(7)	(8)
Constant		12.72	10.13	1.18	3.72		6.95	5.55	0.95	2.24
		(1.06)	(1.12)	(0.25)	(1.01)		(1.64)	(2.16)	(2.04)	(2.99)
Female Dummy		-5.56	-4.17		-2.06		-5.29	-3.33		-1.02
		(0.63)	(0.65)		(0.62)		(0.62)	(0.67)		(0.62)
Married Dummy		1.22	-0.55		-1.50		-0.72	-1.98		-1.70
		(0.70)	(0.72)		(0.72)		(0.77)	(0.78)		(0.75)
Member of church		-1.59	-2.27		-0.94		-1.54	-1.88		-0.71
		(0.98)	(0.97)		(0.92)		(1.02)	(0.99)		(0.92)
Geographical Location	Copenhagen dummy	-1.49	-0.01		-0.25	6 location	p-value	p-value		p-value
		(1.52)	(1.51)		(1.47)	dummies	6.86	8.87		33.53
Age	Age>45 dummy	-0.72	-0.63		-0.56	4 age group	p-value	p-value		p-value
		(0.67)	(0.67)		(0.61)	dummies	0.00	0.00		24.33
Home Ownership			5.49		0.15			3.72		-0.88
			(0.65)		(0.66)			(0.73)		(0.71)
Firm Size	Firm size<10 dummy		5.07		3.47	5 firm size		p-value		p-value
			(1.26)		(1.05)	dummies		0.00		0.00
Industrial Sector	Informal sector dummy		4.37		0.27	22 industry		p-value		p-value
			(1.15)		(0.92)	dummies		0.00		0.00
Self-Reported Income Dummy				5.58	5.59				3.49	3.75
				(0.75)	(0.80)				(0.80)	(0.78)
(Self-Reported Income>20,000DKK)				21.68	21.09				9.79	8.76
				(1.38)	(1.40)				(1.62)	(1.61)
(Self-Reported Income<-10,000DKK)				14.99	14.74				14.56	14.24
				(1.42)	(1.42)				(1.41)	(1.38)
Auditing Flag Dummy				13.22	13.07				12.26	12.37
				(1.58)	(1.53)				(1.61)	(1.56)
Self-Employed Dummy									17.03	13.47
									(1.14)	(1.39)
Capital Income Dummy									-0.75	-0.47
									(1.98)	(1.87)
Stock Income Dummy									0.33	1.21
									(0.65)	(0.66)
Deduction Dummy									-1.12	-0.76
									(0.72)	(0.88)
Audit Adjustment in 2004 or 2005 Dummy									7.22	6.86
									(1.58)	(1.55)
Income controls						6 income			p-value	p-value
						group dummies			0.20	0.02
R-square		1.16%	2.46%	16.15%	16.53%		2.16%	7.76%	18.72%	19.76%
Adjusted R-square		1.14%	2.42%	16.14%	16.48%		2.11%	7.58%	18.66%	19.54%

Notes: This table reports coefficients of the OLS regression of dummy for under-reporting on various dummy regressors. All coefficients are expressed in percent and robust standard errors are reported. Bottom rows report the R-square and adjusted R-squares. All estimates are population weighted and based solely on the 100% audit group (19,680 observations). Standard errors reported in parenthesis. In Panel A (columns (1) to (4)), we include a basic set of dummy variables, while a richer set of variables is included in Panel B (columns (5) to (8)). In Panel B, we do not report the full set of coefficients for geographical, age, firm size, industrial sector, and income groups. We instead only report the p-value from an F-test that the coefficients of those dummies are all equal to zero (for each category). The 6 location dummies are defined as Copenhagen, North Sealand, Middle and South Sealand, South Denmark, Middle Jutland, and North Jutland. The 4 age dummies are for age groups 0-25, 26-45, 46-65, 66+. The 5 firm size dummies are for firms' size: 1, 2-10, 11-100, 101-1000, 1001+. The 6 income group dummies are for each of the bottom three quartiles separately, percentile 75 to 95, percentile 95 to 99, and top percentile. For income categories, self-employed dummy means non zero self-employment income, etc.

Table 4. Tax Evasion vs. Tax Avoidance Elasticities

	Baseline		Differences	
	Before audit income (avoidance + evasion elasticities)	After audit income (avoidance elasticity only)	Difference (evasion elasticity only)	Robustness check: Difference using smaller sample around kink
	(1)	(2)	(3)	(4)
A. Self-Employment Income (MTR jump from 49% to 62% at 318,700 DKK)				
Fraction bunching (percent)	19.12	12.56	6.56	9.57
	(0.90)	(0.76)	(1.18)	(1.86)
Elasticity	0.161	0.085	0.076	0.070
	(0.011)	(0.008)	(0.014)	(0.014)
Number of observations	1,919	1,887	3,806	2,255
B. Stock Income (MTR jump from 28% to 43% at 88,600 DKK)				
Fraction bunching (percent)	39.30	36.42	2.88	1.80
	(2.22)	(2.11)	(3.06)	(3.69)
Elasticity	2.243	1.996	0.247	0.120
	(0.213)	(0.191)	(0.286)	(0.259)
Number of observations	486	519	1,005	737

Notes: This table estimates the effects of marginal tax rates on tax evasion vs. tax avoidance using bunching evidence around kink points of the tax schedule where marginal tax rates jump. Panel A focuses on the self-employed and the top rate kink where marginal tax rates jump from 49% to 62% at 318,700 DKK. Panel B focuses on stock-income and the top rate kink for stock-income where marginal tax rates from 28 to 43% at 88,600 DKK for married filers and 44,300 DKK for single filers (we have aligned single filers by multiplying by two their stock income). As shown in Figure 2, in both cases, there is significant evidence of bunching at the kink both for income before audits and incomes after audits.

In each panel, the first row estimates the fraction of tax filers bunching (income within 1500 DKK of the kink) among tax filers with income within 40,000 DKK of the kink. Column (1) is for income before audit while column (2) is for income after audit. Column (3) reports the difference between column (1) and column (2). Column (4) presents a robustness check the difference when the sample is limited to tax filers within 20,000 DKK (instead of 40,000 DKK) of the kink.

In each panel, the second row estimates the (compensated) elasticity of reported income with respect to the net-of-tax rate using bunching evidence (following the method developed in Saez, 2009). Column (1) is the elasticity for before audit income while column (2) is the elascity for after audit income. Column (3) reports the difference between column (2) and column (1). Column (4) presents as a robustness check the difference in elasticities when the sample is limited to tax filers within 20,000 DKK (instead of 40,000 DKK) of the kink. The elasticity of before audit income combines both the evasion and avoidance elasticities while the elasticity of after audit income is the tax avoidance elasticity. Therefore, the difference in elasticities is the compensated elasticity of tax evasion with respect to the net-of-tax rate.

Table 5. Effects of Randomized Prior Audits on Year to Year Income Changes

	Baseline Audit adjustment	Change in Reported Income (Panels A1, B1) and Probability of Income Increase (Panels A2, B2) from 2006 to 2007			IV Effect of Audit Adjustment on Income Change
		Total Income	Self-Reported Income	Third Party Reported Income	
	(1)	(2)	(3)	(4)	(5)
A. Full Sample					
A1. Amounts [difference between the 100% and the 0% audit groups]					
Net Income	8491	2557	2331	225	0.301
	(827)	(787)	(658)	(691)	(0.098)
Total Tax	3295	1375			0.417
	(257)	(464)			(0.144)
A2. Probability of audit adjustment and income increase [difference between the 100% and the 0% audit groups]					
Net Income	19.09	0.89	2.11	0.24	0.047
	(0.28)	(0.48)	(0.48)	(0.48)	(0.025)
Total Tax	19.17	0.99			0.052
	(0.28)	(0.49)			(0.025)
Number of observations	41,571	41,571	41,571	41,571	41,571
B. Sample Limited to those Receiving No Threat-of-Audit Letter					
B1. Amounts [difference between the 100% and the 0% audit groups]					
Net Income	12835	2904	3086	-182	0.226
	(1,310)	(1117)	(1008)	(962)	(0.091)
Total Tax	5019	1732			0.345
	(406)	(677)			(0.137)
B2. Probability of audit adjustment and income increase [difference between the 100% and the 0% audit groups]					
Net Income	25.75	0.73	2.12	-0.52	0.028
	(0.39)	(0.61)	(0.61)	(0.61)	(0.024)
Total Tax	25.93	0.98			0.038
	(0.39)	(0.61)			(0.024)
Number of observations	26,180	26,180	26,180	26,180	26,180

Notes: This table reports the effects of prior-audits on income changes from 2006 to 2007. Panel 1 focuses on the amounts of income changes while Panel 2 focuses on the probability of a (nominal) income increase. In all cases, we report the differences between the 100% audit group and the 0% audit group in base year. Column (1) reports the difference between the 100% audit group and the 0% audit group in the average amount of audit adjustment in base year (Panel 1) and the fraction with an audit adjustment for under-reporting in base year (Panel 2). Column (2) reports the difference between the 100% audit group and the 0% audit group in the average income increase from 2006 to 2007 (Panel 1) and the fraction with a nominal income increase from 2006 to 2007 (Panel 2). Columns (3) repeats the analysis of column (2) but limited to self-reported income instead of total reported income. Columns (4) repeats the analysis of column (2) but limited to third-party reported income instead of total reported income. Note that (2)=(3)+(4) for amounts in Panel 1.

Column (5) presents the coefficient of an IV regression of income change (Panel 1) and dummy for an income increase (Panel 2) on the baseline audit adjustment for under-reporting using as instrument the 100% audit group dummy. Effectively, we have (5)=(2)/(1). This coefficient in Panel 1 can be interpreted as the causal effect of an additional dollar of audit adjustment on reported income the following year assuming that audits which did not lead to any audit adjustment did not have any causal impact on reported income the following year.

In each panel, we report effects for net income and for total tax liability. Estimates are weighted according to the experiment stratification design. Weights do not reflect population weights. Standard errors are reported in parenthesis.

For panels A1 and B1, all the amounts are in Danish Kroner (US $1 = DKK 5.2 as of 1/2010). Income changes are trimmed at -200,000 DKK and 200,000 DKK. That is, income changes are defined as min(200000,max(income in 2007-income in 2006,-200000). This is done to avoid extreme outcomes which make estimates very imprecise. Less than 2% of observations are trimmed on average.

Table 6. Threat-of-Audit Letter Effects on Individual Upward Adjustments to Reported Income

	No Letter Group	Differences Letter Group vs. No Letter Group									50% Letter - No letter	100% Letter - 50% letter
	Both 0% and 100% audit groups	Both 0% and 100% audit groups			0% audit group only			100% audit group only			Both 0% and 100% audit groups	
	Baseline	Any Adjustment	Upward Adjustment	Downward Adjustment	Any Adjustment	Upward Adjustment	Downward Adjustment	Any Adjustment	Upward Adjustment	Downward Adjustment	Upward Adjustment	Upward Adjustment
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
A. Average amounts of individual upward adjustments												
Net Income	-497	94	84	10	74	77	-3	115	92	23	58	52
	(31)	(42)	(22)	(34)	(55)	(29)	(45)	(64)	(35)	(52)	(26)	(26)
Total Tax	-322	67	50	17	57	46	11	77	54	23	32	36
	(24)	(32)	(18)	(26)	(43)	(24)	(34)	(49)	(28)	(39)	(21)	(21)
Number of obs.	9,397	24,788	24,788	24,788	14,145	14,145	14,145	10,643	10,643	10,643	24,788	24,788
B. Probability of upward adjustments (in percent)												
Net Income	13.37	1.63	1.56	0.07	2.29	1.52	0.76	0.98	1.60	-0.62	1.10	0.93
	(0.35)	(0.47)	(0.28)	(0.40)	(0.62)	(0.37)	(0.53)	(0.73)	(0.44)	(0.61)	(0.33)	(0.33)
Total Tax	13.69	1.52	1.57	-0.05	2.03	1.65	0.37	1.02	1.49	-0.47	1.03	1.07
	(0.35)	(0.48)	(0.29)	(0.40)	(0.63)	(0.37)	(0.54)	(0.73)	(0.44)	(0.61)	(0.33)	(0.33)
Number of obs.	9,397	24,788	24,788	24,788	14,145	14,145	14,145	10,643	10,643	10,643	24,788	24,788

Notes: The table reports the effects of Threat-of-Audit letters on individual adjustments to reported income from the time the letter is received in March to the final May 1st deadline for the tax return filing. Panel A focuses on the average amounts of adjustment. To reduce noise due to extreme observations, all amounts are capped at 10,000 DKK. The cap affects about 1.65 percent of observations for net-income adjustments and 0.75 percent of observations for total tax adjustments (due to net-income adjustments). Panel B focuses on the probability of making an adjustment to net-income or total tax (expressed in percent).

Column (1) reports average adjustments (Panel A) and probability of adjustment (Panel B) among those did not receive the letter. Column (2) reports the difference in average adjustments (Panel A) and probability of adjustment (Panel B) between the letter and no letter groups. Column (3) reports the difference in upward adjustments while column (4) reports the difference in downward adjustments ((3)+(4)=(2)). Cols. (5), (6), (7) repeat cols. (2), (3), (4) but limiting the sample to those not audited in base year (0% audit group). Cols. (8), (9), (10) repeat cols. (2), (3), (4) but limiting the sample to those audited in base year (100% audit group). Column (11) reports the difference in adjustments between the letter group with 50% audit probability and the no letter group. Column (12) reports the difference in adjustments between the letter group with 100% audit probability and the letter group with 50% audit probability.

In each panel, we report effects for net income and for total tax liability. The sample includes only tax filers who did not have any self-employment income in base year (as tax filers with self-employment income were not part of the letter experiment). Estimates are weighted according to the experiment stratification design. Weights do not reflect population weights. Standard errors are reported in parenthesis.

Table A1. Randomization Checks: Audit and Letter Experiments

	A. Audit randomization				B. Letter Randomization				C. Within Letter Randomization			
	0% audit group	100% audit group	Difference 100%-0%	Difference standard error	No Letter group	Letter group	Difference (5)-(6)	Difference standard error	50% Letter group	100% Letter group	Difference (5)-(6)	Difference standard error
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Net Income	265,209	263,485	-1,724	(6,047)	239,936	244,477	4,541	(3,425)	243,878	245,078	1,200	(4,422)
Total Tax	100,968	100,460	-508	(3,010)	82,443	84,230	1,786	(1,588)	84,022	84,438	415	(2,073)
Personal Income	216,418	217,426	1,007	(2,351)	257,022	259,748	2,725	(2,904)	259,374	260,123	749	(3,730)
Capital Income	-13,127	-12,805	323	(1,015)	-16,554	-15,485	1,068	(534)	-15,613	-15,358	255	(626)
Deductions	-11,839	-11,976	-138	(160)	-8,333	-8,304	29	(160)	-8,268	-8,341	-73	(193)
Stock Income	18,141	15,880	-2,261	(4,928)	7,371	8,220	849	(1,777)	7,857	8,584	727	(2,243)
Self-Employment	55,616	54,960	-656	(2,869)	430	299	-131	(209)	527	70	-457	(268)
% with Net Income	99.55	99.52	-0.03	(0.07)	98.73	98.64	-0.09	(0.15)	98.52	98.76	0.24	(0.19)
% with Total Tax	96.71	96.61	-0.11	(0.17)	96.64	96.26	-0.38	(0.25)	96.26	96.25	-0.02	(0.31)
% with Personal Income	94.98	94.85	-0.13	(0.21)	97.29	97.11	-0.18	(0.22)	96.99	97.23	0.25	(0.27)
% with Capital Income	95.67	95.40	-0.27	(0.20)	97.02	96.90	-0.12	(0.23)	96.77	97.03	0.26	(0.28)
% with Deductions	71.69	71.76	0.07	(0.44)	64.18	64.49	0.31	(0.65)	64.79	64.19	-0.60	(0.77)
% with Stock Income	40.30	40.23	-0.07	(0.47)	44.07	43.63	-0.44	(0.67)	43.59	43.68	0.09	(0.80)
% with Self-Employment	40.18	40.37	0.19	(0.47)	0.78	0.79	0.01	(0.12)	0.77	0.82	0.05	(0.14)
Female (%)	39.93	39.59	-0.33	(0.47)	49.80	50.10	0.30	(0.67)	49.83	50.38	0.55	(0.81)
Married (%)	58.46	58.13	-0.32	(0.48)	54.54	53.22	-1.32	(0.67)	53.79	52.65	-1.13	(0.80)
Church membership (%)	85.83	85.71	-0.12	(0.34)	86.82	86.86	0.04	(0.46)	87.06	86.66	-0.40	(0.54)
Copenhagen (%)	3.14	3.13	-0.01	(0.17)	3.17	3.33	0.16	(0.24)	3.32	3.34	0.02	(0.29)
Age	49.28	49.43	0.14	(0.16)	49.09	48.90	-0.19	(0.25)	49.01	48.80	-0.21	(0.30)
% filing in 2007	97.08	96.94	-0.14	(0.16)	100.00	100.00	0.00	(0.00)	100.00	100.00	0.00	(0.00)
Number of observations	23,148	19,630	42,778		9,397	15,391	24,788		7,706	7,685	15,391	

Notes: This table presents randomization checks for the audit experiment (panel A, cols. (1) to (4)) and the letter experiment (panel B, cols. (5) to (8) and Panel C, cols. (9) to (12)). Panel A compares baseline reported incomes in 2006 (before the audit experiment took place). Columns (1) and (2) present the baseline averages for the treatment group and control group respectively. Column (3) presents the difference between the treatment group and the control group. The standard error of the difference is presented in column (4). Panels B and C compare pre-populated tax returns for 2007 incomes before the letters are sent. The columns in panels B and C are constructed as in Panel A. In panel B, the sample is restricted to tax filers not registered as self-employed in the base year as the letter experiment could not be carried out for self-employed. In Panel C, the sample is further restricted to tax filers who received either the 50% threat-of-audit letter or the 100% threat-of-audit letter. Estimates are weighted according to the experiment stratification design. Weights do not reflect population weights. All the amounts are in Danish Kroner (US $1 = DKK 5.2 as of 1/2010).