

## Project Description

# An Experimental Evaluation of Tax Compliance in Denmark

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### 1. Purpose and motivation

Lack of compliance with tax laws jeopardizes revenue funds necessary for government services and undermines the efficiency and fairness of the tax system. Non-compliance (inaccurate reporting) can occur both because of tax evasion (deliberate fraud) and because of honest mistakes made by tax payers due to lack of information or the law being unclear. The aim of this research project is to construct — in cooperation with the Danish Tax Administration (SKAT) — a large-scale randomized experiment that may help to shed light on the following questions about evasion and compliance:

- What is the total extent of non-compliance and tax evasion?
- What is the extent and nature of heterogeneity in non-compliance and evasion? For individuals, how does compliance/evasion vary with income, opportunities to evade, and other individual characteristics? For firms, how does compliance/evasion vary with size, industry and other firm characteristics?
- What are the effects of a more rigorous tax enforcement? In particular, what is the effect of an increased probability of audit on evasion? Is the effect heterogeneous across tax payers?
- How well are tax payers informed about the tax law, and about tax administrators' capability to detect fraud?
- How should tax enforcement resources be allocated across individual tax payers and firms?
- How many resources should be devoted to auditing? Is more auditing desirable?

### 2. Two alternative ways of estimating non-compliance/evasion

The experiment exploits two competing approaches to estimating the degree of non-compliance/evasion. First, the degree of non-compliance will be measured directly by tax administrators through audits. This measure will probably underestimate non-compliance, because the tax authorities are unlikely to detect all evasion. Second, the degree of non-compliance is estimated by announcing to randomly selected tax payers that the returns they

are about to file will be subject to very thorough audits. If the tax payers perceive that a “very thorough” audit allows the tax collection agency to detect all fraud, we would expect them to report their true income. In this case, the effect of the announcement on reported income can be interpreted as the total extent of tax evasion. This way of estimating evasion has previously been adopted by Slemrod et al. (2001) in their study of a tax audit experiment in Minnesota.

The two competing measures may deviate for two reasons: (1) The direct audits capture wrongful reporting in general, including both deliberate evasion and honest mistakes. In other words, the direct audits give a measure of non-compliance in general, whereas the announcement experiment captures tax evasion more narrowly. (2) If agents’ beliefs about tax collectors’ ability to detect fraud in a “thorough audit” are incorrect, the two estimates will tend to differ. For example, if agents believe tax administrators to be better at their job than they actually are, the announcement experiment would tend to make tax payers report more truthfully than what tax administrators can capture in the direct audits. In this case, the announcement experiment would be a better estimate of actual evasion than the audits themselves.

### 3. Experimental Design

The audit experiment will be run on a total of 27,000 agents within four different subgroups: corporate firms, self-employed individuals, employees with complicated tax returns and potentially good opportunities to evade (“heavy” employees), and employees with less complicated tax returns and probably low opportunities to evade (“light” employees).<sup>1</sup> For the heavy employees, the design of the experiment and a timeline for the different stages are illustrated in Figure 1. The experimental design is the same for self-employed individuals but with a slightly different timeline (due to a different deadline for tax reporting). For the corporate firms and the “light” employees, we only generate a **T1**-group and a **C1**-group.

Prior to May 2007, a random sample of agents were selected for very thorough audits.<sup>2</sup> We denote this group by **T** for treatment. At the same time, a control group **C** was created by randomly selecting agents who are not to be audited under any circumstances (i.e., even if aspects of their tax returns would normally call for an audit).

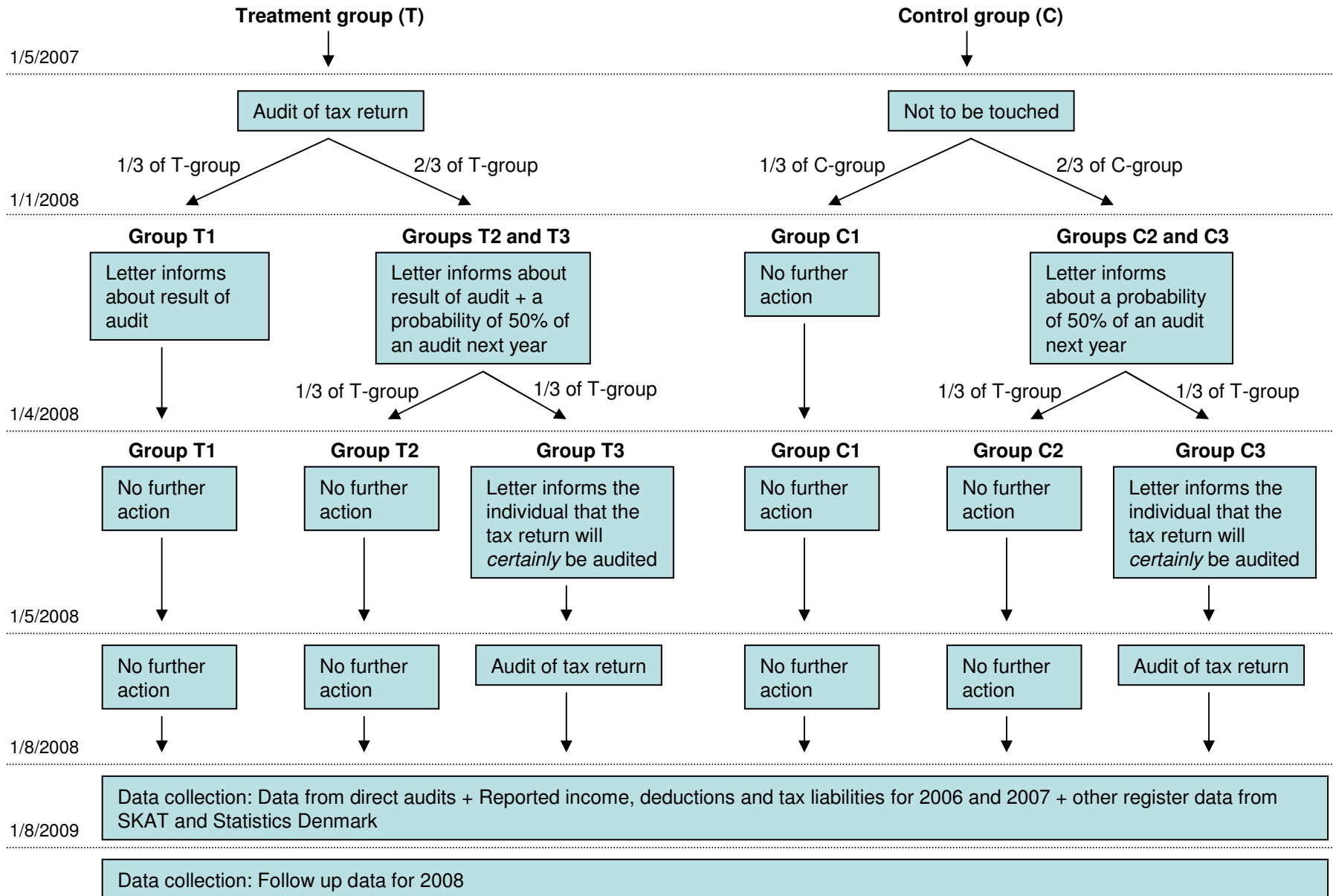
The audits of the **T**-group take place over the Fall of 2007 and the Spring of 2008. Following these audits, the **T**-group is divided into three subgroups equal to 1/3 the size of the original group. **T1**-agents receive a letter informing them that they have been audited, how much extra tax they owe (or are eligible to get back), and in some cases that the result of the auditing may result in a fine. **T2**-agents and **T3**-agents receive a letter containing all the information just described, as well as information that there is a 50% chance that they will be

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<sup>1</sup> In the groups “heavy employees” and “light employees” are included individuals without labour income who are not self-employed.

<sup>2</sup> The selection is random within each of the four subgroups separately. More precisely, a random sample is generated for each group at each tax centre in Denmark.

# Experimental Design for Individuals with High Opportunity to Evade ("Heavy" Employees)



audited again the following year. The letters to **T2** and **T3** announcing the audit threat should be sent after January 1, 2008 so as to prevent effects on real behaviour (say, labour supply), thereby allowing us to isolate the effects of auditing on compliance. Then, at the time where the pre-printed tax returns are sent to the agents in the Spring 2008, the **T3**-agents (but not the **T2**-agents) receive an additional letter telling that they have indeed been selected for auditing again. In other words, at the time of handing in the tax return, **T3**-individuals have been told that their audit probability  $p$  is equal to 1, whereas **T2**-individuals have been told that there is some high probability  $p$ , but still  $p < 1$ , of an audit. After receiving tax returns in May 2008 (for the tax year 2007), **T3**-individuals will be subjected to a second round of audits.

As shown in the figure, the **C**-group which was not audited in the first stage are also divided into **C1**, **C2**, and **C3** and treated symmetrically to the **T**-groups in the following stages.

After the experimental stages, we then link the data obtained from the experiment with data from SKAT and Statistics Denmark for the years 2006, 2007, and 2008.

#### 4. Estimator

We estimate the effects of auditing on a given outcome **X** (e.g. reported income) by considering changes in **X** over time for the treatment and control groups. The estimation will be based on the so-called difference-in-differences estimator, i.e.

$$[ X(\text{T-agents, tax year 2007}) - X(\text{T-agents, tax year 2006}) ] - [ X(\text{C-agents, tax year 2007}) - X(\text{C-agents, tax year 2006}) ]$$

where  $X(\text{agent, year})$  denotes the outcome of interest for a specific group of agents in the experiment in a given year. The exact definition of T- and C-agents in the formula (T1, T2, T3 and C1, C2, C3) depends on the type of treatment we are estimating (see below).

#### 5. Results

*The effect of having experienced an audit on behaviour*

**T1 - C1** gives the effect of having experienced an audit in one year on reporting behaviour in the following year. Note that, if individuals have accurate information on the probability and consequences of being audited, experiencing an audit should not affect future behaviour at all. Hence, this is a test of the information/perception individuals have about the auditing practice. For example, if individuals learn that tax authorities are not as good at detecting evasion as they thought, having experienced an audit can lead to more evasion.

*The effect of an audit probability equal to 1*

**T3 - T1** and **C3 - C1** gives the effect of being informed of a certain, thorough audit (depending on having experienced an audit in the first year or not). These are very interesting treatment effects because, if agents believe (rightly or wrongly) that tax authorities can detect all fraud

upon a "very thorough" audit, being warned about this audit should make them report their true income and hence we get an estimate of the extent of evasion. This means that we obtain an estimate of evasion which we can compare to the direct measures of non-compliance found in the audits.

As discussed above, it is not clear a priori which type of estimate is best, and the two estimates may also capture slightly different things. The effect of announcing audits in advance should affect only evasion (deliberate fraud) and not honest mistakes in reporting due to lack of information or a complex tax code. Moreover, the evasion found in the audits may deviate from the evasion effects found in the announcement experiment if beliefs about the consequences of a "very thorough" audit are different from the actual consequences of such audits. For example, if agents tend to overestimate tax administrators' capability to find fraud, the announcement of a certain audit may make them report more than the administrators are able to detect in the direct audits, in which case **T3 - T1** and **C3 - C1** will get closer to the true amount of evasion than the direct audit. On the other hand, if agents have correct information about the capabilities of tax administrators, the announcement experiment and direct audits should give roughly similar results (apart from non-evasion reasons for imperfect compliance).

Whether agents have correct information can also be studied by comparing **T3 - T1** to **C3 - C1**. Under perfect information, agents will not learn anything new from experiencing an audit in the first year, and then **T3 - T1** and **C3 - C1** should not be different to a statistically significant extent.

#### *The effect of increasing the audit probability*

**T3 - T2**, **T2 - T1**, **C3 - C2**, and **C2 - C1** give the effects of increasing the probability of an audit. If the probability of being audited is denoted by  $p$ , then the **T3/C3**-groups have  $p=1$ , the **T2/C2**-groups have a "high  $p$ ", and the **T1/C1**-groups have whatever  $p$  is currently perceived to be.

#### *Short-run versus long-run effects*

Using data from 2008 and beyond (i.e., *after* both rounds of audits have occurred), we can compare different groups to study the presence of permanent effects of having experienced audits, and we can compare the difference between having experienced two audits versus one audit.

### **6. Time plan, projected outcome, and construction and financing of data**

We have had meetings with SKAT (including the CEO Ole Kjær) and have already started to collaborate. By now SKAT has constructed the random samples for the **T**-group and the **C**-group and has started the auditing process of tax returns for the **T**-group. All resources used by SKAT in relation to the project are financed by SKAT. The data obtained from SKAT needs to be linked with information on individual characteristics obtained from Statistics Denmark.

The data purchase item in our budget reflects the estimated payment to Statistics Denmark. The main part of the data from Statistics Denmark should be available during the spring of 2009 (estimation of the long-run effects may require more data). We plan to have a working paper ready during the fall of 2009. The aim of the project is to publish one or more articles in leading journals.

### **References**

Slemrod, J., Blumenthal, M., Christian, C., 2001. Taxpayer response to an increased probability of audit: evidence from a controlled experiment in Minnesota, *Journal of Public Economics* 79, 455-483.