TAMPERE ECONOMIC WORKING PAPERS

PREFILLED INCOME TAX RETURNS
AND TAX COMPLIANCE:
EVIDENCE FROM A NATURAL EXPERIMENT

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Prefilled income tax returns and tax compliance: Evidence from a natural experiment[*]

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Abstract

Despite the adoption of prefilled tax forms in many countries, little is known about their effects on taxpayers' reporting behaviour. We estimate the effect of the income tax filing system on taxpayers' reporting behaviour, utilising data from a Finnish policy experiment. We find that receiving a (partially) prefilled income tax return lead to a significant reduction in non-prefilled deductions and self-reported income, and an increase in deductions that were prefilled in the new system. However, we do not find effects on individuals' total taxable income or taxes paid. We discuss complexity and compliance costs, salience effects, and changes in the opportunities for tax evasion as possible explanations for our findings.

Keywords: income tax filing, complexity, salience, tax evasion, natural experiment

JEL: H24, H31, H26

1 Introduction

The purpose of this paper is to examine how the details of the income tax filing system affect taxpayers' reporting behaviour. More specifically, we analyse the effects of the introduction of prefilled tax returns on income tax reporting. We utilise a policy experiment that took place in Finland in the mid-1990s, whereby a proportion of taxpayers received a partially prefilled income tax return, whereas other taxpayers had to file a full return (the normal practice). For those who received a prefilled return, items such as labour income had been prefilled based on information received from third parties such as employers. These individuals were required to file a final return only if some income information was incorrect or missing and had the *option* of filing a return e.g. in case they wanted to file for some discretionary deductions. Due to the nature of the rules that determined inclusion of individuals into the experiment, we are able to estimate the causal effect of the income tax filing system on taxpayers' reporting behaviour.

Beyond evaluating the effects of this particular reform, the evidence that we find is likely to be of interest for policy in other countries. Prefilled income tax returns are the current practice in many countries, and their use has been proposed and/or experimented with in others,[1] yet no systematic evidence exists on their effects on tax reporting.[2] One advantage of a shift to prefilled tax returns are savings in tax preparation costs (Goolsbee 2006). In addition to such direct cost savings, it is important to know how taxpayers' reporting behaviour would be affected. Reporting behaviour may be affected if the reform changes the costs of reporting, the likelihood of honest mistakes in reporting (e.g. due to complexity or salience effects), or possibilities for tax evasion. Any changes in reporting behaviour may naturally have an effect on tax revenues as well as the take-home pay of individual taxpayers.

We analyse how the reform affected reporting on five types of items: (i) prefilled income (income from primary and secondary jobs), (ii) non-prefilled income (other earned income and capital income), (iii) prefilled deductions (mortgage interest deduc-

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[1] In a survey of revenue bodies in 56 advanced and emerging economies (including all OECD, EU, and G20 members), almost a half of revenue bodies reported using at least some degree of prefilling (OECD 2015, 256). For an account of an experiment with return-free filing in Callifornia, see Bankman (2005).

[2] Gillitzer and Skov (2013) show that the introduction of third party reporting and prefilling lead to an increase in the number of individuals claiming for charitable contributions in Denmark. Alm et al. (2010) conducted a laboratory experiment where they found that when subjects were provided information on their true tax liability, this had a positive impact on reporting. The treatment in the Alm et al. experiment can be thought of as having analogies with third party information and prefilling.

tion in 1997); (iv) non-prefilled deductions (e.g. costs of travel to work and other costs for acquiring income); and (v) reported wealth (which was non-prefilled for all tax-payers)[3]. This classification covers all items on an individual's income tax return. We find most pronounced effects for non-prefilled deductions, where receiving a partially prefilled return caused a significant reduction in claims. The effect is large, amounting to an over 1/4 drop in the number of individuals claiming for such discretionary deductions. On the other hand, for deductions that were prefilled, there was an increase (albeit smaller) in the number of claims. For non-prefilled income items, there was a fairly small but statistically significant drop in reporting; similarly, there was a drop in reported wealth, which was also not prefilled. For prefilled income items, there was no change in reporting.

Finally, we have estimated the effect of receiving a prefilled tax return on individuals' total taxable income and taxes paid and the reform appears to have had no effects on these aggregate measures. These results are significant for an overall evaluation of the reform. To the extent that taxable income is a sufficient statistic for welfare (net of compliance costs and tax administration costs), the results would point towards the conclusion that prefilled tax returns appear to be a good idea[4]: the reform implied no adverse effects on taxable income, but at the same time it implied a reduction in the costs of compliance and administration.[5]

In addition to assessing the desirability of prefilled tax forms, our results provide interesting insights on taxpayers' filing behaviour. A key feature of our results is that receiving a partially prefilled return leads to a tendency to report less of those items (both positive income items and deductions) that are not prefilled and more of those that are. Potential explanations for our findings are likely to be related to the complexity of the tax system and the associated costs of filing a tax return and/or claiming for individual items, as well as salience effects. We also discuss whether changes in tax evasion may explain our results, and conclude that changes in the opportunities for tax evasion may be a partial explanation for some findings, but is very unlikely to be the whole story.

The paper is organised as follows. In Section 2, we discuss some theoretical considerations related to why and how the details of the income tax filing system might

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[3]At that time, wealth (above a certain limit) was taxable in Finland.

[4]The conclusion could potentially be qualified if there were significant adverse effects on the income distribution (see the Results section for a preliminary discussion).

[5]According to Goolsbee (2006), a scheme with "Simple Returns", where prefilled tax returns would be sent to U.S. taxpayers with sufficiently simple finances, could save up to 225 million hours of tax compliance time and more than $2 billion a year in tax preparation fees.

affect reporting behaviour. In Section 3, we describe the Finnish experiment involving prefilled income tax returns. The data and methodology are described in Section 4. Our preliminary results are presented in Section 5 and the possible explanations for our findings are discussed in Section 6. Section 7 concludes.

2   Theoretical considerations

Why would the details of the income tax filing system matter for reporting behaviour? In this Section, we outline a simple reporting model to provide a framework for understanding the possible mechanisms involved. In the model, individuals weigh the benefits and costs of truthful reporting, taking into account the likelihood of getting caught with tax evasion, as in the classical deterrence model of tax evasion (Allignham and Sandmo, 1972). As in Kleven et al. (2011), the probability of getting caught may differ across different types of income items. Further, we stress that it is the *perceived* probability of detection that matters for the incentive to report truthfully, and the perceived probability may depend on the extent of prefilling. We also incorporate the idea that individuals may make honest mistakes in reporting. The particular type of mistake that we consider is that individuals may not be aware of the obligation to report a given item, or the right to claim for a given deduction; such incomplete awareness is related to the idea of tax salience (e.g. Chetty et al. 2009).

We consider two margins on which reporting decisions are made: the individual decides whether or not to file a return, as well as whether or not to report an individual item, conditional on filing. Previous work on tax compliance concentrates almost exclusively on the intensive margin of reporting (i.e. considering the amount of income to report, and not whether or not to report at all); exceptions include Pitt and Slemrod (1989), Slemrod (1989) and Erard and Ho (2001). Due to possible fixed costs of reporting (see below), when considering deduction claims for example, the extensive margin of reporting is likely to be at least as important as the continuous choice of how much to report. For simplicity, we assume that the individual either does not report an item at all or reports the true amount on that item. (Considering also the amount how much to report on a given item would add a third margin, and would complicate the analysis without providing important new insights in the present context.)

2.1 Model

Reporting individual items We consider four types of items on the tax return: prefilled and non-prefilled income items (positive income items) and prefilled and non-prefilled deductions (negative income items).[6] Clearly, the prefilled items are prefilled only for those individuals who receive a partially prefilled return, but for sake of consistency we use the same categorisation of items for all individuals.

We assume that the individual is risk neutral and all income is subject to a linear income tax rate. We consider a discrete choice of whether a given item is reported or not. An individual item is reported if

$$s_i^P NB_i > 0, \tag{1}$$

where NB_i is the expected net benefit of reporting item i. and s_i^P is the ex post salience parameter that is equal to 1 if the individual is aware of his eligibility for the item or requirement to report it, and zero otherwise. "Ex post" refers to the fact that when reporting individual items, the taxpayer has already incurred the fixed cost of filing (e.g. studying the information material - see below) and salience of different items may have changed (increased) as a result. (In the simplest case we might assume that $s_i^P = 1$ for all items.) Clearly, if $s_i^P = 0$, the individual is not aware of the item and does not report it.

It is important to note that reporting here in practice refers to making amendments to the tax return. For individuals who receive an empty return, the default on a given item is zero, wheras for individuals who receive a partially prefilled return, the default is given by the information printed on the form. The reporting decision is determined by (1) regardless of whether the individual receives a prefilled form or not, since amending a prefilled item is in practice identical to filling the same item on an empty form. The parameter values in (1) may however differ between the different cases, depending on whether the individual receives a partially pre-filled form or not, and on whether the given item is prefilled. (More on this below and in Section 2.2, where we discuss the predictions from the model.)

For positive income items we have

$$NB_i = [(1 - \tau) m_i - k_i] - [(1 - \tilde{p}_i (\varphi_i, \varphi)) m_i + \tilde{p}_i (\varphi_i, \varphi) (1 - \tau (1 + \theta)) m_i], \tag{2}$$

where the terms inside the first square brackets correspond to income from item i if

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[6]In the empirical part, we also consider effects on reported wealth.

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the item is reported, and the second square brackets to expected income if the item is not reported. τ is the linear income tax rate, m_i is the amount of income on item i (measured as the difference to the default - either zero or the prefilled amount - since the benefits of reporting relate to the amount by which the individual's reported income differs from the default); and k_i is the (financial or mental) cost of reporting item i. Further, $\tilde{p}_i(\varphi_i, \varphi)$ is the perceived probability that evasion is detected on item i, which may depend on whether the individual received a prefilled income tax return ($\varphi = 1$; zero otherwise) and whether the given item is prefilled ($\varphi_i = 1$; zero otherwise). Finally, $\theta \in (0, 1)$ is the fine associated with evasion, that is, the fraction of owed tax payments that needs to be paid as a fine if caught with tax evasion. Given (1) and (2), a positive income item is reported if

$$s_i^P \left[(1 + \theta)(\tilde{p}_i(\varphi_i, \varphi) - 1) \cdot \tau m_i - k_i \right] > 0. \tag{3}$$

In the case of deductions, the individual has incurred expenses that may be deducted from taxable income, i.e. there is a possible tax saving (at rate τ) associated with filing for deductions. For deductions (negative income items), the expected net benefit of reporting an item is given by

$$NB_i = \left[-(1 - e_i)(1 - \tau) m_i + e_i \left(-\tilde{p}_i(\varphi_i, \varphi) \theta \tau m_i + (1 - \tilde{p}_i(\varphi_i, \varphi)) \tau m_i \right) - k_i \right] - \left[-(1 - e_i) m_i \right], \tag{4}$$

where the terms inside the first square brackets again give the income from item i if the item is reported, and the last term corresponds to the case when the item is not reported. The parameter $e_i = 1$ if the negative income item is illegitimate (a form of evasion), and zero if it is legitimate (the expenses claimed actually took place). In the case an individual is caught with an illegitimate claim, the fine is again equal to a fraction of the owed tax payments, i.e. $\theta \tau m_i$.[7] Given (1) and (4), a deduction item is then reported if[8]

$$s_i^P \left[-((1 + \theta) e_i \tilde{p}_i(\varphi_i, \varphi) - 1) \cdot \tau m_i - k_i \right] > 0. \tag{5}$$

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[7] The assumption that m_i is exogenous may seem problematic in the case of illegitimate items. One might then think of m_i as corresponding to some "typical" value of a particular deduction. E.g. in the case of trade union membership fees that are tax deductible, a taxpayer may consider deducting such fees even if not belonging to a trade union; the claim would then be illegitimate but m_i would nevertheless be well defined.

[8] The same condition can also be derived by noting that the benefit of reporting a given item relate to the associated tax savings, equal to $(1 - e_i \tilde{p}_i(\varphi_i, \varphi)) \tau m_i$; and the costs relate to the penalty if caught on repoting an illegitimate item, and are equal to $e_i \tilde{p}_i(\varphi_i, \varphi) \theta \tau m_i + k_i$. The item is then reported if the benefit of doing so is larger than the cost.

The filing decision. Prior to deciding whether or not to report individual items, individuals need to decide whether or not to file in the first place. Individuals who did not receive a prefilled form had to file a complete return, whereas for individuals who received a pre-filled form, filing means making some amendments to the pre-filled form (either entering information that was missing from the form, or making corrections to some of the pre-filled items).

Assume that there is a fixed cost of filing, K and there may also be a (possibly probabilistic) fine F for not filing. The individual chooses to file if

$$\sum s_i^A NB_i \cdot I\left(NB_i > 0\right) - K > F, \tag{6}$$

where $I\left(.\right)$ is the indicator function and takes into account the fact that only those items where $NB_i > 0$ will end up being reported in case the individual chooses to file. The ex ante salience parameter s_i^A is equal to 1 if the individual is aware of his eligibility for the item / requirement to report it (prior to e.g. studying any information material associated with tax filing), and zero otherwise. It is natural to assume that $s_i^A \leq s_i^P$ i.e. studying the information material can only increase the individual's awareness of the (costs and benefits) of reporting different items.

It is important to note that for individuals who received a prefilled return, $F = 0$ i.e. they were not required to file a return.[9] On the other hand, for individuals who did not receive a prefilled return, we may assume for simplicity that F is prohibitively high, that is in practice all individuals in the old system had to file a return.[10]

2.2 Predictions

Complexity and the costs of reporting A key cost component is likely to be the fixed cost of filing a tax return, K. The tax system as well as the tax return form itself are highly complex, and there may be significant fixed costs related to learning the details of the system, studying the instructions material provided etc, in order to report one's income correctly for tax purposes.[11] Those who are required to file a

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[9]Those who received a prefilled return were only required to file if some income information on particular items on the tax form was incorrect or missing, in which case they would face a penalty for not reporting that particular item; the fine θ (see above) would then be applicable.

[10]This is a simplifying assumption that is strictly speaking not required for our results; what we would need is that for those who received a non-prefilled return, $F >> 0$ i.e. they are much more likely to file a return than individuals who received a prefilled form.

[11]The fixed cost of filing taxes may also be related to the negative psychological emotions associated with having to think about taxation or complex tasks in general. In Finland, individual taxpayers very rarely use tax preparer services.

complete income tax return have no choice but to incur these fixed costs (as we have assumed that F is prohibitively high for them). On the other hand, those who receive a prefilled return, may choose to avoid these costs. Therefore, in the case of receiving a partially prefilled return, if the expected gain from filing is smaller than the fixed cost of filing, the individual will not file.

In addition to the fixed cost of filing a return, there may be costs associated with reporting any individual item on the tax return, denoted above by k_i.[12] A key component here is likely to be the opportunity cost of time spent on activities related to reporting, in particular calculating the correct figure to report on each item.[13] The opportunity cost of time is likely to be convex: it is fairly easy to find a small amount of time for any particular activity, but since there is only a fixed number of hours in a day, additional time units become increasingly costly. Many items were prefilled for those individuals who received a prepopulated tax return. Therefore, considering possible effects on reporting any discretionary items, a convex opportunity cost of time (k_i increasing when more items are reported) would point towards individuals who received a prepopulated return filing for *more* such items, as compared to individuals who had to file a complete return.

Taking together the two lines of argument related to costs of reporting, a taxfiler's cost function (as a function of the number of items reported n, assuming here for simplicity that $s_i^A = 1$), given by $C\left(n\right) = K + \sum_{i}^{n} k_i$, may first be concave and then convex: there may be a fixed cost related to learning the details of a complex tax system, but thereafter the function may be convex due to convex opportunity costs of time. Therefore the effect of receiving a prefilled tax return on reporting of any discretionary items such as deductions, based on these arguments, is unclear a priori.

If the fixed complexity costs are prominent, we would expect them to dominate over the variable costs, and would expect to see a reduction in reporting any non-prefilled items. On the other hand, for items that are prefilled in the prepopulated tax return, one would expect to see an increase in the number of people claiming a positive amount: again, for individuals who receive an empty return, the default on a given item is zero, wheras for individuals who receive a partially prefilled return, the default is given by the information prefilled by the tax authority. Therefore, for prefilled items, there is no

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[12]See Pitt and Slemrod (1989) for evidence on the cost of itemising deductions in the U.S.

[13]There were no significant differences in any physical costs incurred by individuals subject to the two different systems of tax filing. Making adjustments to the pre-filled tax return was in practice identical to reporting similar items in the old system. Also other rules e.g. regarding retaining receipts related to deduction claims were identical across the two systems.

cost associated with reporting the prefilled amount (as this is the amount that prevails if the taxpayer does nothing); the costs of reporting are in this case associated with *changing* the information that is prefilled by the tax authority.

All in all, fixed cost of reporting therefore create a bias towards the status quo: filing more of those items that are prefilled on the tax return, and less of those that are not.

Salience   Receiving a prefilled tax return might also affect reporting behaviour through salience effects. Recent findings in the literature on behavioural public economics (see e.g. Congdon et al. 2011) indicate that individuals may be less than fully attentive to taxes. In general, tax salience refers to the degree of visibility of taxation (see e.g. Chetty, Looney and Kroft 2009, Finkelstein 2009). Previous literature on tax salience has been concerned with the visibility of tax rates (or tax inclusive prices), whereas we refer to the visibility of other parts of the tax code, namely individuals' awareness of particular tax items.[14]

In our case, individuals who received a prefilled tax return in most cases did not have to take any action to comply with their legal duties as taxpayers, whereas other individuals were required by law to fill out a tax return. This may have made some features of the income tax system more visible for them, i.e. they might have learned about the existence of some (non-prefilled) items that they would otherwise have been unaware of. For individuals who receive an empty return, only condition (1) and therefore only s_i^P is relevant. On the other hand, for individuals who receive a prefilled return also an extra condition, namely (6), has to be satisfied. Given this condition and $s_i^A \leq s_i^P$, salience effects would unambiguously lead to a reduction in reporting a positive amount on both positive and negative non-prefilled income items.

Simliarly to the costs of reporting, salience effects lead to a bias towards the status quo in income tax reporting. Whereas the arguments related to costs of reporting would assume reporting decisions to be the result of a rational cost-benefit calculus, salience effects may imply that an individual may neglect the obligation / possibility to report some items altogether, in which case his/her reporting decisions would not be the result of optimising behaviour.

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[14]Further, whereas the previous literature is mostly concerned with the effects of salience on real behaviour such as consumption (Chetty, Looney and Kroft 2009) and labour supply decisions (e.g. Chetty and Saez 2013), we are in the current paper interested in the effects of salience on reporting behaviour.

Opportunities for tax evasion A third line of argument is related to the opportunities for tax evasion. The perceived probability that evasion is detected is a key determinant of the extent of evasion, as it affects the expected costs of evasion: The higher the probability of detection, the lower is the extent of tax evasion. As shown by expressions (3) and (5) above, a higher probability of detection would obviously lead to a higher likelyhood of reporting a positive amount on positive income items and a lower likelihood of claiming for deductions (assuming that some deduction claims may be illegitimate).

In recent literature, it has become apparent that audit probability is not a good proxy for the probability of detection. For example, Kleven et al. (2011) and Slemrod (2007) stress the importance of third-party reporting: the authorities receive information on certain income items also from third parties (e.g. regular wages from employers), and the probability of detection is higher on these items (e.g. due to the possibility of computerised cross-checking of information) and the opportunities for evasion correspondingly lower, than on items not subject to third-party reporting.

In our case, the information available to the tax authorities was the same in both systems, and the actual detection probability does not depend on whether the third party information is prefilled or not. Nevertheless, the *perceived* probability of detection may be affected by prefilling. One possibility is then the following:

$$\tilde{p}_i\left(1,1\right) \geq \tilde{p}_i\left(0,1\right) \geq \tilde{p}_i\left(0,0\right). \tag{7}$$

That is, receiving a prepopulated return would increase the perceived probability of detection, but more so for those items that are actually prefilled on the form. The second inequality follows from possible spillovers to other items: Receiving a prefilled form makes the individuals realise that the tax authorities have some information on their income, and they may be worried that this information may extend beyond what is printed on the form. If (7) holds, receiving a partially prefilled form would reduce evasion across the board.

On the other hand, we conjecture that another possibility is that

$$\tilde{p}_i\left(1,1\right) \geq \tilde{p}_i\left(0,0\right) \geq \tilde{p}_i\left(0,1\right), \tag{8}$$

that is, receiving a partially prefilled form increases the perceived detection probability only for those items that are actually prefilled on the form, and reduces it for other items. The story would go as follows: upon receiving a prepopulated return, the individual realises that the tax authority holds some information on his/her income,

but assumes that the authorities would have printed all their information on the form, and that it is therefore safe to evade income on other items. If this were the case, then receiving a partially prefilled form would reduce evasion on those items that are prefilled, but increase evasion on other items.

Putting together the three lines of argument outlined above, one may think of a broad notion of tax compliance, defined in terms of the accuracy of reporting and taking into account both under- and over-reporting. Such a notion would encompass all three mechanisms discussed above: First, costs of reporting may naturally reduce the accuracy of reporting. Second, (incomplete) salience points to the possibility of honest mistakes and omissions in reporting. Third, tax evasion refers to a particular type of non-compliance, namely the case of willful underreporting. Compliance costs and honest mistakes in reporting may lead not only to underreporting, but also to overreporting (e.g. due to unclaimed deductions). This phenomenon is typically not detected in audit experiments, such as in Kleven et al. (2011), where the focus naturally is on detecting underreporting (see also Gillitzer and Skov 2013). In our setting, it may be possible to detect evidence of both over- and underreporting.

3   The experiment

The paper utilises data from the so called Finnish tax proposal experiment implemented between 1995 and 2004. During the experiment, a proportion of non-entrepreneur taxpayers did not have to file a return, but instead received a prefilled return ("tax proposal") where their tax liability had been calculated based on third-party reports (e.g. wage income reported by employers and transfers reported by benefit agencies) and other relevant information available to the government. The main objective of moving to a system with prefilled income tax returns was to save on tax filing expenses, even though it was also mentioned that the accuracy of reporting should not be compromised.

Those individuals who had to file a complete return, say for 1995, were required to do so by the end of January 1996. Those individuals who had been selected into the experiment were notified of this in mid-January. The prepopulated return was sent to them in late May and they had until mid-June to submit their corrections. If they did not do so, the tax authorities' original proposal was implemented.

A key piece of information for our purposes are the rules that determined a taxpayer's inclusion into the experiment. There were differences in inclusion criteria be-

tween groups of municipalities: different municipalities used different selection models, whereby both the number and types of individuals included into the experiment varied across municipalities. Within each model, individuals were selected according to variables such as age, income, lagged deductions and wealth holdings.[15] The rules created variation in treatment status across individuals, depending on where the individual lived. The purpose of the selection models was to first start off the experiment with a modest number of taxpayers with relatively simple tax returns; and then to expand the experiment so that more and more of different kinds of taxpayers became treated in subsequent years.

The experiment started in 1995 with approximately 300 000 individuals (7 % of taxpayers) receiving a prefilled return. In the first year of the experiment, there were four different selection models used: the most restrictive model was one where only pensioners and young individuals with low earned income in a given municipality were included in the experiment; and the most inclusive model involved all regular taxpayers in a given municipality. The other two selection models were between these two extremes in the share of individuals selected: the models involved regular wage earners in a given municipality, and differed in the exact details of the selection rules. One of the above four models was implemented in 9.8 % out of the 439 municipalities[16].

In subsequent years, the number of participating individuals increased each year, through two channels: The experiment was extended into new municipalities, and the selection models were changed (usually) to include more taxpayers in a given municipality. The selection models became increasingly complex over time.[17] The number of individuals who received a prefilled return each year is reported in Table 1. Today most regular, non-entrepreneur taxpayers receive a prefilled return.

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[15]Inclusion into the experiment depended on one's own and one's spouse's information, as both spouses were always part of the same reporting regime: Individuals whose spouse was not eligible to receive a prepopulated return would not receive one either.

[16]Our data excludes the 16 municipalities in the autonomous island of Åland.

[17]In the most complex model, selection depended on the previous year values of approximately 70 variables, and also in other models selection often depended on dozens of variables.

Table 1. Number of taxpayers receiving a prefilled return.

Year	prefilled return	Normal return	Total	% prefilled
1995	303,981	3,819,187	4,123,168	7.4
1996	1,325,855	2,813,613	4,139,468	32.0
1997	2,367,027	1,797,996	4,165,023	56.8
1998	2,299,431	1,884,912	4,184,343	55.0
1999	2,850,301	1,359,241	4,209,542	67.7
2000	2,864,301	1,369,787	4,234,088	67.6
2001	2,875,631	1,378,058	4,253,689	67.6
2002	2,920,112	1,359,233	4,279,345	68.2
2003	2,947,364	1,352,664	4,300,028	68.5
2004	2,950,826	1,365,848	4,316,674	68.4

Since our identification strategy (see section 4) is partly based on geographical variation in the implementation of the experiment, an important piece of information relates to how decisions were made regarding which selection model to use in each municipality. At the time, Finland was divided into 10 tax office areas that were in turn divided into a total of 93 tax districts. There were altogether 455 municipalities in 1995, and each municipality belonged into one tax district. When the experiment was launched, each tax office chose one or sometimes two default selection models that were applied to the tax districts within that tax office area. Before making a final decision on the models to be applied to each district and municipality, the tax offices were able to apply the different models to data from previous years' tax collection, to examine for example how many taxpayers would be selected from each municipality under any given model. Some municipalities and districts were then subject to exceptions, meaning that some other model than the default model of their tax office area was finally applied to that municipality/district. The main reason for such exceptions was to include a sufficient number of taxpayers into the experiment.[18] Therefore, while not completely random, the selection of municipalities into the experiment appears unrelated to our outcomes of interest (e.g. expected future trends in deduction claims or reported income), and therefore does not interfere with our identification strategy. Identification will be discussed in more detail in the next section.

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[18]It is also important to note that the tax offices are independent entities whose purpose is to ensure efficient and accurate tax collection, and their functions are unrelated to municipal politics, for example.

4 Data and methodology

Our analysis is based on two data sources. First, we have obtained detailed documentation of the experiment from the tax authorities. Notably, this includes detailed information on the criteria based on which individuals were selected into the experiment each year. This information is crucial for our identification strategy. Second, for carrying out the main part of the analysis, we use individual level data on a representative panel (20 %) of Finnish non-entrepreneur taxpayers (and their spouses) from Statistics Finland's tax register for 1993-1997. Altogether, there are approximately 1 million individuals in the data, and we follow them over the entire period. The data covers all variables contained in the tax register: there is information on various forms of income as well as tax payments (for example labour income, capital income, transfers, asset holdings, debt, taxes paid, very detailed information on various types of deductions), as well as background information (age, gender, marital status, place of residence, level of education).

In the analysis, we look at the effects of receiving a prefilled return for the first time. We utilise data from the first three years of the experiment (1995-7), when the experiment expanded considerably from covering 7% of regular taxpayers in 1995 to covering 57 % of taxpayers in 1997 - see Table 1.[19] We pool the data for the three years in all regressions. The outcome variables are formed by classifying all items in the tax return into five categories, and our analysis therefore provides a full analysis of the effects of introducing prepopulated tax returns on income tax reporting. The first three categories consist of positive income items and wealth, and the last two cover deduction items:

(i) Prefilled income: This category consists of income from primary and secondary jobs.

(ii) Non-prefilled income: This category consists of other (self-reported) earned income and capital income. Capital income includes dividends, rental income, capital gains and interest income, and other smaller items.[20]

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[19]Analysing later years when more and more individuals are treated becomes problematic, since it is increasingly difficult to find suitable non-treated controls for the treated individuals.

[20]The share of individuals with capital income is fairly low in our data - see Table A1 in the Appendix. This is mainly due to the fact that the sample excludes self-employed individuals (as they were not part of the experiment). Note also that unlike in some other countries, interest income on bank deposits is not part of income taxation and hence not included in our data (it is taxed at source and not reported on the income tax return). Further, mortgage interest payments are not reported as negative capital income, but rather as a deduction item. Finally, we have excluded individuals who had reported capital gains from sales of local telecommunications company shares in 1997: this was a one-off event associated with the privatization of the local telecommunications company in Helsinki,

(iii) Wealth. Information on the amount of personal wealth was non-prefilled for all taxpayers.

(iv) Prefilled deductions: This category consists of the mortgage interest deduction in 1997. Prior to 1997 no deduction items were prefilled, and prepopulation was introduced for this single deduction item in 1997.

(v) Non-prefilled (discretionary) deductions: This category includes deductions for the costs of travel to work, interest payments for mortgages (prior to 1997) and student loans, costs for acquiring income and other smaller items.[21]

The prefilled income and deduction items were subject to third-party reporting in both systems (regardless of whether the individual received a prepopulated return or not), whereas the non-prefilled items were not subject to third party reporting in either system. Third party reporting is naturally a prerequisite for using prepopulated returns. However, since third party information affected all taxpayers in a similar fashion, whereas we have variation in prepopulation across taxpayers, we can disentangle the effects of prepopulation from those of third party information.[22] Further, the fact that individuals were told of their inclusion in the experiment only after the tax year had ended, together with the fact that we are focussing on individuals who received a prepopulated tax return for the first time, ensure that the responses that we observe are only related to tax reporting, not to real behaviour.[23]

For income items, we use data on the self-reports. If there is no self-report, we use the third-party report (i.e. the figure printed on the prefilled form; if there is no separately recorded self-report, this implies that the individual has not amended the proposal made by the tax authority in this respect, and hence accepts the prefilled figure). The deduction data that we have on the other hand relates to approved claims, and we have no separate information on original deduction claims. This has both disadvantages and advantages. A disadvantage is clearly that we do not have accurate information on the original claims. Nevertheless, approved claims should be

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which caused a large local peak in the number of individuals with capital gains.

[21]In the Finnish system, there are a number of deductions / allowances that taxpayers receive automatically (that are not reported at all on the income tax return), as well as a number of discretionary deductions that they can apply for. The latter is similar to itemisation in the U.S. case. Itemisation behaviour in the U.S. has been studied by Pitt and Slemrod (1989) and Slemrod (1989).

[22]This is in contrast to Gillitzer and Skov (2013) and Kleven et al. (2011), where the effects of prepopulation and third party information cannot be separated. In Gillitzer and Skov, prepopulation and third party reporting of charitable deductions were introduced simultaneously for the entire population. In Kleven et al. there is no exogenous variation between individuals in either third party information or prepopulation.

[23]For example, if opportunities for tax evasion change, this may trigger real behavioural responses, as tax evasion opportunities have an effect on effective tax rates. However, due to the timing of the experiment, this is not an issue in our case.

a good proxy for original claims, as they should in the vast majority of cases be equal to original claims (as individual tax returns are rarely audited). An advantage is that the data that we have contains those figures that are relevant for the final outcome of taxation (tax revenue, individual take-home pay).

To examine the effects of receiving a prefilled tax form on the above outcome variables, we use a sample consisting of treated individuals and a control group (see below), and estimate models of the following type:

$$y_{it} = \alpha_i + \gamma_t + \delta D_{it} + \beta' X_{it} + \varepsilon_{it},$$

where y_{it} is the outcome variable of interest for individual i in year t. D_{it} is a dummy variable indicating whether individual i received a prefilled tax form in year t and δ is the effect on the outcome variable. We control for year dummies γ_t as well as for individual fixed effects α_i (through estimating the equation in first differences) and time-varying individual characteristics X_{it}.[24] Finally, ε_{it} is the error term. In the estimations, we cluster the standard errors by municipality.

Let us next turn to describe our identification strategy. Given the nature of the rules that determined whether an individual will receive a prefilled income tax return or not, and the fact that these rules varied both across individuals according to their place of residence, as well as over time, we are able to identify the causal effect of the method of income tax filing on taxpayers' reporting behaviour. We will essentially use a difference-in-difference methodology, where information on the selection rules is used to form an appropriate control group for treated individuals. As was explained above, individuals were selected into the experiment according to e.g. their income histories and other characteristics. We form the control group by requiring that individuals in the control group have these same characteristics: The sole reason why the control individuals were not treated was that they lived in a different municipality that applied some other selection model.[25] It is also noteworthy that the selection criteria were not known to taxpayers at the time, and therefore self-selection into the experiment is not an issue.

We feel that the reform provides an unusually attractive set-up: Selection of individuals into the experiment was based on observables, and since we have information on those observables, we can select the control group by those same criteria. As was

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[24]We control for education (six categories), number of children (a dummy for each number), marital status (four categories) and province of residence (20 provinces) in all regressions.

[25]More details of how we have constructed the control group are given in the Appendix.

explained in Section 3, the selection models were highly complex and in many cases selection depended on previous year values of dozens of variables. We have information on the vast majority of those variables, but we are for the moment missing a couple of them.[26] Nevertheless, with the information that we currently have, we can match actual treatment with fairly high accuracy: 90,2% of individuals whom we predict to have received a prefilled return, actually did so.

The identifying assumption is that the year-to-year changes in the outcome variable for the treatment and control groups would have been similar in the absence of the treatment. As usual in a difference-in-difference set-up, we can provide support for this assumption through examining the pre-treatment changes (and associated pseudo-tests) in the outcome variable in the two groups. Further, we also exploit the panel nature of the data and control for individual fixed effects. Hence the effects that we estimate are identified from within-person variation. The effects that we identify with this procedure are average treatment effects on the treated.[27]

5   Results

Income items   We first turn to results regarding positive income items on the tax return. We discuss results for prefilled and non-prefilled items separately.

First, let us examine effects on reported income from primary and secondary jobs. These income items were prefilled for those who received the prepopulated tax return. For these items, we do not expect to find large effects on reporting, as they were subject to third party reporting in both systems. Further, these items were also subject to income tax withholding, and hence taxpayers must have been well aware that their income from these sources is known to the tax authority, regardless of whether this information was prefilled or not.[28] There is nothing to gain from understating one's income on these items, nor can one plausibly forget about it.

Column (1) of Table 2 reports the effect on the fraction of individuals reporting any income from primary and secondary jobs, whereas column (2) reports the effect

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[26]Even though we have access to the contents of the entire tax register, some variables were not saved in that register. We are currently in the process of determining whether this information can be obtained from other registers.

[27]Especially in the first year of the experiment, the participants were a highly selective group (e.g. pensioners, low income individuals). To arrive at population average treatment effects (in future work), we will need to use a weighting procedure.

[28]E.g. Slemrod and Gillitzer (2014, 43) note that reports by the U.S. Department of the Treasury indicate that evasion rates on items subject to both third-party reporting and witholding are very low, 1%.

on the average amounts reported (in the Finnish currency Markka, FIM). Indeed we find no effects on these items. The distributions of earned income from primary and secondary jobs for the treatment and the control group are presented in Figure 1.[29] The figure shows that the distributions were very similar in the treatment and control group, both before and after the reform.

[Figure 1 about here.]

Second, we look at effects on other earned income and capital income. These items were not prefilled in the income tax return even for those individuals who received a prepopulated return. The results are reported in columns (3) and (4) of Table 2. We find that receiving a partially prefilled income tax return caused a small but significant drop - amounting to 1,3 %-points - in the fraction of individuals with any income on these items. While the effect is not large, it is non-negligible in relative terms: only 9,4 % of individuals in the treatment group had some income in these categories prior to the reform, so the reform reduced the fraction of people with income on these items by approximately 1/7.

However, in monetary terms the effect on non-prefilled income items (column (4) of Table 2) was not significant. The main reason is that it was those individuals with only small amounts of income on these items that stopped reporting. This can be seen in Figure 2, where we present the histograms of the amounts of non-prefilled income for individuals in the treatment and control groups, both before and after the reform. The number of individuals with income on these items appears somewhat higher in the control group before the reform. After the reform, the number of individuals with small (but positive) amounts of income in these categories declined in the treatment group, whereas it increased in the control group. Formal analyses confirm the visual observation that the reform caused a significant reduction in frequencies at the low end of the distribution, but that there was no such reduction in frequencies for higher levels of income. We estimated models in which the dependent variable was a dummy indicating reported income on the relevant items larger than some amount K. The p-value of the treatment effect falls below 0.05 as K is increased to approximately FIM 1000.

[Figure 2 about here.]

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[29]In Figures 1-4, we present the entire distribution of the relevant items on the tax return, excluding zeroes and the top 5% (to make changes elsewhere more easily detectable).

Table 2. Effect of receiving a prefilled tax return on reporting behaviour: income items.

	(1)	(2)	(3)	(4)
	prefilled income	prefilled income	Non-prefilled income	Non-prefilled income
	(dummy)	(FIM)	(dummy)	(FIM)
prefilled return	0.0018	304.8	. -0.0134**	-19.31
	(0.0017)	(317.5)	(0.0043)	(103.32)
relative effect			-14 %	
N	375,998	375,998	373,655	373,655

Robust and clustered (municipality) standard errors in parentheses.

All specifications include individual controls and fixed effects.

** p<0.01, * p<0.05

Deductions Let us next turn to deduction claims, focussing first on those deduction items that were prefilled for those who received the prepopulated return. At the start of the policy experiment, none of the deductions were prefilled. However, in 1997, prefilling was introduced for the mortgage interest deduction. In columns (1) and (2) of Table 3, we report the effect of receiving a prefilled income tax return (for the first time) in 1997, on mortgage interest deduction claims. In column (1), we find an approximately 5%-point increase in the proportion of individuals claiming for the mortgage interest deduction. This increase amounts to almost 1/4 more claims relative to the previous year, when 21,5 % of individuals in the treatment group claimed for the mortgage interest deduction. The setting here is similar to Gillitzer and Skov (2013), who analyse the effect of the prefilling of the charitable tax deduction in Denmark, and find a large positive effect on the number of claims.

However, there was no effect on the average magnitude of claims (column (2) of Table 3). Again, this is explained by the finding that the new claims were small in magnitude. The histograms of mortgage interest deduction claims for the treatment and control groups, before and after the reform, are shown in Figure 3. Mortgage interest deduction claims increased in both groups in 1997, but more so in the treatment group, especially for low claims. Again, using a similar methodology as above, we find that differences in the changes in claims between the treatment and the control group are significant only at the low end of the distribution, for claims less than approximately FIM 6000.

[Figure 3 about here.]

Finally, let us turn to results regarding claims for non-prefilled deduction items. In column (3) of Table 3, the dependent variable is a dummy for whether the individual claimed for any of such deductions.[30] The results indicate that receiving a prefilled return reduced the fraction of individuals claiming for non-prefilled deductions by 12.6 %-points. This is a fairly large effect: in the year preceding their inclusion into the experiment, approximately 44 % of the individuals in our treatment group filed for these deductions. Receiving a prefilled return therefore reduced the number of individuals claiming for deductions by over 1/4 relative to the previous year.

Column (4) of Table 3 shows the effect on the amount of non-prefilled deductions that individuals filed for on average. The dependent variable is now the total sum of money associated with all relevant items on the tax return. The amount of deductions filed for decreased on average by about FIM 470, which corresponds to about € 138 in 2013 (deflated by the average earnings index). This reduction reflects the fact that the fraction of zeros increased, as we showed above.

Table 3. Effect of receiving a prefilled tax return on reporting behaviour: deductions.

	(1)	(2)	(3)	(4)
	prefilled deductions (dummy)	prefilled deductions (FIM)	Non-prefilled deductions (dummy)	Non-prefilled deductions (FIM)
prefilled return	0.0513**	-27.87	-0.1255**	-469.10**
	(0.0098)	(98.41)	(0.0088)	(129.76)
relative effect	-24 %		-29 %	-13 %
N	375,998	375,998	375,998	375,998

Robust and clustered (municipality) standard errors in parentheses.

All specifications include individual controls and fixed effects.

** $p < 0.01$, * $p < 0.05$

It is interesting to note, however, that the average amount of deductions (conditional on claiming a positive amount) increased by an extra FIM 1,292 in the treatment group compared to the recipients of the conventional return. (Note that this figure incorporates both the change in the composition of the group claiming for deductions, and changes in the claimed amounts.) In Figure 4, we take a closer look at the changes in the distribution of non-prefilled deduction claims. In the treatment group there was

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[30]That is, this dummy equals 0 if *all* items associated with non-pre-filled deductions in the individual's tax return were zero; and 1 otherwise.

a significant drop in frequencies at low deductions. In the control group, on the other hand, there was a much smaller or no drop. Again, formal analyses confirm the visual observation that the reform caused a significant drop in frequencies at low deductions, but that there was no such drop in frequencies at high deductions: The estimated treatment effect becomes insignificant for deductions claims above FIM 7,000.

[Figure 4 about here.]

Wealth   At the time of the reform, wealth was taxable in Finland, if the amount of wealth exceeded FIM 1 100 000 (this was roughly equivalent to 254 000 € in 2014). Wealth taxation was based solely on self-reports, and it was therefore another non-prefilled item on the tax return. We find that receiving a partially prefilled income tax return caused a highly statistically significant decline in both the number of individuals reporting a positive amount of wealth, as well as in the average amount of wealth reported. The results are reported in Table 4. The result is analogous to the finding that there was a reduction in non-prefilled income. Figure 5 shows that the effect comes from a very large reduction in reporting at the low end of the wealth distribution.

Table 4. Effect of receiving a prefilled tax return
on reporting behaviour: wealth.

	(1)	(2)
	wealth	wealth
	(dummy)	(FIM)
prefilled return	-0.0341**	-3285.25**
	(0.0025)	(832.14)
relative effect	-6,6 %	-4,3 %
N	375,998	375,998

Robust and clustered (municipality) standard errors in parentheses.

All specifications include individual controls and fixed effects.

** p<0.01, * p<0.05

[Figure 5 about here]

Total taxable income and taxes paid   Finally, we have estimated the effect of receiving a prefilled tax return on individuals' total taxable income and taxes paid. Figures 6 and 7 show the distributions of total taxable income and taxes paid in the treatment and control group before and after the reform. Regression results are reported in Table 5. The results indicate the introduction of prefilled tax returns had no effect on these aggregate measures.[31]

Table 5. Effect of receiving a prefilled tax return
on reporting behaviour: aggregates.

	(1)	(2)
	total taxable income	total taxes paid
	(FIM)	(FIM)
prefilled return	323.57	43.44
	(284.53)	(170.57)
N	375,998	375,998

Robust and clustered (municipality) standard errors in parentheses.

All specifications include individual controls and fixed effects.

** $p<0.01$, * $p<0.05$

[Figures 6 and 7 about here.]

These results are significant for an overall evaluation of the reform. To the extent that taxable income is a sufficient statistic for welfare, the results would imply that prefilled tax returns appear to be a good idea: the reform implied no adverse effects on taxable income, but at the same time it resulted in a more efficient use of information available to the tax authority and therefore reduced the costs of tax filing and administration.

This conclusion could potentially be qualified if the reform had adverse effects on the income distribution. It is therefore also important to consider whether the effects of the reform differed across income groups? Did the reform imply significant losses for any income group? While we have not yet analysed the effects on the income distribution in detail, preliminary analyses suggest that total taxable income or total taxes paid did not change significantly in any income group.

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[31]It should be noted that that the effects reported in all the tables are average treatment effects on the treated. To obtain population effects - such as effects on total tax revenue - one needs to obtain weighted estimates (in progress).

Placebo tests Finally, to provide support for our identifying assumption, we have also examined a pseudo-treatment corresponding to each of the specifications in Tables 2 to 5, where we pretend that the individuals in the treatment group received a prefilled return one year earlier than they actually did. The estimated effects of the pseudo-treatment are reported in Table A2 in Appendix 2. The estimated effects are small and far from being statistically significant in all of the 12 cases. These tests provide very convincing support for our set-up.

6   Discussion

Let us next turn back to the theoretical arguments on why the details of the tax filing system might affect reporting behaviour, and to a discussion of which of the explanations outlined in Section 2 are most likely to be the driving forces behind our results.

Let us first turn to the third possible explanation, namely changes in the opportunities for tax evasion. In Section 2, we outlined two possibilities as to how receiving a prepopulated return might affect the perceived probability of detection. It is immediately clear that if the perceived probabilities were as in (8), a story relating to the possibilities for tax evasion would not be consistent with our findings: in this case we should observe an increase in evasion on non-prefilled items. However, our most prominent finding is the large reduction in claiming for non-prefilled deductions, which would be consistent with less, not more evasion.

On the other hand, if perceived probabilities of detection were as in (7), a story related to the opportunities for tax evasion would in principle be consistent with a reduction in non-prefilled deductions. However, for this type of a story, it would be a puzzle why we see a disappearance of small deductions, as an auditor's attention would most likely be drawn to exceptionally large deduction claims. Further, if (7) holds, receiving a prefilled return should cause a reduction in evasion across the board. We observed both increases in reported net income (the large decline in non-prefilled deductions) on some items, as well as reductions in reported net income on other items (a reduction in non-prefilled income and an increase in prefilled deductions). Therefore, while we of course cannot rule out the possibility that tax evasion opportunities would have been significant in determining the responses of some individuals, the above findings suggest that arguments related to tax evasion opportunities clearly cannot be the whole story.

Turning next to explanations related to complexity and the costs of reporting, all

our results are in line with this story. Broadly speaking, we find an increase in the tendency to report whatever is prefilled on the tax return, and a reduction in the tendency to report those items that are not prefilled. The large reduction in claiming non-prefilled deductions is consistent with individuals having a fixed complexity cost of tax filing: Many individuals who received a prefilled return decided not to file, as by doing so they could *choose* to avoid the fixed complexity cost of filing. Avoiding the fixed cost was not an option for individuals who were required to file a complete return. Further, an important feature of our results is that the effects that we observe occur predominantly through effects on the number of individuals claiming for small deductions: for those individuals, the costs of filing exceeded the (expected) benefits. A similar reasoning would explain the reduction in the number of individuals reporting any non-prefilled income: it may be, for instance, that individuals are willing to tolerate the risk of getting caught with tax evasion (i.e. leaving small amounts of income unreported) if they can avoid the fixed cost of filing a return.

The fact that there was an increase in the number of individuals claiming for deductions that were actually prefilled is also compatible with a story related to costs of reporting. (Note that the relevant cost here, however, is a variable cost of filing for any single item, rather than a fixed cost of filing in general.)

A part of the explanation for our findings may also be related to salience effects. This is in particular the case regarding non-prefilled items, where the fraction of zeros increased for both positive income items and deductions. However, the fact that we see responses predominantly at the low end of the distribution (of deductions and non-prefilled income) points more towards individuals conducting a rational calculus between benefits and costs of reporting, rather than towards purely accidental omissions in reporting.

A final point to discuss is to what extent our results are likely to extend to countries implementing prefilled tax returns today. First, the details of filing systems may differ across countries and over time. For example, during the time when the reform was implemented in Finland, paper filing was used, whereas electronic filing is now becoming increasingly common. Are the complexity costs of filing taxes likely to be very different today? To the extent that complexity costs arise from the need to study information materials and from having to learn the details of the tax system, we conjecture that those costs have not radically declined over time. Second, the generalisability of our results may to some extent depend on the similarity of some features of tax systems (e.g. deductions) across countries. The Finnish tax system is certainly very similar to tax systems in other Scandinavian countries, but it is also interesting to note for

example that the Finnish and the U.S. tax systems are surprisingly similar in this respect.

Finally, regardless of the details of particular tax systems, our analysis implies that the information available to taxpayers is likely to have important effects for the success of a reform introducing prefilled tax returns: tax authorities should take care in ensuring that taxpayers have adequate information on their obligations to report certain non-prefilled items, as well as their eligibility for discretionary deductions. To the extent that omissions in reporting non-prefilled items were due to complexity costs, the results also point towards the adverse effects of tax complexity on compliance.

7 Conclusions

Prefilled income tax returns are used in many countries, and their use has been planned and/or experimented with in others. Yet no systematic evidence on their effects exist. It is clear that prefilled returns reduce administrative costs for the tax authority, as well as compliance costs for taxpayers, but their effects on reporting behaviour have been unexplored to date.

We utilised a Finnish policy experiment to study the effects of prefilled income tax returns on reporting behaviour. We find that receiving a partially prefilled income tax return increased taxpayers' tendency to report deduction items that are prefilled on the tax return, and reduced the tendency to report both income and deduction items that are not prefilled. We discussed three possible explanations for our findings - changes in tax evasion opportunities, salience effects, and compliance costs, which we argue mainly result from the complexity of tax filing and the tax system in general. We argued that while some of our findings are consistent with the first two explanations, the overall pattern of responses is best explained by complexity costs.

Our results, in particular those related to the large effects on deduction claims, provide interesting information on taxpayers' behaviour, as they indicate that relatively minor changes in the tax system may have fairly large effects on (reporting) behaviour. The results therefore point towards the need for a better understanding of various design features of tax systems that have until recently received relatively little attention in the literature (see e.g. Slemrod and Gillitzer 2014).

We also find that receiving a prefilled tax return had no effect on individuals' total taxable income and taxes paid. These results are significant for an overall evaluation of the reform. To the extent that taxable income is a sufficient statistic for welfare, the results coupled with the savings in administrative costs associated with prefilled tax

returns, point towards the conclusion that prefilled tax returns appear to be a good idea. However, it has to be borne in mind that there may be large effects on reporting some particular items, and even though the effects on various types of income and deduction items cancel each other out in our particular case, this may not hold in general. The precise way in which a reform introducing prefilled tax forms is implemented may therefore be important for the success of the policy, and our findings also provide useful information for tax authorities planning to implement prefilled tax returns. For example, tax authorities should take care in ensuring that taxpayers have adequate information on their obligations to report certain non-prefilled items, as well as their eligibility for discretionary deductions. The results also point towards the adverse effects of tax complexity on compliance.

Appendix 1. Selecting the control group

We form the control group as follows: For each treated individual, we find a control individual by applying exactly the same criteria that caused the treated individual to be included in the experiment, but who was not treated because she lived in another municipality that either did not participate in the experiment, or used a different selection model. In selecting the control group for individuals living in, say, municipality A, from individuals living in another municipality B, we take into account the selection models used in both municipalities. The examples below clarify the procedure.

For example, assume that an individual living in municipality A, using selection model 1 (e.g. selecting pensioners only), was selected into the experiment. Consider another individual living in municipality B, who also fulfils the criteria of model 1 (i.e. is a pensioner), but was not selected into the experiment because his municipality did not participate. This individual in municipality B can now serve as a control for the treated individual in municipality A.

A more general case is the one where municipality B also participated in the experiment but used a different set of selection criteria than municipality A. In this case, when selecting a control from municipality B, we also utilise the selection criteria that were applied in municipality B: e.g. if municipality B used a model that selected low-income individuals only, we use untreated individuals in municipality B as potential controls only for high-income individuals in other municipalities.

Further, in later years of the experiment (1996->), we also require that the control individuals not only satisfy the same selection (or exclusion) criteria as the treated individual for that year, but also for the previous years of the experiment. For example, assume that an individual lives in a municipality where only pensioners were treated in 1995, and we observe that the individual is treated for the first time in 1996. We then use the selection criteria for 1996 to find a control for this individual, as explained above, but further restrict the pool of eligible controls to individuals who were non-pensioners in 1995.

Note that there are in general many individuals who could serve as a control for any treated individual (i.e. who fulfil the selection criteria in the way defined above). We select one control individual for each treated individual randomly from this pool of eligible controls. Using all eligible controls would lead to an unbalanced sample, in the sense that there are more eligible controls for some treated individuals than for others. Hence using a sample where all eligible controls are included might lead to a situation where the fractions of, say, employed and unemployed individuals differ between the

treatment and the control group, and therefore to biased results.

Appendix 2. Additional tables

Table A1. Descriptive statistics.

	Treatment group		Control group	
	mean	sd	mean	sd
Prefilled income	62254	79435	56020	71024
Share	0.6284	0.4832	0.6181	0.4858
Non-prefilled income	1284	21424	1561	25760
Share	0.0956	0.2940	0.1258	0.3316
Prefilled deductions	3403	11155	3256	6389
....Share	0.4740	0.4993	0.4339	0.4956
Non-prefilled deductions	2806	9401	2969	8864
Share	0.3540	0.4782	0.4059	0.4910
Age	44.485	17.991	44.635	18.111
Male	0.4715	0.4991	0.4859	0.4998
Education category 1	0.4112	0.4920	0.4262	0.4945
category 2	0.3690	0.4825	0.3805	0.4855
category 3	0.1210	0.3262	0.1151	0.3191
category 4	0.0448	0.2068	0.0395	0.1948
category 5	0.0496	0.2172	0.0354	0.1849
category 6	0.0041	0.0645	0.0030	0.0547
Number of children	0.4586	0.9116	0.4725	0.9358
Single	0.5600	0.4963	0.5348	0.4987
Married	0.4308	0.4951	0.4572	0.4981
Divorced	0.0059	0.0768	0.0060	0.0774
Widow	0.0032	0.0567	0.0018	0.0427

Notes: Descriptive statistics for the treatment year and the previous year.

Table A2. Effects of pseudo-treatment on income items and deductions.

	(1)	(2)	(3)	(4)
	prefilled income	prefilled income	Non-prefilled income	Non-prefilled income
	(dummy)	(FIM)	(dummy)	(FIM)
prefilled return	0.0021	317.188	. -0.0024	72.11
	(0.0025)	(377.5)	(0.0017)	(106.40)
N	375,998	375,998	373,640	373,640

	(5)	(6)	(7)	(8)
	prefilled deductions	prefilled deductions	Non-prefilled deductions	Non-prefilled deductions
	(dummy)	(FIM)	(dummy)	(FIM)
prefilled return	0.0012	-25.48	-0.0061	-62.46
	(0.0022)	(81.98)	(0.0109)	(47.94)
N	375,998	375,998	375,998	375,998

	(9)	(10)	(11)	(12)
	wealth (dummy)	wealth (FIM)	taxable income (FIM)	total taxes paid (FIM)
prefilled return	0.0007	387.27	373,52	179.87
	(0.0012)	(304.48)	(315,60)	(125.46)
N	375,998	375,998	375,998	375,998

Robust and clustered (municipality) standard errors in parentheses.

All specifications include individual controls and fixed effects.

** $p<0.01$, * $p<0.05$

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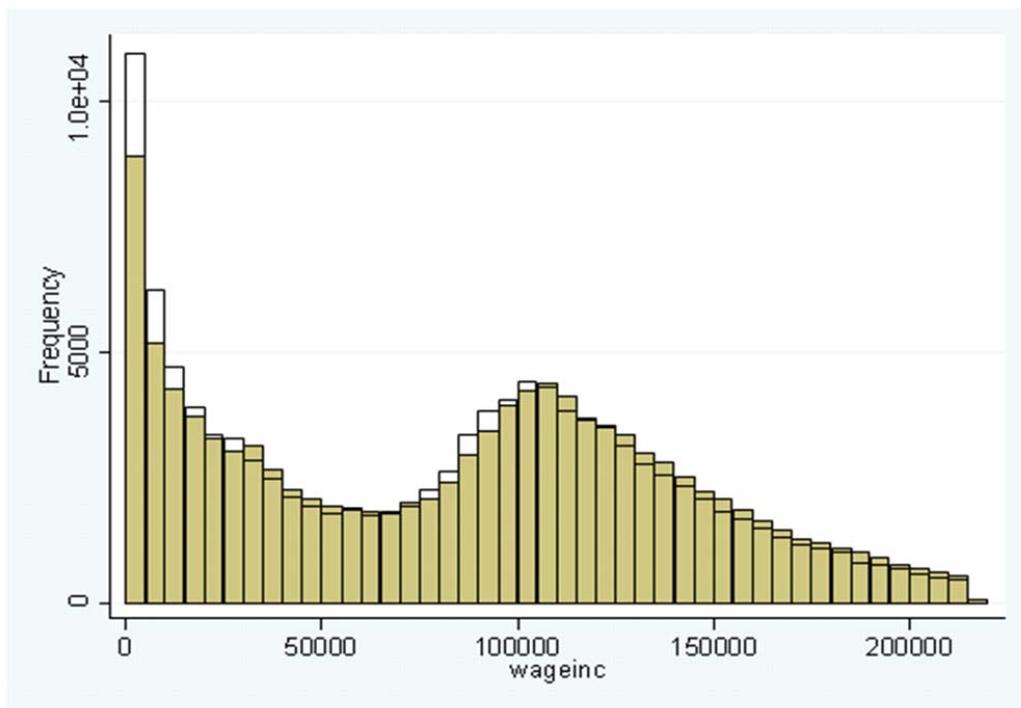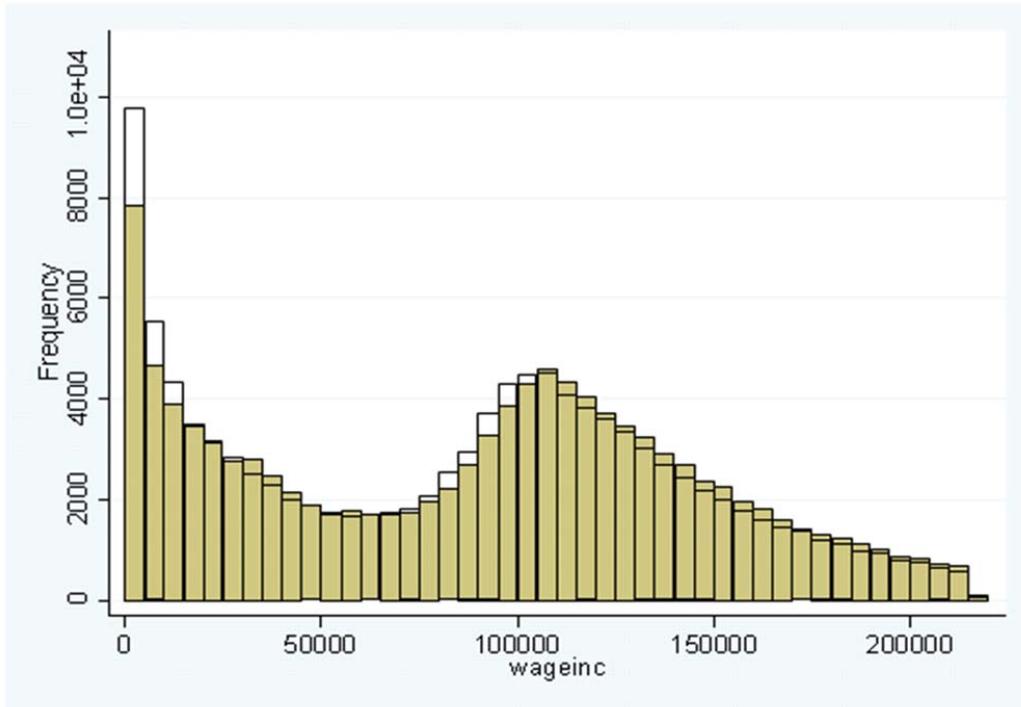
Figure 1. Distribution of earned income from primary and secondary jobs before (white) and after (green) the reform, in treatment group (top panel) and control group (bottom panel).
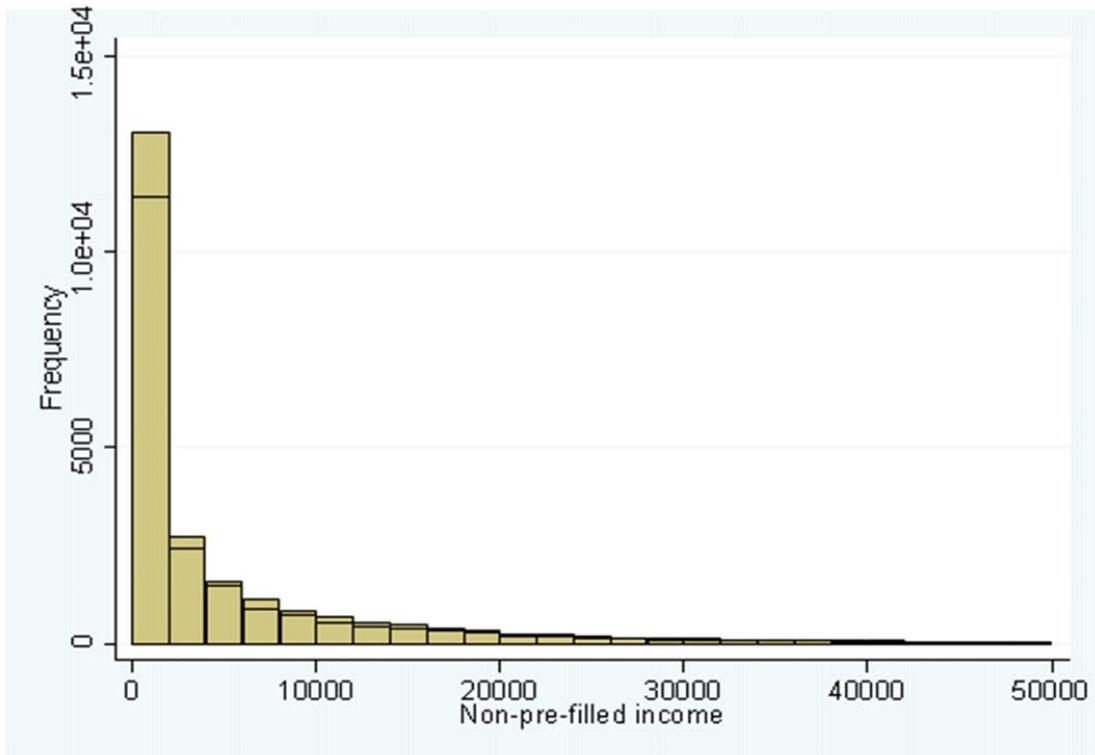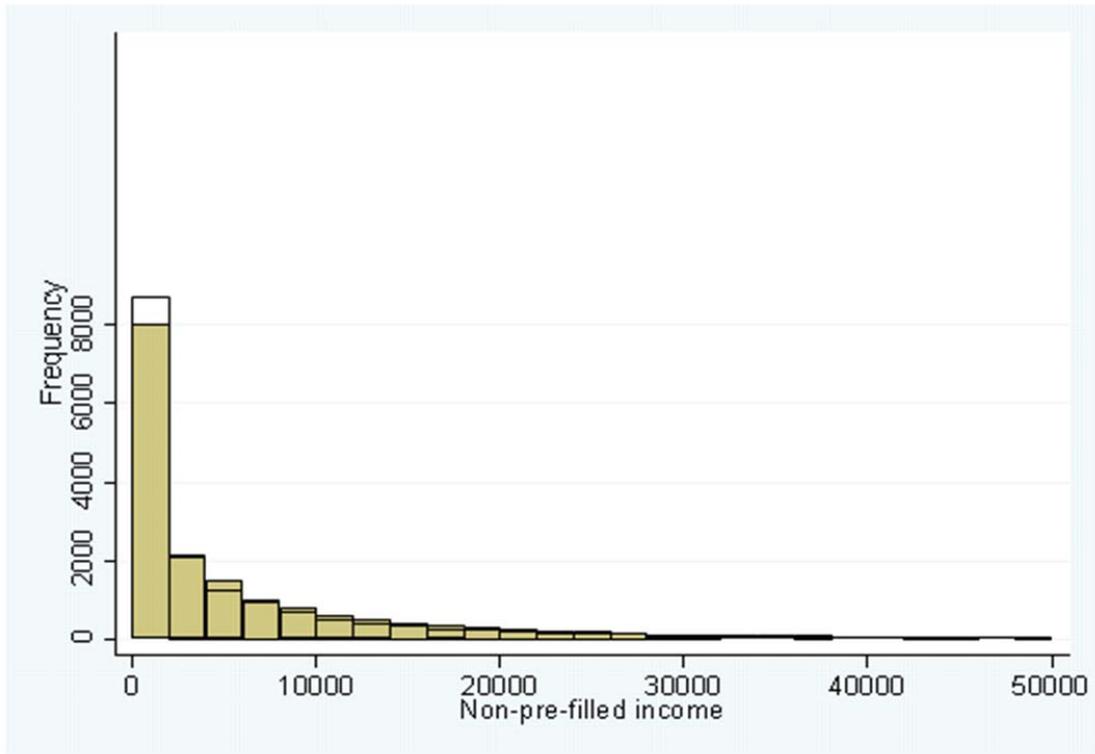
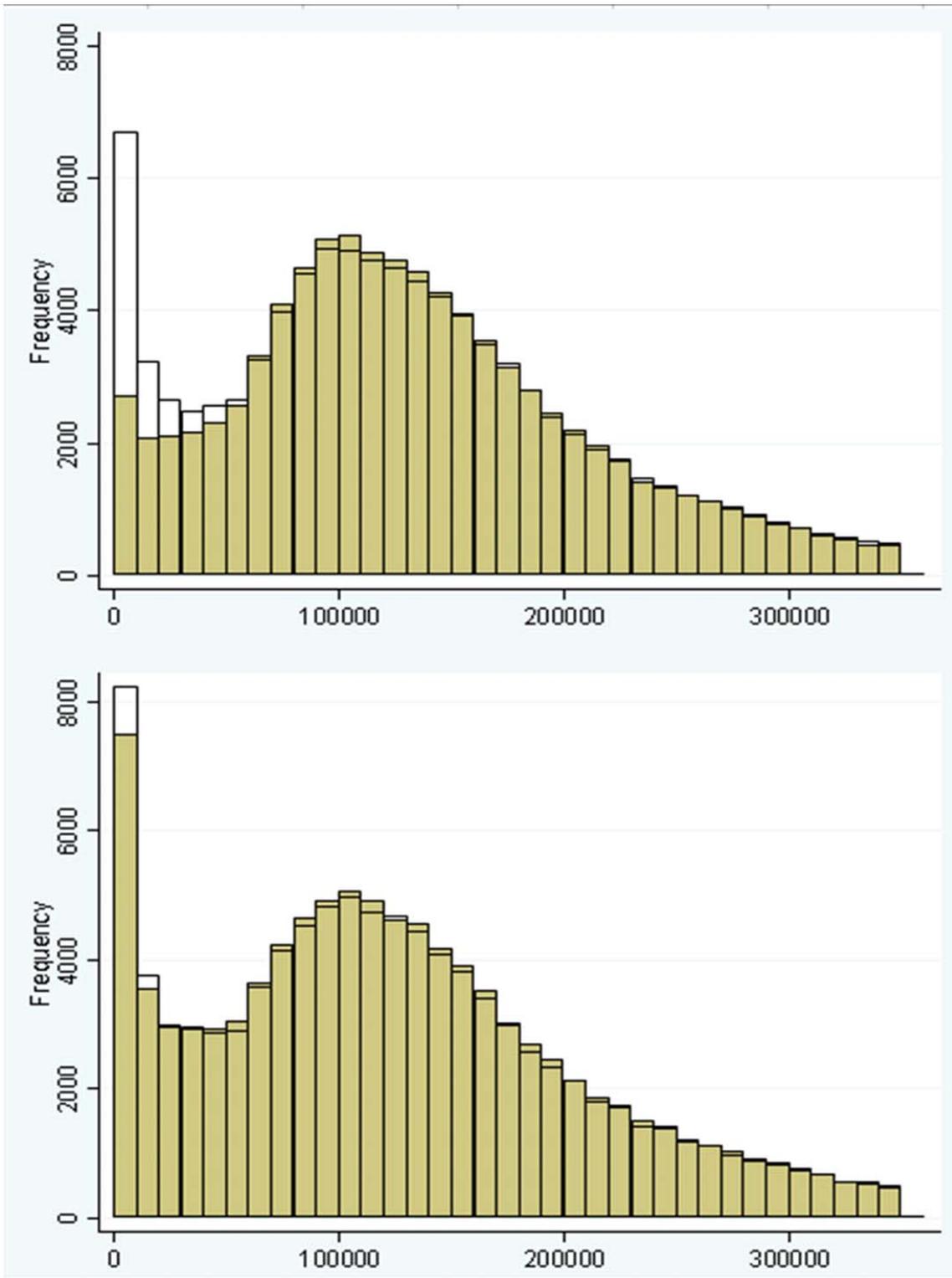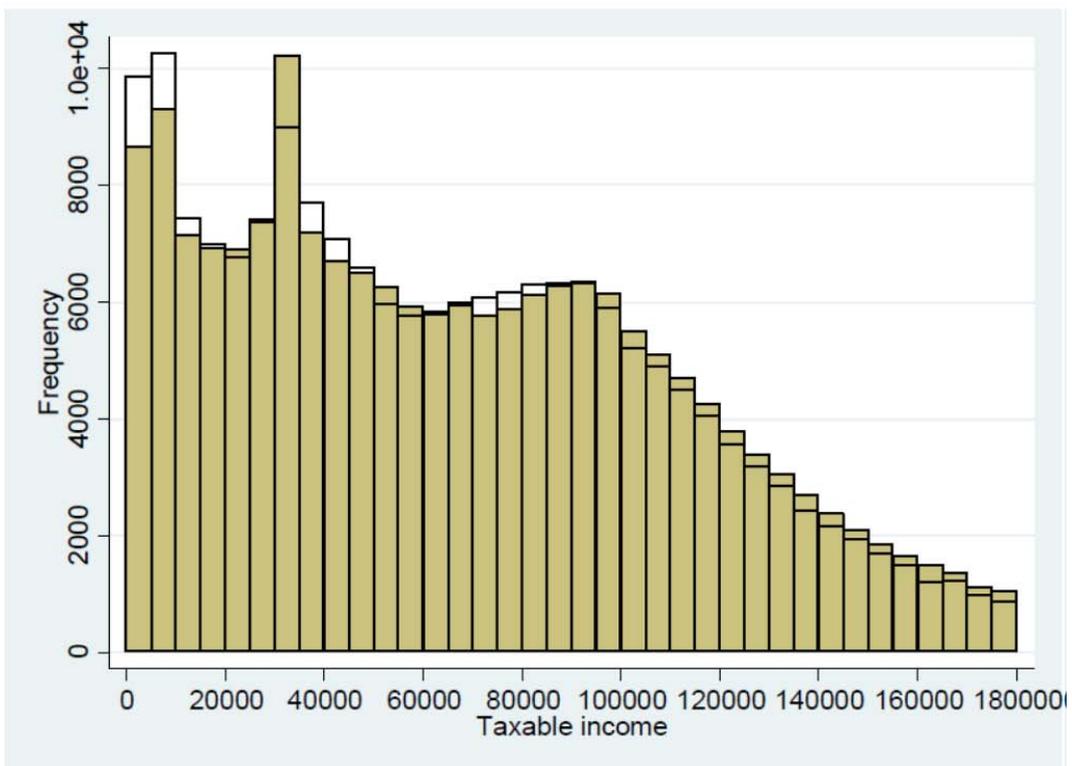**Figure 2. Distribution of other earned income and capital income before (white) and after (green) the reform, in treatment group (top panel) and control group (bottom panel).**
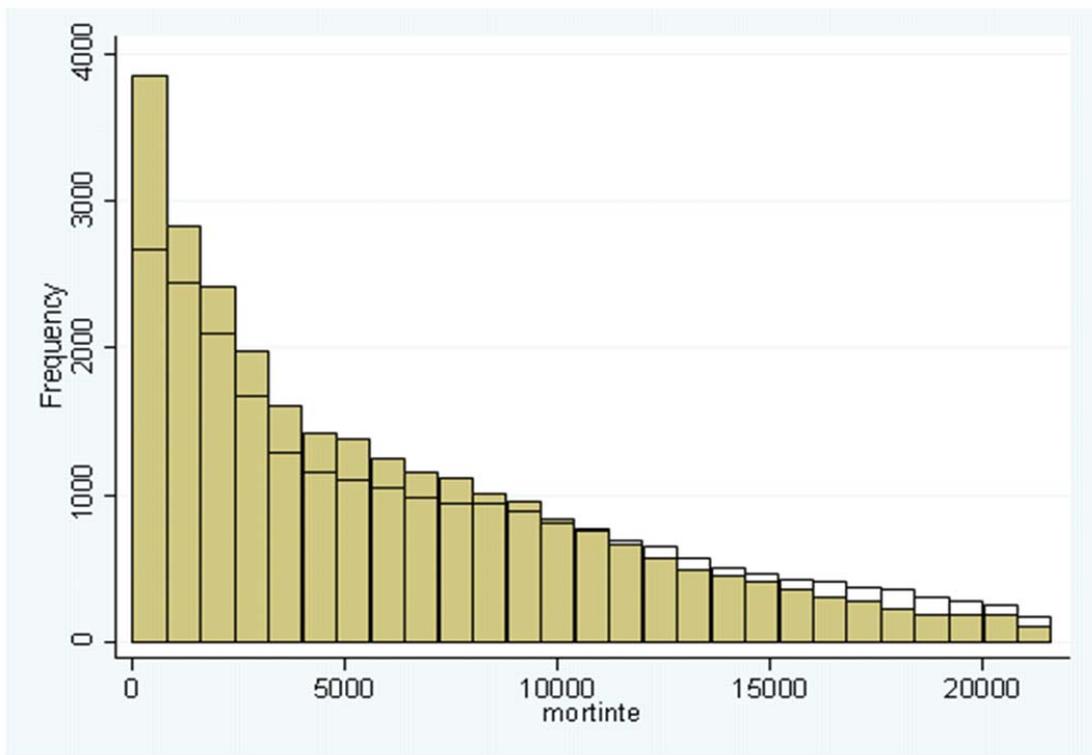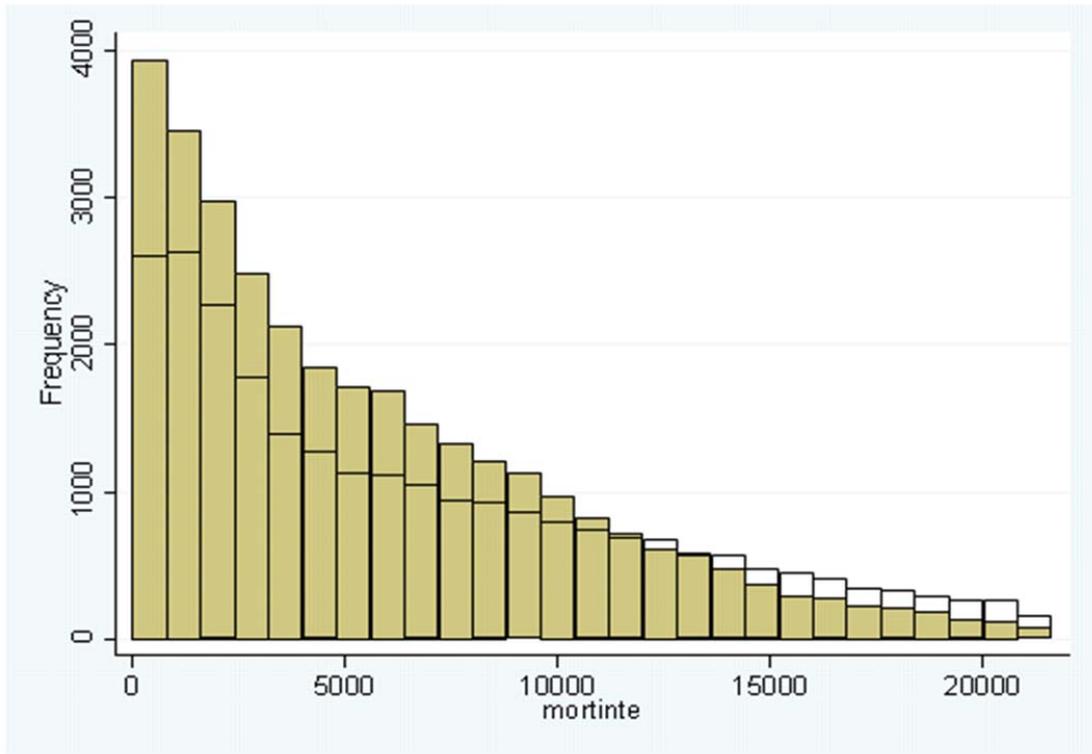
Figure 3. Distribution of mortgage interest deduction before (white) and after (green) the reform, in treatment group (top panel) and control group (bottom panel).
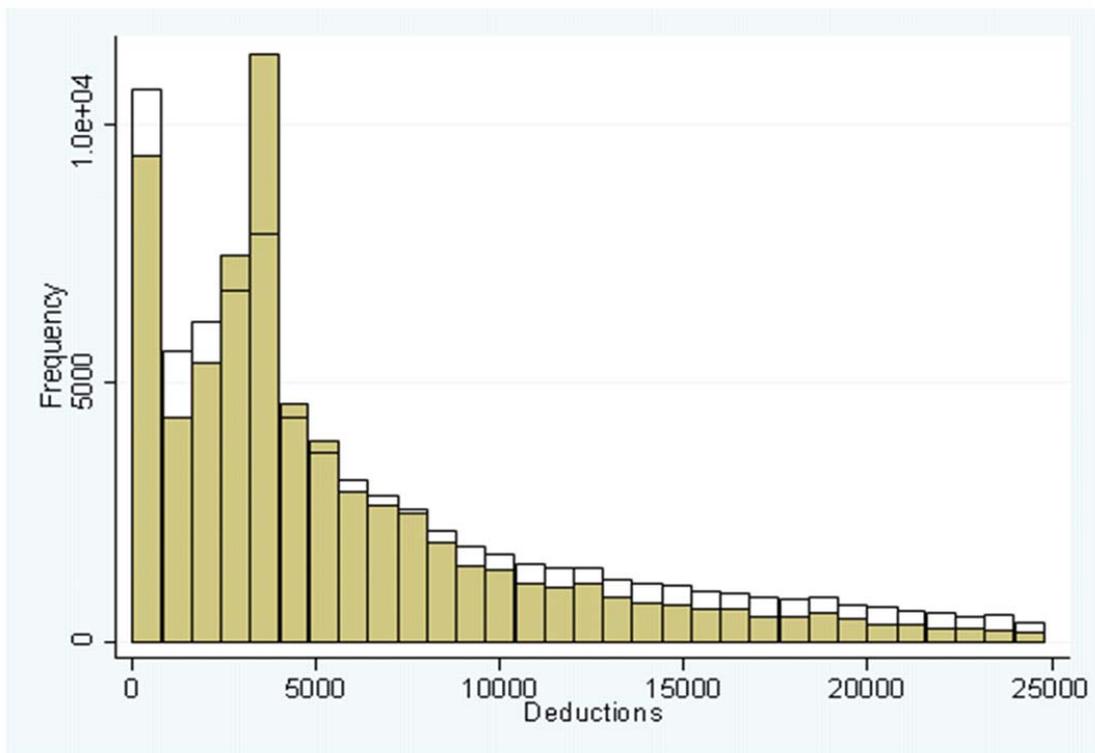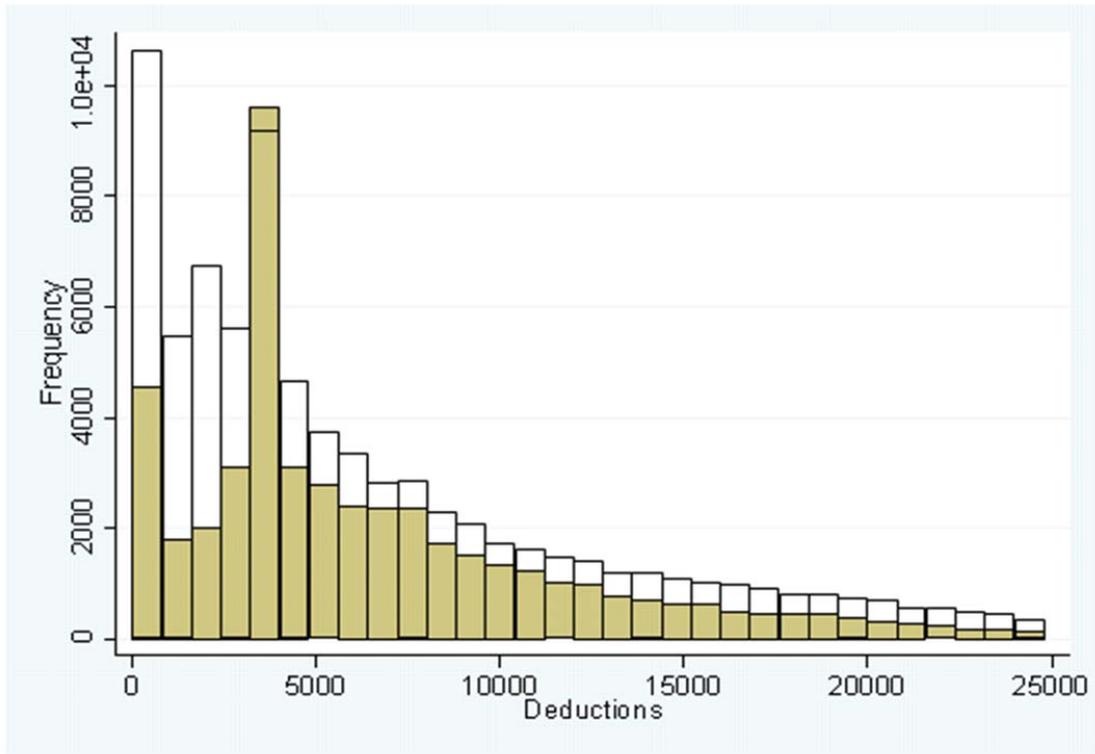
Figure 4. Distribution of discretionary deductions before (white) and after (green) the reform, in treatment group (top panel) and control group (bottom panel).

Figure 5. Distribution of reported wealth before (white) and after (green) the reform, in treatment group (top panel) and control group (bottom panel).
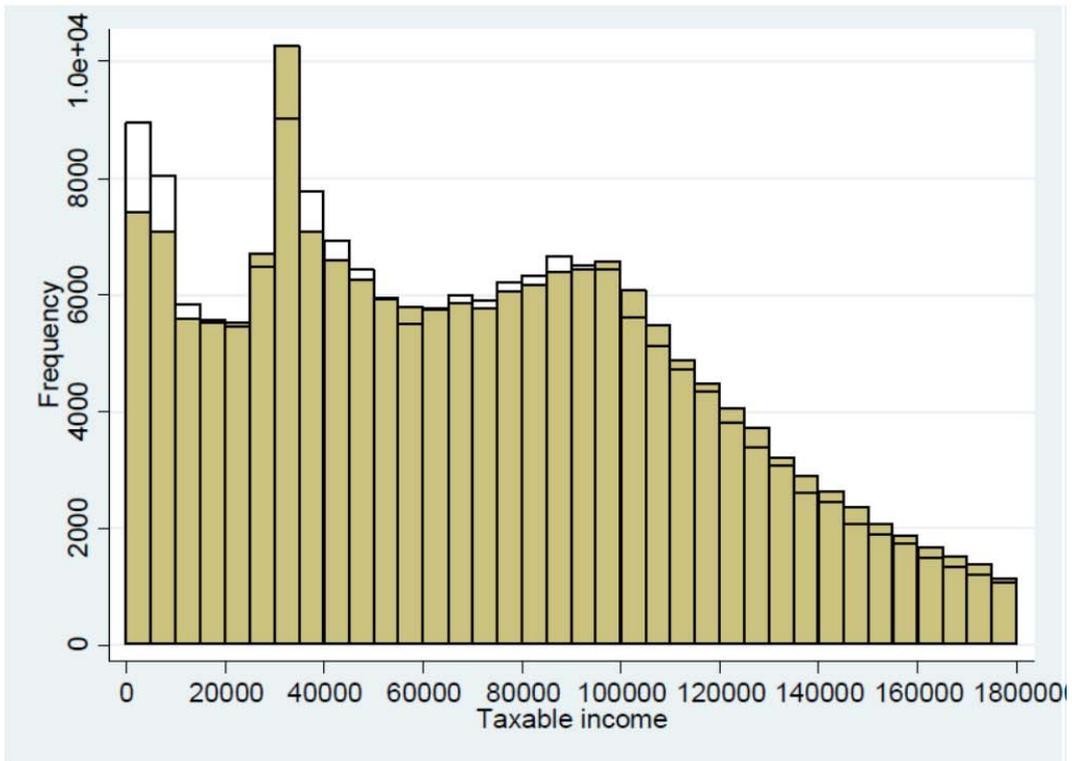
Figure 6. Distribution of taxable income before (white) and after (green) the reform, in treatment group (top panel) and control group (bottom panel).
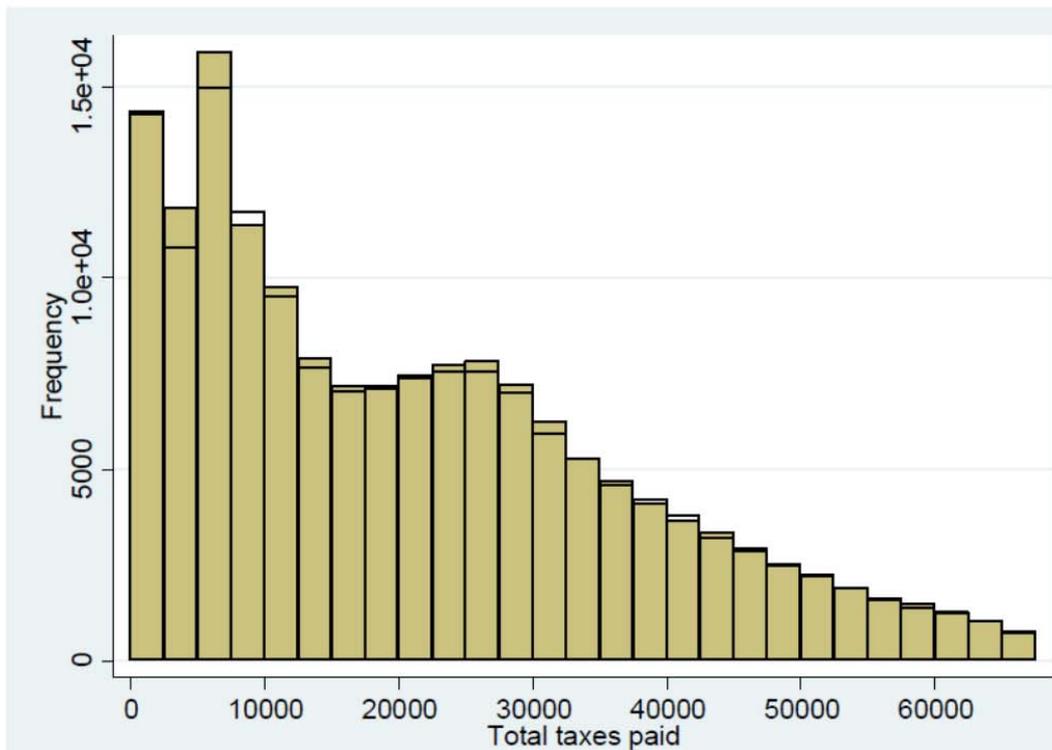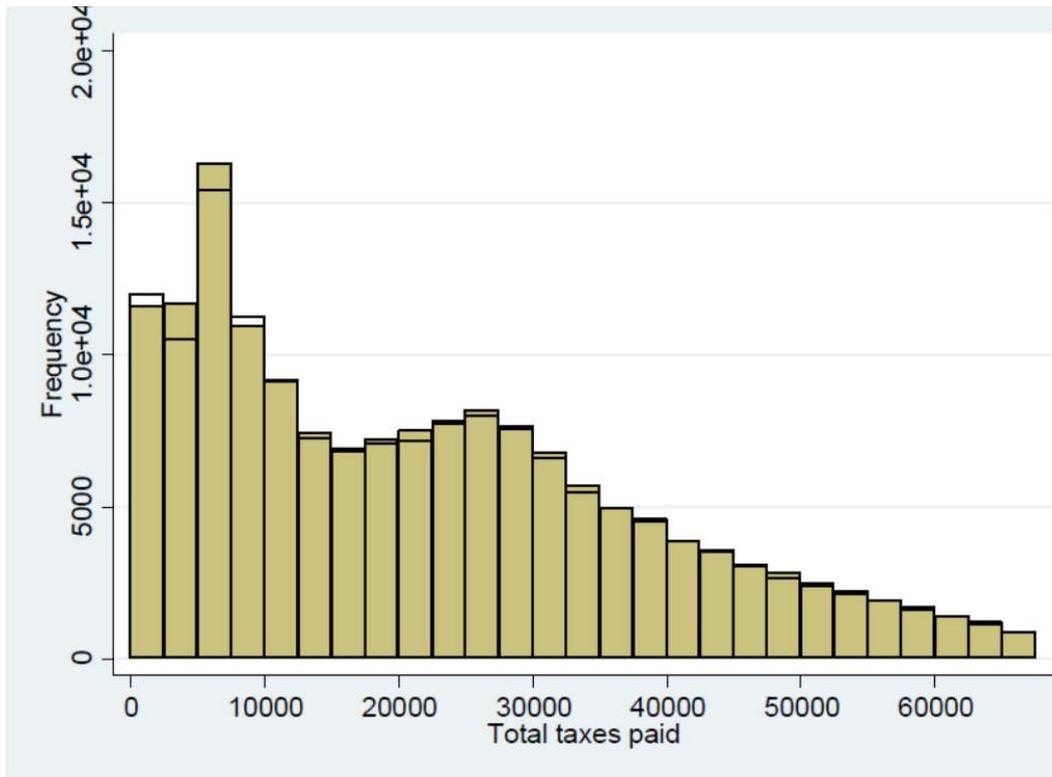
Figure 7. Distribution of total taxes paid before (white) and after (green) the reform, in treatment group (top panel) and control group (bottom panel).