Tax Compliance in the Rental Housing Market: Evidence from a Field Experiment*

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Abstract

We study rental income tax compliance using novel third-party information and a large-scale randomized field experiment. The third-party information combines register data on the ownership and occupancy of apartments. The RCT used this new third-party information in the targeting and design of experimental treatments, and increased the propensity to report rental income and the amount of reported rental income net of expenses. Our research design also allows us to identify members of ownership networks and analyze spillover effects in tax enforcement between them. We find positive reporting spillovers. We do not find evidence of real effects on asset market transactions.

JEL: H26, H31

Keywords: Tax compliance, field experiment, rental market, spillovers, real effects of tax enforcement.

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1 Introduction

Rental income from buy-to-let housing is in most developed countries heavily taxed and a significant source of tax revenue.[1] Housing constitutes a large share of household wealth, and hence tax treatment of housing may have broad efficiency and equity implications. Rental income tax however has features that potentially make it relatively easy to evade for taxpayers and difficult to enforce for tax authorities: Ownership of buy-to-let housing is highly dispersed, with a large number of landlords each renting out a small number of apartments. Also, rental income is typically not subject to third-party reporting or tax withholding, and therefore differs from e.g. wage earnings where third-party reporting has been shown to be crucial for enforcement (Kleven et al., 2011). However, previous literature does not provide a comprehensive analysis of rental income tax non-compliance or enforcement. Moreover, while previous literature has shown that information spillovers can enhance the effectiveness of tax enforcement, there is scope for a unified analysis of enforcement spillovers in different types of networks.[2]

This paper studies rental income tax compliance and enforcement by leveraging a novel source of third-party information combined with a randomized field experiment (RCT).[3] We built the new information source by combining two crucial pieces of information from different administrative registers – information on ownership of properties and on place of residence. Owners of apartments that are occupied by someone else than the owner are identified as suspected landlords, and hence suspected recipients of rental income. In cooperation with Finnish tax authorities, we designed a large-scale RCT, where suspected landlords identified with the new information were randomized into either receiving one of four treatment letters or no letter. We build the RCT design in such a way that it enables an analysis of both spatial and ownership network spillovers of the treatment letters.

The strongest of our four treatment letters informed the recipients of the use of targeted information on the ownership of suspected rental apartments in tax enforcement, thus alerting the recipient to a heightened probability of getting caught of non-compliance. The other letters provided weaker signals of tax enforcement by informing the recipient about how to report various taxes including rental income, or providing a general notification of intensified enforcement of rental income taxation.

We find that the treatment letters in our RCT affected the reporting behavior of suspected landlords relative to those not receiving a letter. In particular, the strongest

[1]In many OECD countries, rental property is the most heavily taxed type of asset (OECD, 2018).

[2]We review the relevant literature at the end of the introduction.

[3]We use the term "third-party information" in this paper in a broad sense, to refer to information available to the tax authority from other sources than from direct reports by the taxpayers themselves.

treatment informing suspected landlords of the usage of the new type of third-party information in tax enforcement caused a marked increase in the propensity to report rental income. This result highlights that our new information accurately identified suspected landlords, even though some of them did not previously report any rental income. For example, the strongest treatment letter increased the propensity to report rental income among those who did not file such a report in the previous year by more than 30%. We also find treatment effects on the euro-amount of income reported net of expenses.

These findings are important for tax enforcement: If no third-party information was used, the group of landlords who never report any rental income would likely largely go undetected. Our novel third-party information allowed targeting enforcement measures also at this group of suspected evaders. Further, an enforcement strategy relying on tax audits would be particularly costly in the case of small-scale renting. Alternative methods to tax audits, such as the use of novel information sources in tax enforcement, are particularly attractive in this institutional setting where small-scale activity makes up a significant share of the market.

Our RCT was designed from the outset to allow a rigorous study of various spillover effects. First, to take networks formed by co-owners of apartments into account, we constructed the treatment groups such that at most one individual from each ownership network receives a letter. This allows studying spillover effects of the letters to the other members of the network. Moreover, we utilize a randomized block design, where we randomize dense rental neighborhoods into three treatment blocks with varying treatment intensity. If local spillovers were to exist, they should be most pronounced in the high-intensity block where a larger fraction of suspected landlords received a treatment letter.

We find a positive and significant spillover effect on tax reporting within ownership networks. Those in the ownership network who did not receive a letter, responded to a letter received by their co-owner. The spillover effects are quantitatively important: The total impact of the treatment letters on the number of rental income reports increases by about 14% because of this spillover effect. Co-ownership of rental apartments clearly constitutes a more closely connected network than geographic proximity. Therefore, information spillovers driven by co-ownership can be expected to be stronger than spillovers driven by geographic proximity. Indeed, although we find important spillovers among co-owners, we do not detect any within local rental markets even in cases where almost two-thirds of the landlords operating in the local market were treated.

Finally, we provide one of the first analyses of potential real effects of tax enforcement, through analyzing the effects of our experimental treatments on housing market transactions, and on the ownership of other assets. However, we do not find large nor statistically significant effects on these outcomes.

We contribute to filling several gaps in the literature on tax compliance and enforcement. First, we are the first to conduct a comprehensive analysis of rental income tax enforcement.[4] In most OECD countries rental income is heavily taxed, but not subject to third-party reporting. However, based on earlier literature, we have little understanding on the extent of tax non-compliance from rental income, and how to create effective tax enforcement strategies to tackle non-compliance.

Second, we contribute to the broader literature on tax compliance by creating a new information source that provides a signal of economic activity and can be used in tax enforcement as a substitute to direct third-party income reports when those are not readily available, and by designing an RCT to study the effectiveness of that information in tax enforcement. As the new information was created by combining different administrative registers, it is relatively low-cost to build and of good quality, and a similar procedure could be utilized in other contexts. Our study complements RCTs that have used third-party information based on register data in very different settings: Harju et al. (2020) utilized such third-party information on evading taxes from imports of used cars, and Bott et al. (2020) on foreign income of suspected tax evaders.[5] Other previous studies have used naturally occurring variation in third-party reporting.[6]

More specifically, our study is related to the literature on tax compliance nudges (for a meta-analysis, see Antinyan and Asatryan, 2024), but goes beyond nudging in various ways. The entire experiment – the targeting of the information letters, as well as their content, and also the implementation of the interventions – hinges on the newly-built information source. Typically, only one of these elements, i.e. information provision, would be involved in a pure nudge experiment. Further, our treatment is not a pure nudge in the sense that in making this new information source available for tax enforcement, it changes economic incentives through increasing the probability that tax evasion is detected. A pure nudge might change the salience of this information to the recipients.

[4] More specific questions have been addressed in the literature. Castro et al. (2022) conduct an information experiment to study rental income reporting, but focus on individuals who were likely tax evaders. Wenzel and Taylor (2004) examine the effects of a requirement to itemize deductions from rental income and López-Laborda et al. (2023) analyze the effects of a warning system to deter tax evasion from vacation rental income.

[5] Kotakorpi ⓡ al. (2024) provide complementary evidence on how randomized third-party information affects both tax reporting and pricing decisions in a double-auction lab experiment.

[6] For example, Kleven et al. (2011) study whether self-employment income not subject to third-party reporting exhibits more tax evasion than wage income. Pomeranz (2015) compares VAT declarations involving line-items that are covered by paper trail to line-items that are not. Naritomi (2019) studies third-party information due to a campaign that incentivized consumers to send in their receipts from their purchases to the authorities.

Our experiment does not only do that, but truly extends the information base that can be used in enforcement.

Third, we explicitly design the experiment to be able to detect tax enforcement spillovers in two different types of networks: ownership networks and local rental markets. In the previous literature, geographic spillovers between individuals have been studied in the context of TV license fee collection (Rincke and Traxler, 2011; Drago et al., 2020), income tax filing (Meiselman, 2018), and property tax compliance (Carrillo et al., 2021; Cruces et al., 2024).[7] Spillover effects between firms have been analyzed by Pomeranz (2015), Boning et al. (2020), Lediga et al. (2022), and Bellon et al. (2023) focusing on spillovers between firms connected through VAT chains, same industry, common tax preparer, or geographic proximity.[8] However, we are not aware of previous studies analyzing the tax enforcement spillovers generated by joint ownership of assets or studies analyzing tax enforcement spillovers in different types of networks in a unified setting. In addition, with the exception of Cruces et al. (2024), the potential for spillovers has not previously been taken into account from the outset when designing tax enforcement experiments.

Fourth, research on the effects of tax enforcement on real economic activity is extremely scarce overall. A few papers examine the real responses of firms to evasion opportunities or tax enforcement (Kopczuk et al., 2015; Harju et al., 2025). In a context closer to ours, Bomare and Le Guern Herry (2022) examine how enforcement may affect allocations of off-shore wealth into different types of assets. We provide novel analysis of potential real responses to enforcement by individual taxpayers, by using administrative data on housing market transactions and other aspects of portfolio choice.

2 Institutions and Experimental Design

2.1 Institutional Setting

Rental markets and private rental activity. One third of all Finnish households live in rental housing.[9] More than half of the rental units in the private rental market are owned by households or individuals.[10]

[7] Alstadsaeter et al. (2019), Frimmel et al. (2019), and Paetzold and Winner (2016) analyze tax evasion and avoidance spillovers within the family and workplace but do not focus on the effects of enforcement measures.

[8] The potential for enforcement spillovers to other taxes reported by the same individual or firm has been studied by Brockmeyer et al. (2019), Castro et al. (2022), and Bagchi and Dušek (2021).

[9] See, e.g. http://www.oecd.org/social/affordable-housing-database/housing-market/ for information on the tenure distribution in different countries.

[10] The private rental market constitutes two thirds of the overall rental market. The rest can be characterized as social housing where housing units are owned by municipalities and non-profit organizations

In the private rental market, legislation on rental agreements is very flexible. In this respect the institutional setting in Finland is very similar to countries like the U.K. or the U.S. Rent-setting is not subject to any restrictions, and in the case of long-term rental agreements, the rent is typically reviewed annually. The size of annual rent increases must be specified in the lease agreement and is typically based on the cost-of-living index. Valid reasons for contract termination include unpaid rents, sale of the dwelling, or personal use by the landlord.

The private rental market is dominated by private landlords who typically own only a few dwellings. For the purposes of this study, we identify likely landlords by combining register data on ownership and apartment occupancy in a manner explained in Section 2.2.1. In what follows, we refer to these individuals as suspected landlords.

Table 1 illustrates the phenomenon under study. First, small-scale renting is highly prevalent, as some 85% of all suspected landlords own only one suspected rental apartment. Second, out of those owning one suspected rental apartment, roughly 75% reported some rental income and roughly two thirds of all rental income tax revenue is collected from them in 2016. As the ownership of rental units is widespread across households and small-scale renting makes up a large share of tax revenue, enforcement may be costly for tax authorities. This underlines the importance of looking for ways to steer taxpayers to comply without tax audits.

Table 1: Rental income and tax reporting by suspected landlords.

Number of suspected rental apartments	Share of suspected landlords	Propensity to report rental income	Share of total tax revenue	Mean rental income
1	0.853	0.754	0.665	3035
2	0.106	0.888	0.200	7319
3	0.028	0.917	0.084	11633
4	0.008	0.944	0.028	13850
5	0.003	0.940	0.013	16673
6 or more	0.002	0.964	0.011	23320

Notes: The table shows the propensity to report rental income, share of suspected landlords and total tax revenue, and mean rental income reported in 2016 by the number of suspected rental apartments owned in 2015. The data used includes all individuals in our base population (see Section 2.2.1) in the control block (see Section 2.2.4) (N = 15,913).

Taxation of rental income. The Finnish tax system is a dual system combining

not subject to capital income taxation.

progressive taxation of labor income with a separate tax on all capital income, including net rental income. At the time of the experiment, the tax rate on capital income was 30% up to an annual threshold of 30,000 euros and 34% on capital income exceeding the threshold.

The rental income tax is a non-negligible source of tax revenue. In 2015, total reported rental income net of expenses amounted to 1.6 billion euros. The corresponding tax revenue was more than 480 million euros. This constituted roughly 20% of the revenue from capital income, 6% of central government income tax revenue and 1.1% of all central government tax revenue.[11]

Overall, Finland is a relatively high-compliance country. For example, the VAT gap in Finland has been estimated by the EU commission to be one of the lowest in the EU, though the level is comparable to many other EU countries.[12]

Turning next to the tax-filing procedure, pre-populated income tax returns are sent out to taxpayers each year in late April. The pre-populated return contains information on incomes that are subject to third-party reporting. The taxpayer is required to submit a revised return to the tax authority if any income information is missing from the pre-populated return. The taxpayer can also apply for discretionary deductions (e.g. expenses for travel to work). The taxpayers have to submit their corrections in May; otherwise, the original proposal is implemented.

As income from rental property was not subject to any third-party reporting at the time of our study, all individuals with rental income had to revise the pre-populated tax return and submit the revision to the tax authority. Rental income is reported on a separate form (see Appendix B), and income and deductible expenses have to be reported separately. In the experiment, treated individuals received different types of communication from the Finnish Tax Administration shortly after receiving the pre-populated tax return. These treatments are described in detail in Section 2.2.3.

In the absence of third-party information, tax enforcement needs to rely on self-reported income or on auditing taxpayers whose reporting behavior raises a red flag. Obviously, the red flags indicating non-compliance are difficult to implement based on

[11]OECD (2018) provides a report on the taxation of household savings, showing for example that 34 out of 40 countries covered in the report tax rental income, while two apply a tax on imputed rather than actual rental income. The report provides a detailed comparison of the principles of taxation of rental income (among other assets) between countries, but we are not aware of internationally comparable statistics on rental income tax revenue.

[12]The three countries with the lowest VAT gap in the EU were the Netherlands, Finland, and Spain, according to Commission estimates for 2021. Finland's VAT gap was estimated to be 0.4 %, compared to 5.3 % for EU27 overall. It should be noted however that the VAT gap does not relate solely to tax compliance, but is a broader measure of the effectiveness of tax collection. (European Commission et al., 2023).

reporting behavior only, if an individual never reports any rental income. The idea of our information experiment is to create an environment where compliance is high even without large-scale audits.

2.2 Experimental Design

2.2.1 Constructing the base population and co-ownership network

The base population for the experiment was formed using the tax authority's register on apartment ownership and an extract from the national population register on apartment occupancy.[13] We linked the information from these registers using personal identification numbers that uniquely identify individuals across different national registers. Both data sets are based on the situation at the end of year 2015.[14]

In these data we classify apartments that are occupied by someone else than one of the owners as suspected rental apartments. The owners of these apartments are classified as suspected landlords. We drop apartments with more than 15 tenants or more than 5 owners to remove outliers and potentially erroneous entries in the data. We also exclude individuals who have entrepreneurial activity (based on tax data for the pre-experiment year 2015), so that the data include regular individual taxpayers only.

For each apartment with multiple owners, we use ownership shares to identify the main owner of the apartment and allocate the apartment to this owner.[15] This procedure leads to a population of suspected landlords, who are main owners of a suspected rental apartment, and each suspected rental apartment has one (main) owner.

We restrict this base population of suspected landlords in two consecutive ways to minimize unintentional spillovers across experimental treatment groups, and to allow a rigorous analysis of spillovers within an ownership network. The purpose of these restrictions is to ensure that the base population includes only one suspected landlord from each household, and one suspected landlord from each co-ownership network (as defined below).

First, to keep only one person from each household, we first construct households using information on home addresses. For each household, we keep only the person that owns the most suspected rental apartments.[16]

[13] We focus on apartments in apartment buildings and leave out detached houses which are predominantly owner-occupied and often located in rural areas with thin rental markets.

[14] We drop apartments that have been sold in November or December 2015 because they are likely to be vacant immediately following the transaction.

[15] In case of equal ownership shares, the main owner is randomly chosen.

[16] We count only apartments where the individual is the main owner. In case of equal number of apartments, we calculate the total of ownership shares for each individual and keep the person with

Second, to guarantee that at most one person in each co-ownership network receives the treatment, we identify main owners who own apartments together.[17] This defines the co-ownership network of main owners. From each such network, we randomize one person into the base population for our main analysis.[18]

Finally, the co-owner population for the network spillover analysis is formed as follows. The main owners who were not randomized into the base population for the main analysis are allocated to the co-owner population. In addition, the minority owners who own an apartment together with a main owner in the base population of our main analysis are allocated to the co-owner population.[19] This procedure identifies a clearly defined first-order network of co-owners where spillovers can be rigorously analyzed.[20] Descriptive statistics of the base population for the main analysis, as well as the co-owner population for the network spillover analysis, are provided in Section 3.1.

We do not expect everybody in the base population to have received rental income in 2015. Rather, the fact that an individual is identified as a suspected landlord in our data, provides an imperfect indication of likely rental income. This is because of the end of year snapshot nature of the data, and potential reporting lags and errors in the registers. For example, our measure does not capture rental apartments sold before the end of the year or apartments rented out earlier during the year but vacant at the end of the year. Further, the register of apartment ownership and apartment occupancy may sometimes be updated with a lag. Finally, some of the suspected rental apartments may have been occupied free of rent (e.g. by a friend or a relative of the owner, as we cannot identify family ties besides marriage or cohabitation in the data).

2.2.2 Timeline of the experiment

Figure 1 illustrates the timing of tax filing and the experimental design.

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the biggest overall ownership share. In case the ownership shares are also equal, we randomly choose one person from the household. Finally, we drop individuals who own more than 15 suspected rental apartments.

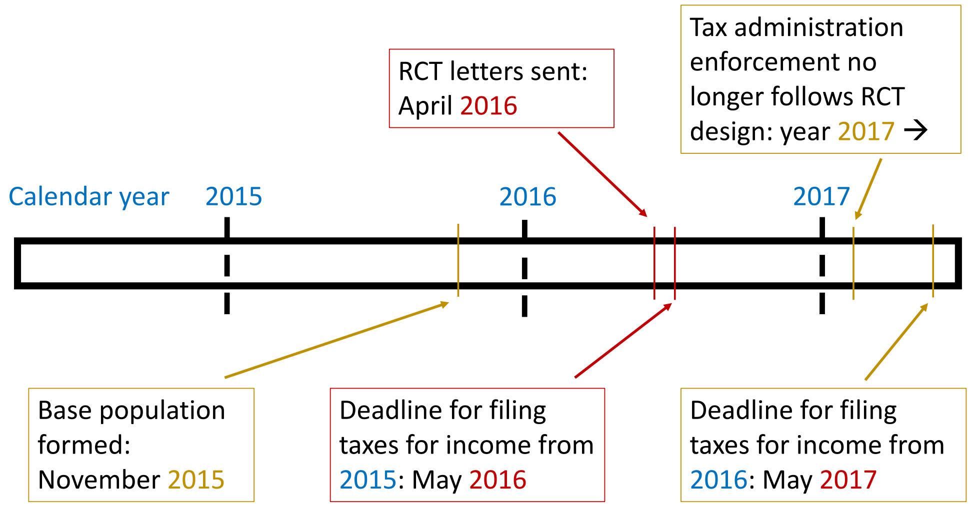
[17]In practice this means that if two individuals own a suspected rental apartment together, the minority owner must be a main owner of another apartment.

[18]In addition, the base population of course includes suspected landlords who own apartment(s) alone, which is the most common case.

[19]Note that these minority owners are not a main owner in any of the suspected rental apartments in our data. They are included in the co-owner analysis to obtain a more comprehensive estimate of spillovers in the co-ownership network.

[20]The network analysis considers the first-order network, i.e. those individuals who have a direct link via joint ownership of an apartment. Therefore the estimate of network spillovers is a lower-bound of spillovers that might occur in the complete network.

Figure 1: An illustration of the timeline of the experiment.



The base population was formed as described above using information on apartment residency and ownership in November 2015. The treatment letters were sent out in April 2016 by the Finnish Tax Administration. The deadline for filing tax returns was in May 2016, leaving time for recipients of the letters to adjust their reporting of rental income from 2015. However, as the letters were sent out in 2016, all real outcomes for 2015 were already fixed. Therefore, any effects we will observe in the income tax reporting data in 2016 will be pure reporting responses. However, as we will explain in more detail in Section 3.1, we have a separate data source that provides register data on asset transactions, which allows us to identify real responses (e.g. apartments bought and sold) during 2016.

Our contract with the Finnish Tax Administration for running the experiment concerned year 2016 only. Therefore, when interpreting our results we need to take into account that Finnish Tax Administration no longer necessarily followed the experimental design in their audit selection in 2017 (related to tax reporting on income from year 2016), i.e. the year following the initial experiment. Thus, analyzing the longer term impacts of our experiment on tax reporting is not possible.

2.2.3 Treatments

In this subsection, we describe the experimental treatments. At the end of the section we turn to a more detailed discussion of the role and special nature of third-party information in the experiment.

The experiment consisted of four treatments: 1) Letter 1 with a reminder to file tax returns and to report deductions and missing income, such as rental income; 2) Letter 2 providing information on how to file rental income; 3) Letter 3 notifying the recipient of a general increase in the intensity of rental income tax enforcement; 4) Letter 4 on intensified enforcement of rental income taxation and a mention of the use of third-party information on the ownership of dwellings. Letter 4 also indicated that according to the information available to the Finnish Tax Administration, the recipient owns at least one apartment that may have been rented out in 2015. All treatment Letters 2-4 also contained the information provided in treatment Letter 1.

In total, approximately 45,000 treatment letters were sent to suspected landlords who were randomly selected from our base population. The numbers of letters in each treatment arm, and other details of the experimental design, are described in Subsection 2.2.4. The enforcement measures described in Letter 3 and Letter 4 were implemented by the Finnish Tax Administration in summer 2016. The full letters are shown in Appendix B (translated from Finnish by the authors).

According to the deterrence model of tax evasion (Allingham and Sandmo, 1972), the extent of evasion depends crucially on the probability that evasion is detected. Recent literature has emphasized that the detection probability is particularly high for income items subject to third-party reporting (Kleven et al., 2011). Our experimental Letter 3 and Letter 4 are designed to directly affect the recipients' perception of the detection probability – Letter 3 through a signal of a general increase in enforcement intensity, and Letter 4 additionally through notifying landlords about the use of third-party information in tax enforcement. On the other hand, Letter 2 is more focused on information provision and may be particularly effective in cases where suspected landlords have not been fully aware of their reporting obligations. Letters 3 and 4 can therefore be thought of as being linked to the "enforcement paradigm" and Letter 2 to the "service paradigm" of tax authorities, according to the classification presented in Alm (2012).

However, because the letters were sent by the Finnish Tax Administration, and it does not routinely send such reminders to taxpayers, all letters likely have increased the recipients' subjective detection probability. We therefore expect the treatments to affect the recipients' reporting behavior, in particular if there has been non-compliance in the baseline. The effects of the letters on tax reporting are estimated from a comparison of reporting in each of the letter groups to a control group of suspected landlords who did not receive a letter.

Let us now turn to a more detailed discussion of the role of third-party information in the experiment.

First, regarding the nature of the new information source, we use the term "third-party information" in a broad sense, referring to information used in enforcement but not reported to the tax authority by the taxpayer themselves. In our case, the information comes from combining data in novel ways from different administrative registers. In this sense, the information originates from a third party, even if it is not a case of direct third-party reporting to the tax authority. Further, the type of information we use is special in the sense that it provides a signal of economic activity (rental activity), but does not provide precise information on its scale (the level of income). This feature will be reflected in our results, as we discuss in Section 4.1.

Second, it is important to note that the new information source that we employed was a key element that made the experiment feasible in the first place. Therefore, even though the new third-party information is used directly in Letter 4 only, we regard the entire experiment as providing evidence on the effectiveness (or not) of using this novel source of third-party information in tax enforcement. This is because the targeting of *any* of the letters to a group of suspected landlords would not have been feasible, had we not used this new information source.

2.2.4 Block design

Table 2 describes our randomized block design, which is similar to the design in Crépon et al. (2013). The block design allows us to vary the treatment intensity in different geographic areas and analyze whether the treatment letters created spatial spillovers within local rental markets, in addition to direct effects on letter recipients. When treatment intensity is higher in a local area, we also expect to see more spillovers in that area, if this is a relevant spillover channel. Such local spillovers may arise through information exchange between landlords in local rental markets or more broadly through changes in market conditions caused by more intense enforcement of the rental income tax.

To analyze spatial spillovers, we first assign each suspected landlord in our base population to a postcode area based on where their suspected rental apartments are located. Those who own apartments in several postcode areas are allocated to the postcode area with most apartments.[21]

Spatial spillovers are more likely to arise in large cities and towns with high population density. Rural areas and small towns are mostly populated by owner-occupiers living

[21]In case of equal number of apartments in two postcode areas, we use the sum of ownership shares to allocate the owner to the postcode area. In case of equal ownership shares, we randomly assign the owner to one postcode area.

in detached houses and rental markets tend to be thin. Motivated by these regional differences, we select into the block design only postcode areas with a reasonably dense rental market. We leave out rural municipalities with less than 5,000 apartments as well as postcode areas with less than 60 apartments and on average less than five apartments per building. With these restrictions we have 263 postcode areas in the block design.

We then use the following two-step procedure to create blocks with different treatment intensities. First, we form groups of postcode areas of similar size and then, within each strata, randomly assign each of the 263 postcode areas into one of three blocks: i) control block with no letters; ii) low-intensity block where 24% of suspected landlords in the base population receive a letter; iii) high-intensity block where 62% of suspected landlords in the base population receive a letter.[22]

Table 2: Experimental design.

	Control block	Low intensity block	High intensity block	Outside block design	Total
No letter	17,515	18,864	11,621	24,033	72,033
Letter 1	0	1,700	2,484	4,742	8,926
Letter 2	0	1,730	2,367	4,835	8,932
Letter 3	0	1,110	6,425	1,386	8,921
Letter 4	0	2,292	12,776	2,788	17,856
Total	17,515	25,696	35,673	37,784	116,668
Postcode areas	62	90	111	1,031	1,294

Notes: The table shows the number of different letters sent to suspected landlords and the size of the no-letter group in the three blocks with different treatment intensity (0%, 24%, 62%) and the number of postcode areas in each block. "Outside block design" refers to the number of different treatment letters sent to suspected landlords in postcode areas that are outside the block design.

In the second step, in each block, the relevant fraction (0%, 24% or 62%) of all suspected landlords in the base population are randomly selected to the treatment. In addition, the share of the stronger treatment Letter 3 and Letter 4 was higher in the high-intensity block. For instance, Letter 4 comprised roughly a third of the treatment letters in the low-intensity block and a half in the high-intensity block.

[22] As an example, Figure A1 in Appendix A illustrates the block design for Helsinki, the capital city and largest municipality in our data. There are approximately 80 postcode areas in Helsinki. The postcode areas satisfying our criteria for reasonably dense rental market are randomly assigned to control, low-intensity or high-intensity block (for data confidentiality reasons, we are not able to show which ones).

The first three columns in Table 2 show the number of individuals allocated to the different letter groups and the no-letter group in the three blocks with different treatment intensity. Similarly, the second to last column shows random allocation of the suspected landlords in our base population outside the block design into the different letter groups and the no-letter group.

3 Data and Empirical Strategy

3.1 Data

Our data cover the whole population of Finland and contain information on individual rental income (gross and net), place of residence, ownership of apartments, transactions of apartments, ownership and transactions of other assets, family status (id of spouse/partner included), age and gender. The tax reporting data (covering information on rental income) are for income accrued in years 2012 through 2016. Taxes on these incomes are always reported in the following year, so that tax filing in our data takes place between 2013 and 2017. Data on transactions of apartments and other assets come from a separate administrative register that records asset transactions at the time of the transaction. The construction of the estimation samples was described in Section 2.2.1.[23] Summary statistics of key variables in the data are reported in Appendix A Table A1 for the main estimation sample and in Table A7 for the co-owner population used in the network spillover analysis. Co-owners own on average fewer apartments and have a lower propensity to report rental income than individuals in the main estimation sample. This is because the co-owner sample also includes minority owners.

Given that rental markets outside the block design are quite different from those in the blocks, we only utilize data from the three blocks in our main analysis. This choice also allows us to analyze spatial spillovers and to isolate the direct treatment effects of the letters from such spillovers. We report the results for suspected landlords outside the block design for completeness in Appendix A (Figure A2 and Table A2).

Table 3 describes reporting of rental income before (2015) and after (2016) the treatment for the main estimation sample. Comparing the different treatment groups before the treatment indicates that the randomization has been successful as the groups are very

[23] We further trim the data by excluding individuals with extreme values for the following variables: net rental income, spouse's net rental income, number of apartments, number of sold apartments and number of bought apartments. We drop the individual from the sample in all years if the value of any of these variables is above the 99.5 percentile or below the 0.5 percentile in any year during the study period. This reduces the sample size by 5.9% and decreases standard errors considerably.

similar in terms of the pre-treatment propensity to report and reported net rental income. This is to be expected by construction. More detailed balancing tests are provided in Section 3.2.

Overall, a comparison of 2015 and 2016 indicates that the propensity to report rental income is higher after the treatment. A first indication that the treatment had some effect on the propensity to report rental income is visible in the table: The propensity to report is higher in 2016 in the letter groups than in the no-letter group.

Table 3: Reporting of rental income before and after the treatment.

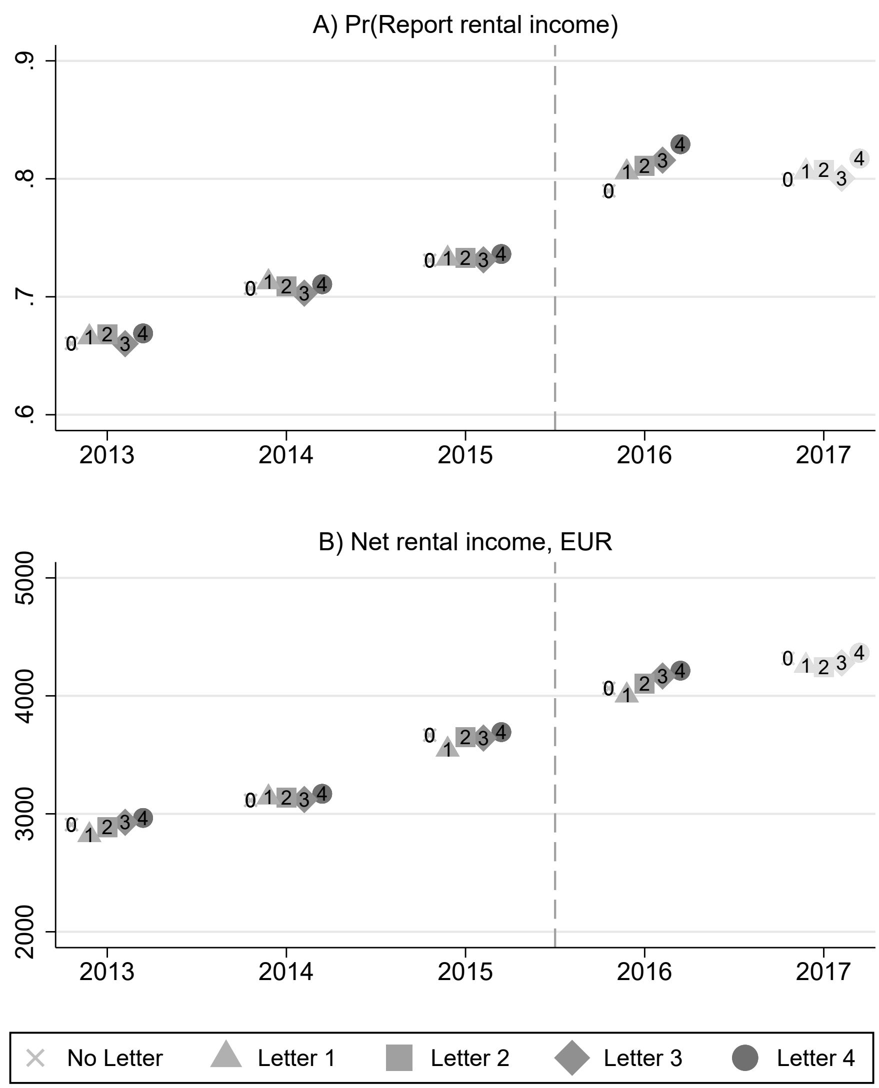
	Reported rental income 1/0		Net rental income, EUR	
	2015	2016	2015	2016
High and low intensity block				
No letter	0.731	0.790	3667	4064
	(0.443)	(0.407)	(4814)	(4955)
Letter 1	0.732	0.806	3544	4004
	(0.443)	(0.396)	(4658)	(4865)
Letter 2	0.733	0.811	3649	4105
	(0.442)	(0.391)	(4865)	(4864)
Letter 3	0.731	0.816	3642	4168
	(0.443)	(0.388)	(4826)	(4978)
Letter 4	0.736	0.829	3692	4214
	(0.441)	(0.376)	(4813)	(4897)
Control block				
No letter	0.713	0.775	3523	3895
	(0.452)	(0.417)	(4802)	(4888)

Notes: The table shows the propensity to report rental income and mean net rental income before the treatment in 2015 and after the treatment in 2016 in the treatment groups. Standard deviations are in parentheses. (N = 73,323 in 2015 and N = 71,845 in 2016)

However, the propensity to report rental income increases also in the no-letter group. One specific reason may be related to turnover combined with the fact that the base population is drawn based on end-of-year 2015 data: We follow this fixed group of individuals over time, and while all individuals in our base population owned a suspected rental apartment in 2015, some of them may not have yet owned one in 2014. This mechanically increases accrued rental income from 2014 to 2015, and hence the propensity to report rental income from 2015 to 2016. In addition, observed developments over time may naturally be due to general trends in the rental market.

These developments are further illustrated in Figure 2, which shows the propensity to report and net rental income for the different treatment groups and the no-letter group in years 2013–2017. The figure shows that indeed in our sample the propensity to report increases over time before the experiment. This is consistent with the mechanical effect of selecting the sample in 2015 based on knowledge of ownership of suspected rental apartments at that point in time.

Figure 2: Reporting of rental income in treatment groups - high and low intensity blocks



Notes: The figures show the development of the mean propensity to report rental income and mean net rental income in the letter groups and the no-letter group in the high and low intensity blocks.

3.2 Balancing tests

This subsection presents the balancing tests examining whether key background characteristics are balanced between individuals in the different treatment groups and the control group. The tests are carried out running regressions where an outcome is regressed on the indicators for the different letter groups. The omitted group is the no-letter group, and the coefficients of the different letter groups allow to test whether there is a significant difference between each letter group and the no-letter group for different outcome measures before the experiment. The balancing tests are carried out in a regression framework, because we also need to include indicators for the high and low intensity blocks due to the nature of the block design in our experiment. The tests are conducted using data for 2015, which is the year before the treatments.

Table 4 presents the results of the balancing tests. The outcomes included are the main outcomes of interest in the study: a dummy for reporting rental income and the level of reported net rental income in euros. We also investigate the balance of a number of background variables: number of suspected rental apartments, age, a dummy for female and a dummy for being married or cohabiting. Finally, we use the sample before trimming of extreme values to test whether the likelihood of trimming is balanced.

Table 4 shows that the randomization was successful and most of the variables examined were in balance before the treatment. By examining different coefficients, it is also apparent that letter groups 1 through 4 are in balance compared to each other, not just compared to the no-letter group. There are a couple of significant coefficients, but given the large number of tests, we expect some coefficients to turn out statistically significant at the 5% level by pure chance. Thus, we conclude that the treatments are reasonably well balanced overall.

Table 4: Balancing tests for pre-treatment outcomes and background characteristics.

	Reporting rental income	Net rental income	Suspected rental apartments	Age	Female	Has spouse	Trimmed
Letter 1	0.00071	-131.2	0.00203	0.439	0.0108	0.0102	0.00406
	(0.00824)	(67.65)	(0.00868)	(0.289)	(0.00855)	(0.00893)	(0.00467)
Letter 2	0.00137	-25.49	-0.000091	0.2	-0.00134	-0.00013	-0.00581
	(0.00726)	(93.98)	(0.0100)	(0.280)	(0.00772)	(0.00834)	(0.00432)
Letter 3	-0.00135	-43.02	0.000688	0.178	-0.0003	-0.0148	0.00335
	(0.00594)	(72.41)	(0.00881)	(0.239)	(0.00751)	(0.00740)	(0.00357)
Letter 4	0.00362	7.728	-0.00333	0.148	0.00581	-0.00244	-0.00407
	(0.00514)	(51.75)	(0.00661)	(0.190)	(0.00601)	(0.00521)	(0.00294)
Low intensity	0.0166	128.7	-0.00117	-0.215	0.00186	0.00911	0.00186
block	(0.0129)	(198.4)	(0.0128)	(0.428)	(0.00819)	(0.00872)	(0.00454)
High intensity	0.0203	167.2	-0.00562	0.201	0.0129	0.00764	0.00197
block	(0.0120)	(199.9)	(0.0120)	(0.451)	(0.00849)	(0.00874)	(0.00474)
Baseline mean	0.728	3630.1	1.202	58.01	0.533	0.602	0.0705
N	73323	73323	73323	73323	73323	73323	78884

Notes: Balancing tests done in regression framework using data from 2015. The omitted group is the no-letter group. Different outcomes are tested in separate regressions.

3.3 Empirical strategy

We use the panel property of the data by utilizing a Differences-in-Differences (DiD) strategy which compares outcomes in the treatment groups over time. This strategy controls for possible idiosyncratic differences across treatment groups, and thus reduces the noise in the estimates. We can credibly compare the groups because they are randomized from the base population. We estimate the following model:

$$y_{it} = \alpha_t + \sum_j \sum_\tau \mathbb{1}[t = \tau]\beta_{j\tau} Letter_j + \sum_k \sum_\tau \mathbb{1}[t = \tau]\gamma_{k\tau} Block_k + \mu_i + E_{it} + \epsilon_{it} \tag{1}$$

where y_{it} is the outcome for individual i at time t. We consider the effects of the different treatment letters ($Letter_j$) separately, and include dummies for high or low intensity blocks ($Block_k$). The coefficients $\beta_{j\tau}$ then are year-specific coefficients that identify the effects of the different letters in different years compared to reference year 2015 just before the treatment. The $\gamma_{k\tau}$ coefficients identify the yearly effects of being in a high or low-intensity block (over and above the direct effect of receiving a letter), relative to the control block, and control for spatial spillovers. We control for general changes in the outcomes over time through year fixed effects (α_t) and time-invariant individual-level factors through individual fixed effects (μ_i).

We also control for the enforcement measures (E_{it}) associated with the experiment but we cannot disclose the estimates.[24] We cluster standard errors at the postcode level – the same level at which we randomize geographic treatment blocks.

We first estimate Equation (1) with data from three years before the treatment and two years after the treatment (years 2013–2017) and present the findings in the form of event-study graphs plotting the regression estimates of $\beta_{j\tau}$. We then estimate the following pooled DiD model with data from three years before and one year after the experiment (years 2013–2016)[25]:

$$y_{it} = \alpha_t + \sum_j \beta_j Letter_j * After + \sum_k \gamma_k Block_k * After + \mu_i + E_{it} + \epsilon_{it}, \qquad (2)$$

where $After$ indicates the post-treatment year 2016.

After reporting the main estimates, we analyze heterogeneous responses first visually by plotting the estimates of Equation (1) for sub-samples based on pre-treatment reporting status and the number of suspected rental apartments owned before the treatment. In order to disentangle which characteristics are the key drivers of the observed heterogeneity, we estimate a DiD model which includes interactions of the letter dummies with dummies for different subgroups of the base population:

$$\begin{aligned} y_{it} = \alpha_t &+ \sum_j \sum_g \beta_{jg} Letter_j * Group_g * After + \sum_j \delta_j Letter_j * After \\ &+ \sum_g \kappa_g Group_g * After + \sum_k \gamma_k Block_k * After + \mu_i + E_{it} + \epsilon_{it} \end{aligned} \qquad (3)$$

The model interacts the letter dummies with dummies $Group_g$ corresponding to each subgroup of interest: a dummy for not reporting rental income in 2015, a dummy for

[24]Our data contain information on which individuals were subject to the additional enforcement measures as indicated in Letters 3 and 4, even though the exact contents of the measures are not disclosed to us by the Tax Administration. We can thus control for the effects of these measures in our analysis. In addition, the Tax Administration may have carried out some business-as-usual enforcement measures, but those would be orthogonal to our (randomized) treatments.

[25]Data for 2017 is excluded from the DiD regressions for the reasons explained in 2.2.2, namely that the experimental design might no longer hold in that year. Results for year 2017 are however included in the graphical analysis for completeness, but they are marked with a lighter color.

owning only one suspected rental apartment, as well as a dummy for age below 40.[26] Similar to our main analysis in Equation (2), the model uses data from the pre-treatment years (years 2013 through 2015) and one year after the treatment (2016). The model also controls for group specific trends through the interaction of the group dummies and the $After$-dummy, and the main effect of the treatment through the interaction of letter dummies with the $After$-dummy.

After the main analysis, we present results on two types of spillovers in Section 4.2. The spillovers are estimated as follows.

First, to estimate spillover effects within ownership networks, the analysis is conducted on the sample of individuals consisting of the co-owners of the suspected landlords in our base population. Construction of the co-owner sample was explained in Section 2.2.1. To analyze network spillovers, we provide event-study figures plotting the $\beta_{j\tau}$ coefficients from Equation (1), and DiD estimates based on Equation (2), estimated on the co-owner sample. In this analysis, the treatment dummies indicate whether each individual in the spillover analysis co-owned an apartment with an individual in the base population who received a treatment letter.

Second, as explained above, the $\gamma_{k\tau}$-coefficients in Equation (1) identify the yearly effects of being in a high- or low-intensity block (over and above the direct effect of receiving a letter), relative to the control block. These coefficients provide estimates of spatial spillovers. To illustrate their magnitude, we provide event-study figures where we plot the $\gamma_{k\tau}$ coefficients for years 2013–2017 for the high- and low-intensity blocks.

Finally, we report the real effects of the experimental treatments on asset allocations in Section 4.3, where we run an event-study analysis corresponding to Equation (1) with housing market transactions as the outcome. This analysis is complemented by studying differences between letter groups in transactions of other assets. The data on other assets is available for year 2016 only, and therefore the latter analysis is run as a simple OLS on data for that year.

The experiment has been pre-registered at the AEA RCT Registry.[27] The analysis of the effects on tax reporting (income and deductions) has been registered as primary outcomes, and spillovers in personal networks and within local rental markets, as well as effects on asset ownership, have been pre-registered as secondary outcomes. The heterogeneity analysis has not been pre-registered and is therefore exploratory in nature.

[26] Heterogeneity according to gender was also analyzed, but no significant differences were found.
[27] https://www.socialscienceregistry.org/trials/2575

4 Results

4.1 Direct reporting effects

Graphical analysis. This subsection presents the event-study analysis plotting the $\beta_{j\tau}$-coefficients from Equation (1).

Figure 3 shows the development of the propensity to report rental income (Panel A) and the reported net rental income (Panel B) for suspected landlords in our base population in the treatment blocks.[28] The figure shows that the treatment letters affected the reporting of rental income by suspected landlords: both the propensity to report rental income (the extensive margin of reporting) and the average euro amount of reported net rental income increased. The effects appear somewhat stronger for the propensity to report. The effect is visible in all letter groups, but seems to be strongest for Letter 3 and Letter 4 and weakest for Letter 1.

Because the treatment letters were sent out in spring 2016, the effects in 2016 are pure reporting responses only. Tax reporting in 2016 concerns income earned in 2015, and therefore all real activity that affect those incomes have already taken place before receiving a treatment letter.

Figure 3 (as well as corresponding figures below) also show reporting for 2017. The estimates show that the effects largely disappear. However, as explained above, that could arise simply because the experimental design does not necessarily hold any longer for that year. We include the estimates for 2017 for completeness in the graphical analysis, but have indicated them with a lighter color.[29]

As noted in Section 2.2.3, compared to previous literature, the type of third-party information that we utilize has interesting features: The information consists of a signal of suspected existence of economic activity that confers a tax liability on the individual (rental activity), but provides no indication of the scale of this activity (actual income levels). Accordingly, the effects that we find are strongest on the extensive margin of reporting. Nevertheless, there are also significant effects on the amount of rental income reported. Interestingly, we find a positive effect on *net* rental income. This is in contrast with findings in some previous studies where taxpayers have scaled up deductible costs

[28] Figure A2 and Table A2 in Appendix A show the corresponding results for suspected landlords in our base population outside the treatment blocks.

[29] Given that property income is a relatively stable income source, enforcement might have long-lasting effects according to Advani et al. (2023), whose evidence however relates to audits. The meta-analysis of tax enforcement nudge experiments by Antinyan and Asatryan (2024) does not examine effects extending beyond 12 months of the initial interventions. E.g. De Neve et al. (2021) find that the effects of information treatments fade out within 1–2 years, depending on the details of the treatments.

to offset the effect of *specific* third-party information on the amount of taxable income (Slemrod et al. 2017; Carrillo et al. 2017). We provide a break-down of the observed responses to effects on reported gross income vs. deductions below when we turn to DiD analysis.

We next study whether the effects are heterogeneous across subgroups. In Figures 4 and 5 we divide the main estimation sample into those who reported rental income in 2015, that is, one year before the treatment, and to those who did not report any rental income in 2015, despite both groups being owners of suspected rental apartments in that year.[30] In Figures 6 and 7, in turn, we divide the base population according to the number of suspected rental apartments owned in 2015.

Figure 4 shows that the effect on the probability of reporting any rental income in 2016 is remarkably strong among those who did not report any rental income in 2015, about 10 percentage points for Letter 4. The effect is positive and statistically significant also among those who did report in 2015, but the point estimates are much smaller. Figure 5, in turn, presents corresponding results for net rental income. Here, the difference between those who did or did not report rental income in 2015 is not as large.

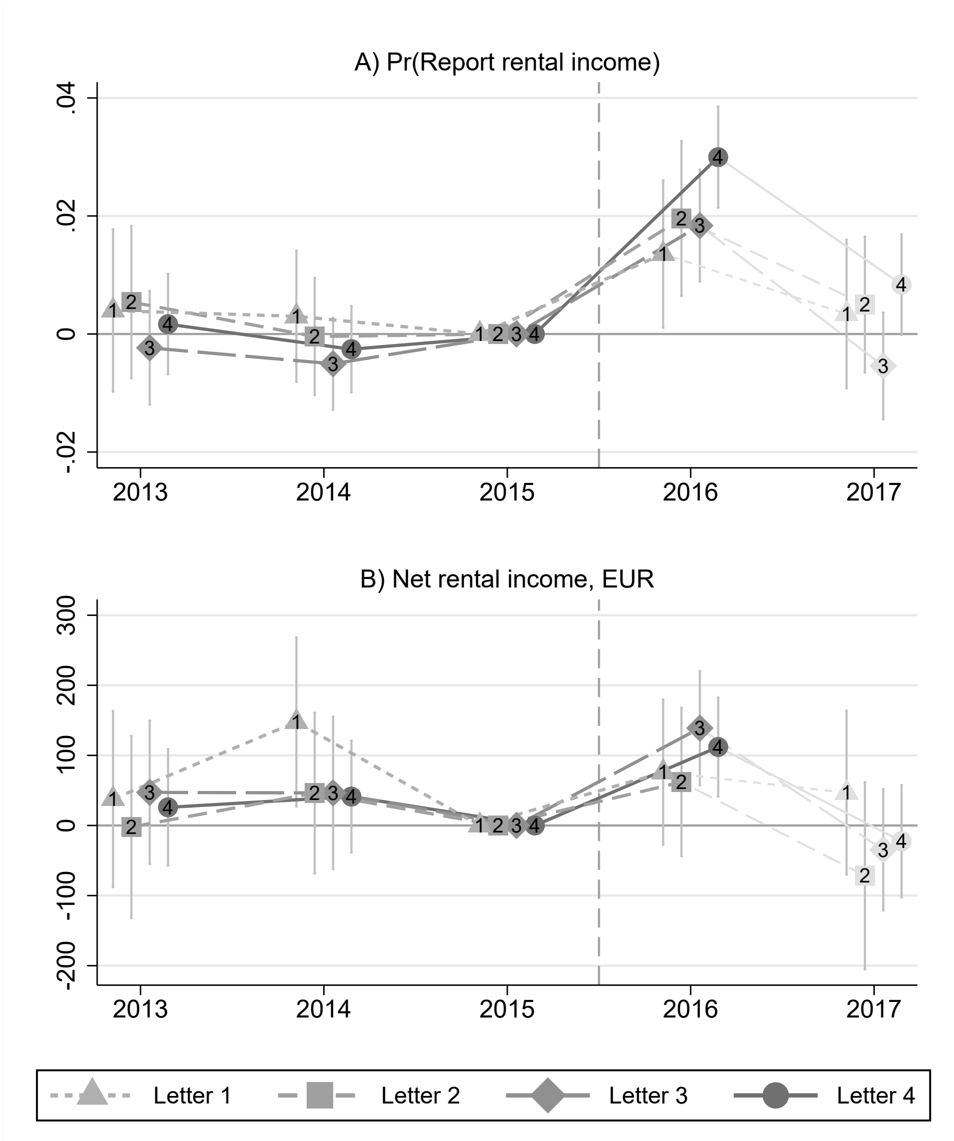
Figures 6 and 7 show that the effects for both the probability of reporting and net rental income are more pronounced among those who had only one suspected rental apartment in 2015 than among those who had two or more suspected rental apartments. The mean of rental apartments in the latter group is 2.4 apartments.

All in all, the event-study graphs for the divided samples show that the effects are concentrated on those who reported no rental income in the previous year and on those with only one suspected rental apartment. These results are interesting from the point of view of effective tax enforcement. First, they indicate that non-compliance was likely prevalent among those who did not report rental income in the previous year. This group would have been difficult to detect in tax enforcement in the absence of our new source of third-party information. The result also highlights that findings from studies where interventions are targeted at likely evaders (e.g. Bott et al. 2020) may not generalize to the average taxpayer. Second, as Table 1 shows, around 85% of suspected landlords have only one rental apartment, and in total this group makes up a large share of tax revenue. While auditing such a large group of taxpayers, each engaging in small-scale renting, may be potentially costly, it is noteworthy that the letter intervention is particularly effective among this group. From an equity perspective on the other hand, this implies that mostly

[30] Reporting of rental income is quite persistent over time. Out of those who did not report rental income in 2015, roughly 12% did so in 2014.

individuals with relatively low rental income are affected.[31]

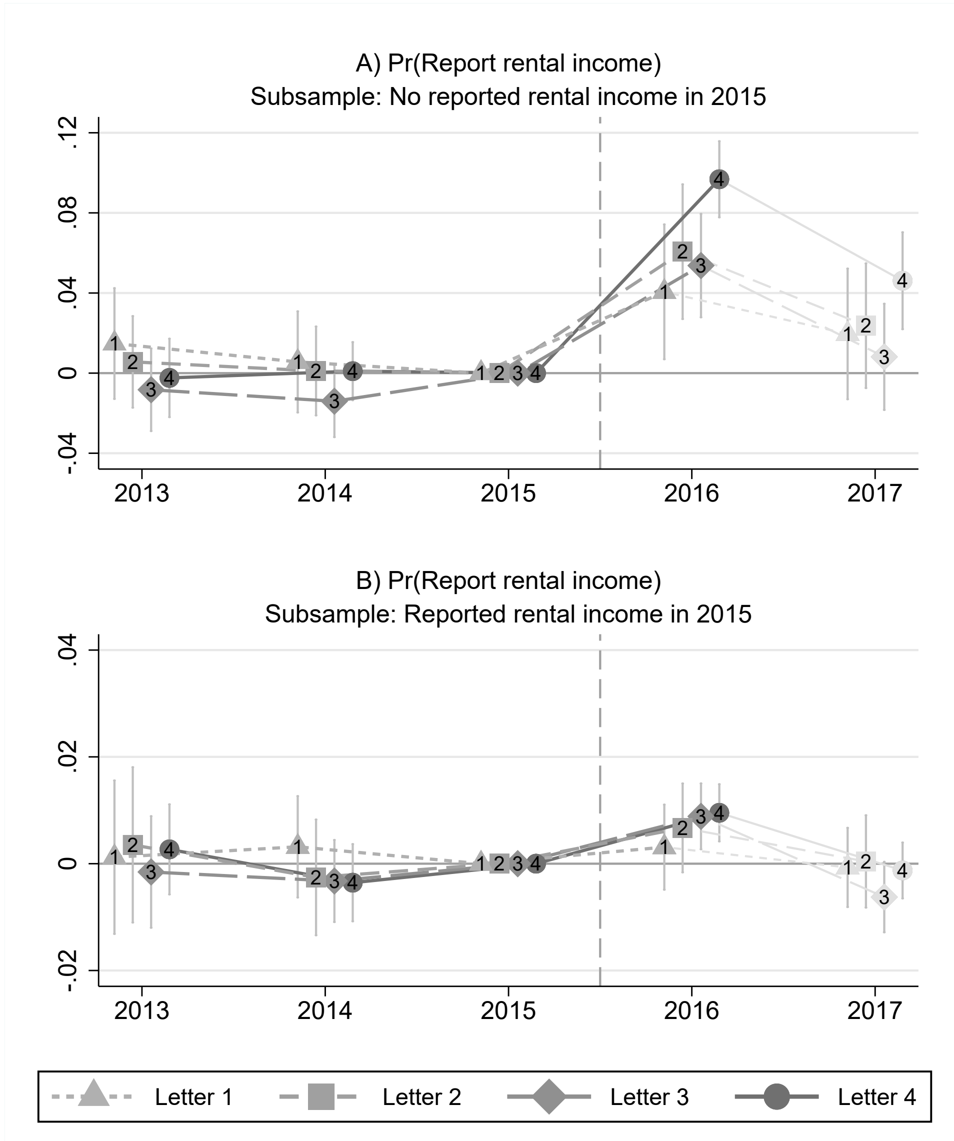
Figure 3: Propensity to report rental income and net rental income in letter groups.



Notes: The figures show regression coefficients on treatment letter by year dummies (ref. no letter and year 2015). Controls include individual fixed effects, treatment block by year dummies and additional enforcement measures related to the treatments. Vertical lines indicate 95% confidence intervals (clustering at postcode level). Estimates for 2017 are shown in light gray to indicate that the experimental design no longer holds in that year.

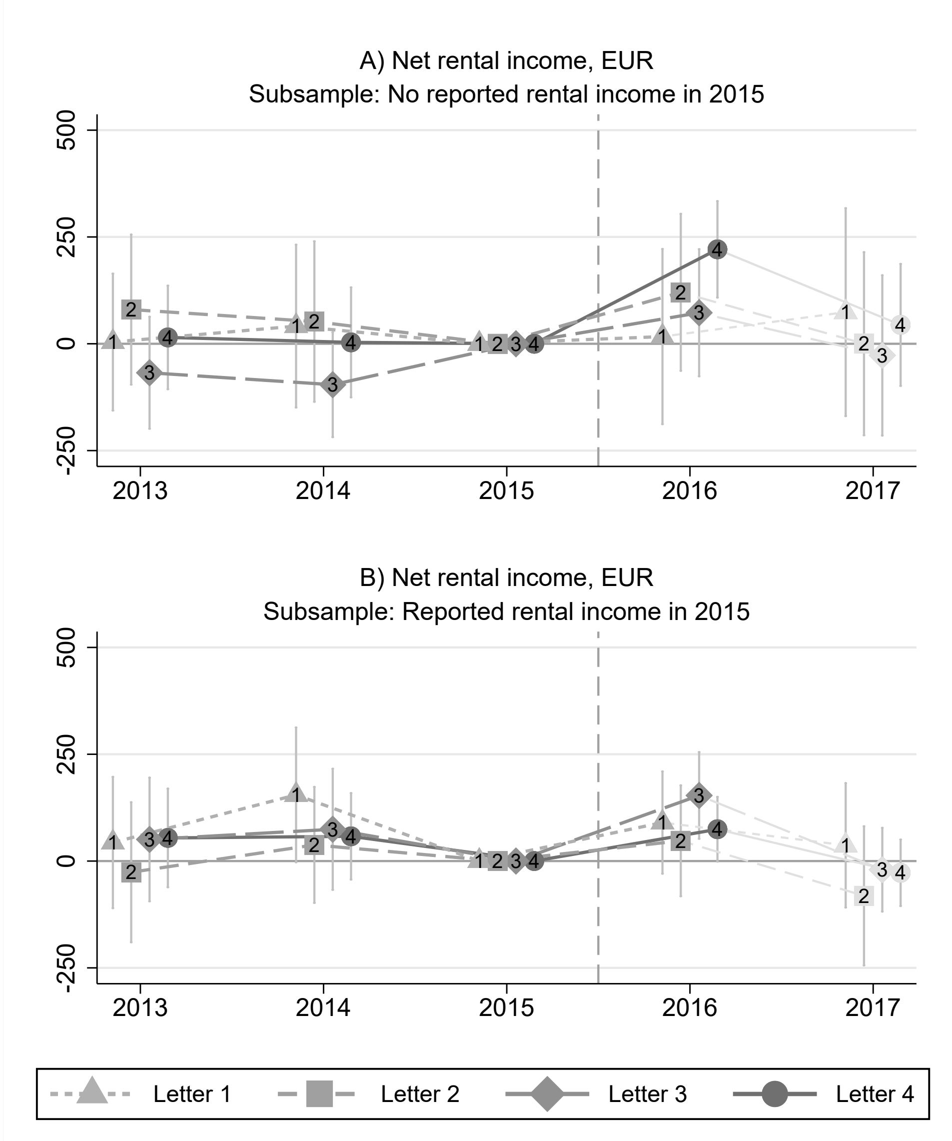
[31]Unfortunately our data does not include information on total incomes or wealth, and hence a more extensive analysis of equity effects is not feasible.

Figure 4: Propensity to report rental income in letter groups – by reporting status before treatment.



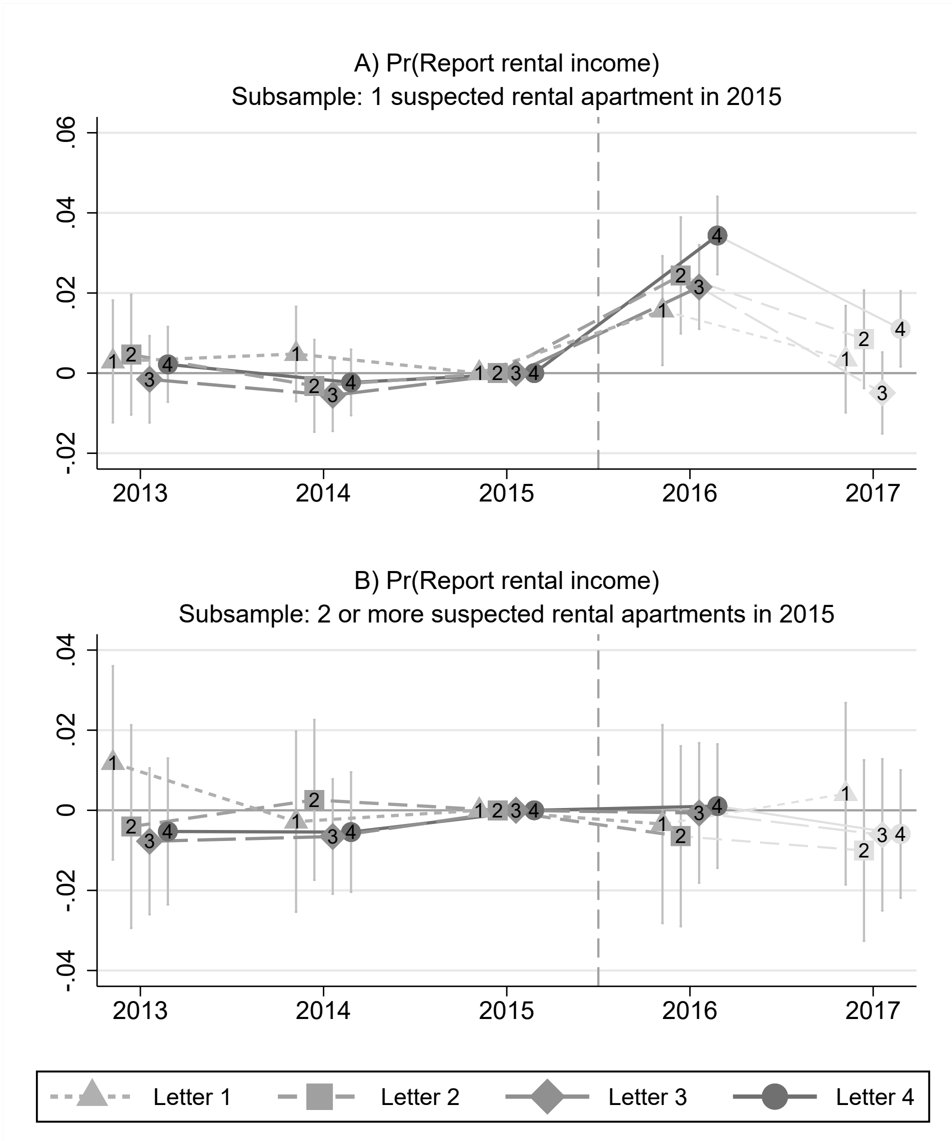
Notes: The figures show regression coefficients on treatment letter by year dummies (ref. no letter and year 2015). Controls include individual fixed effects, treatment block by year dummies and additional enforcement measures related to the treatments. Vertical lines indicate 95% confidence intervals (clustering at postcode level). Data for 2017 is shown in gray to indicate that the experimental design no longer holds in that year.

Figure 5: Net rental income in letter groups – by reporting status before treatment.



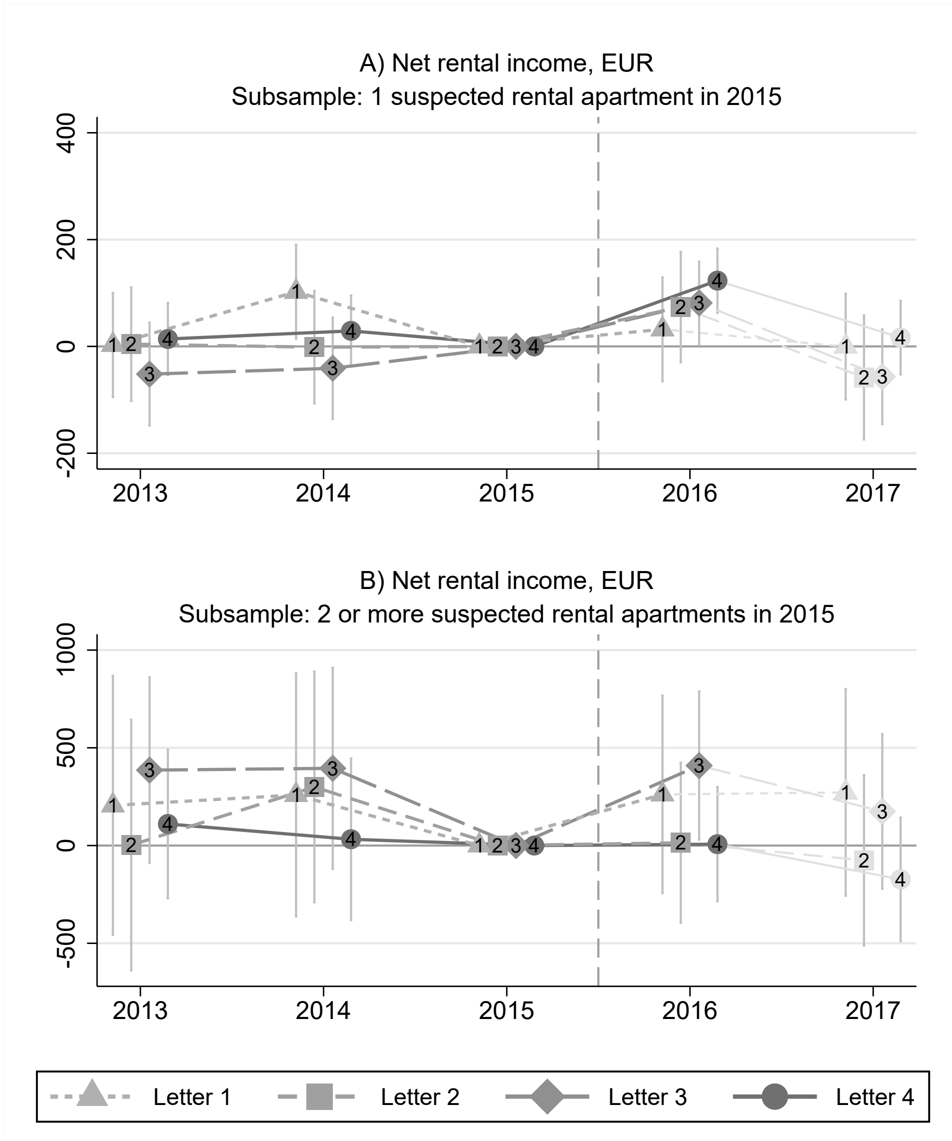
Notes: The figures show regression coefficients on treatment letter by year dummies (ref. no letter and year 2015). Controls include individual fixed effects, treatment block by year dummies and additional enforcement measures related to the treatments. Vertical lines indicate 95% confidence intervals (clustering at postcode level). Data for 2017 is shown in gray to indicate that the experimental design no longer holds in that year.

Figure 6: Propensity to report rental income in letter groups – by number of suspected rental apartments owned before treatment.



Notes: The figures show regression coefficients on treatment letter by year dummies (ref. no letter and year 2015). Controls include individual fixed effects, treatment block by year dummies and additional enforcement measures related to the treatments. Vertical lines indicate 95% confidence intervals (clustering at postcode level). Data for 2017 is shown in gray to indicate that the experimental design no longer holds in that year.

Figure 7: Net rental income in letter groups – by number of suspected rental apartments owned before treatment.



Notes: The figures show regression coefficients on treatment letter by year dummies (ref. no letter and year 2015). Controls include individual fixed effects, treatment block by year dummies and additional enforcement measures related to the treatments. Vertical lines indicate 95% confidence intervals (clustering at postcode level). Data for 2017 is shown in gray to indicate that the experimental design no longer holds in that year.

DiD regression results. To put precise numbers on the effects, we next estimate DiD models, where the effects are estimated for each letter group relative to receiving no letter (β_j coefficients from Equation (2)). Table 5 shows the results for reporting of rental income on the extensive margin, gross rental income, deductions and net rental income.

The first column shows that the effects of Letters 2 and 3 amount to about a 2 percentage point increase in the propensity to report rental income, while Letter 4 increased the propensity to report by around 3 percentage points. The latter amounts to a relative effect of 4% compared to the baseline reporting rate of 78.5%. The estimate for Letter 1 is smaller than for the other letters and is not statistically significant at the 5% level.[32]

Table 5 also provides a breakdown of the response in reported net rental income into gross rental income and deductions. As noted above, examining these responses separately is of interest, as previous research has found that taxpayers may react to third-party information on income by increasing reported deductions.

The table shows that Letters 3 and 4 cause a significant increase in reported gross rental income. We also find a significant increase in deductions in response to receiving Letter 4. However, the increase in deductions does not match the increase in gross rental income, and hence we observe an increase in reported net rental income overall. The effects on the level of net rental income are nevertheless slightly smaller in relative terms than those found for the propensity to report.[33] These results may be related to the nature of our third-party information, as explained above in connection with the graphical analysis. Another reason may relate to the fact that the individuals who start to report typically engage in small-scale renting, and hence may have relatively low rental income.

To provide a simple estimate for the direct revenue implications of our information treatment, we could consider sending Letter 4 to everyone in our base population, that is, all 116 668 individuals we identified as suspected landlords. This would yield 3.2 million euros more tax revenue, or 2.3% more tax revenue relative to how much taxes are remitted from rental income in the baseline.[34] These effects arise from a relatively inexpensive

[32]We have also tested for the statistical significance of differences between Letter 1 and Letters 2–4. The difference between Letter 4 and Letter 1 is statistically significant at the 5% level for the propensity to report rental income and gross rental income. The other coefficients of Letters 2–4 do not differ from Letter 1 at the 5% level.

[33]The proportionate effect of Letter 4 relative to the baseline mean is 2.4 percent for gross rental income, 2.5 percent for deductions and 2.3 percent for net rental income. We also estimated the proportionate effects of the treatment letters directly with Poisson Pseudo-Maximum Likelihood. The results are reported in Table A3 and they are similar to the proportionate effects based on linear regression.

[34]In the calculation we used the estimated effect of 93.15 euros more net rental income from receiving Letter 4, the number of individuals in the base population and flat capital income tax rate of 30%, as well as 4002 euros remitted rental income in the baseline on average. Admittedly, there may be some inaccuracy in this calculation. First, we have used the standard tax rate of 30% for rental income, while some individuals would pay a higher tax rate of 34%. Second, if the individual has made losses on their

combination of merging and analyzing data from different administrative registers, and sending letters by mail. We do not have access to information on the costs accrued by the Tax Administration to run the experiment, but our results can be utilized within the Tax Administration for a more detailed cost-benefit calculation. Our conjecture is that at least once the relevant procedures are in place, these types of interventions are likely on balance cost-effective. Moreover, the true revenue significance of the findings would likely be broader, if the findings are used to improve the targeting of audits in subsequent years; or if there are new innovations using similar procedures of building new sources of third-party information for other income items.

Table 5: Effects of treatment letters.

	Reporting rental income (0/1)	Gross rental income	Deductions	Net rental income
Letter 1	0.0110	19.08	2.338	16.74
	(0.00603)	(71.46)	(48.71)	(55.07)
Letter 2	0.0182	80.33	30.6	49.73
	(0.00660)	(71.83)	(52.45)	(49.98)
Letter 3	0.0193	129.7	8.935	120.8
	(0.00488)	(59.36)	(36.34)	(50.01)
Letter 4	0.0305	175.8	82.61	93.15
	(0.00433)	(46.69)	(31.08)	(34.82)
Baseline mean	0.785	7307	3304	4002

Notes: The table shows DiD estimates of the effects of treatment letters (ref. no letter) using data from years 2013-2016. Sample size is 289,363. Controls include individual fixed effects, treatment block by year dummies and additional enforcement measures related to the treatments. Standard errors clustered at postcode area level (263 clusters) are in parentheses.

Turning to heterogeneity analysis, Table 6 shows the results of an analysis in which different background characteristics are interacted with the letter dummies and the after dummy (Equation (3)). This allows us to study in a joint model which characteristic is the key driver behind the heterogeneous responses observed in the graphical event-study analysis above. Table A9 in Appendix A reports the overlap between background characteristics, and shows, for example, that those who did not report rental income in 2015 are more likely to own only one suspected rental apartment in 2015 than the average suspected landlord in the whole sample.

rental activity, those losses are under certain conditions deductible from other tax bases; such deductions are not taken into account in the calculation.

Panel A of Table 6 shows the effects on the propensity to report rental income and Panel B on the net rental income. The first column shows the main effects of the letters. The second column shows the results for suspected landlords who did not report any rental income in 2015 compared to those who did report. In the joint model it seems that this is the subgroup that responds to the treatment letters, that is, the only statistically significant estimates are for this group. This is also a subgroup where non-compliance appears more likely. Indeed, the baseline propensity to report in the control group in year 2016 in this subgroup is only about 32%. Receiving Letter 4 therefore causes about a 10 percentage point, or an over 30%, increase in the propensity to report rental income relative to the baseline in this group. The effect on net rental income is also large for this group, amounting to 20% of the baseline mean for Letter 4.

The third column shows no differential effects for suspected landlords who own only one suspected rental apartment. This result deviates from Figure 6. This finding can be explained by noting that many of those who did not report rental income in 2015 also have only one suspected rental apartment (Table A9 in Appendix A). The findings in columns 2 and 3 together suggest that prior reporting status, rather than the number of rental apartments per se, is the key driver behind the heterogeneous response between groups. Nevertheless, finding large responses among owners of a single apartment may provide useful information for the targeting of enforcement.

The fourth column shows that Letter 2 had a stronger effect on those younger than 40, suggesting that reminders and instructions are more effective for less experienced taxpayers. However, as the other letters do not have a differential effect by age, we would not like to draw strong conclusions regarding heterogeneity driven by age.

Table 6: Effects of treatment letters - interactions with background characteristics.

	Main effect of letters	No reported rental income in 2015 *Letter*After	One suspected rental apartment in 2015 *Letter*After	Age below 40 *Letter*After
Panel A: Reporting rental income (0/1)				
Letter 1	-0.0113	0.0347	0.0136	0.0231
	(0.0131)	(0.0177)	(0.0151)	(0.0177)
Letter 2	-0.00973	0.0540	0.00937	0.0415
	(0.0101)	(0.0173)	(0.0128)	(0.0165)
Letter 3	0.00465	0.0686	-0.00065	-0.00733
	(0.00725)	(0.0129)	(0.00832)	(0.0148)
Letter 4	0.00671	0.0966	0.00103	-0.00359
	(0.00670)	(0.0104)	(0.00793)	(0.0102)
Baseline mean	0.785	0.316	0.762	0.75
Panel B: Net rental income				
Letter 1	138.8	8.068	-173.5	169.7
	(224.1)	(117.7)	(233.8)	(130.6)
Letter 2	-73.47	114.7	77.38	178.3
	(175.0)	(99.87)	(187.6)	(140.6)
Letter 3	260.1	137.6	-225.2	110
	(163.4)	(72.30)	(164.1)	(121.1)
Letter 4	48.54	193.3	-14.48	82.86
	(125.7)	(55.61)	(128.2)	(86.09)
Baseline mean	4002	977	3118	3635

Notes: The table shows DiD estimates of the effects of treatment letters (ref. no letter) using data from years 2013-2016. Sample size is 289,363. The estimates are from a single model where the letter dummies are interacted with dummies for the characteristics of suspected landlords and the after dummy. The first column shows the main effect of treatment letters. Columns 2-4 show the interaction of characteristics of suspected landlords, letter dummies and the after dummy. Controls include individual fixed effects, treatment block by year dummies, additional enforcement measures related to the treatment letters, letter groups interacted with the after dummy, and landlord characteristics interacted with the after dummy. Standard errors clustered at postcode area level (263 clusters) are in parentheses.

4.2 Spillover effects in reporting

In this subsection, we discuss spillover effects in rental income reporting. We first focus on enforcement spillovers within ownership networks. Co-ownership is a natural type of close relationship where information sharing and hence spillovers may occur. As discussed in Section 2.2.1, we can use administrative data to identify individuals who co-own an apartment and have taken these networks into account in the experimental design.

Taxpayers in the base population included in the block design had 10,220 co-owners in 2015 (other than spouses). Table A6 in Appendix A shows the proportions and the number of co-owners exposed to the different treatments through their co-ownership network.

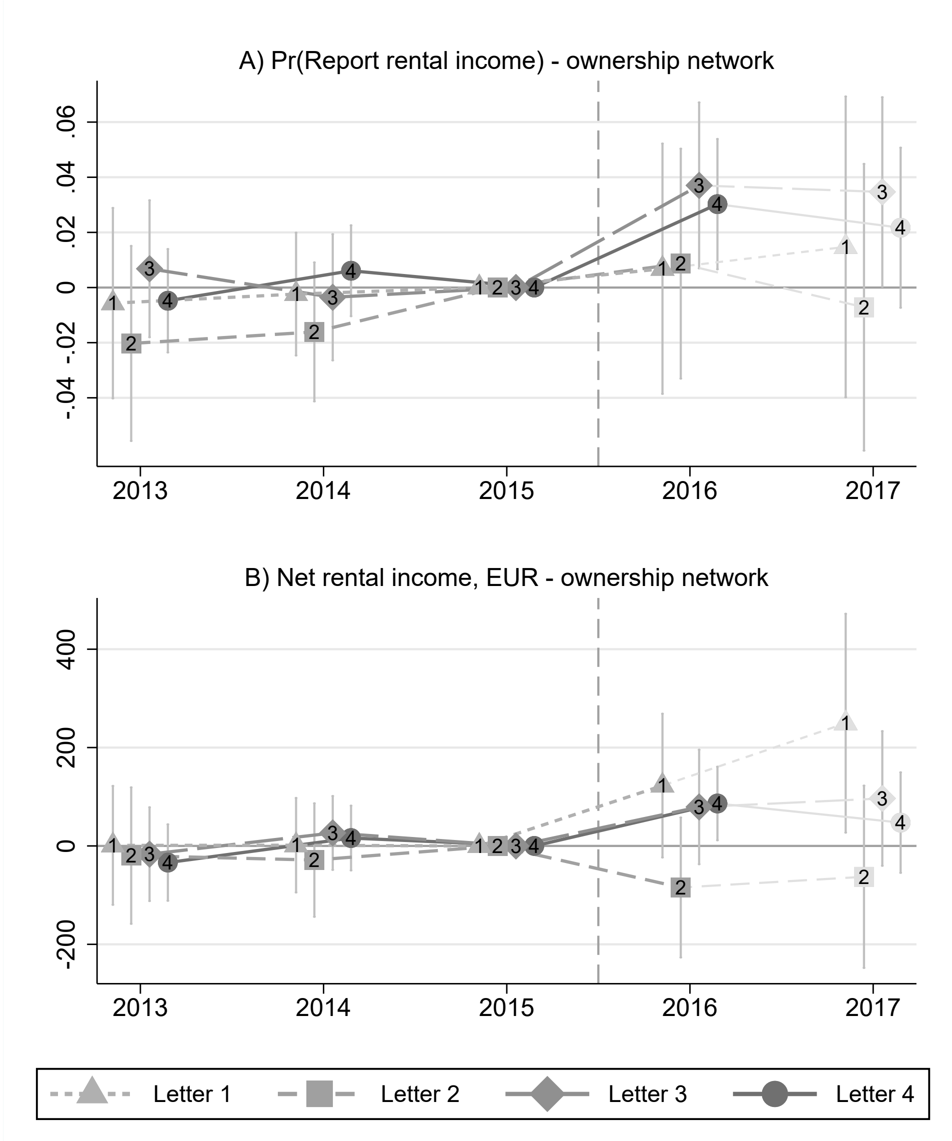
As above for the direct effects, we first provide a graphical analysis, plotting the $\beta_{j\tau}$-coefficients from Equation (1), estimated on the population of co-owners. We then turn to a DiD analysis, estimating Equation (2) for co-owners.

Figure 8 shows the event-study graph for reporting behavior within the ownership networks. The figure indicates significant reporting spillovers among co-owners. Panel A shows that the propensity to report rental income increases especially among suspected landlords whose co-owner received Letter 3 or 4. In panel B, the effect on reported net rental income is overall less clear, but the spillover effect of Letter 4 is statistically significant also for this outcome.

Table 7 presents the DiD estimates for the spillover effects of treatment letters on the reporting of rental income and net rental income. The table confirms that Letter 3 or 4 received by a co-owner led to an increase in the reporting of rental income. Interestingly, the point estimates are of a similar order of magnitude as in the main analysis (Table 5). For net rental income only Letter 4 created significant effects among co-owners.

The estimates reported in Figure 8 and Table 7 are potentially biased if there is heterogeneity in network degree (number of co-owners in the base population) and treatment effects are heterogeneous (Aronow and Samii, 2017). In our setting, variation in network degree is limited as 85 percent of co-owners have only one co-owner in the base population. Nevertheless, to address potential aggregation bias, we draw on Aronow and Samii (2017) and estimate a model, where observations in the co-owner sample are weighted by the inverse of the probability that they are indirectly exposed to a treatment letter. We can calculate these probabilities from Table 2. In addition, the number of co-owners in 2015 interacted with year dummies is added to control for network degree. The results of this robustness check that are reported in Table A8 in Appendix A are very similar to Table 7.

Figure 8: Reporting spillovers in ownership network.



Notes: Data includes individuals who owned apartments together with individuals in our main estimation sample (spouses excluded). The figures show regression coefficients on co-owner's treatment letter by year dummies (ref. no letter and year 2015). Controls include individual fixed effects. Vertical lines indicate 95% confidence intervals (clustering at postcode level). Data for 2017 is shown in gray as the experimental design no longer holds in that year.

When quantifying the significance of spillovers, it should be noted that not all suspected landlords in our main estimation sample have co-owners. Taking this into account, the size of the spillover effect, measured by the number of additional rental income tax reports arising due to the spillover, amounts to 14% of the direct effect. For total reported net rental income, the indirect effect through spillovers is 9% of the direct effect.[35]

Table 7: Spillovers in ownership network.

Dep. Var.	Reporting rental income (0/1)	Net Rental income
Letter 1	0.00984	110.3
	(0.0249)	(79.49)
Letter 2	0.0211	-66.93
	(0.0235)	(76.71)
Letter 3	0.0378	75.59
	(0.0145)	(59.72)
Letter 4	0.0274	77.24
	(0.0119)	(38.55)
Baseline mean	0.528	1308.5

Notes: The table shows DiD estimates for the effects of treatment letters (ref. no letter) using data from years 2013-2016. Sample size is 39,575. Co-owners include individuals who owned apartments together with individuals in our main estimation sample but not living in the same household. Controls include individual fixed effects. Standard errors clustered at postcode area level (707 clusters) are in parentheses.

Next, we utilize the block design with varying treatment intensity to analyze spatial enforcement spillovers (see Section 2.2.4). Such spillovers may arise through information exchange between landlords owning rental units in the same local housing market. Therefore, if such spillovers exist, they should be most pronounced in the high-intensity block where roughly two thirds of suspected landlords received a treatment letter.

Figure A3 in Appendix A shows that reporting behavior of those landlords in the high- and low-intensity treatment block who did not receive a letter did not differ from the reporting behavior of suspected landlords in the control block. That is, we do not find evidence of information spillovers within local rental markets.

Taken together our results on co-ownership networks and spatial spillovers indicate that it is important to take into account information transmission within networks when evaluating the overall effectiveness of tax enforcement measures. Our experiment with two different types of spillovers also highlights that spillovers may arise when there is a strong

[35]The total indirect effect of treatment letters is calculated by applying point estimates from Table 7 to the number of co-owners indirectly exposed to each letter. We perform a similar calculation for direct effects using estimates from Table 5. We then calculate the ratio of the total number of additional reports or total increase in net rental income due to spillovers to the direct effects.

presumption of connection (joint ownership) but not necessarily otherwise. Our findings thereby corroborate the view that enforcement spillovers are especially likely when there are close personal relationships.

4.3 Real effects

We next analyze whether the experiment influenced housing transactions or other portfolio choices of suspected landlords. Stricter enforcement increases an evader's effective tax rate from rental income and reduces the perceived after-tax return to rental housing. Stricter enforcement may then induce affected individuals to reduce the number of apartments held and instead invest in other assets.[36]

Figure A4 in Appendix A shows that the treatment did not affect housing transactions of suspected landlords. The result is perhaps not surprising: The effect on tax reporting is on average relatively small, leading to modest increases in effective tax rates. As housing is a lumpy investment, effects of relatively small changes in effective tax rates may in the end be difficult to detect. We therefore analyze also trade in other assets, namely listed stocks and shares in mutual funds. The idea is that any smaller changes in housing investments (e.g. reductions in money spent on improvements) that we cannot observe directly in our data, may show up as investments in other assets.[37] The results of this analysis, documented in Table A4 in Appendix A, do not reveal significant effects on portfolio choice.[38]

Together with the observation that there are no geographic spillovers in reporting, these results suggest that significant spillovers through the local housing market are unlikely.

5 Conclusions

In this paper we built a new source of third-party information that can be used in the enforcement of rental income taxation by combining information from different administrative registers. We carried out an RCT to study the effectiveness of this information

[36]Reporting practices differ from asset class to another. While rental income taxation is based on self-reported income, taxation of many other types of capital income such as income from dividends, mutual funds, or savings accounts involves third-party reporting.

[37]As data on these transactions are available to us only from year 2016, we cannot estimate DiD type models. Instead, we estimate a cross-sectional OLS model comparing letter groups in 2016.

[38]We also repeated the analysis with a subsample containing those who did not report rental income in 2015. The results are reported in Table A5, and they are similar to Table A4.

in tax enforcement. The information allowed to target enforcement measures at a relevant group of taxpayers, whose rental activity might otherwise go unnoticed. Notifying taxpayers of the use of this information in tax enforcement clearly increased reporting of rental income. The reactions are particularly large for suspected landlords who had not previously reported any rental activity, increasing the reporting rate compared to the control group by over 30%.

We also provided a comprehensive analysis of tax enforcement spillovers taking into account informational spillovers within ownership networks and spatial spillovers within local rental markets. Analyzing spillovers is crucial because taking into account direct effects only may understate the effects of enforcement. Typically, the analysis of spillovers is difficult because networks, where spillovers may be expected to arise, may be unobserved or not well-defined. Rental housing is often co-owned which creates well-defined networks between owners. Taking into account spillovers within ownership networks increases the estimated effects of enforcement by 14 % on the extensive margin of reporting, and by 9 % for the amount of net rental income reported. However, we do not find evidence of spatial spillovers within local rental markets. These findings are important in highlighting that spillovers may be significant within well-defined networks even if they do not arise in other settings: while both geographic proximity and co-ownership can give rise to information sharing, co-ownership clearly constitutes a more closely connected network. Co-ownership may be important in many other contexts and a similar approach can be applied to estimating enforcement spillovers, for example, within networks of small business owners.

A further contribution of the paper was to provide an example of analyzing potential real effects on tax enforcement on market allocations, using administrative data on asset market transactions. We did not find clear effects on housing market transactions, nor on the ownership of other assets. As real effects of tax enforcement are understudied, this type of analysis provides an important avenue for further research also in other tax enforcement contexts where data on indicators of real economic responses are available.

As for the policy implications of our findings, the first and primary implication of our results is to highlight the possibilities of more effective tax enforcement through creating novel third-party information by combining information from different administrative registers. This type of information can provide a signal of economic activity to the tax authority, even when the taxpayer has not reported any income from such activity. Overall, the intervention we implemented created significant increases in tax revenue through a relatively low-cost combination of merging data from administrative registers and sending letters by mail to taxpayers. Moreover, the true revenue significance of the findings is likely broader, as the findings can be used to improve the targeting of audits in subsequent years or if there are new innovations using similar procedures of building new sources of third-party information for other income items. A natural conjecture is to systematically go through income items not currently subject to third-party reporting, and assess whether relevant information may instead be created by novel combinations of register data. For example, in our setting the information we used relates to ownership and occupancy of apartments, while information on rent levels was not available. More broadly, there may be multiple applications for different income sources e.g. related to self-employment income that are currently not subject to third-party reporting.

Our results may also provide several more nuanced lessons for enforcement policy. First, our results suggest that an effective enforcement strategy might combine the use of two types of information: third-party information that provides a signal of potential income, as well as information on previous tax reporting behavior (in our case no report in the previous year). Second, the tax authorities might consider publicizing at least the type of information used in tax enforcement, in contrast to the tendency of many tax authorities to rather hide this information; c.f. also Slemrod (2016) for a brief discussion on secrecy related to tax audit rules. In our case, letting taxpayers know that the tax authority used information on the suspected ownership of rental apartments in tax enforcement significantly improved compliance.Third, our results highlight that information spillovers significantly strengthen the effectiveness of enforcement measures, and hence may affect the associated cost-benefit calculus as well as the optimal targeting of enforcement.

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Tax Compliance in the Rental Housing Market: Evidence from a Field Experiment

Online Appendix

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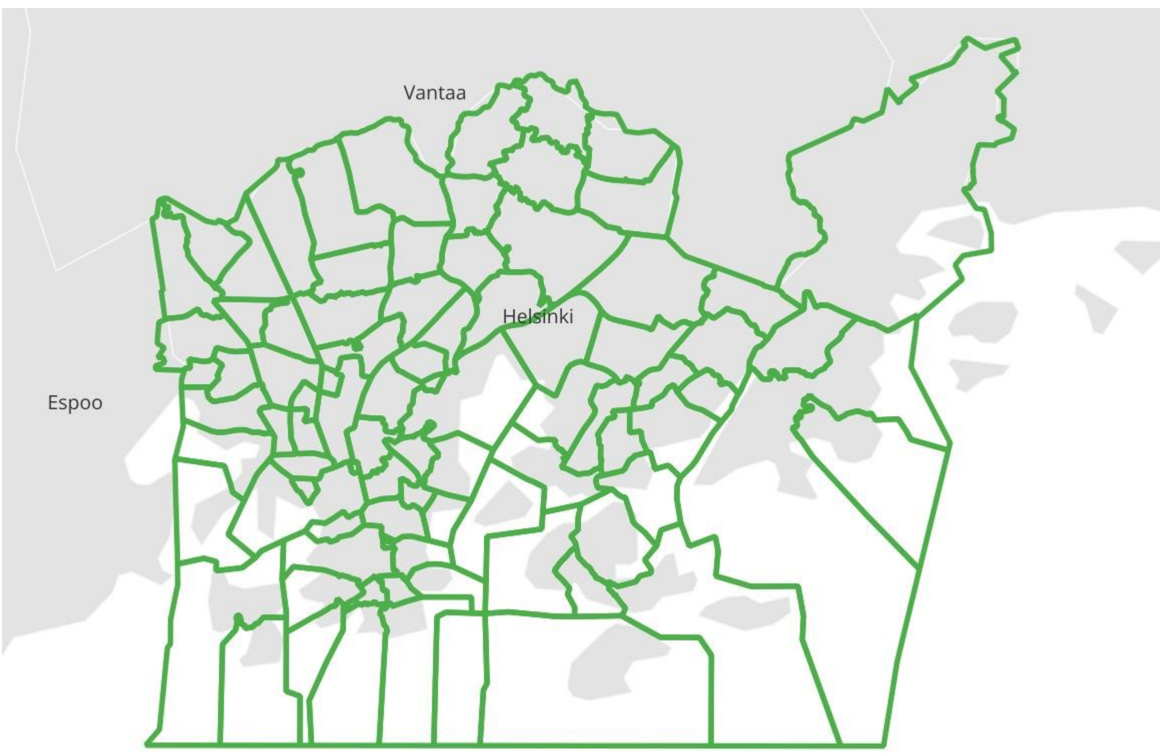
A Additional tables and figures

Figure A1: An illustration of the block design for Helsinki.



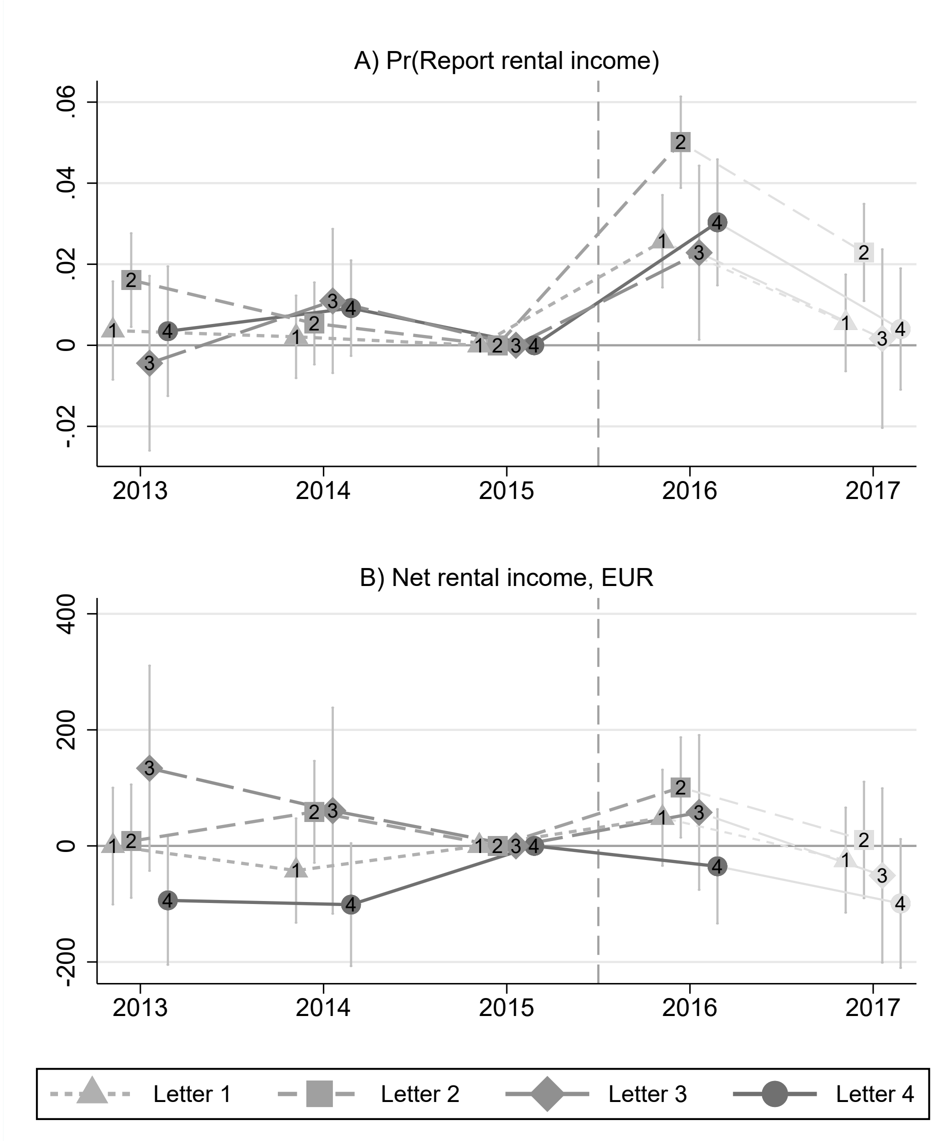
Notes: Map shows zip-code areas in Helsinki that are used to create the high-, low-intensity blocks as well as control blocks.
Source: Contains data from the Map Service of the City of Helsinki Regional Divisions database (9/2025) and data from the National Land Survey of Finland Division into Administrative Areas Database (1/2023).

Table A1: Summary statistics for key variables 2013–2017.

	Obs	Mean	Std.Dev.	Median	1st percentile	99th percentile
Reported rental income (0/1)	359699	0.737	0.440	1	0	1
Net rental income	359699	3582.4	4818.3	2410	-2039.4	22998
Gross rental income	359699	6688.3	7809.5	5165	0	38031.0
Deductions	359699	3105.9	4155.0	2080.66	0	20171.5
Apartments bought	356415	0.0745	0.2811	0	0	1
Apartments sold	356415	0.0604	0.2524	0	0	1
Suspected rental apartments in 2015	359699	1.204	0.589	1	1	4
Age	359699	57.9	16.0	59	25	91
Female	359699	0.532	0.499	1	0	1

Notes: Table shows summary statistics for suspected landlords in our control and treatment blocks for years 2013-2017.

Figure A2: Reporting of rental income in letter groups in areas outside the block design.



Notes: Figures show regression coefficients on treatment letter by year dummies (ref. no letter and year 2015) for data outside the block design. Controls include individual fixed effects, year fixed effects and additional enforcement measures related with the treatments. Vertical lines indicate 95% confidence intervals (clustering at postcode level). Data for 2017 is shown in gray to indicate that the experimental design no longer holds in that year.

Table A2: Effects of treatment letters in areas outside the block design.

Dep. Var.	Reporting rental income (0/1)	Net rental income
Letter 1	0.0239	61.75
	(0.00576)	(43.97)
Letter 2	0.0433	79.41
	(0.00610)	(43.84)
Letter 3	0.0210	2.151
	(0.0107)	(80.14)
Letter 4	0.0275	33.03
	(0.00844)	(54.21)
Baseline mean	0.744	2913.7

Notes: Table shows DiD estimates of the effects of treatment letters (ref. no letter) for data outside the block design using data from years 2013-2016. Sample size is 140,046. Controls include individual fixed effects, year dummies and additional enforcement measures related to the treatment letters. Standard errors clustered at postcode area level (1022 clusters) are in parentheses.

Table A3: Poisson estimates for proportionate effects of treatment letters.

	Gross rental income	Deductions	Net rental income
Letter 1	0.00496	0.00147	0.00767
	(0.0105)	(0.0150)	(0.0129)
Letter 2	0.012	0.00795	0.00627
	(0.0105)	(0.0154)	(0.0129)
Letter 3	0.0163	0.000888	0.0202
	(0.00891)	(0.0127)	(0.0111)
Letter 4	0.0228	0.0267	0.0196
	(0.00690)	(0.00988)	(0.00835)
Baseline mean	7307	3304	4002

Notes: Table shows Poisson Pseudo Maximum Likelihood DiD estimates of the proportionate effects of treatment letters (ref. no letter) using data from years 2013–2016. Sample size is 289,363. Controls include individual fixed effects, treatment block by year dummies and additional enforcement measures related to the treatments. Standard errors clustered at postcode area level (263 clusters) are in parentheses.

Table A4: Effects of treatment letters on investment portfolio.

	Apartments bought	Apartments sold	Buy shares 0/1	Sell shares 0/1	Buy shares EUR	Sell shares EUR
Letter 1	-0.00469	-0.00029	-0.00267	-0.00343	-438.6	-18.31
	(0.00473)	(0.00563)	(0.00591)	(0.00737)	(395.2)	(575.5)
Letter 2	-0.00185	0.00371	0.00111	-0.00165	91.08	566.9
	(0.00502)	(0.00607)	(0.00600)	(0.00740)	(432.8)	(617.1)
Letter 3	0.00369	0.00049	-0.00273	-0.00244	-523.3	259.9
	(0.00438)	(0.00519)	(0.00484)	(0.00603)	(325.4)	(487.4)
Letter 4	3.57E-05	0.0018	0.000776	-0.00539	-182	424.1
	(0.00330)	(0.00357)	(0.00387)	(0.00479)	(281.7)	(399.0)
Baseline mean	0.0602	0.0718	0.133	0.231	3997.9	7582.4
N	286924	286924	71370	71370	71370	71370

Notes: Columns 1–2 show DiD estimates for the effects of treatment letters (ref. no letter) using data from years 2013–2016. Controls include individual fixed effects, treatment block by year dummies and additional enforcement measures related to the treatment letters. Columns 3–6 show OLS estimates of letter dummies using data from 2016 on transactions of stocks in listed companies and shares in mutual funds. Standard errors clustered at postcode area level (263 clusters) are in parentheses.

Table A5: Effects of treatment letters on investment portfolio – those who did not report rental income in 2015.

	Apartments bought	Apartments sold	Buy shares 0/1	Sell shares 0/1	Buy shares EUR	Sell shares EUR
Letter 1	-0.0106	0.00673	-0.0115	0.00248	-1031.8	-186.6
	(0.0108)	(0.0117)	(0.00986)	(0.0138)	(593.1)	(908.1)
Letter 2	-0.00042	0.00106	0.00157	0.0146	872.5	1661.3
	(0.00900)	(0.0105)	(0.0104)	(0.0140)	(1006.0)	(1344.3)
Letter 3	0.00138	-0.00169	-0.0102	-0.0117	-1046.3	-232.3
	(0.00881)	(0.0107)	(0.00826)	(0.0110)	(440.4)	(775.5)
Letter 4	-0.00768	-0.00374	-0.00011	-0.0044	51.09	765.5
	(0.00582)	(0.00716)	(0.00681)	(0.00900)	(536.3)	(710.3)
Baseline mean	0.0529	0.0662	0.0966	0.195	2754.1	5479.4
N	75869	75869	19029	19029	18922	18922

Notes: Columns 1–2 show DiD estimates for the effects of treatment letters (ref. no letter) using data from years 2013–2016. Controls include individual fixed effects, treatment block by year dummies and additional enforcement measures related to the treatment letters. Controls include individual fixed effects, and additional enforcement measures related to the treatment letters. Columns 3–6 show OLS estimates of letter dummies using data from 2016 on transactions of stocks in listed companies and shares in mutual funds. Standard errors clustered at postcode area level (263 clusters) are in parentheses.

Table A6: Potential exposure to treatment spillovers in ownership network.

	Share of co-owners	Count
Letter 1	0.046	475
Letter 2	0.047	483
Letter 3	0.091	935
Letter 4	0.172	1760
No letter	0.645	6588

Notes: Data includes people who owned apartments in 2015 together with people in our main estimation sample (the block design) but living in a different household. The table shows the proportions and the number of these co-owners potentially exposed to different treatments through their co-owners.

Table A7: Summary statistics for key variables 2013–2017 – co-owners.

	Obs	Mean	Std.Dev.	Median	1st percentile	99th percentile
Reported rental income (0/1)	48982	0.466	0.499	0	0	1
Net rental income	48982	1267.5	1980.7	0	0	8385.6
Gross rental income	48982	2467.9	3957.0	0	0	14880.0
Deductions	48982	1101.3	2456.9	0	0	8590.1
Suspected rental apartments in 2015 (main owner)	48982	0.152	0.433	0	0	2
Suspected rental apartments in 2015	48982	1.195	0.619	1	1	4
Age	48982	53.1	18.9	53	19	111
Female	48982	0.530	0.499	1	0	1

Notes: Table shows summary statistics for individuals in our control and treatment blocks for years 2013–2017 for co-owners of suspected landlords in the main estimation sample (block design).

Table A8: Spillovers in ownership network – WLS with additional controls.

Dep. Var.	Reporting rental income (0/1)	Net Rental income
Letter 1	0.0209	137.1
	(0.0255)	(71.10)
Letter 2	0.0248	-76.2
	(0.0246)	(76.07)
Letter 3	0.0394	71.01
	(0.0156)	(60.37)
Letter 4	0.029	71.2
	(0.0126)	(38.60)
Baseline mean	0.528	1308.5

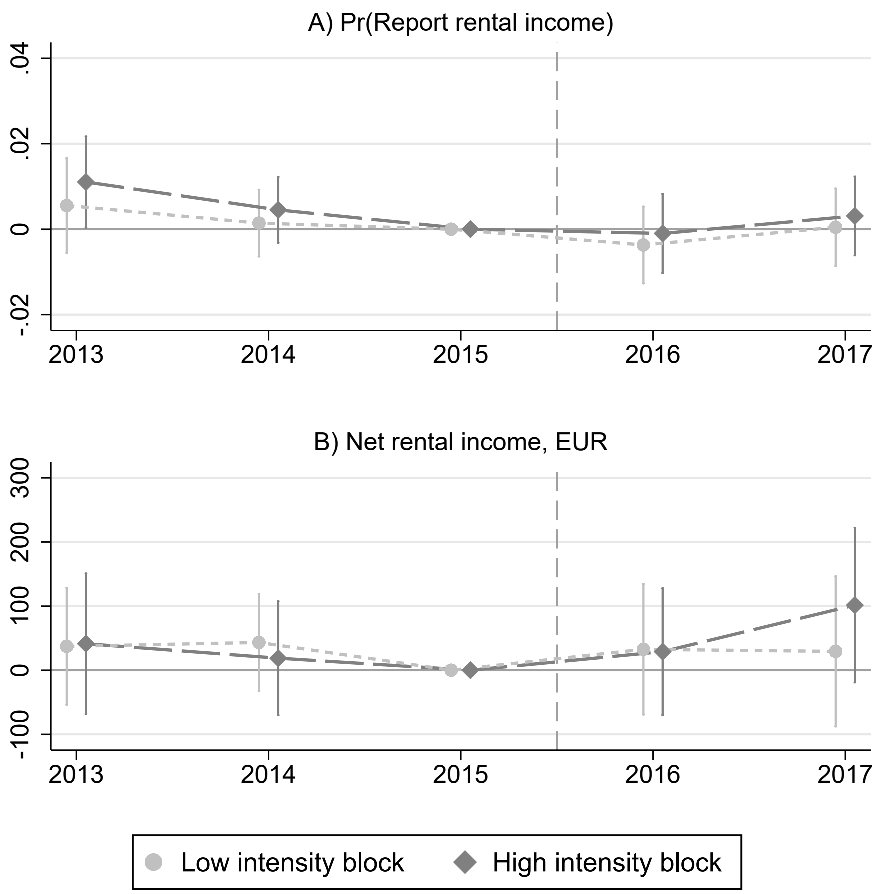
Notes: Table shows DiD estimates for the effects of treatment letters (ref. no letter) using data from years 2013-2016. Sample size is 39,575. Co-owners include individuals who owned apartments together with individuals in our main estimation sample but not living in the same household. Observations are weighted by the inverse of the probability of receiving an indirect treatment. Controls include individual fixed effects and the number co-owners in the base population in 2015 interacted with year dummies. Standard errors clustered at postcode area level (707 clusters) are in parentheses.

Table A9: Overlap between subgroups.

	Share: no reported rental income in 2015	Share: one suspected rental apartment in 2015	Share: age below 40 in 2015	N
All	0.265	0.854	0.168	71,845
Subroup				
No reported rental income in 2015	1	0.948	0.246	19,029
One suspected rental apartment in 2015	0.294	1	0.181	61,374
Age below 40 in 2015	0.387	0.921	1	12,077

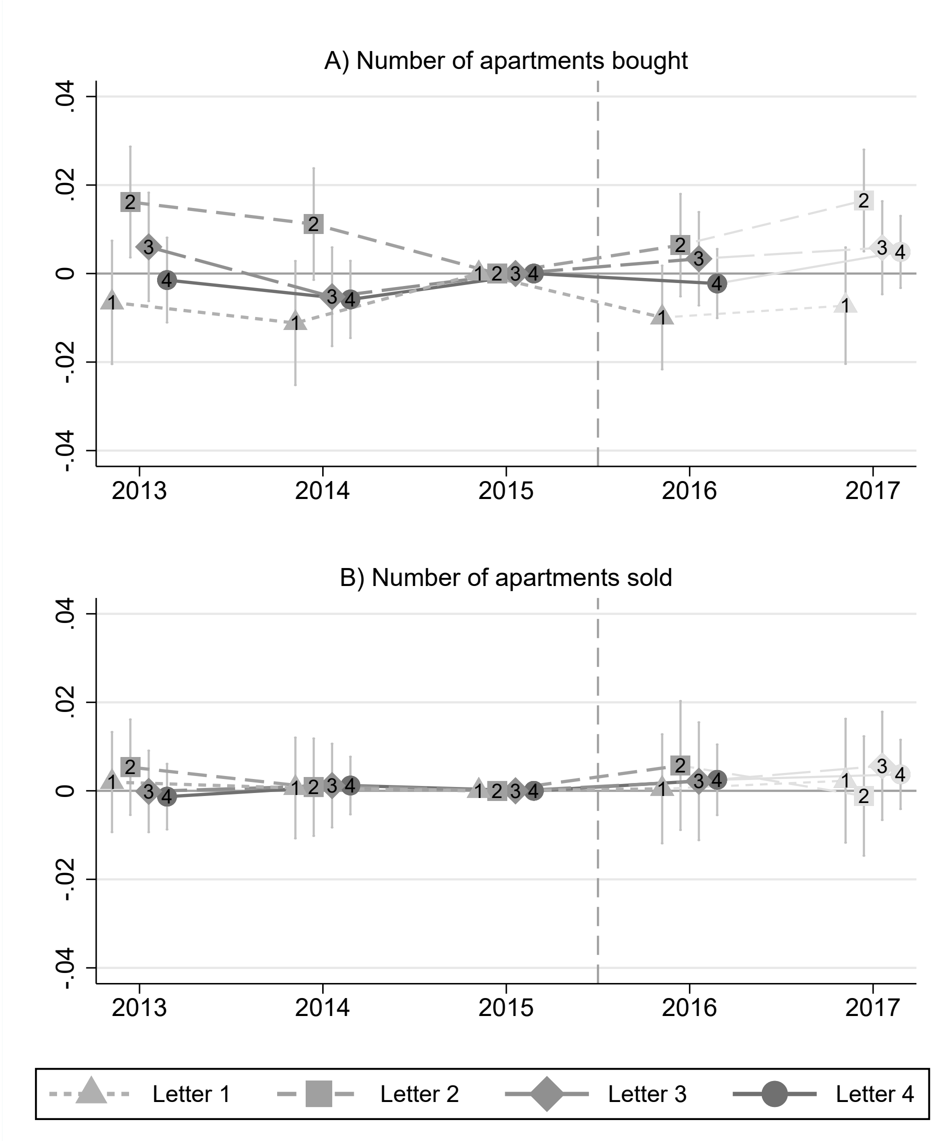
Notes: Table shows the share of suspected landlords belonging to different subgroups in the main estimation sample in 2016. The shares are reported by subgroups to analyze overlap between them.

Figure A3: Reporting spillovers in the local rental market



Notes: Figures show regression coefficients on treatment block by year dummies (ref. control block and year 2015). Controls include individual fixed effects, treatment letter by year dummies and additional enforcement measures related to the treatments. Vertical lines indicate 95% confidence intervals (clustering at postcode level).

Figure A4: Housing transactions in letter groups



Notes: Figures show regression coefficients on treatment letter by year dummies (ref. no letter and year 2015). Controls include individual fixed effects, treatment block by year dummies and additional enforcement measures related to the treatments. Vertical lines indicate 95% confidence intervals (clustering at postcode level).

B Rental income form and treatment letters

VERO SKATT

e-File
tax.fi/mytax

7H RENTAL INCOME
RENTAL APARTMENTS

Tax Administration
P.O. Box 700
00052 VERO

Use this form to report any rental income earned from renting out an apartment in a housing company. If you are a co-owner of the apartment, report only your portion of the rental income and the related expenses.

Do not deduct interest on this form; report it as interest on expenses incurred in acquiring or maintaining income. More information is available in the instructions for filling in the form.

Do not attach any receipts to the form; place them somewhere for safekeeping. The Tax Administration will ask you for them, if necessary. For further tax instructions concerning rental income, go to tax.fi.

Report rental income from real estate rented out on form 7K and rental income from other property on form 7L. Use form 16B to report rental income earned abroad.

1 Personal details and tax year

Name	Personal identity code or business ID	Tax year

2 Apartment in a housing company I

Name of housing company or real estate holding company	Business ID	Apartment number

Personal identity code or business ID of the tenant	Name of tenant

Tenancy period (ddmmyyyy–ddmmyyyy)					
	—		Share of ownership in apartment		(%)

30111

	€	c
2.1 Your share of gross rental income per year		
2.2 Monthly maintenance charges and water charges paid per year (only your share)		
2.3 Other costs per year (only your share)		
2.4 Net taxable rental income per year (positive difference between income and expenses) +		
2.5 Net taxable loss from rental operations per year (negative difference between income and expenses) -		

2 Apartment in a housing company II

Name of housing company or real estate holding company	Business ID	Apartment number

Personal identity code or business ID of the tenant	Name of tenant

Tenancy period (ddmmyyyy–ddmmyyyy)					
	—		Share of ownership in apartment		(%)

	€	c
2.1 Your share of gross rental income per year		
2.2 Monthly maintenance charges and water charges paid per year (only your share)		
2.3 Other costs per year (only your share)		
2.4 Net taxable rental income per year (positive difference between income and expenses) +		
2.5 Net taxable loss from rental operations per year (negative difference between income and expenses) -		

VEROH 3011e 1.2018

Date	Signature	Telephone number

The information entered on this form will be read by computer, by optical character recognition. The computer system does not process anything you may have written outside the spaces. Only fill in forms printed out from tax.fi, do not use photocopies. Photocopies may have inferior quality, making optical character recognition difficult.

VERO SKATT

NOTICE

Finnish Tax Administration PO Box 325 FI-
00052 Vero, Finland

Ref.

Check your pre-completed tax return

You have received a pre-completed tax return containing information on your earnings and deductions in 2015. Review the tax return with care. If the information is correct and nothing is missing, you need not do anything. If the information is incorrect, or some pieces of information are missing, you must correct or supplement the tax return. Information to be supplemented may include rental income, travel expenses between your home and place of work, or tax credit for household expenses, for example.

You can supplement and correct the information in the pre-completed tax return in the *Tax return online* service (vero.fi/veroilmoitus). The service will remain open until the tax return deadline indicated on your tax return. If you supplement your tax return online, you need not use the tax return form or its appendix forms.

If you use a paper form to submit your tax return by regular post, you must also send the required appendix forms. For example, you must use form 7H to announce your rental income from a unit in a housing company and form 14A to get your tax credit for household expenses. The required appendix forms are listed in the instructions on how to complete the tax return. Don’t forget to enter the required pieces of information in the correct part of the tax return form in addition to the appendix forms.

For more information, please visit vero.fi/henkilöasiakkaat > Veroilmoitus (Individual taxpayers > Tax return) or call the service number specified in your pre-completed tax return.

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If you received rental income in 2015, announce the rental income and related expenses. The most common expenses to be deducted from rental income include maintenance charges, annual repair costs and real estate tax. If you received rental income from several sources (such as a unit in a housing company and a summer home), you must separately announce the income and expenses of each property. If you own a unit in a housing company with another person, you must only announce the share of rental income and expenses corresponding to your share of ownership. Calculate the amount of taxable rental income by deducting the expenses from the rental income.

Example of calculating rental income
The taxpayer owns one unit in a housing company, which they rented out for the entire year of 2015, with the rent being EUR 1,000 per month. The taxpayer/landlord paid a maintenance charge of EUR 250 per month. Other expenses related to the renting of the apartment totalled EUR 1,500. The taxable rental income is the difference between the rental income and expenses, or 12 x EUR 1,000 - 12 x EUR 250 - EUR 1,500 = EUR 7,500. Hence, the taxable rental income is **EUR 7,500**.

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The Finnish Tax Administration is boosting the monitoring of tax to be paid for rental income. Hence, additional information on rental income and related expenses will be requested more often than before. The additional information is needed for the Tax Administration to verify that the rental income and expenses specified in your tax return are correct.

If you received rental income in 2015, you must announce all rental income you received and related expenses. If necessary, the Tax Administration can request receipts or other additional information on your rental income and expenses. If we need additional information on your rental income, you will receive a request to supplement your tax return after the tax return deadline. Do not enclose your receipts with your tax return, however; the Tax Administration will separately request them if necessary.

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The rental income information for 2015 will be compared to information on landlords' property ownership more comprehensively than before. Special attention will be paid to tax returns where the rental income information is not consistent with the property ownership information. According to the information available to the Tax Administration, you own at least one unit in a housing company, and the apartment may have been rented out in 2015.

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