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ABSTRACT

We study rental income tax compliance using a large-scale randomized field experiment and register data with third-party information on the ownership of apartments. We analyze the responses of potential landlords to treatment letters notifying them of stricter tax enforcement. We also study spillover effects of tax enforcement within the household and between landlords within local rental markets. We find an increase in reported income after an enforcement letter is sent to landlords. We also find positive reporting spillovers between spouses, as well as between landlords in a subgroup of more likely evaders.

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1. Introduction

Rental income is an interesting form of taxable income in that it is largely lacking in third party reporting. There are reasons to believe that this might create opportunities for tax evasion.[2] According to a recent study, rental property is the most heavily taxed type of asset in many OECD countries (OECD 2018). Further, the ownership of rental units tends to be widespread across households, which makes different types of enforcement measures costly for tax authorities.

There is increased awareness of the potential consequences of evasion for the efficiency of taxation. For instance, in the U.K., it is estimated that a significant tax loss is likely in the rental market.[3] In addition, a recent report concluded that roughly half of the landlords in one borough of London do not report their rental income.[4]

We analyze tax enforcement and compliance in the rental housing market using a large scale field experiment in Finland and register data on the entire population of Finnish households owning housing units. In the experiment, a randomly selected subset of potential landlords was subjected to stricter enforcement of the rental income tax. The affected landlords were notified of the enforcement measures through letters sent by the Tax Authority.

We also analyze whether intensified enforcement has spillover effects beyond those individuals who receive a treatment letter. Spillover effects in tax reporting may arise if the information on the treatments spreads between landlords, or within the family. Using a randomized block design similar to Crépon et al. (2013), we analyze spillover effects from intensified enforcement across landlords within local rental markets.

Analyzing spillovers is essential for obtaining an accurate understanding of the overall implications of stricter enforcement. Crépon et al. (2013) ague that in the context of labour market policies, ignoring spillovers may severely bias our understanding of the effects of a policy if making some subgroup of individuals more employable has a negative externality on other jobseekers. In the context of tax enforcement, ignoring spillover effects may lead to misleading conclusions about the effects of intensified enforcement. Depending on the sign of the spillover effect, the effects of tax enforcement may be understated or overstated if one only looks at the direct effect. Ignoring some of these responses also leads to biased estimates of the compliance gap (i.e. the amount of tax revenue that can be recouped by more intensive enforcement) (Gemmell and Hasseldine 2013, Slemrod 2017).

A few earlier papers have studied regional enforcement spillovers between individuals in the context of TV license fee collection (Rincke and Traxler 2011, Drago et al. 2015) and income tax filing (Meiselman 2018). Pomeranz (2015), Boning et al. (2018) and Brockmeyer et al. (2018) analyze

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[2] Several recent studies have analyzed the role of third-party reporting in other cases and have found it to be an important factor in understanding tax evasion. See e.g. Kleven et al. (2011) and Harju et al. (2017).

[3] "Tax evasion in 2014 and what can be done about it"
http://www.taxresearch.org.uk/Documents/PCSTaxGap2014Full.pdf.

[4] "Half of landlords in one London borough fail to declare rental income", The Guardian, August 13, 2017.

enforcement spillovers in firm networks. We contribute to this literature by analyzing both regional spillovers as well as spillovers between family members.

To study the above questions, our study utilizes a large scale field experiment together with register data on Finnish households owning housing units. The register data enables identifying apartments occupied by someone else than the owner (or a family member); the owners of such apartments are classified as potential landlords in our study.

Our experiment comprised several treatments that allow us to disentangle different determinants of non-compliance. First, ignorance (e.g. about reporting requirements concerning income vs. expenses) and compliance costs may affect the level of non-compliance, and one of our treatments aimed at reducing these costs, through providing simplifying information on the tax filing procedure and requirements. Our second treatment signaled a general increase in enforcement intensity to the recipients. Finally, our third treatment informed the taxpayers of the use of third-party information in tax enforcement, and allows us to assess the effectiveness of third-party information in deterring tax evasion, compared with a general increase in enforcement intensity. Whereas the importance of third-party information has been acknowledged in earlier literature (e.g. Slemrod 2007, Kleven et al. 2011), literature utilizing randomized variation in third-party information is scarce.[5] Harju et al. (2017) have implemented randomized variation in the salience of third-party information, albeit in a quite different context, namely tax evasion on car imports.

Overall, despite likely opportunities for evasion, prior literature on rental income tax evasion is very scarce. Wenzel and Taylor (2004) carried out an experiment where landlords were asked to itemize expenses in tax returns, which led to a 5-7.5% reduction in reported expenses compared to receiving an information letter only.

We find that the treatment letters had an effect on the reporting behavior on potential landlords. The effect is most pronounced on the extensive margin (i.e. the number of individuals that report a positive amount of rental income), while effects on the intensive margin (i.e. the euro amount of rental income reported) are smaller. The strongest treatment, notifying taxpayers of the use of third-party information in tax enforcement, has the strongest effect. We also find some, albeit somewhat weak, indication of spillover effects in tax reporting, concentrated on areas where more than half of the potential landlords received a treatment letter. In particular, we find spillover effects in reporting behavior between spouses. Both direct and spillover effects are stronger for potential landlords who did not report any rental income in the previous year; a group that can be considered as more likely evaders.

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[5] In Kleven et al. (2011), variation in 3rd party reporting comes from certain types of income being subject to 3rd party reporting, while others (notably self-employment income) are not. In studying firm responses to an audit experiment, Pomeranz (2015) compares those line-items in the VAT declaration of firms that are covered by the paper trail (transactions between two firms) to line items that are not (sales to final consumers). Naritomi (2016) compares retail transactions (where the extent of 3rd party information increased due to a campaign that incentivized consumers to send in their receipts to the authorities) and wholesale transactions (not affected by the campaign). In none of these studies was 3rd party information in itself subject to randomization.

The paper is organized as follows. In the next section, we describe the institutional setting. Section 3 presents the experiment. In section 4, we present and discuss our preliminary findings. Section 5 provides concluding remarks.

2. Institutional background

Overall more than 60% of Finnish households live in owner-occupied housing. The share is lower in large cities, for instance, in the capital city of Helsinki the share of owner-occupiers is slightly less than 50%. The rental market can be divided into social housing and the private rental market. The social housing sector with regulated rents constitutes almost 50% of the rental markets. The social housing units are owned by municipalities and non-profit organizations that are not subject to regular capital income taxation.

In the private rental market, rents can be freely set and the rental income is subject to a 30% capital income tax rate (34% if taxable income exceeds an annual threshold of 30,000 euros). Roughly half of the private rental units are owned by individual households. The rest are owned by large institutional landlords.

The rental income tax is a non-negligible source of tax revenue, with rental income net of expenses amounting to 1.4 billion euros, and the corresponding tax revenue about 400 million euros (or 0.5% of total tax revenue) in Finland in 2013. Tax compliance related to rental income has also wider relevance in relation to the overall efficiency of the capital income tax system. This is especially important if tax evasion opportunities vary between different types of capital income.

In the analysis, we focus on rental apartments owned by households. Figure 1 illustrates the nature of the phenomenon under study. The figure shows the distribution of the total number of individuals in 2013 with "extra" flats (i.e. owning one or more apartments in addition to the one that they live in themselves), that are potentially rented out. The figure also shows the number of individuals reporting rental income, as well as tax revenue, by the number of "extra" flats owned by the individual. For example, out of those individuals owning one such flat, only roughly a quarter actually reported some rental income to the tax authority. On the other hand, individuals with larger numbers of owned flats are also more likely to report rental income. These findings have some significance for tax enforcement, and the prevalence of small-scale renting implies that such activity is important in terms of revenue.

A priori, there are many potential reasons for the descriptive patterns shown in Figure 1. Firstly, people may hold extra flats as second homes or they may be occupied by close relatives free of rent. The likelihood of renting out at least one flat probably increases with the number of extra flats. Secondly, reporting behavior may be correlated with the number of extra flats. It may be the case that non-professional, small-scale landlords are not familiar with tax filing requirements, and/or find the compliance cost of tax filing to be too high. On the other hand, they may judge the likelihood of being caught with evasion to be particularly low. Our empirical design enables us to disentangle the

relative importance of these factors. Finally, it is also possible that some of these individuals are not landlords due to inaccuracies in the data.

Turning next to the tax-filing procedure, prepopulated income tax returns are sent out to taxpayers in late April each year. They contain information on incomes that are subject to third-party reporting. Thereafter, the taxpayer is required to submit a revised return to the tax authority if any income information is missing from the prepopulated 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. Rental income is reported on a separate form, and income and deductible expenses have to be reported separately.
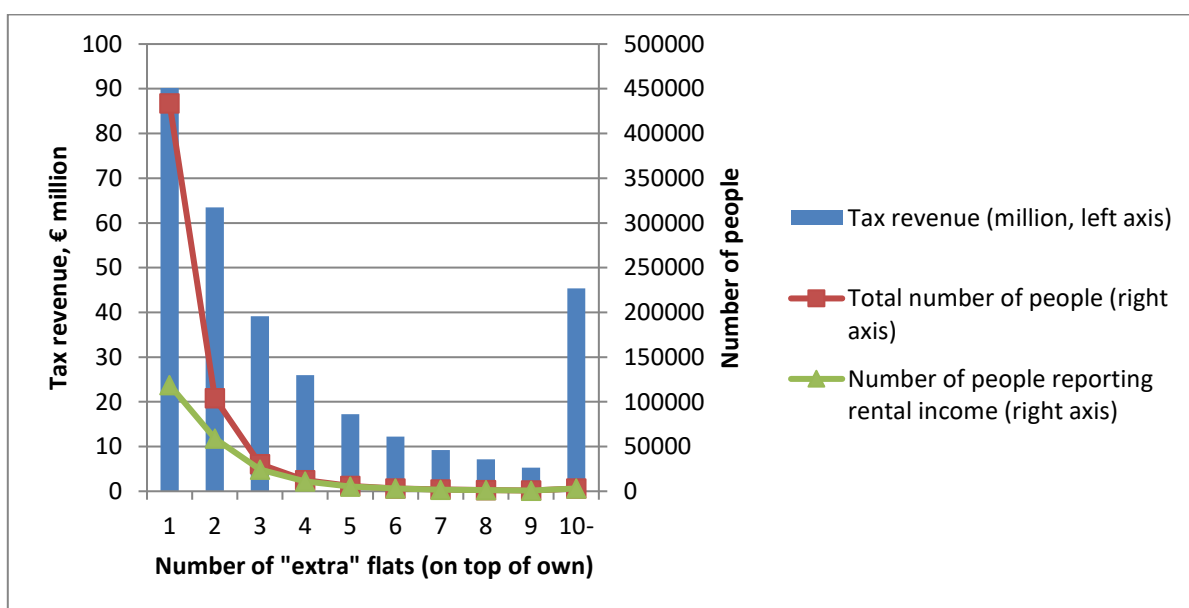
Figure 1. Rental income tax reporting by potential landlords

3. The experiment

The base population for the experiment was formed as follows: Using personal identification codes that uniquely identify individuals across different government registers, we can identify flats that are occupied by someone else than the owner or their family member. Individuals owning at least one such flat are classified as potential landlords in our data and they form the base population used in the analysis.

The treatment letters were sent out in April 2016, and we first study their effects on subsequent rental income tax reporting that took place in spring 2016. The letters were sent out just prior to the time when taxpayers received their prepopulated income tax returns. Reporting concerned income earned in 2015, and therefore any effects that we find in this analysis are pure reporting responses; any real responses are ruled out by the timing of the experiment. However, we also

provide results on income tax reporting in spring 2017. In this case, any effects that we find on rental income reported in 2017 (concerning income earned in 2016) may incorporate both reporting and real responses.[6]

More specifically, we implemented four treatments: 1) A neutral reminder to file tax returns; 2) Information on how to file rental income; 3) Letter on intensified enforcement of rental income taxation; 4) Letter on intensified enforcement of rental income taxation and a mention of third-party information on ownership of dwellings. All treatment letters (2) – (4) contained also the neutral information provided in treatment letter (1), and therefore group (1) served as a baseline for the actual treatments of interest. Summary statistics of all key variables in the data are reported in the Appendix.

Table 1 describes our experimental design. We used a randomized block design, similar to the design in Crépon et al. (2013), to assign individuals randomly to the four treatment groups. To be able to analyze potential spatial spillovers of the treatments, we identified municipalities with a reasonably dense rental market and divided them into 90 geographical units (parts of town, defined by zip codes). We assigned these areas randomly into three blocks with varying intensity of treatment: i) control block where no letters were sent; ii) low intensity block where 24 percent of potential landlords in the base population were sent a letter; iii) high intensity block where 62 percent of potential landlords received a letter. In addition, the share of the stronger treatment letters (3 and 4) was higher in the high intensity block. Figure 2 shows an illustration of the block design for Helsinki, the largest municipality in our data. The blocks in the figure are randomly assigned to control, low intensity or high intensity groups (for data confidentiality reasons, we are not able to show which ones).

Table 1. Experimental design.

	Not in blocks	Control block	Low intensity block	High intensity block	Total
No letter	28178	19208	21320	14995	83701
Letter 1	4779	0	1713	2502	8994
Letter 2	4871	0	1739	2383	8993
Letter 3	1397	0	1118	6476	8991
Letter 4	2813	0	2310	12863	17986
Total	42038	19208	28200	39219	128665

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[6] In addition to the reporting of rental income, tax enforcement may also affect real behavior (portfolio choice) of landlords. As stricter enforcement increases effective tax rates, it may have similar effects on trade as an increase in the statutory tax rate. That is, some landlords may for example be induced to reduce their real estate holdings in response to stricter tax enforcement. The effects on rental income reported in 2017 may therefore be a combination of real effects (e.g. true rental income being lower than it would be in the absence of the treatment) and reporting effects (e.g. tax evasion being lower than it would be in the absence of the treatment).

Table 2 describes reporting of rental income before the treatment (tax year 2014) in cells defined by the treatment group and whether the area belongs to the three blocks with varying geographical intensity of treatment. The comparison of rows in each column shows that the randomization has been successful as the groups are very similar to each other in terms of the pre-treatment propensity to report, reported gross rental income and reported net rental income. Individuals in the three blocks (control, low intensity treatment, high intensity treatment) are on average more likely to report and have higher rental income than individuals outside the blocks. This is to be expected by construction. Given that landlords and rental markets outside of the blocks are quite different from those within the blocks, in the current version we utilize data from the blocks only. (This choice is also justified by our focus on spillover effects; data from outside of the blocks is less well suited for analyzing spillover effects.)
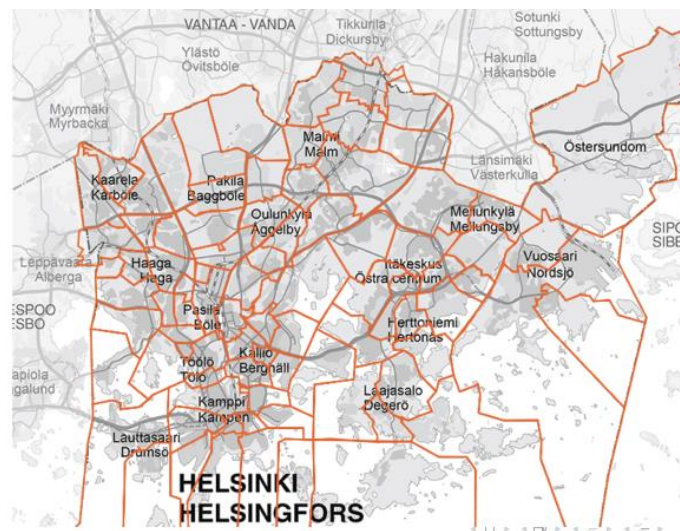
Figure 2. An illustration of the block design for Helsinki.

Table 2. Reporting of rental income before treatment.

		Reported rental income 1/0		Gross rental income		Net rental income	
		Not in blocks	In blocks	Not in blocks	In blocks	Not in blocks	In blocks
No letter	Mean	0.692	0.735	6755.6	8176.1	3385.9	4487.4
	Std. Dev.	0.461	0.441	24131.1	23729.1	10342.2	14018.2
Letter 1	Mean	0.680	0.743	7125.9	8046.4	3649.9	4483.5
	Std. Dev.	0.467	0.437	32590.6	16957.8	21530.9	13613.2
Letter 2	Mean	0.672	0.741	6211.6	7790.9	3211.6	4279.2
	Std. Dev.	0.470	0.438	13515.3	12031.8	8407.1	7634.1
Letter 3	Mean	0.700	0.738	6083.9	8197.0	2994.5	4633.3
	Std. Dev.	0.459	0.440	13272.6	17958.7	5709.8	11257.4
Letter 4	Mean	0.682	0.744	6442.1	7929.7	3332.2	4358.7
	Std. Dev.	0.466	0.436	14558.5	16210.5	9626.3	9005.0

Figure 3 provides first descriptive evidence on the effects of our experimental treatments. It shows the development of the fraction of potential landlords reporting rental income in tax years 2013-2016. Tax years 2013-2014 are pre-treatment years, and tax year 2015 is the first treatment year. The development is shown separately for the control blocks, those who received no letters in the treatment blocks, as well as recipients of treatment letters 2-4 combined. The fraction develops similarly in all the groups before the treatment, and also the pre-treatment levels are not statistically significantly different from each other. This is in line with the observation from Table 2 above that the randomization appears to have been successful.

The figure indicates that the treatment letters caused a statistically significant increase in the fraction of potential landlords who report a positive amount of rental income in tax year 2015. In the following year, the fraction reporting rental income declines somewhat. On the other hand, other landlords in the treatment blocks do not seem to be affected on average (a measure of possible spillover effects, also to be discussed in more detail below).

Finally, an important point to note from Figure 3 is that approximately 77 % of the potential landlords in the control blocks reported a positive amount of rental income in 2015. This number provides an estimate of baseline compliance (at the extensive margin). While there may be some measurement error in this compliance estimate (as it is in principle possible that no rent was paid even if the apartment was occupied), it is safe to say that the degree of non-compliance in rental income taxation is likely to be sizeable. Also, landlords' reactions to the treatment letters are a first indication of underlying non-compliance. It is therefore of interest to analyze how compliance can be improved. In the next Section, we turn to an econometric analysis of the effects of stricter tax enforcement in the rental housing market.

Figure 4 shows the development of the amount of net rental income for those receiving letter 4 (the strongest treatment) in high-intensity treatment blocks; for those who did not receive any letter in high-intensity blocks; and for those in the control blocks. The figure is based on an individual-level fixed-effects regression and also includes block-level net rental income as a control. The figure shows that in the treatment year (tax year 2015), net rental income increased for those receiving letter 4, but was not affected for other groups. In the following year, tax year 2016, net income is still on a higher level in the group that received the letter. In that year, there is a slight increase visible also for the group in the high intensity treatment blocks who did not receive any letter. The latter effect is consistent with a positive spillover effect, though the effect does not appear to be statistically significant.
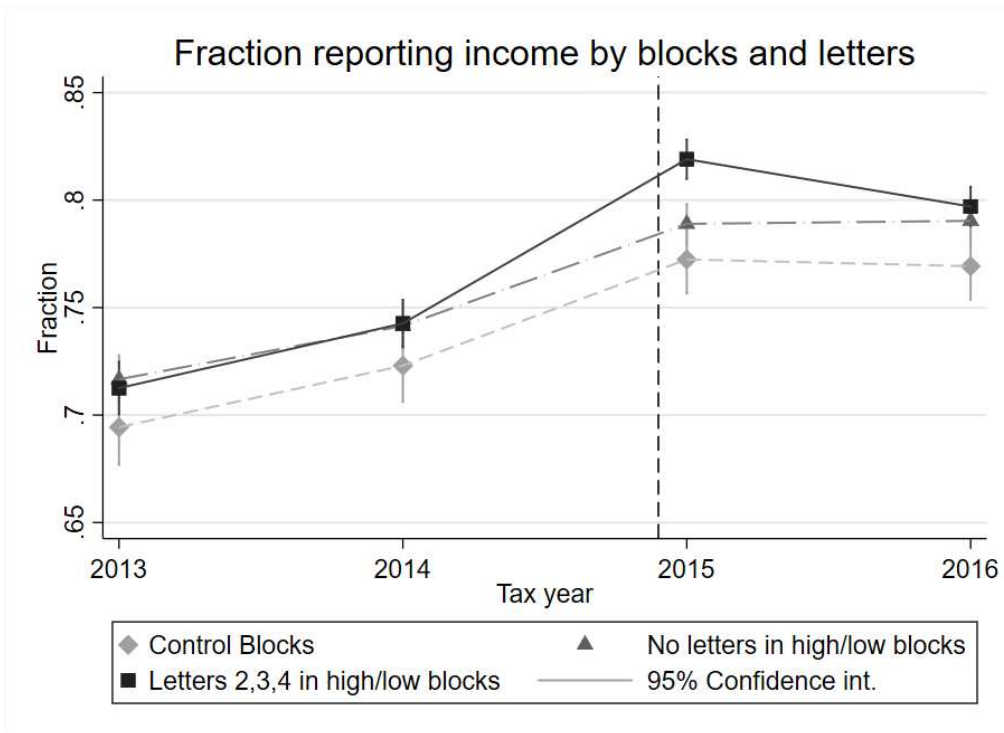
Figure 3. Fraction of potential landlords reporting rental income, by blocks and letters, tax years 2013-2016.
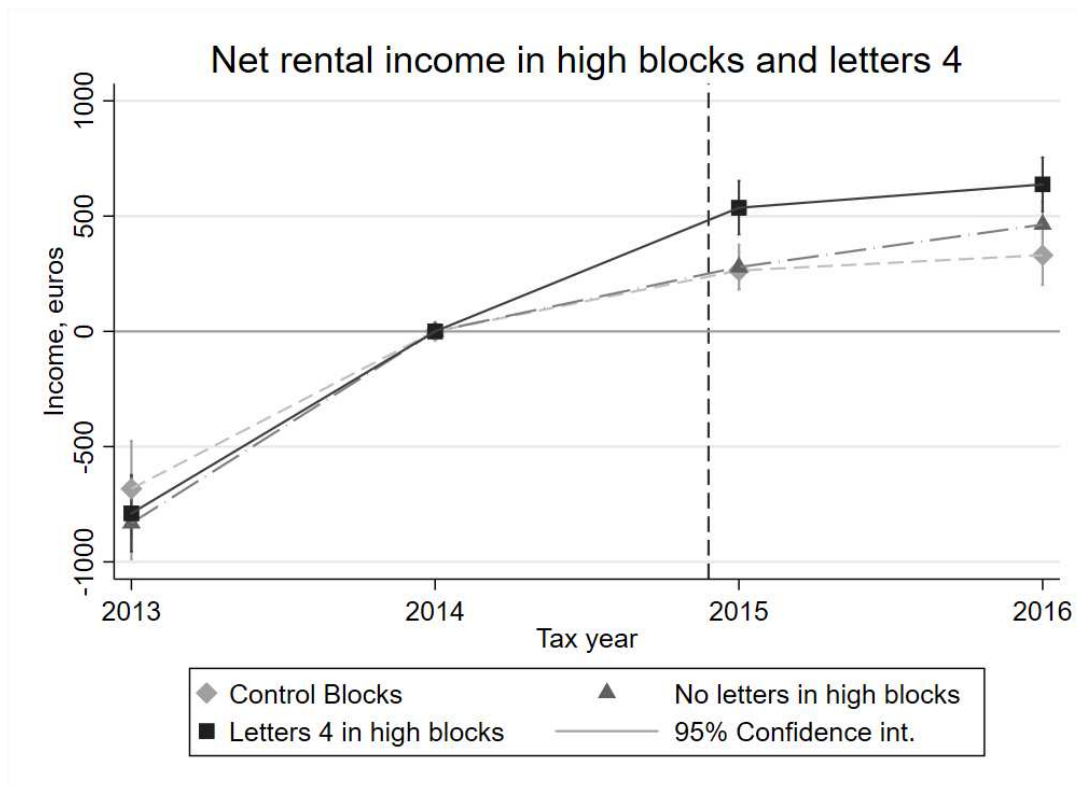

Figure 4. Net rental income reported by landlords, by blocks and letters, tax years 2013-2016. Based on an individual level fixed-effects regression.

4. Results

We use the following Difference-in-Differences type model to estimate the effects of the various treatments in our experimental design:

$$y_{it} = \alpha + \zeta After_t + \beta_j \sum_j Let_j * After_t + \gamma_k \sum_k Blo_k * After_t + \sum Let_j + \sum Blo_k + \varepsilon_{it} \quad (1)$$

where y is the outcome for individual i and time t. We control for general changes in outcomes in the after period (either tax year 2015 or 2016) with dummies $After_t$. We consider the effects of the different treatment letters (Let_j) separately, and include dummies for high or low intensity blocks (Blo_k). β_j then identify the effects of the different letters on outcome y, and γ_k identify the effects of being in a high or low treatment block (over and above the direct effect of receiving a letter), relative to the control blocks. ε is the error term. We cluster standard errors at the local area (block) level defined by zip codes. (In addition, we control for the number of apartments owned (before treatment) to gain precision.)

We first turn to regression results at the block level for tax year 2015, the first year of treatment. Table 3 analyses reporting behavior in low and high intensity areas compared to control areas with no letters. This specification does not include controls for the different letter treatments separately, but only analyses the effects of the treatment blocks.[7] The estimates reported in Table 3 therefore capture the combined effect of all four treatments, while treatment intensity differs between the high and low blocks. Recall that low intensity block means that 24 percent of potential landlords in the base population were sent a letter, while in the high intensity blocks 62 percent of potential landlords received a letter. In addition, the share of the stronger treatment letters (3 and 4) was higher in the high intensity block (cf. Table 1).

Table 3 shows that the fraction of potential landlords reporting a positive amount of rental income is slightly higher in the low intensity treatment blocks than in control blocks; and even higher in the high intensity blocks. The difference of the high intensity block to the control block, as well as between the high intensity and low intensity blocks, are statistically significant. This is first evidence that the treatment letters had an impact on reporting rental income, and that the intensity of treatment might matter. Further, also the reported net rental income is highest in the high intensity block.

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[7] That is, the specification corresponds to equation (1), with the coefficients on Let_j as well as the β_j coefficients being restricted to zero. The coefficients reported in Table 3 correspond to the γ_k coefficients from this specification.

Table 3. Effects by geographical intensity of the treatment.

Dep. Var.	(1) Reported rental income (0/1)	(2) Rental income (gross)	(3) Rental income (net)	(4) Spouse reported rental income (0/1)	(5) HH Rental income (gross)	(6) HH Rental income (net)
Low intensity	0.00321	53.62	103.5	0.00348	78.38	147.2*
block	[0.00402]	[85.10]	[69.31]	[0.00294]	[98.45]	[77.35]
High intensity	**0.0162*****	112.9*	132.6***	0.00393	171.4**	195.4***
block	**[0.00357]**	[67.01]	[45.88]	[0.00243]	[81.49]	[58.38]
N	173254	173254	173254	106446	173254	173254
R-sq	0.084	0.68	0.64	0.055	0.457	0.434
Baseline mean	0.748	8534.7	4635.6	0.428	10618	5770.8

Notes: All specifications include dummies for different numbers of apartments owned (before treatment) as controls. Standard errors clustered at the postcode level are in brackets. Significance is denoted by asterisks: p<0.01 *** p<0.05 ** p<0.1 * Bold font indicates significant difference to low intensity block (at 5 percent level).

Table 4 turns to analyse the effects of the different letter treatments, showing the effects of the treatments on the immediate reporting of rental income for tax year 2015 (propensity to report, gross rental income and net rental income). The table shows separately the effects of all four treatment letters (the β_j coefficients) and the blocks (the γ_k coefficients). The first observation is that all letters caused a statistically significant increase in the propensity to report (column 1). In addition, the letters with actual information on rental income tax enforcement (letters 3-4) had a significantly stronger effect (by 1.5 – 2% points) than the neutral letter (letter 1). While providing simple information on reporting procedures and requirements (letter 2) increased compliance, suggesting that simple mistakes may play a role in non-compliance, the letters on intensified enforcement (letters 3 and 4, targeting individuals who have evaded taxes on purpose) were more effective. The strongest treatment, which notified landlords of the use of third-party information in tax enforcement (letter 4), had the largest effect.[8] Compared to the baseline compliance rate of 77 %, the relative effect of letter 4 is to increase compliance by about 3.5 %.

The above estimates concern effects on compliance at the extensive margin. The estimates for the effects on the amount of rental income reported (compliance at the intensive margin, columns 2 and 3) are positive, and statistically significant for the stronger treatments 3 and 4.

Further, the results in Table 4 allow us to analyze reporting spillovers. We analyze two types of spillovers. First, we utilize the block design in order to analyze spillovers between landlords in local rental markets. Regional spillover effects are measured by the coefficients γ_k in equation (1). In this specification, we find no evidence of regional reporting spillovers. Note that while the coefficients in Table 4 capture all spillovers within a block (i.e. also those occurring between recipients of different types of letters), another way to look at spillovers is to isolate effects on those who did not

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[8] It should be noted that estimates for letters 3 and 4 give the combined effect of receiving the letters, and the associated randomized enforcement measures. If we separately control for enforcement measures received by landlords, the results on the effects of the treatment letters remain intact.

receive any treatment letter at all.[9] Such a specification is illustrated in Table A2 in the Appendix, concentrating on spillovers within high intensity treatment blocks. The results are similar to those shown in Table 4, namely the estimated spillover effects are not statistically significant.

Table 4. Effects of letters and geographical intensity of the treatment

Dep. Var.	(1) Reported rental income (0/1)	(2) Rental income (gross)	(3) Rental income (net)	(4) Spouse reported rental income (0/1)	(5) HH Rental income (gross)	(6) HH Rental income (net)
Letter 1	0.0102*	188	139.4	0.00172	202.3	108
	[0.00585]	[204.0]	[105.6]	[0.00543]	[213.1]	[120.1]
Letter 2	0.0231***	90.02	83.65	-0.00861**	-38.07	40.44
	[0.00598]	[106.2]	[96.94]	[0.00427]	[262.1]	[142.9]
Letter 3	**0.0265*****	220.7**	124.8	0.0105***	274.2***	120.9
	[0.00390]	[92.38]	[82.63]	[0.00375]	[97.88]	[86.80]
Letter 4	**0.0316*****	229.2**	180.8***	0.00436	327.8**	232.0***
	[0.00368]	[110.6]	[62.23]	[0.00294]	[143.7]	[80.26]
Low intensity block	-0.00248	9.116	70.14	0.00314	30.69	114.3
	[0.00402]	[88.74]	[70.49]	[0.00297]	[100.9]	[77.69]
High intensity block	-0.00059	-16.24	38.77	0.00127	7.979	90.01
	[0.00392]	[71.33]	[53.32]	[0.00290]	[85.39]	[66.72]
N	173254	173254	173254	106446	173254	173254
R-sq	0.084	0.68	0.64	0.055	0.457	0.434
Baseline mean	0.748	8534.7	4635.6	0.428	10618	5770.8

Notes: All specifications include dummies for different numbers of apartments owned (before treatment) as controls. Standard errors clustered at the postcode level are in brackets. Significance is denoted by asterisks: p<0.01 *** p<0.05 ** p<0.1 * Bold font indicates significant difference to low intensity block or Letter 1 (at 5 percent level).

We may also focus on spillovers between family members, where information flows are of course particularly likely. The direction of possible spillover effects between spouses is not obvious a priori. A threat effect induced by the treatment letters would suggest positive spillovers. On the other hand, if spouses jointly own a flat and previously only one of them has (mistakenly) reported income on the entire flat, the letter with information on how to report rental income (letter 2) may alert them to the fact that both of them should report their rental income according to their ownership share. In this case the spillover may also be negative.

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[9] While the latter approach may seem to be the more natural interpretation of spillover effects in some respects, the approach taken in Table 4 is in some sense more comprehensive. Hearing about other landlords receiving similar treatment letters may either reinforce or dilute the direct impact of the treatment on letter recipients; the γ_k coefficients take such effects into account, in addition to spillovers on non-treated landlords.

Column 4 of Table 4 indicates in most cases positive and sometimes statistically significant spillovers between spouses at the extensive margin, i.e. in the likelihood of reporting any rental income. In the case of Letter 2 however the spillover effect is negative, suggesting that the mechanism outlined in the paragraph above may be operational in the case of the information treatment. (The net effect of Letter 2 on the extensive margin of reporting at the household level (the sum of the coefficients in columns 1 and 4) remains positive however.)

Columns 5 and 6 of Table 4 take into account spillovers between spouses in reporting at the intensive margin, through looking at the effects of the treatment letters on reported rental income at the household level. For the letters that notified the recipients of intensified enforcement (Letters 3 and 4), the effects are somewhat stronger than in columns 2 and 3 looking at the treated individual only. This indicates the presence of positive spillovers in tax enforcement within the family.

In Table 5, we report the results for a subgroup of particular interest, namely those individuals in the sample that did not report any rental income in tax year 2014, i.e. before the treatment. While some of these individuals may indeed not have owned or rented out a flat in the previous year, this is nevertheless a subgroup where non-compliance appears more likely. Indeed, baseline compliance (at the extensive margin) in the control block in 2015 in this group is only about 15 %.

The effects on the propensity to report are now much stronger than in Table 4. Given the low baseline compliance rate in this subgroup, the relative effect on the compliance rate (at the extensive margin) of intensified enforcement 4 is very large in this group: receiving the strongest treatment letter (Letter 4) causes an over 50 % increase in the propensity to report rental income in this group.

For this subgroup, we also find somewhat stronger indications of spillover effects. We find significant and positive reporting spillover effects of intensified enforcement between spouses, as in Table 4. Now there are also statistically significant and positive regional spillover effects at the block level, indicating that information on the letters has spread between landlords within local rental markets. The regional spillovers serve to reinforce the direct positive impact of the treatment.

The overall gist of the results regarding reporting spillovers is that the (positive) effects of enforcement on tax reporting may be significantly understated if spillover effects are ignored. Spillovers appear to occur within the household, and we also find some indications of positive spillovers between landlords in regional rental markets.

Table 5. Effects of letters and geographical intensity of the treatment – subsample with no reported rental income in 2014.

Dep. Var.	(1) Reported rental income (0/1)	(2) Rental income (gross)	(3) Rental income (net)	(4) Spouse reported rental income (0/1)	(5) HH Rental income (gross)	(6) HH Rental income (net)
Letter 1	0.0330**	-204.6	-331.1***	0.02	-255.3	-413.3**
	[0.0155]	[274.3]	[119.8]	[0.0160]	[330.1]	[178.3]
Letter 2	0.0578***	176	53.87	-0.0196	480.7	193.4
	[0.0163]	[244.0]	[155.7]	[0.0163]	[516.9]	[265.1]
Letter 3	0.0613***	-178.1	-87.64	0.016	-205	-117.5
	[0.0111]	[195.4]	[133.9]	[0.0133]	[218.0]	[148.3]
Letter 4	**0.0883***	294.1	88.77	0.0232**	533.9	**229.4**
	[0.00965]	[288.3]	[148.7]	[0.00988]	[408.5]	**[242.2]**
Low intensity block	0.0058	236.6	212.8	0.0198**	414.9	292.7*
	[0.0137]	[238.1]	[156.1]	[0.00926]	[262.7]	[167.0]
High intensity block	0.0123	315.9	282.8**	0.00514	333.1	322.9**
	[0.0141]	[196.2]	[118.3]	[0.0105]	[231.3]	[147.6]
N	45422	45422	45422	23522	45422	45422
R-sq	0.215	0.227	0.282	0.049	0.164	0.19
Baseline mean	0.153	1076.3	535	0.118	1593.1	796.7

Notes: All specifications include dummies for different numbers of apartments owned (before treatment) as controls. Standard errors clustered at the postcode level are in brackets. Significance is denoted by asterisks: p<0.01 *** p<0.05 ** p<0.1 * Bold font indicates significant difference to low intensity block or Letter 1 (at 5 percent level).

Next, we repeat the analysis of the main treatment effects for tax year 2016 (that is, reporting in spring 2017, i.e. one year after the treatments took place). The results are reported in Table 6. Whereas the immediate effects reported above are guaranteed to incorporate reporting effects only, the results in subsequent years may incorporate both reporting and real responses to more intense tax enforcement as landlords have had the opportunity to adjust their real estate holdings.

We find smaller effects on the propensity to report rental income than in the analysis above, where we focused on the immediate effects. We also no longer find any effect on the reported net rental income (the intensive margin effect). This may be due to the impact of the letters on reporting being diluted over time (a reporting effect); or some landlords having reduced their apartment holdings due to a higher effective tax rate on rental income (a real effect). At this stage we are unable to disentangle these two effects. Analysing the potential real effects of tax enforcement remains an interesting issue for further research.

Table 6. Effects of treatment letters – for tax year 2016.

Dep. Var.	(1) Reported rental income (0/1)	(2) Spouse reported rental income (0/1)	(3) HH Rental income (gross)	(4) HH Rental income (net)
Letter 1	0.0031	0.00269	226.2	135.5
	[0.00577]	[0.00589]	[247.3]	[188.0]
Letter 2	0.0049	-0.00593	-434.7	-103.1
	[0.00512]	[0.00481]	[421.5]	[114.2]
Letter 3	0.000879	0.000913	167.5	-33.3
	[0.00391]	[0.00436]	[237.6]	[97.68]
Letter 4	0.00903**	0.00216	27.37	123.5
	[0.00415]	[0.00338]	[151.2]	[82.71]
Low intensity	0.00182	0.00238	36.51	108.2
block	[0.00377]	[0.00302]	[128.3]	[83.24]
High intensity	0.00517	0.00136	107.2	148.0*
block	[0.00389]	[0.00316]	[144.5]	[87.25]
N	173254	106446	173254	173254
R-sq	0.077	0.053	0.455	0.374
Baseline mean	0.746	0.431	10874.2	5854.2

Notes: All specifications include dummies for different numbers of apartments owned before treatment as controls. Standard errors clustered at the postcode level are in brackets. Significance is denoted by asterisks: p<0.01 *** p<0.05 ** p<0.1 * Bold font indicates significant difference to low intensity block or Letter 1 (at 5 percent level).

5. Concluding remarks

We have reported the results from a large-scale randomized experiment focusing on rental income tax compliance. The experiment was conducted in spring 2016 and our data covers two years of reporting behavior (spring 2016 and 2017) combined with a rich set of other tax related information about individual landlords. This enables us to analyze the effects of the treatment on the immediate reporting behavior as well as the behavior one year after the treatment.

Our findings suggest that different types of treatment letters had an effect on the reporting behavior on potential landlords. The effect is most pronounced on the extensive margin (that is, the propensity to report any rental income) while we also find some effects on the intensive margin. The results are strongest for individuals who had reported any rental income in the year prior to treatment, and are thus more likely to be tax evaders: the strongest treatment, providing information on the use of third-party information in tax enforcement, increased the propensity to report rental income in this group by over 50 %.

Our experimental design allows for studying spillovers in the local rental markets, since we have utilized a randomized block design where the intensity of treatment varies between zip code districts. We find some evidence of spillovers in reporting behavior between landlords within local rental markets in the subgroup that did not report any rental income in the previous year. Reporting

spillovers are particularly evident between spouses. The letter that notified potential landlords of the use of third-party information in the enforcement of the rental income tax increased reported net rental income at the household level by approximately 230 €, which translates to a 70 € revenue gain per household receiving this type of treatment letter. The results on spillover effects overall indicate that the (positive) effects of enforcement on tax reporting may be significantly understated if spillover effects are ignored.

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Appendix. Additional tables

Table A1. Summary statistics for key variables.

	Obs	Mean	Std. Dev.	Median	1st percentile	99th percentile
2015 (treatment year)						
Reported rental income (0/1)	86627	0.795	0.403	1	0	1
Gross rental income	86627	8965	23293	6000	0	64556
Net rental income	86627	4925	14355	3029	-1774	38050
Spouse reported rental income (0/1)	53223	0.448	0.497	0	0	1
HH Gross rental income	86627	11348	30470	7140	0	81250
HH Net rental income	86627	6219	18468	3791	-2213	47861
Owned apartemnts	86627	2.343	2.393	2	1	11
Potential rental apartments	86627	1.126	0.910	1	0	5
Apartments bought	86494	0.094	0.384	0	0	1
Apartments sold	86494	0.076	0.306	0	0	1
No letter (1/0)	86627	0.641	0.480	1	0	1
Letter 1 (1/0)	86627	0.049	0.215	0	0	1
Letter 2 (1/0)	86627	0.048	0.213	0	0	1
Letter 3 (1/0)	86627	0.088	0.283	0	0	1
Letter 4 (1/0)	86627	0.175	0.380	0	0	1
Control block (1/0)	86627	0.222	0.415	0	0	1
Low intensity block (1/0)	86627	0.326	0.469	0	0	1
High intensity block (1/0)	86627	0.453	0.498	0	0	1
2016 (after treatment)						
Reported rental income (0/1)	86627	0.788	0.409	1	0	1
Spouse reported rental income (0/1)	53223	0.452	0.498	0	0	1
HH Gross rental income	86627	11798	29651	7395	0	83560
HH Net rental income	86627	6398	17397	3958	-2667	48591
Owned apartemnts	86627	2.332	2.514	2	0	11
Apartments bought	84043	0.077	0.430	0	0	1
Apartments sold	84043	0.084	0.361	0	0	1

Table A2. Effects by letter group and spillovers in high intensity block (tax year 2015).

Dep. Var.	Reported rental income (0/1)	Rental income (gross)	Rental income (net)
No Letter (high block)	0.00641	-72.58	-24.2
	[0.00460]	[93.49]	[58.07]
Letter 1 (high block)	0.0175**	-82.27	135.4
	[0.00759]	[169.0]	[119.5]
Letter 2 (high block)	0.0250***	-42.85	176.3*
	[0.00847]	[138.5]	[105.1]
Letter 3 (high block)	0.0325***	138.9	139.2**
	[0.00484]	[93.82]	[70.08]
Letter 4 (high block)	0.0400***	219.8	231.4***
	[0.00465]	[136.2]	[71.90]
N	58427	58427	58427
R-sq	0.458	0.843	0.832

Notes: Sample includes individuals in control block and high intensity block. All specifications include as controls lagged dependent variable, a dummy for reporting rental income in 2014 and the number of apartments owned in the end of 2015. Standard errors are clustered at the level postcode level are in brackets. Significance is denoted by asterisks: p<0.01 *** p<0.05 ** p<0.1 *

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