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April 2015

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ABSTRACT

Tax Compliance and Information Provision: A Field Experiment with Small Firms^{*}

We study a field experiment on tax compliance in Slovenia. Small accounting companies were randomly assigned to an untreated control group and two treatment groups. Companies in the first treatment group received a letter that highlighted the importance of paying taxes and informed about the likelihood of becoming subject to an audit. In the second treatment group, tax officers from the tax authorities handed out in person the same letter that companies in the first treatment group received by post. The results indicate that such letters can increase compliance, and trigger even more compliance if handed over in person. These findings are in line with the theoretical predictions that we derive to rationalize the experiment.

JEL Classification: H20, H32, H50, C93

Keywords: tax compliance, audits, randomized field experiment, tax authority, information provision

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1 Introduction

Tax noncompliance is a significant problem in many countries around the world.¹ There is compelling evidence that a large portion of overall noncompliance can be attributed to taxpayers that self-report taxable income rather than those that are subject to third-party reporting and tax-withholding (Slemrod 2007; Kleven et al. 2011). One important question that arises then is: how can compliance be improved among taxpayers that self-report their income? Mechanisms that have been suggested in the literature include, among others, improving tax morale (e.g., Luttmer and Singhal 2014), information on audit probabilities (e.g., Kleven et al. 2011) and closer interaction with the tax authorities (e.g., Alm 2012). In this paper, we study these mechanisms in the context of a field experiment with small firms – a group of taxpayers that is usually considered to have high propensities of noncompliance while yet being scarcely investigated in the literature.

We analyze a field experiment that was conducted in cooperation with the Slovenian tax authorities in January 2014. 142 small accounting companies in a municipal region of Slovenia (called Kranj) were randomly divided into three groups: an untreated control group and two treatment groups. In the first treatment group, firms received a letter by post that reminded them of the importance of paying taxes and informed about the likelihood (of 10%) of becoming subject to an audit. In the second treatment group, the same letter that was sent to firms in the first treatment group was handed over in person to company representatives by tax officers from the financial administration. These so-called mobile units were the same for each company and were instructed not to provide any extra information which were not stated in the letter. Using official tax-reporting data provided by the Slovenian authorities, this field-experimental set-up allows us to study (i) how a letter containing moral appeals and salient audit probabilities affects tax compliance of small firms, and (ii) whether the interaction channel with the tax authority – in person or via mail – affects compliance.

To identify the effect of the randomized intervention, the Slovenian tax authorities provided data on earnings reported for tax purposes of all firms in the experiment for the years 2012 and 2013. Since the experiment took place before the companies reported their 2013 earnings to the tax authorities, we can rely on a simple difference-in-differences design where we compare the evolution in earnings between the years 2012 and 2013 across the three groups of companies. The experimental results indicate that the treatment letter increased compliance, and leads to even more compliance if handed over in per-

¹For example, the Internal Revenue Service estimated the tax gap – i.e., the difference between taxes owed and taxes paid in a fiscal year – in the United States in 2006 to be around 450 billion US Dollars which is roughly 17% of all true tax liabilities (IRS 2012). For the UK, the government agency HM Revenue & Customs reports that the 2010/2011 tax gap is 32 billion Pounds (about 54 billion US Dollars) which corresponds to 6.7% of all tax liabilities (HMRC 2012). Tax noncompliance is suggested to be an even more severe problem in developing and transitional countries (Schneider and Enste 2000).

son. That is, changes in taxable earnings between 2012 and 2013 are higher in the first treatment group (letter sent by post) relative to the control group, and higher in the second treatment group (letter handed over by mobile units) relative to the first treatment group. These effects are fairly sizable. For example, the average percentage change between the self-reported tax base in 2012 and 2013 is 1.87% in the control group, 12.63% in the “letter” treatment group and 20.28% in the “visit” treatment group. However, our non-parametric tests and parametric regressions reveal that these differences across the groups are not distinguishable from zero in a statistical sense. The lack of statistical significance is likely to be due to the low number of observations.² The field-experimental results therefore provide suggestive evidence that is intuitively appealing, but we are not able to reject the null of no treatment effects. Our findings are in line with the theoretical predictions that we derive to rationalize the experiment.

Our paper speaks to the large literature on tax compliance and contributes to the recently growing literature using field experiments to study compliance behavior (e.g., Slemrod et al. 2001; Kleven et al. 2011; Pomeranz 2015; Kosonen and Ropponen 2013; Hallsworth et al. 2014; Del Carpio 2014; Dwenger et al. 2014). We particularly add to an understanding of how (i) moral appeals, (ii) information about audits and (iii) interactions with tax authorities may affect compliance: First, older studies find no sizable effects of moral appeals on compliance (e.g., Blumenthal et al. 2001; Torgler 2004). However, recent work by Hallsworth et al. (2014) and Del Carpio (2014) finds positive compliance effects of morale appeals in the form of, for example, information on overall compliance levels in the population. Second, information about audit probabilities and threat-of-audit letters are consistently shown to have positive effects on compliance, especially on self-reported income (e.g., Slemrod et al. 2001; Kleven et al. 2011; Pomeranz 2015). Third, while we are not able to distinguish the effect of moral appeals and audit information in our experimental context, our paper provides indications that a more personal way of transmitting information to taxpayers triggers a stronger effect on compliance than non-personal interactions. This supports the notion put forward by Alm (2012) (also see Alm et al. 2010 and Gangl et al. 2015) that an improved and more personal way of interaction between taxpayers and tax-authorities may improve compliance.

In a broader perspective, our paper provides additional indications that tax compliance decisions cannot solely be viewed as risky gambles where taxpayers maximize their expected utility (Allingham and Sandmo 1972). In particular, we show that non-pecuniary issues – such as the transmitting mechanism that should not affect purely rational taxpayers who trust the institutions in place – may play a role also for small companies which are usually assumed to be more rational than individual households.

²In section 4.3, we report the results of a power analysis which indicates that we would have needed roughly 200 observations per cell to obtain statistically significant results.

In addition, we generally contribute to an understanding of tax-compliance behavior of firms. Although it is often presumed that firms, especially small firms with self-reported earnings, tend to have high likelihoods of non-compliance, evidence is very scarce. De-Backer et al. (2012), Kosonen and Ropponen (2013), Carrillo et al. (2014), Slemrod et al. (2015) and Pomeranz (2015) are notable exceptions that are complementary to our paper in understanding tax-compliance behavior of firms. However, as opposed to our paper, these studies do not contribute to the question of whether small firms are subject to behavioral inconsistencies in the context of tax compliance.

We also add to the behavioral-economics literature showing that personal communication may trigger stronger effects than less personal and more anonymous ways of communicating. For example, Ben-Ner and Putterman (2009) highlight that personal communication, compared to anonymous communication, increases trust in social dilemmas (see also Balliet 2009 for a review). Our paper hence suggests that some of the results in behavioral economics are also likely to apply to the economics of tax evasion.

The paper proceeds as follows. Section 2 gives an overview of the field experiment and provides summary statistics. Section 3 derives predictions regarding the treatment effects which help to rationalize the field experiment. Section 4 presents and discusses the results, and Section 5 concludes the paper.

2 The field experiment

This section gives an overview of the field-experimental design and procedures, illustrates organizational issues and provides summary statistics for all firms that were part of the experiment.

2.1 Experimental design

142 small accounting firms in the Slovenian region of Kranj were chosen to be part of the field experiment. The companies did not know that they were subject to a randomized intervention. All participating firms were randomly assigned to be either in a control group, a “letter” treatment or a “visit” treatment. In total, 32 firms were selected to be in the untreated control group, 80 firms were in the letter treatment and received a letter from the tax authorities, and 30 firms were randomly selected into the visit treatment which received a visit from the customs officers (see below for pre-treatment summary statistics). The reason we assigned a larger number of firms into the letter treatment was that we are aware of the fact that this treatment intervention may be suffering from noise. Although we presume that official letters from the authorities are actually read, it is unobservable whether letters are really read and, if so, by whom. Thus, we increased the number of firms that received a letter to make meaningful inferences from

our between-treatment comparisons. All experimental interventions took place between January 13th and January 22nd, 2014.

Firms in the **control group** were not treated in any way. Firms in the **letter treatment** received a letter by the local tax authority by post. The translated letter is displayed in the Appendix. The letter first included a reminder that paying taxes is “a civic duty” and that “taxes are important to maintain public schools, public infrastructure and public health provision”. In a next step, the firms were informed that they are part of a special investigation which implies that 10% of all accounting firms in the region of Kranj would be subject to tax audits. The letter also contained the information that these audits will be regarding the tax returns for the year 2013 which are due in April 2014 (hence after the treatment intervention).³ The general structure and wording of the letter is in line with field experiments in the literature (e.g., Kleven et al. 2011).

Firms in the **visit treatment** were personally approached by so-called mobile units. These units were staffed with two tax officers who are employees of the local tax authority. The mobile units were instructed to ask for the highest company representative available and to hand over a letter. This letter was the exact same letter that firms in the letter treatment received by post. The tax officers read the letter to the company representative but were instructed not to respond to any questions asking for additional information *not* contained in the letter. As a result, firms in the visit treatment were provided the same information as firms in the letter treatment, but the transmission channel was different: personal interaction with the tax authority rather than a letter sent by post.

2.2 Motivation of the experimental design

The overall aim of the field experiment is to identify practices which may help to increase tax compliance of small companies. The focus is on small profit-maximizing firms with good evasion opportunities because they are presumed to have high propensities of noncompliance and it is in the interest of policy-makers and tax-administrations to improve compliance among this group.⁴ Though suspected to evade large amounts of taxes, small firms have not been at the center of the literature and it is not clear whether they are also affected by non-pecuniary factors that go beyond standard profit-maximizing considerations.

The results of this experimental design allow us to study (i) how a letter containing moral appeals along with a statement about an audit probability of 10% affects com-

³The last paragraph further informed the companies that a few selected taxpayers who are clients of accounting companies in Kranj are being selected for audits as well.

⁴The Slovenian tax authorities provided us with anecdotal evidence that noncompliance of small firms is a considerable problem in their country.

pliance of these small firms and (ii) whether the way this letter is transmitted to the group of small-firm taxpayers – in-person delivery (face-to-face) or by letter sent by post – makes a difference. The design does not allow us to identify the exact channel along which a potential treatment effect of the letter runs in (i). Such an effect may either be attributable to the moral appeals in the letter or to the stated audit probability of 10%. However, we are mainly interested in whether the way of information transmission affects compliance. We therefore decided to use a treatment letter similar to the one in Kleven et al. (2011) that has been shown to be effective in raising tax compliance. That is, while this formulation of the letter does not allow to disentangle the effect of morale appeal and the stated audit probability, it is likely to affect compliance and is therefore suitable for our experiment.

In principle, a tax authority has several possibilities to interact with taxpayers: letters sent by post, email, phone calls or personal visits from tax officers. So why did we focus on postal letters and personal visits? First, email addresses and phone numbers of relevant corporate representatives are usually not known to the financial administration. In addition, even if these communication channels were available, it is uncertain whether they would have the desired effect of triggering attention. The reasons are, among others, spam filters, administrative staff answering the phone and generally low salience. Second, postal letters and personal visits constitute two extremes of formal interaction: very formal (letter) vs. very personal (visit). Hence, we presumably estimate an upper-bound effect of the transmission channel: relative to the visit group, the effect of other, less personal interactions may be smaller than the face-to-face effect in our experiment. Third, the personal face-to-face interaction is interesting from an academic perspective because face-to-face interactions have been shown to make a difference in other contexts and it is desirable to study whether this way of communicating also constitutes a difference in the context of evasion.

Our experimental design also allows us to study if small firms' compliance decisions are affected by non-pecuniary factors that go beyond simple mechanisms of profit-maximizing. The literature has shown that tax compliance decisions of individuals are affected by non-pecuniary factors such as conditional cooperation and social norms (Frey and Torgler 2007; Traxler 2010; Hallsworth et al. 2014; Del Carpio 2014; Lefebvre et al. 2014), tax morale or intrinsic motivations (Halla 2012; Dwenger et al. 2014), cognitive biases such as overestimation of audit probabilities (Erard and Feinstein 1994; Scholz and Pinney 1995) and a lack of information about cheating possibilities (Paetzold and Winner 2014). However, it is yet to explore whether such factors also affect firms that are often assumed to be pure profit-maximizer in the spirit of the seminal Allingham and Sandmo (1972) model. Our experimental design can contribute to the exploration of this important question because rational firms should not be affected by the way the treatment

letter is transmitted: since the audit probability and all other factors that are relevant in the simple Allingham-Sandmo framework are constant across treatment groups, any treatment effect of the visit treatment relative to the letter treatment is attributable to non-pecuniary factors and would suggest that small firms' evasion decisions may also be subject to "behavioral" biases.

2.3 Data, outcome variable and summary statistics

We seek to identify how the treatment interventions described above affected tax compliance. For this purpose, the tax authority of Kranj provided data from tax returns of all firms that were part of the experiment. As an outcome variable in our analyses, we focus on the tax base (before tax reliefs) which is based on self-reported earnings and losses of the firms. The tax base variable therefore constitutes the basis for the firm's taxable income. We have access to data regarding the tax years 2012 and 2013. Tax returns for a tax year t are due in April of year $t + 1$. That is, since the interventions took place in January 2014, we have data on the reported tax bases of each firm before the treatment intervention and after the treatment intervention. In order to ensure credibility and avoid deception, the tax authorities audited the 2013 tax returns (due in April 2014) of 14 firms, corresponding closely to the audit probability of 10% that was stated in the treatment letters.

Table 1 presents summary statistics separated by control and treatment groups for the pre-treatment year 2012. We were provided information on profits, number of employees, number of branches of this firm and the tax base (our outcome variable) of all 142 accounting firms that were part of the experiment (32 in control group, 80 in letter treatment and 30 in visit treatment). The comparison of variables between treatment groups indicates that our randomization process was successful: there are no statistically significant differences in the 2012 (pre-treatment) levels of all variables between the three groups according to non-parametric Mann-Whitney-U tests. The summary statistics indicate that the firms in the experiment are rather small. Their average tax bases are around 10,000 EUR, they generate average profits of about 12,500 EUR, and they have on average 2.3 employees who mostly work in only one branch.

2.4 Implementation of the experiment

The field experiment was organized in cooperation with the tax authorities in Slovenia. We, the authors of this paper, designed the experiment taking into account the constraints (e.g., capability to run a certain number of audits) and possibilities (e.g., availability of mobile units) of the tax authority. We randomized the firms into three groups, formulated the treatment letter and provided instructions on the implementation of the experiment.

Table 1: Summary statistics by treatment group, 2012, pre treatment

| Group | Tax base | Profit | Employees | Branches | N |
|---------|------------------------|------------------------|----------------|----------------|----------|
| Control | 9087.00 (11506.68) | 10628.93 (12433.47) | 2.81 (2.84) | 1.22 (0.42) | 32 / |
| Letter | 10650.31 (19615.78) | 14051.23 (23905.72) | 2.21 (2.52) | 1.29 (0.53) | 80 / |
| Visit | 10017.27 (12152.41) | 10824.84 (12186.07) | 2.03 (2.25) | 1.43 (0.68) | 30 / |
| Total | 10164.28 (16597.33) | 12598.38 (19684.27) | 2.31 (2.54) | 1.30 (0.55) | 142 / |

Notes: Summary statistics by treatment status before treatment in year 2012. *Tax base* is the firm’s tax base (before reliefs) in that year (in EUR). *Profit* indicates profits made during the year (in EUR). *Employees* is the number of employees the firm has, and *Branches* is the number of the firm’s branches. *N* indicates the number of firms in each group. Reported are means with standard deviations in parentheses. Firms in the control group were untreated. Firms in the first treatment group (Letter) received the treatment letter by post. Firms in the second treatment group (Visit) were handed over the treatment letter in person.

The actual implementation of the experiment was carried out by the Slovenian tax authority. That is, they, for example, sent the letters to the firms and organized the work of the mobile units. The tax authority also provided the tax return data that we use for empirical analyses.

3 Predictions

Based on and informed by existing literature on tax compliance and communication, this section derives predictions regarding the effects of our randomized treatment interventions. As we emphasized before it is not clear from an ex-ante perspective if small companies act purely rational or if they are subject to inconsistent “behavioral” behavior. However, the answer to the question of whether firms are purely rational or not yields different predictions for our experimental outcome. In the following, we make two contradicting assumptions about the behavior of small firms: under the first assumption, firms are purely rational and not subject to any biases, and under the second assumption firms are subject to inconsistent, biased behavior that has been shown to be prevalent among individuals. We then proceed and derive behavioral predictions based on both these assumptions.

Assumption A: *Small firms are rational taxpayers that maximize their profits and are not subject to behavioral inconsistent behavior.*

Assumption B: *Small firms are non-rational taxpayers that maximize their profits but they are subject to behavioral inconsistent behavior.*

3.1 Predictions under assumption A: Rational Taxpayers

Purely rational taxpayers are not affected by any non-pecuniary factors, such as morale appeals, and behave according to the Allingham-Sandmo framework. That is, taxpayers have full information over all relevant factors and their tax compliance decisions are exclusively affected by tax rates, fines and audit probabilities. In our experimental set-up, purely rational firms could be affected by the treatment letter because the audit probability of 10% stated therein may be higher than past audit probabilities, and hence signals an increase in the audit probability. In contrast, the morale appeal stated in the letter does not affect compliance. Hence, we derive prediction A1 as follows:

Prediction A1: *Firms are rational taxpayers that are not influenced by moral appeals. Any effects of the treatment letter on compliance, relative to the control group, are due to an increase in the audit probability.*

Prediction A1 states that we can expect a compliance effect of the treatment letter relative to the control group for rationally behaving firms. In contrast, however, we do not expect that rational taxpayers are affected by the way the letter is transmitted. Delivering the letter face-to-face in person does not alter any factors included in the Allingham-Sandmo framework. In particular, since the audit probability is explicitly stated to be 10% in both treatments, the transmission channel should not make a difference.⁵ Following this line of argumentation, we propose prediction A2:

Prediction A2: *Firms are rational taxpayers that are not affected by the way a message is transmitted. There is no change in compliance rates in the visit treatment relative to the letter treatment.*

⁵Rationally-behaving taxpayers might respond differently to the visit than to the letter because the visit is a more costly signal than the letter sent by post. If taxpayers do not believe in the audit probability announced by the tax authorities in the treatment letter and consider it a bluff, they might interpret the visit as a more credible signal for an audit than the letter. As a result, they might respond more strongly to the visit relative to the letter. However, we do not find this plausible because Slovenia, a member of the European Union and OECD, has stable institutions where it seems unlikely that the government deceives its taxpayers in official government letters by means of announcing an audit probability that will not be enforced. We therefore assume that taxpayers believe in the audit probability that is explicitly stated to be the same in the letter and visit treatments; that is, taxpayers should not have a reason to believe that the audit probability is different between the two treatment groups. Our assumption of credible institutions is backed by World Bank data in which Slovenia scores high in terms of Government Effectiveness (score: 0.91) and Rule of Law (0.9). See WorldBank (2013).

3.2 Predictions under assumption B: Biased Taxpayers

Although it is usually assumed that firms behave more rationally than individuals, it is nevertheless possible that behavioral biases are even prevalent for the group of firms in our experiment. After all, these firms are usually very small and managed by individuals. As a consequence the same behavioral inconsistencies and biases that individuals have may also be relevant for the group of small accounting companies. In our experiment, several behavioral mechanisms may be at work and affect the treatment effects. While it is possible that these different mechanisms interact and influence behavior simultaneously, for reasons of expositional clarity we concentrate on discussing them separately:

First, stating audit probabilities explicitly in a letter may increase the salience of audits and informs taxpayers about the mere fact that audits are possible. These mechanisms imply that the treatment letter increases compliance, even if the actual audit probability has not been increased (Chetty et al. 2013; Paetzold and Winner 2014).⁶ Second, recent field-experimental evidence by Hallsworth et al. (2014) and Del Carpio (2014) shows that moral appeals disclosed in letters to taxpayers have a positive effect on compliance. Given this evidence in the literature, the moral-appeal part of our treatment letter could increase compliance.

Third, the mere fact that the firms receive a treatment letter from the tax authorities could affect compliance. This might be for two reasons: On the one hand, communication and improved interaction between the tax authority and the taxpayers may affect trust towards the authorities. For example, the behavioral literature on communication finds that communication increases trust in situations of social dilemmas (Charness and Dufwenberg 2006; Balliet 2009; Ben-Ner et al. 2011) and triggers higher voluntary contributions in public good games (Isaac and Walker 1988; Ostrom et al. 1992; Brosig et al. 2003). This suggests that compliance increases in response to communication and interaction between the tax authorities and the taxpayer. In the context of tax compliance, a few studies have stressed that an improved interaction of tax authority and taxpayer, for example in the form of better service quality of the tax authorities, may also have a positive effect on compliance (Alm et al. 2010; Alm 2012; Gangl et al. 2015). On the other hand, receiving a letter from the tax authorities might affect compliance for another reason: the letter may exert pressure and therefore increases compliance. For example, DellaVigna et al. (2012) conduct a field experiment in the context of charitable giving and show that social pressure in form of interaction with a fundraiser increases voluntary

⁶It may be that firms had heavily overestimated the probability of audit before the experiment (Kahneman and Tversky 1979). The letter could then have helped to adjust the perceived audit probability towards zero, hence decreasing compliance. However, anecdotal evidence provided by the tax authority confirms that the overall level of compliance among small firms in Slovenia has been very low, which makes it appear unlikely that the firms had a perceived audit probability of more than 10% before the experiment.

contributions. In a similar vein, Funk (2010) shows, in a natural field experiment in Switzerland, that voter turnout significantly dropped when the government allowed the possibility to vote by mail. The reason that citizens felt less obliged to vote is that social pressure of fulfilling a civic duty was removed through the reform: before the reform it was easily observable for everybody to see if someone went to vote, especially in small communities.

These three mechanisms are not at force in the control group and therefore all suggest that the treatment letter could have a positive effect on compliance. While our experiment cannot help to disentangle which of the mechanisms is most prevalent, we can yet derive prediction B1:

Prediction B1: *The treatment letter from the tax authorities increases tax compliance in the letter treatment relative to the control group.*

Our second treatment intervention – the personal visit of tax officers – alters the way the treatment letter is transmitted: the content of the information delivered is exactly the same as in the letter treatment, but the information is now transmitted face-to-face. Since no relevant tax parameters (including the audit probability that is explicitly stated and the same in both treatment groups) are changed, a rational taxpayer would not be affected by the channel of transmission. However, the behavioral literature shows that face-to-face communication may trigger different outcomes than more anonymous ways of communication (such as the letter sent by post). For example, Ben-Ner and Putterman (2009) show that cooperation is higher in the presence of face-to-face interactions relative to anonymous interaction. They propose that the lack of anonymity and personal sympathy could be the drivers of this result. Applied to our context, this implies that transmitting information face-to-face may be more effective in improving compliance than the rather anonymous letter.

Another reason why the face-to-face transmission of the letter may have a larger effect on compliance is that the visited firms are likely to have a stronger feeling of being under investigation by the authorities. The firms may be under the impression that the tax authorities will only visit taxpayers which they suspect to be potential evaders – despite the actual audit probabilities that are equal in both treatments. Since visits are more costly and laborious than writing letters, this effect could be larger in the case of face-to-face transmission. This is in line with the evidence put forward by DellaVigna et al. (2012): the effects of social pressure are higher in the presence of personal contact.

While it is again not possible to disentangle the mechanism at work, the above arguments all point in the direction that increasing the intensity of communication by face-to-face visits yields higher compliance. Thus, we derive prediction B2:

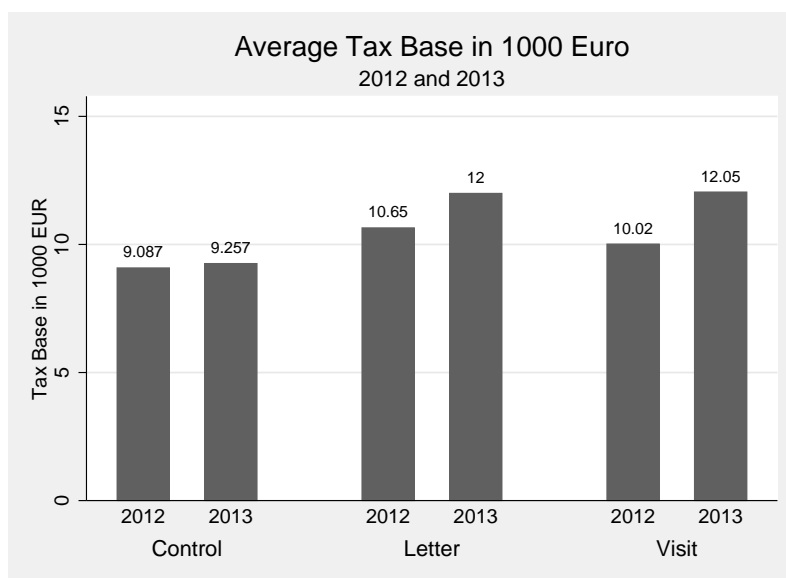
Prediction B2: *More personal (face-to-face) transmission of the information increases tax compliance in the visit treatment relative to the letter treatment.*

4 Results

4.1 Non-parametric comparisons

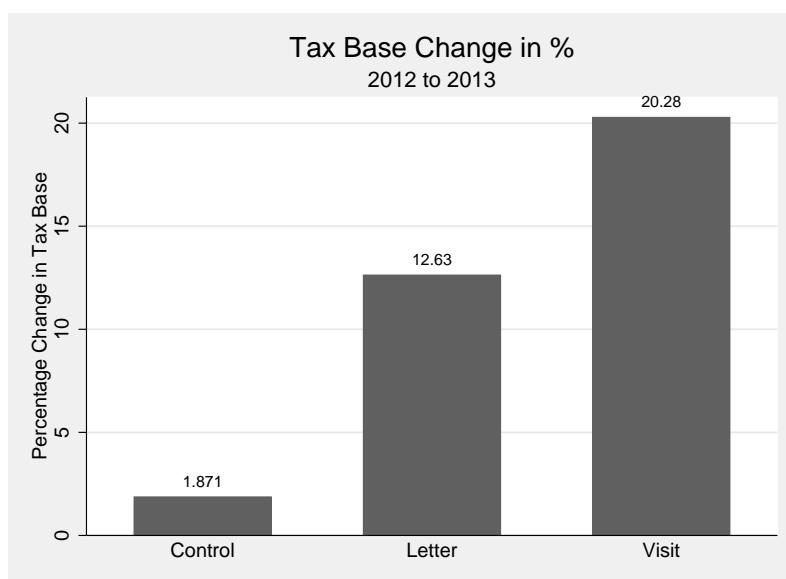
In this section, we provide an overview of tax-base levels in the years before and after the treatment interventions, and compare the control group and treatment groups with respect to the evolution in tax base between the years 2012 and 2013. Figure 1 depicts the mean tax bases in each treatment group in the years 2012 and 2013 and Figure 2 displays the relative average changes in tax base levels between the years 2012 and 2013. The figures indicate that the average increase in tax bases between the years 2012 and 2013, i.e., before and after the treatment intervention, was greater in the treatment groups relative to the control group. In addition, the average tax base increased by more in the visit treatment than in the letter treatment. The sizes are quite sizable: while the average tax base almost did not grow in the control group (change: 1.87%), it increased by 12.63% in the letter treatment and 20.28% in the visit treatment. However, non-parametric Mann-Whitney-U tests, where we compare the differences in tax bases between 2012 and 2013 across the groups, reveal that these differential developments are not statistically significant.

Figure 1: Tax bases by Treatment Group, 2012 and 2013



Notes: Displayed are the means of tax base (in 1000 EUR) by treatment group in 2012 and 2013 (before and after treatment interventions). $N = 142$. Firms in the control group were untreated. Firms in the letter treatment received the treatment letter by post. Firms in visit group were handed over the treatment letter in person.

Figure 2: Tax bases changes by Treatment Group, 2013 vs 2012



Notes: Displayed are the average relative changes in tax base between years 2012 and 2013 (before and after treatment interventions) by treatment group. $N = 142$. Firms in the control group were untreated. Firms in the letter treatment received the treatment letter by post. Firms in the visit treatment were handed over the treatment letter in person.

4.2 Parametric regressions

This section explores whether the non-parametric results translate into estimates from parametric regressions. In the following, we present results based on the following difference-in-differences (DiD) regression model:

$$\phi_{igt} = \alpha + \beta \cdot Treat_g + \eta_t + \delta_1(1[g = 2] \cdot 1[t = 2013]) + \delta_2(1[g = 3] \cdot 1[t = 2013]) + \mathbf{X}_{igt} + \epsilon_{igt}, \quad (1)$$

where subscripts indicate a firm i in group $g = 1, 2, 3$ (with $g = 1$: control group, $g = 2$: treatment letter, $g = 3$: visit treatment) and year t (either 2012 or 2013). The dependent variable ϕ_{igt} is the tax base that is reported for tax purposes. $Treat_g$ is a categorical indicator variable for a firm's group and η_t is a year fixed effect. The variables of interest are the interactions between the treatment group indicators and the indicator for the post-treatment year 2013: $(1[g = 2] \cdot 1[t = 2013])$ and $(1[g = 3] \cdot 1[t = 2013])$. The coefficients for these variables indicate the differential evolutions of tax bases between the years 2012 and 2013 for the two treatment groups relative to the omitted control group. That is, these are the usual DiD coefficients that indicate if the treatment groups' tax bases evolved differently than the control group's tax bases. We add a vector of control variables \mathbf{X}_{igt} which includes the number of employees and the number of branches the firm has. ϵ_{igt} is an error term. We use standard OLS regressions and cluster standard errors on the firm level.

Table 2 depicts the results from this regression model. The coefficients of interest for the interaction terms confirm the non-parametric results above. The average tax bases in both treatment groups increased by more than the average tax base in the control group. The DiD coefficients in model (I) (without control variables \mathbf{X}_{igt}) are 1175 and 1861 for treatment groups 1 and 2, respectively. The results therefore confirm that the treatment letter increases average compliance, and that this effect is even stronger if the letter is delivered in person rather than sent by post. The coefficients perfectly mirror the descriptive statistics (see, for example, figure 1) as these coefficients can also be calculated as the plain differences in differences: $(12.00 - 10.65) - (9.257 - 9.087) = 1.18$ for the letter treatment and $(12.05 - 10.02) - (9.257 - 9.087) = 1.86$ for the visit treatment (all numbers in 1000 EUR). The coefficients are, however, not statistically significant, and we are not able to reject the null hypothesis of no treatment effect. Both the coefficient and the level of significance remain unchanged when we add control variables to the regression specification (see model (II) in table 2).

Table 2: DiD Regressions: Effects of treatments on tax base

| | (I) | (II) |
|---------------------------------|---------------------------|--------------------------|
| <i>Reference group: Control</i> | | |
| Letter \times 2013 | 1175.239 (2003.513) | 1175.239 (2010.759) |
| Visit \times 2013 | 1861.440 (2207.443) | 1861.440 (2215.427) |
| Letter Treat | 1563.302 (2996.443) | 3417.208 (2757.402) |
| Visit Treat | 930.263 (2997.982) | 3358.661 (3289.533) |
| Treat Year 2013 | 170.030 (1344.378) | 170.030 (1349.240) |
| employees | | 3070.735*** (503.532) |
| branches | | -166.763 (2120.598) |
| constant | 9087.007*** (2027.152) | 653.808 (3360.892) |
| N | 284 | 284 |
| R2 | 0.004 | 0.221 |

Notes: Difference-in-differences regressions based on equation 1. Standard errors in parentheses clustered on the firm level. Estimates are based on a sample of 142 firms and two periods (years 2012 and 2013). Significant levels are * < 0.10 , ** < 0.05 , *** < 0.01 . Dependent variable: Tax base (in EUR). Independent variables of interest are the interactions of the treatment-group indicators with the post-treatment-year dummy. Coefficients of interest are relative to the omitted control group. Firms in the letter treatment received a treatment letter by post. Firms in visit treatment were handed over the same treatment letter in person. Year 2013 observations are post treatment. Specifications (II) include control variables for number of employees and number of branches.

4.3 Discussion of results

Our empirical analysis reveals that the average increase in the tax base between the pre-treatment year 2012 and the post-treatment year 2013 was higher in the “letter” group relative to the control group, and higher in the “visit” group relative to the “letter” group. This provides some suggestive evidence that the treatment letter affects compliance, and that this effect is larger if the letter is transmitted face-to-face. The latter result particularly suggests that our Assumption B is preferable to Assumption A (and hence predictions B1 and B2 are to be chosen over predictions A1 and A2): the average differences in differences indicate that small firms may be subject to inconsistent biased behavior, and do not behave as rational as modeled in the seminal Allingham-Sandmo framework. These results seem reasonable because firms, especially small firms, are man-

aged by individuals that have been shown to behave inconsistently in many different contexts. A further argument which supports our conclusion that small firms are subject to behavioral inconsistencies is that firms report considerable amounts for tax purposes although the penalties for noncompliance are quite low in Slovenia. The authorities provided information on penalties which reveal that the penalty structure is such that rational taxpayers – as modeled in the Allingham-Sandmo model – would always evade fully. However, as we have shown, this is not what we observe and tax bases are usually larger than zero.

However, despite being intuitively appealing, one has to be cautious in interpreting the results. The coefficients of the treatment effects in both the parametric and non-parametric analyses are not different from zero in a statistical sense. That is, there is a non-negligible likelihood that our coefficients are purely driven by chance. The lack of statistical significance may to some extent be due to the relative low number of observations, and it would be desirable, though currently not politically feasible, to collect additional field-experimental observations. For example, a power analysis (with a significance level of 0.1 and test-power of 0.8) suggests that we would have required 195 observations per group to detect a statistically significant difference between the observed means of the control group and visit treatment group.

Even if one interprets our results as an indication of treatment effects rather than the result of pure chance, we are not able to disentangle which of the mechanisms that we propose in section 3 are driving the results. The literature (especially Hallsworth et al. 2014 and Del Carpio 2014, and to some extent also Blumenthal et al. 2001) shows that morale appeals could improve tax compliance. In addition, the literature (Slemrod et al. 2001) also shows that increased audit probabilities improve compliance. That is, we argue that the effect of the treatment letter is driven by both the morale appeal as well as the stated audit probability.

Our second treatment intervention shows that firms' tax compliance is likely to be influenced by the way the information is transmitted. Since this should not affect purely rational taxpayers, we show that small firms, just like individuals, are driven by non-pecuniary factors. This effect may be due to several different mechanisms that we cannot disentangle. As outlined in Section 3, these mechanisms of behavioral inconsistencies are: (i) the mere effect of personal communication (in line with the literature, see Balliet 2009) and (ii) the perception of being under close investigation. We consider it plausible that the effect of the transmission channel, relative to the letter sent by post, is driven by a combination of both mechanisms. Following an argument put forward by Chetty (2015), it may not be necessary from a policy perspective to disentangle the mechanisms at work. A policy maker can benefit from the mere fact that our treatments may have an impact on compliance, even without knowing why exactly these interventions have the

desired effect.

5 Concluding remarks

We study a randomized field experiment on tax compliance conducted in cooperation with the Slovenian tax authorities. The experiment focuses on small accounting firms – a group of taxpayers that self-reports taxable income and potentially has high propensities to be noncompliant with tax laws. Our results provide suggestive evidence that a treatment letter, which reminds the firms of the civic duty to pay taxes and informs about an audit probability of 10%, may increase tax compliance. This effect is even larger if this letter is handed over to the firms in person. However, one should treat these results with caution as they are not statistically significant. The lack of significance is to some extent likely to be due to the low number of observations.

One careful lesson from our experiment is that informing taxpayers about their civic responsibility to pay taxes and the audit probability may help to increase compliance. An even more important (yet careful) lesson might be drawn from the result that the letter was even more powerful when handed over in person. This may suggest that an improved and more personal interaction of taxpayers and tax authorities can help to increase compliance. Our results correspond to the notion of Alm (2012) that he labels the “service paradigm” and according to which taxpayers and tax authorities should interact more closely.

It was one of the main aims of this paper to investigate whether an augmented form of transmitting information to taxpayers affects compliance. While it would have been desirable to disentangle the potential mechanisms through which the communication-effect works, it may already be valuable information for tax authorities and policy makers that more personal communication can help to combat evasion – even without knowing why exactly the intervention works. That is, providing first suggestive evidence that more personal interaction may work can help tax authorities to improve compliance. However, caution is required despite the compelling effects because we lack statistical significance. Therefore, future research should (i) explore the effect of communication on compliance more closely and provide more robust evidence, and (ii) disentangle the mechanisms through which communication affects taxpayers.

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Appendix

A Treatment letter

The English translation of the treatment letter is displayed below (the original Slovenian version is available upon request).

As part of the effort to ensure a more effective and fair tax collection, [*name of tax authority in Kranj*] would like to remind you that paying taxes is a civic duty. Taxes are important to maintain public schools, public infrastructure and public health provision. In this respect, we trust that you will honestly pay your taxes.

Additionally, the [*name of tax authority in Kranj*] has selected all accounting companies in Kranj for a special investigation. For that purpose, 10% of all accounting companies in Kranj will be randomly selected and subject to a special tax audit. The audits will be regarding the tax returns for the year 2013, which are due in April 2014. Hence, there is a probability of 10% that your return for the tax year 2013 will be closely investigated. If errors or omissions are found, you will be contacted by [*name of tax authority in Kranj*]. As always, you have the possibility of changing or adding items on your return until 31. March. 2014. This possibility applies even if you have already made adjustments to your return at this point.

Further, the [*name of tax authority in Kranj*] has selected a number of taxpayers who are clients of accounting companies for a special tax audit in 2014. Some of these taxpayers may be clients of your company.

Faithfully yours,

[*Signature of Head of Taxation Unit of tax authority*]