

TESTING ENFORCEMENT STRATEGIES IN THE FIELD: THREAT, MORAL APPEAL AND SOCIAL INFORMATION

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Abstract

We run a large-scale natural field experiment to evaluate alternative strategies to enforce compliance with the law. The experiment varies the text of mailings sent to potential evaders of TV license fees. We find a strong effect of mailings, leading to a substantial increase in compliance. Among different mailings, a threat treatment which makes a high detection risk salient has a significant deterrent effect. Neither appealing to morals nor imparting information about others' behavior enhances compliance on aggregate. However, the information condition has a weak positive effect in municipalities where evasion is believed to be common. (JEL: C93, H26, K42)

1. Introduction

Consider a taxpayer who earns an income which is subject to self-reporting requirements. If she does not report the income, she saves on taxes, but faces the risk of detection, which may entail fines and legal consequences. Rational taxpayers trade off the gains from evasion against the costs of detection (Allingham and Sandmo 1972).

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To achieve compliance, enforcement has to be sufficiently strong: for a given sanction level, taxpayers have to expect a sufficiently high detection risk. While several studies provide evidence on deterrence,¹ it is argued that compliance is not only shaped by formal law enforcement, but also by informal institutions, like social norms (see, e.g., Sandmo 2005). The question then arises whether enforcement should only be based on deterrent threats, or whether one can also draw upon alternative behavioral motives.

This paper offers new insights on this question by testing different enforcement strategies in a natural field experiment. The experiment was run in Austria in 2005. Austrian households that own a TV or radio are required by law to self-register for paying an annual fee for public broadcasting. The fee amounts to more than 200 per year. An enforcement problem exists because public broadcasting channels can be received without paying the fee. Evaders face a non-negligible detection risk and the threat of sizeable fines (see Rincke and Traxler 2011). The enforcement authority granted us access to data of more than 50,000 individuals who were identified as potential evaders. In cooperation with the authority, we sent mailings to randomly drawn 95% of this sample. The remaining individuals served as a control group and did not receive any mailings.

All mailings included a cover letter and a response form with a postpaid envelope. We randomly assigned individuals to treatment conditions that differed in the wording of the cover letter. The authority's standard letter, which was routinely used in their mailing campaigns before our study, served as baseline. The letter explained that the authority is legally obliged to clarify why, according to their database, the recipient is not paying fees and asked for a response within 14 days. We extended this letter along three dimensions: a *threat* treatment stressed a high detection risk and increased the salience of possible legal and financial sanctions; a *moral appeal* emphasized that compliance is also a matter of fairness; and a *social information* treatment highlighted the high level of compliance. We evaluate the effect of sending a mailing as well as the impact of the different mailing treatments by comparing registrations—that is, the number of evaders who start paying fees.

The main findings of the experiment are as follows. First, the mailings have a sizeable impact on compliance. Among the untreated control group, the fraction of individuals who start to pay the fee within 50 days of the experiment is only 0.8%. For the mailing recipients, the corresponding fraction is 7.7%. Second, the comparison across the different mailing treatments reveals a significant effect of the threat. On top of the basic effect of receiving a mailing, the threat increases compliance by one additional percentage point. Third, on aggregate, neither the moral appeal nor the social information treatment have an effect beyond that of the baseline mailing. However, the social information has a heterogeneous effect. When evasion is believed to be common (rare), the treatment has a weakly positive (negative) impact on registrations.

1. For recent contributions in the context of crime that solve the identification problem, see Drago, Galbiati, and Vertova 2009; Draca, Machin, and Witt 2011; Machin and Marie 2011. Evidence on tax enforcement is discussed in what follows.

The first result establishes a quantitatively important effect of addressing evaders by personalized mailings. There are several explanations for this finding. The mailings clearly reduce the transaction costs of registering. Mailings might also serve as “nudges” (Thaler and Sunstein 2008) and remind cheaters of their legal duty to register. Moreover, the mere fact of receiving a mailing signals that the authority suspects the recipient to cheat. This “alert effect” might be further amplified since all letters frame the contact with the enforcement authority as a legal interaction. Compared to evaders in the no-mailing condition, mailing recipients might therefore perceive a higher sanction risk.

While we cannot separate these different explanations, we provide complementary evidence that alludes to the relevance of the alert effect. We conducted a survey which exposed participants from a different sample to the treatments from the field experiment and analyzed treatment effects on various perception domains. The survey revealed that mailings cause a significant increase in the perceived detection risk. This suggests that the mailings indeed signal surveillance and thus – jointly with the decline in transaction costs and a possible reminder effect – have a pronounced impact on compliance.

Our second finding is that on top of the mailing effect, the threat causes a further rise in registrations. Compared to the baseline mailing, the effect corresponds to a 15% increase in compliance. In interpreting this observation, it is important to note that our treatments did not intervene with the actual enforcement level. The threat treatment stressed that—conditional on not responding—a member of the enforcement authority would personally request information from the mailing recipient. However, this was the standard enforcement practice that was applied to all individuals. The threat made this point salient, but the objective sanction risk remained constant across treatments. Adding the threat nevertheless increases compliance. In line with recent contributions that emphasize the link between policies, subjective perceptions and compliance behavior (see, e.g., Lochner 2007; Hjalmarsson 2009), our perception survey indicates that the threat effect can be traced back to an increase in the perceived detection risk. The evidence suggests that the threat shapes perceptions about the costs of cheating and that individuals adjust their behavior to these perceptions—that is, the threat deters evaders.²

Three pieces of evidence complement this result. First, we exploit information on heterogenous types of individuals in our sample. We show that the threat has a strong effect on evaders who face potentially severe sanctions in case of detection, whereas it has no effect on types who are not exposed to an actual sanction risk. Second, we consider the timing of the responses. While all mailings asked to respond within two weeks after receiving the mailing, evaders typically delay registrations and hence the payment of the fee. The threat shifts registrations from later weeks to the first 14 days. This fits the picture of evaders trying to avoid an inspection which follows not responding in time.

2. It is reassuring to see that the perception survey also mirrors the pronounced effect of sending mailings and the relatively modest additional increase in compliance from the threat: the impact of receiving any mailing on the expected detection risk is substantially larger than the additional effect of a threat mailing.

Finally, we also studied the longevity of the effect from the threat. Half a year after our experiment, only 2.36% of those who initially registered had deregistered from paying license fees. However, the frequency of deregistrations was roughly twice as high for registrations in response to the threat than from mailings without a threat (2.96% versus 1.63%). This is exactly what one would expect if the threat is more likely to enforce compliance among individuals who are at the margin to evasion, as these individuals are also most likely to later cancel their registration. Although the de-registration effect is statistically significant, it is quantitatively negligible and hardly reduces the long-run effectiveness of the threat.

Our first set of results adds to the evidence from field experiments in the domain of tax enforcement. In a recent study, Kleven et al. (2011) sent letters to a random sample of Danish taxpayers, who were informed that their tax returns would be audited (with certainty or with a chance of 50%). A control group received no letter. In line with our results, Kleven et al. found that the audit-threats have a positive impact on self-reported income. In a similar setup, the Internal Revenue Service (IRS) approached taxpayers in Minnesota with mailings (Blumenthal, Christian, and Slemrod 2001; Slemrod, Blumenthal, and Christian 2001). The communication of a high auditing risk had mixed results: low and middle-income taxpayers increased reported income, whereas high-income taxpayers reported *lower* tax liabilities (Slemrod, Blumenthal, and Christian 2001).³ The authors argue that this effect might be due to strategic responses of taxpayers with a low reported income who prefer to enter a (bargaining-like) interaction with the IRS.

Beyond the institutional context, the present paper differs from these studies in several aspects. Most importantly, our sample was not a random draw from a predefined population, but a selected sample of potential evaders. Moreover, the audit-threat letters explain to taxpayers that they were randomly selected for these audits. In our case, the letters clearly communicate that the recipients were selected because the authority suspects them of cheating. Finally, we do not measure differences in reported income, but a binary outcome variable: whether or not an evader starts to comply. Despite all these differences, it is interesting to note that our threat treatment produces a quantitatively similar effect to the audit-threats in Kleven et al. (2011).

Turning to the third finding of the field experiment, our results show that adding moral suasion or social information to the mailing does not increase compliance. To evaluate the ineffectiveness of the moral suasion, it is again important to recall that our mailings were targeted at a selected sample of cheaters. It is thus unlikely that the mailing recipients are guided by moral values that support compliance. Hence, our finding does not imply that moral standards are irrelevant for the compliance of the overall population. Still, the null-result is important as it indicates that moral appeals are not an attractive policy to enforce compliance among those who deviate from the law.

3. In an experiment with the Australian tax authorities, a deterrent letter did not have any significant impact either (Wenzel and Taylor 2004). Pomeranz (2010) discusses a field experiment on VAT evasion of firms that does find deterrence effects.

While our evidence is in line with studies on tax enforcement that found no effects of moral appeals either (Blumenthal, Christian, and Slemrod 2001; Wenzel and Taylor 2004), the null result seems at odds with recent contributions documenting positive effects of normative appeals. Mazar, Amir, and Ariely (2008) and Pruckner and Sausgruber (2013), for instance, find that normative messages are effective in preventing people from cheating. These papers crucially differ from ours, though, as they are based on population subsamples that are drawn independently of particular moral standards. Our data suggest that honesty reminders might not work if one applies them to the most dishonest part of the population.

The social information treatment, which communicated that 94% of all Austrians follow the law and pay the fee, could affect compliance by altering perceptions about the formal and informal sanction risk for cheating. To see this, note that norm-enforcing social sanctions are considered to be stronger the more people follow a norm (Elster 1989). The social information treatment may therefore shape perceptions regarding the strength of informal law enforcement. Equivalently, the “broken windows theory” (Wilson and Kelling 1982) suggests that the provided information may also change perceptions about formal law enforcement. This implies that the impact of the social information on behavior crucially relies on individuals’ prior beliefs about others’ compliance. With noncommon priors, the provided information can affect beliefs—and thus compliance—in either direction.

Consistent with this prediction, we find evidence that the social information has significantly different effects when evasion is believed to be common and when it is believed to be rare. In the former case, the treatment has a weak positive, in the latter a weak negative effect on compliance. This observation contributes to the research on conditional cooperation (Gächter 2007) as it provides evidence on individuals conditioning their compliance on the (perceived) compliance of others. The finding also relates to the literature on social influence (Cialdini 1998). Closest to our study is the work by Schultz et al. (2007), who informed households about their own and their neighbors’ energy consumption. In response to the information, households that consumed more (less) than others decreased (increased) their energy consumption. Similarly, Costa and Kahn (2010) show that the effect of providing households with information on their electricity consumption depends on household ideology. Beshears et al. (2010) find that information on co-workers’ saving behavior has heterogeneous effects on the retirement savings decisions of unionized and non-unionized workers. These results closely resemble our findings on the heterogeneous effect of social information.

In summary, our study provides comprehensive evidence on the effectiveness of alternative enforcement strategies from a large-scale field experiment. We show that addressing cheaters with mailings has a substantial impact on compliance. In our context, the effect corresponds to a net revenue per mailing of €15 and, with 100,000 annual mailings sent by the authority, to €1.5 million of additional revenues per year. The effectiveness of the mailings is further increased by a threat, which makes the risk and the consequences of a detection more salient. Apart from the enforcement of

license fees our results might be informative to several other enforcement domains. Tax authorities, for instance, frequently address taxpayers with letters that point out problems in their tax returns (e.g., IRS mail audits). Similarly, people suspected of breaching copyright by illegally downloading music or videos are addressed with warning letters. Like in our context—and different from general audit-threats—such letters signal surveillance. It appears plausible that our findings generalize to these applications.

The remainder of the paper is structured as follows. Sections 2 and 3 present the institutional background and the design of the field experiment. In Section 4, we discuss several hypotheses regarding the impact of our treatments. The results are analyzed in Section 5. Section 6 provides a discussion and presents complementary survey evidence. Section 7 concludes.

2. Institutional Background

A significant share of radio and television broadcasting around the world is provided by public broadcasters that are mainly financed through TV and radio *license fees*. In Europe, the total amount of fees collected adds up to roughly €20 billion.⁴ A typical license fee system is in place in Austria. According to the Austrian *License Fee Act*, households must file a registration and pay annual license fees if they own a TV or a radio. The size of the fee is substantial. In 2005, it ranged from €206 to €263 (varying between federal states). The amount is due per “household” (defined in a broad sense, including apartment-sharing communities, etc.), regardless of the number of household members, TVs, and radios. An enforcement problem exists since public broadcasting programs can be received without paying the fee.

The license fee system is managed by “Fee Info Service” (*Gebühren Info Service*, henceforth GIS), a subsidiary of the Austrian public broadcasting company. GIS is responsible for collecting and enforcing the fees. To identify potential evaders, GIS compares residence data with its own database. Residents who have not registered a TV or radio (and no other household member is known to pay the fee) are treated as potential evaders. This selected group is first addressed by mailings, which are sent by GIS in regular mailing campaigns. The License Fee Act requires mailing recipients to respond and to provide correct information on why they do not pay fees. Data on those who do not respond are then handed over to GIS enforcement division. This division employs field inspectors who check potential evaders at their homes (see Rincke and Traxler 2011). In fact, detections by field inspectors are quite frequent. In 2004, door-to-door inspections resulted in a clearance rate of one-third. A detected evader is registered and typically has to pay fees for several past months. In addition, a case may be reported to the authorities who can impose a fine of up to €2,180. If someone

4. Own computation for the year 2005, based on information provided by the *Broadcasting Fee Association*. On the funding of public broadcasting, see also Head (1985).

does not comply with the payment duty after an official report, legal proceedings will be initiated.⁵

GIS enforcement activities are reflected in a high compliance rate: in July 2005, 94% of all Austrian households were registered for license fees and paid a total of €644 million (0.3% of GDP). The 94% give a reasonable proxy for the overall compliance level, as only 1% of households own neither a TV nor a radio. Once someone is registered, she typically continues to pay license fees in the following years. To stop paying, one has to deregister actively by stating that one no longer operates a broadcasting receiver. In addition, moving households have an easy opportunity to start evading by de-registering at the old place without registering at the new place. Hence, compliance is in constant flux.

3. Experiment Design and Data

We experimentally manipulated mailings that were sent by GIS to potential license fee evaders. Following standard GIS procedures, a mailing contained a cover letter, an information sheet and a response and registration form with a postage prepaid envelope. In the letter, GIS explains that—according to their data—the mailing recipient has not registered any TV or radio and is required by law to clarify the facts by returning the response form within 14 days. The information sheet listed several key paragraphs of the License Fee Act. In particular, it provided information about the payment duty, the size of the fee and the maximum fine that can be imposed in case of detection.

3.1. Treatments

The experiment varied the text of the cover letter.⁶ Everything else—the response deadline, the response form, and the info sheet (and thus the information on possible fines)—was the same across treatments. GIS' standard letter, which was used in their regular mailing campaigns before our study, served as *baseline* (T1). This letter was extended along three dimensions: we introduced a *threat* (T2), *social information* (T3), and a *moral appeal* (T5). We also interacted the threat with the two other dimensions (*threat* × *info*, T4; *threat* × *moral*, T6), resulting in six different mailing treatments. In addition, we implemented a *no-mailing* condition (T0). This untreated control group did not receive any mailing.

The threat treatments contained a paragraph that communicates a significant detection risk by pointing out GIS' standard enforcement practice. The treatment also increased the salience of potential consequences of noncompliance:

5. Austrians seem to be aware of the possible sanctions. A national survey finds that 55% of respondents expect severe or very severe "sanctions if one is detected evading license fees For participation in the black labor market [absenteeism from work], the corresponding figure is 60% [38%] (see Traxler and Winter 2012).

6. The full text of the letters used in the experiment is provided in the Online Appendix.

If you do not respond to this letter, a staff member of GIS will contact you in order to request information from you personally. If you refuse to provide information or if there is a well-founded suspicion that you provide disinformation, GIS is obligated to order an inquiry by the responsible federal authorities. Please keep in mind that in this case you may face legal consequences and considerable costs.

The social information treatments included a paragraph which provided information about the actual level of compliance with the law (as estimated by GIS):

Do you actually know that almost all citizens comply with this legal duty? In fact, 94%—a vast majority of all households—have registered their broadcasting receivers.

Finally, the moral appeal extended the baseline letter by the following sentences:

Those who do not conscientiously register their broadcasting receivers not only violate the law, but also harm all honest households. Hence, registering is also a matter of fairness.

3.2. *Sample and Implementation*

GIS provided us with data on 50,498 potential evaders who have not received a mailing before—at least, not at the current place of residence. From these data we first took a 5% random subsample and assigned it to the no-mailing group (T0). The remaining data were randomly allocated to the six mailing treatments (T1–T6). Table 1 provides summary statistics and demonstrates that the assignment of treatments was orthogonal to observable individual and municipality characteristics. Variance analyses confirm the null hypothesis of equality of means across treatments for all variables in Table 1.

GIS routinely receives updated residence data from municipalities. By comparing these data with their own data on license fee registrations, potential cheaters are identified. Our sample consists of the most recent wave of data on potential cheaters that were available in August 2005, and that were not addressed in prior mailing campaigns. The sample is certainly not representative of the overall population. It is a selected sample of evaders plus individuals that were selected by mistake. One can classify three different types of individuals in the sample. First, there are evaders, individuals that have not registered their TVs/radios. As it takes several months until new entries (e.g., a household that recently moved) appear in the residence data available to GIS, it appears plausible to denote those who are not yet paying fees by the time they receive a mailing as deliberate evaders rather than forgetful procrastinators. Second, the sample contains individuals who comply with their payment duty or live in a household that does so.⁷ Finally, the sample includes individuals who neither own a TV nor a radio. As we will discuss in what follows, these different types face different response options and different incentives to respond.

The mailings were sent by GIS in two waves during September and October 2005. Each wave contained all treatments in equal proportions. Between September and December, GIS made no changes in their field inspection policy, nor did

7. Such types are in the sample either because GIS failed to track that someone else in the household is already paying fees or because a complying individual changed her address or name (e.g., after marriage) without reporting the changes to GIS.

TABLE 1. Mean values of individual and municipality characteristics per treatment.

	Individual characteristics			Municipality characteristics			Number of observations
	Gender (% of males)	Age (years)	Population size	Pop. density (inh./km ²)	Compliance rate (in %)		
Total	63.3 (48.2)	36.4 (11.8)	43,941 (75,792)	825 (2,278)	93.5 (5.8)	50,498	
T0	64.6 (47.8)	36.9 (12.1)	45,815 (77,148)	817 (2,140)	93.5 (5.8)	2,586	
T1	63.4 (48.2)	36.5 (11.8)	43,377 (75,306)	856 (2,417)	93.5 (5.7)	7,984	
T2	63.7 (48.1)	36.3 (11.9)	44,543 (76,469)	811 (2,188)	93.5 (5.8)	7,821	
T3	62.6 (48.4)	36.5 (11.8)	43,903 (76,001)	796 (2,175)	93.5 (5.8)	7,998	
T4	63.3 (48.2)	36.1 (11.8)	43,319 (75,326)	835 (2,307)	93.5 (5.7)	8,101	
T5	62.5 (48.4)	36.4 (12.1)	44,301 (75,938)	848 (2,368)	93.4 (5.7)	8,084	
T6	64.3 (47.9)	36.6 (11.8)	43,610 (75,289)	805 (2,243)	93.4 (5.8)	7,924	
Anova: <i>p</i> -values	0.17	0.33	0.76	0.58	0.86		

Notes: Age is only available for a subsample of 16,281 recipients. Population density is measured by the number of inhabitants per square kilometer. Similar to the overall compliance rate, GIS approximates local compliance by the share of households who are registered for license fees relative to the total number of households living in a municipality. Compliance rates are pre-experiment. Standard deviations are in parentheses. The Anova tests the null hypothesis of equal means across treatments.

they inform anybody about our study. The mailing recipients were not aware of participating in an experiment. This feature distinguishes our study as a natural field experiment.

Mailing responses and unsolicited registrations in the no-mailing group were measured by GIS' computer system. The system allowed for an accurate measurement of behavior: it tracked responses via the reply forms that were sent along with the mailings, registrations that were made online or by calling the GIS service hotline, and it detected responses from other individuals in the mailing recipient's household (e.g., the spouse).

The mailing responses were classified into four categories.

- A new registration for license fees.
- An update of contract details (name, address, etc.), or a statement that someone in the household is already paying the fee.
- A statement of the recipient that there is no broadcasting receiver in the household.
- Any response that cannot be classified into the other categories.

The response categories can be linked to the type classification. Those who are liable to pay fees can either respond to the mailing by registering or by asserting that they have no radio or TV. Those who are already law-abiding may clarify their status by an update response, which requires a valid registration number (e.g., from the registration of another household member). If GIS cannot match the number with their data, the response is classified as category D. Finally, the few without radio or TV may respond in category C. Of course, all types can simply ignore the mailing.⁸

One can unambiguously link registrations and update-responses to particular types. Only evaders can register for license fees and only law-abiding individuals can update contract details. The primary focus of GIS is to maximize revenues—that is, registrations. Their secondary target are contract updates, as they help GIS to improve the targeting of their enforcement measures on the noncompliant population. In the following, we will consider registrations and contract updates. This allows us to study whether strategies that are successful in enforcing compliance—that is, strategies that trigger a high rate of registrations—cause any undesirable impact on law-abiding individuals—in the form of less-frequent contract updates. As evaders and law-abiding mailing recipients face quite different incentives to respond, a type-specific response analysis is also interesting from a theoretical point of view, as it allows us to infer the channels driving the response to our interventions.

8. One could think of several other possibilities. For instance, an evader could assert incorrectly that someone else in the household pays fees. Based on the registration number, this would be discovered and coded as D-response. Honest households might make mistakes and register a second time. Such errors would be discovered by GIS that is obliged to cancel any double registration.

4. Expected Treatment Effects

4.1. Registrations

Consider a cheater who chooses between registering and not registering her TV. If she does not register and continues to evade the fee, she will be detected with some probability. In this case, the evader has to pay the fee, supplementary payments and maybe a fine. Rational decision makers trade off these potential costs with the benefits from evading. The decision depends on their risk preferences and—with imperfect information—the perceived sanction risk (Sah 1991).

4.1.1 No-Mailing Treatment. Comparing the no-mailing group (T0) with the mailing treatments (T1–T6), the mere fact of receiving a mailing signals that GIS suspects the recipient of violating the law. Evaders in the no-mailing group do not receive this signal. Hence, we suppose that mailing recipients perceive a higher risk of formal sanctions and are therefore more likely to register than individuals in the no-mailing condition. Recall further that all mailings included a registration form and a prepaid envelope. Individuals in the control group had to obtain the form themselves. Hence, the mailings decrease the transaction costs of registering, which should again increase registrations (on this point, see also Huck and Rasul 2010). Moreover, mailings might serve as a “nudge” (Thaler and Sunstein 2008), remind cheaters of their legal duty and—by stating a response deadline of 14 days—reduce procrastination (Ariely and Wertenbroch 2002). While we cannot separate these different channels, there are two reasons to doubt that reminder effects are quantitatively important in our context. First, GIS runs intensive media campaigns that remind people of the legal obligation to register radios and TVs. During the time of the experiment, there were on average three daily spots in countrywide broadcasted channels. Thus, cheaters already receive quite frequent reminders. Second, as discussed in Section 3.2, the individuals in our sample are more likely to be deliberate evaders rather than forgetful procrastinators.

Turning to the mailing treatments, it is important to recall that our six treatments did not change the “true” incentives for compliance—the actual detection risk and the magnitude of the sanctions were constant across all treatments. Thus, any behavioral change in response to our mailing treatments must be brought about by changes in the recipients’ perceptions. Recall further that all mailings frame the contact with the authority as one based on legal enforcement. The question is then whether our different mailing treatments strengthen or weaken the effect of the baseline mailing.

4.1.2 Threat. The threat aims at enforcing compliance through intervening with the perceived sanction risk.⁹ By communicating a high detection risk and increasing the

9. The threat letter may also convey information on how society views non-compliance. However, the survey results (discussed in what follows) suggest that the threat affects behavior via altering perceptions regarding the legal, but not the social sanction risk.

saliency of financial and legal consequences, we expect the treatment to increase the expected costs of cheating. In turn, this should result in more registrations. Of course, this requires (some) individuals to have subjective expectations.¹⁰ While this case finds ample empirical support (e.g., Lochner 2007), there is still no clear-cut evidence of the extent to which threats change risk perceptions and thus behavior.

4.1.3 Social Information. By changing perceptions about the others' compliance, the social information treatment may alter perceptions about the formal and informal enforcement of compliance. According to the "broken windows theory" (Wilson and Kelling 1982), signs of noncompliance signal lax enforcement and thereby trigger further deviations from the law. A similar bandwagon effect emerges if one considers social norms or conformity motives (Elster 1989). Given that the informal enforcement of a certain behavior is the stronger, the more people follow the behavior, we should observe that individuals condition their compliance on the compliance of others.¹¹ Thus, adding information about a 94% compliance rate should have a positive effect on registrations by individuals who initially expect lower levels of compliance. The opposite holds for those who initially consider compliance to be higher than communicated. Hence, the impact of the social information crucially relies on people's prior beliefs. Evidence from the literature on social influence lends further support to this prediction (see, e.g., Schultz et al. 2007).

4.1.4 Moral Appeal. The appeal stresses that license fee evaders violate not only a legal, but also a fairness norm, and that they harm honest households. Psychology research has illustrated the importance of personal norms and moral concepts, as well as their saliency, as driving forces of behavior (Cialdini 1998). Provided that our letter makes a relevant moral concept more salient, one might expect the moral appeal to have a positive impact on registrations. This prediction can be further motivated by evidence showing that honesty reminders are effective to prevent people from cheating (e.g., Mazar, Amir, and Ariely 2008; Pruckner and Sausgruber 2013). However, the prediction is at odds with field experiments on tax enforcement that find no evidence on the effectiveness of moral suasion (Blumenthal, Christian, and Slemrod 2001; Wenzel and Taylor 2004). From a traditional economic perspective this is not surprising. Quite on the contrary, the treatment could even backfire as the moral appeal could "be read as a sign that the enforcement system cannot cope and must resort to rhetoric instead" (Bardach 1989, p. 62).

10. Manski (2004) offers a review of the subjective expectations literature. For recent contributions on the role of subjective expectations for law enforcement, see Lochner (2007) and Hjalmarsson (2009).

11. The case for conditional compliance is also made in Traxler and Winter (2012), who find that half of the participants in a national survey in Austria are willing to impose informal sanctions on license fee evaders. They further show that the inclination to sanction declines with the belief about the pervasiveness of noncompliance.

4.2. *Contract Updates*

Individuals that already comply with their payment duty are equally obliged to respond to the mailings. However, GIS never imposes sanctions on complying individuals when they do not respond. Hence, there are no formal consequences for not responding. At the same time, responding entails only minor transaction costs since the return mailing is prepaid. It is therefore difficult to assess the treatments' impact—in particular, the effect of the moral appeal and the social information—on the response from law-abiding individuals. Of course, they might not be aware that they face no risk of formal sanctions. They might simply want to avoid an interaction with a field inspector – an event they may find embarrassing even if there are no material consequences at stake. If the expected embarrassment from an inspection drives the response, or if the law-abiding expect economic sanctions, the threat should result in more update responses. The impact of the threat should be smaller, however, the lower the expected costs from an inspection are. This has straightforward implications for the comparison of evaders and honest types. Given that the threat increases the perceived inspection risk of both types, honest guys should be less sensitive to the threat than evaders, as the former should expect lower sanctions than the latter.

5. Results of the Experiment

We now turn to the results from the field experiment. Sections 5.1 and 5.2 provide a non-parametric analysis of the treatment effects. Section 5.3 analyzes the longevity of the effects. Finally, Sections 5.4 and 5.5 discuss estimation results and offers evidence on interaction effects with municipality characteristics.

5.1. *Overall Effect of Sending Mailings*

First, we compare the frequency of registrations in treatments T1–T6 to the unsolicited registrations in the control group (T0) in intervals of 25 days.¹² Within 25 days after sending the first mailings, only eight out of the 2,586 individuals (0.31%) registered for license fees in the no-mailing group. In contrast, 2,794 out of 47,912 mailings (5.83%) resulted in a registration within the first 25 days after sending the respective mailing. The difference is highly significant ($p = 0.000$, according to a two-sided test on the equality of proportions). In the second 25 days, the registration rate was 0.50% in the control and 1.83% in the mailing treatments ($p = 0.000$). Beyond 50 days, we do not observe any differences in registration rates. Hence, the impact of the mailings on registrations is limited to the first 50 days.

After 50 days, 0.81% of the individuals in the no-mailing condition were registered. In the mailing treatments, the cumulated registration rate was 7.67%—nearly ten times higher. Assuming that newly registered households pay license fees for at least one

12. A similar picture emerges if weekly or biweekly intervals are considered.

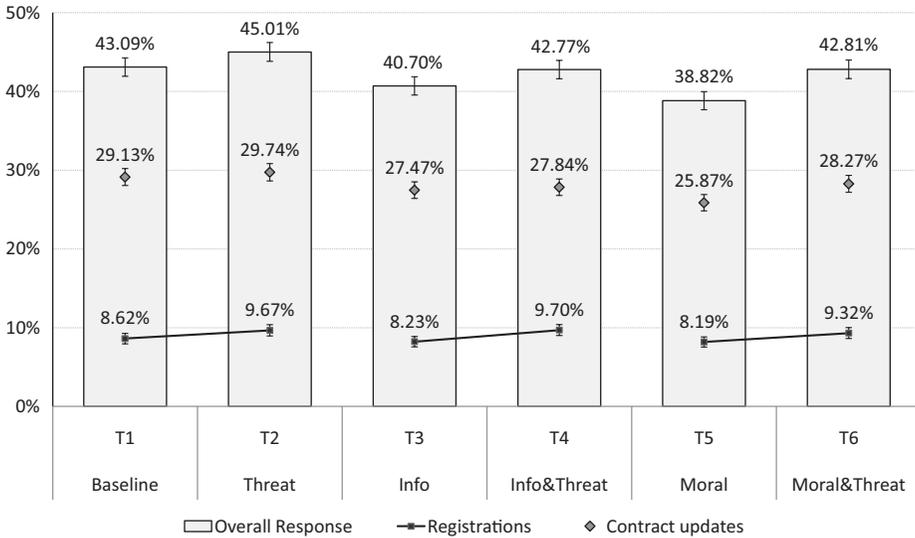


FIGURE 1. Mailing response within 50 days. Percentages are relative to the number of *delivered* mailings. Vertical lines indicate 95% confidence intervals.

year (which, according to GIS, is a very conservative assumption), the expected net revenue per mailing (net of the marginal cost for printing, postage, etc.) amounts to 15. As discussed previously, there are several possible explanations for the strong impact of the mailings. While our experiment was not designed to separate these channels, Section 6.1 presents further evidence that alludes to the relevance of one particular channel.

5.2. Effects of the Mailing Treatments

Figure 1 summarizes the results for the mailing treatments T1–T6. The figure displays registrations, updates, and the overall response relative to delivered mailings.¹³ Motivated by the previous results, we focus on the response within 50 days. All our results are robust to extending this observation period.

Consider first registrations. Recall that only evaders can register. The share of registrations thus measures the treatments’ success in enforcing compliance. Figure 1 shows that in the baseline treatment 8.62% of recipients responded with a registration for license fees. A comparison of the registration rates reveals a significant positive effect of the threat between T1 and T2 ($p = 0.034$), T3 and T4 ($p = 0.003$), and between T5 and T6 ($p = 0.020$, see bars combined by line in Figure 1). In contrast,

13. On average, 14.41% of the mailings could not be delivered because of erroneous addresses. Due to the random assignment, this share of nondelivered mailings does not statistically differ between treatments (χ^2 -test: $p = 0.793$). In the following, we report responses relative to mailings delivered rather than mailings sent. Note that we cannot measure any equivalent to the nondelivered mailings in T0.

neither the social information nor the moral appeal have any significant effects (T1 versus T3: $p = 0.420$, T1 versus T5: $p = 0.369$). This finding does not change when the two treatments are interacted with the threat (T2 versus T4: $p = 0.948$; T2 versus T6: $p = 0.493$).

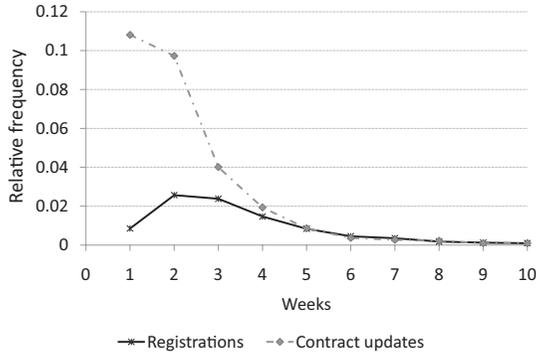
As the precise distribution of evading and complying individuals (and individuals without radio/TV) in our sample is not identified, it is difficult to judge whether the observed registration rates correspond to particularly low or high *type-specific* response rates. If, say, two [one] out of three mailing recipients were evaders (and assuming that all of them read the mailings), a registration rate of 8.62% would imply that a fraction of 12.93% [25.86%] of all evaders changed their behavior in response to the mailings.¹⁴ Hence, we cannot obtain point estimates for the overall persuasion rate (see DellaVigna and Gentzkow 2010). Due to the randomization, however, the unobserved type distribution should not differ between treatments. Moreover, the fraction of evaders who read the mailing should not vary either. One can therefore interpret the relative changes in registrations as the treatments' impact on the persuasion rate of cheaters. Along these lines, the results indicate that the threat increases the frequency of evaders that stopped cheating by 12.2 (T2 relative to T1), 17.9 (T4/T3) and 13.8% (T6/T5), respectively. On average, this resulted in an increase in compliance by an additional 15% on top to the basic mailing effect.

We now turn to contract updates. Figure 1 indicates that 29.13% updated their information on a valid registration in treatment T1. The threat has no effect on update responses between T1 and T2 ($p = 0.437$), nor between T3 and T4 ($p = 0.625$). It is significant only when it interacts with the moral appeal (T5 versus T6: $p = 0.002$). However, the moral framing per se has a significantly negative impact on update responses (T1 versus T5: $p = 0.000$). In treatment T6, the threat only partially counterbalances this negative effect, but the response rate is still below the one in the non-interacted threat treatment (T2 versus T6: $p = 0.059$). The social information treatment also triggers a significant decline in contract updates (T1 versus T3: $p = 0.031$; T2 versus T4: $p = 0.014$).

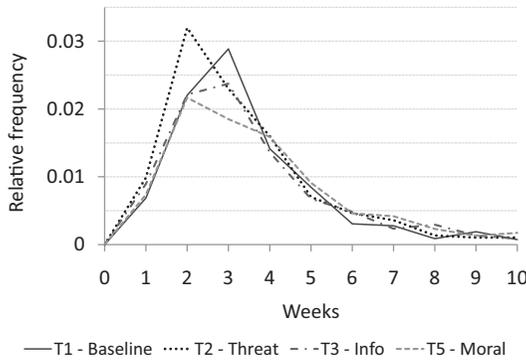
The overall response rate, which varies between 39 and 45%, documents again a positive effect of the threat (T1 and T2: $p = 0.024$; T3 and T4: $p = 0.014$; T5 and T6: $p = 0.000$) and negative effects from the social information (T1 versus T3: $p = 0.005$; T2 versus T4: $p = 0.008$) and the moral appeal (T1 versus T5: $p = 0.000$; T2 versus T6: $p = 0.010$). Given GIS' target of maximizing registrations and contract updates, but also concerning the overall response, the threat treatment T2 was the most successful mailing strategy.

Figure 2 displays the time pattern of the response in weekly intervals. Panel (a) compares the timing of registrations and contract updates. Apart from the difference in levels, the figure shows that update responses arrive mainly in the first two weeks after sending the mailings. Hence, most contract updates meet the requested 14-day response deadline. In contrast, registrations come with some delay. The main bulk of

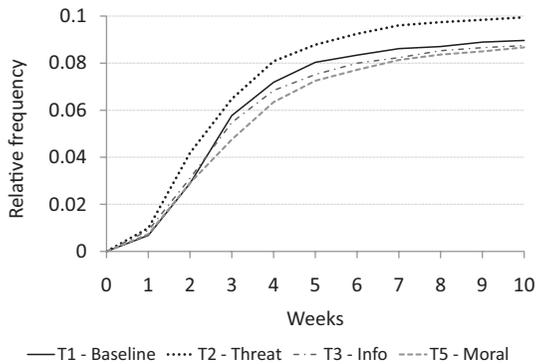
14. See also footnote 17 and, for a closer discussion, Appendix IV in Fellner, Sausgruber, and Traxler (2009).



(a) Frequency of registrations and contract updates relative to delivered letters.



(b) Frequency of registrations across the four main treatments relative to delivered letters.



(c) Cumulative frequency of registrations across the four main treatments.

FIGURE 2. Time patterns of response.

registrations arrives in the second and third week. A possible explanation for this is that evaders have a clear incentive to delay their response in order to postpone the start date for paying license fees. For those who already comply with TV license fees, this is not the case.

Panel (b) illustrates the treatments' impact on the timing of registrations (we focus on the non-interacted treatments). In the baseline (T1), the mode of registrations is in the third week. Compared to this benchmark, the threat treatment (T2) produces an inter-temporal substitution of registrations from the third to the second week, with a major drop thereafter. This suggests that the two-week deadline is taken more seriously in the presence of the threat. While the threat increases registrations particularly in the first two weeks, the other two treatments show hardly any difference to the baseline within this period. In the third week, the social information (T3) and the moral appeal (T5) produce fewer registrations than the baseline mailing. As captured by the cumulative response in Panel (c), however, the gap in registrations declines over time (in particular for T5). Only the positive effect of the threat persists over time.

5.3. *Temporary versus Permanent Effects*

An important question concerns the longevity of our treatment effects. Recall that those who register for license fees continue to pay fees unless they deregister. Deregistrations occur if households move or if they—correctly or incorrectly—assert that they no longer operate a broadcasting receiver. To study how many people deregistered, we had a look at the status of registrations half a year after the experiment. GIS provided us with information on the status of 2,291 out of the 3,671 registrations in response to the mailings.¹⁵ Among this group, only 2.36% had deregistered after six months. 97.64% of those who initially registered continued to pay fees.¹⁶

Figure 3 shows that deregistrations are not equally distributed across treatments. For registrations in response to mailings that contained the threat, deregistrations are more frequent. The difference in deregistrations is modest between T1 and T2, but it is quite pronounced for the remaining treatments. Overall, the deregistration rate in the threat treatments is nearly twice as high as in the treatments that do not include the threat (2.96 versus 1.63%; $p = 0.037$, according to a two-sided test on the equality of proportions; single treatments show no significant differences.) This observation is not surprising, given that the threat is more likely to enforce compliance among individuals who are at the margin between evasion and compliance—who are also most likely to later cancel their registration. Note further that the de-registration effect is only statistically significant in reducing the long-run effectiveness of the threat. Quantitatively, the effect is negligible. Among those treated with a threat, the post-experimental compliance rate is still one percentage point higher.

15. The GIS database identified only those cases where people register under exactly the same name and address as printed on the mailings. If, for instance, the addressee registered under a slightly different name (e.g., including a middle name) or if the spouse responded to the mailing, GIS was not able to track the status of the registration without considerable effort. Changes in the database structure also prevent an analysis of deregistrations at a later point in time.

16. One might argue that many people pay the fees habitually (see Gerber, Green, and Shachar 2003, for a related study on this issue). The fact that habit formation depends on the way of getting people to act for the first time is illustrated by Landry et al. (2011). In our context, however, the low number of deregistrations might be driven by the fact that the continuation of the payments is the default (Thaler and Sunstein 2008).

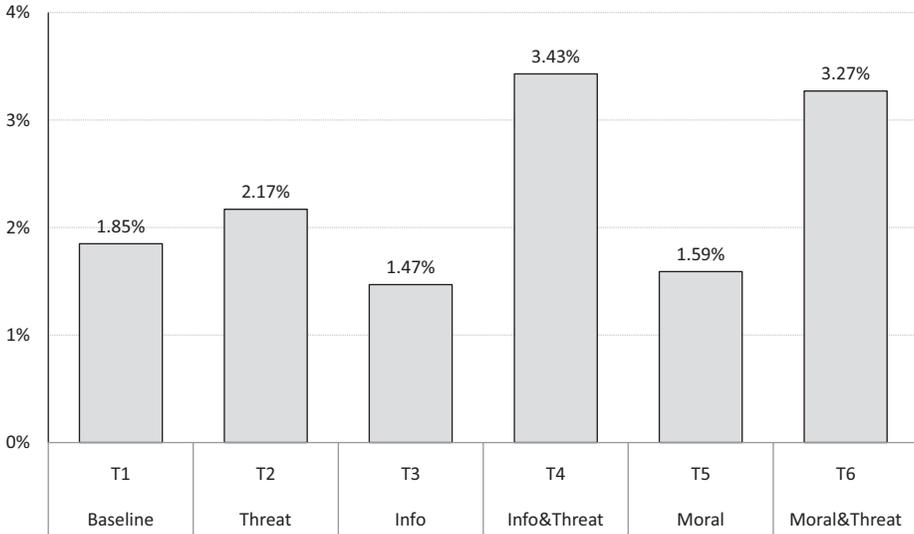


FIGURE 3. Frequency of deregistrations.

5.4. Regression Analysis

5.4.1 Registrations. We complement the analysis by running regressions on registrations, update responses and the overall response. As dependent variable we first use a dummy P_i^{reg} that indicates whether individual i registered within 50 days.¹⁷ We consider regressions of the form

$$P_i^{reg} = \alpha + \beta_0 Mailing_i + \beta_1 Threat_i + \beta_2 Moral_i + \beta_3 Info_i + \varepsilon_i. \tag{1}$$

The variable $Mailing_i$ indicates if the individual was in a mailing or in the untreated control group. $Threat_i$, $Info_i$ and $Moral_i$ are treatment dummies indicating if individual i received a mailing that included the threat (T2, T4, and T6), social information (T3 and T4) or the moral appeal (T5 and T6), respectively. Hence, β_0 , captures the basic effect of the mailings (relative to the no-mailing condition) and β_1 , β_2 and β_3 measure the treatments’ impact on registrations (relative to the baseline mailing T1). To account for potential interactions between the treatments, we additionally include

17. The variable—which is insensitive to responses from non-evaders (individuals who already comply or those without a radio or TV)—is determined by the share of evaders in the sample and their response behavior. The exact frequency of evaders is not identified and one cannot simply use the bounds on the type distribution that are implied by update responses, as these responses are also sensitive to the treatments. Our estimations assume that each mailing recipient could produce a registration. While we obtain consistent estimates of the treatment effects on the observed registration frequency for our sample, we will underestimate the *type-specific* treatment effects. A similar caveat applies to the analysis of contract updates. (See also Appendix IV in Fellner, Sausgruber, and Traxler 2009.)

TABLE 2. Treatment effects on registrations, contract updates, and overall responses.

Dependent variables:	Registration		Contract update		Overall response	
	(I)	(II)	(III)	(IV)	(V)	(VI)
Mailing	0.065*** (0.003)	0.066*** (0.003)				
Threat	0.010*** (0.002)	0.009** (0.004)	0.011** (0.004)	0.006 (0.008)	0.027*** (0.005)	0.019** (0.009)
Moral	-0.004 (0.003)	-0.004 (0.004)	-0.024*** (0.005)	-0.033*** (0.008)	-0.032*** (0.006)	-0.043*** (0.008)
Info	-0.002 (0.003)	-0.004 (0.004)	-0.018*** (0.005)	-0.017** (0.008)	-0.023*** (0.006)	-0.024*** (0.008)
Threat × Moral		0.001 (0.006)		0.018 (0.011)		0.020* (0.012)
Threat × Info		0.004 (0.006)		-0.002 (0.011)		0.001 (0.012)
Constant	0.008*** (0.002)	0.008*** (0.002)	0.289*** (0.004)	0.291*** (0.005)	0.427*** (0.005)	0.431*** (0.006)
Observations	50,498	50,498	41,007	41,007	41,007	41,007

Notes: All specifications are estimated with a linear probability model. The dependent variables are dummies indicating a registration [(I) and (II)], a contract update [(III) and (IV)], or any response [(V) and (VI)] within 50 days. The estimations in (I) and (II) are based on the full sample, whereas columns (III)–(VI) use the sample of delivered mailings. Robust standard errors are in parentheses.

***Significant at the 1% level; **significant at the 5% level.

Threat × Moral_i and *Threat × Info_i* in equation (1). All equations are estimated using the linear probability model.¹⁸ The results are reported in Table 2.

The regressions confirm our findings from above. Receiving a mailing has a strongly positive effect on registrations. Among the different mailing treatments, the threat significantly increases individuals' propensity to register. Neither the moral appeal nor the social information have a significant effect. The outcome from column (I) remains unchanged when we account for treatment interactions: none of the interaction terms in column (II) are statistically significant. An *F*-test does not reject the null hypothesis that both interaction terms equal zero ($p = 0.728$).¹⁹

The evidence documents the effectiveness of the mailings (see Section 5.1) and the impact of the threat. Threatening evaders with inspections clearly works as an enforcement strategy, whereas the moral appeal and the social information fail. Including the threat increases the registration rates by slightly more than one percentage point. In comparison to the mailings without the threat paragraph, the effect corresponds to 15% more registrations or an additional increase in the net revenue per

18. In the following, we estimate equations with many interaction terms as explanatory variables. Computing correct interaction effects in nonlinear models becomes tedious and computationally quite intensive (see Ai and Norton 2003). Therefore, and to ease comparability between estimations, we employ the linear probability model throughout the whole paper. The results from Table 2 are basically identical to those from probit estimations, which are available from the authors upon request.

19. As a robustness check, we included additional control variables (recipients' gender and several municipality characteristics) in the estimations. This exercise leaves the estimates basically unaffected, demonstrating that the controls are orthogonal to our treatments.

mailing by €2.50. Related to the basic impact of the mailings, this effect is modest in size. This might be explained by the fact that all mailings included information on possible legal and financial sanctions. The threat only increased the salience of these consequences. In addition, all mailings signal surveillance thus reducing the scope for the threat to further increase the perceived risk of detection. In Section 6.1, we provide additional evidence that supports this interpretation.

In contrast to the threat, the moral appeal and the social information do not increase compliance. Concerning the social information, this observation is not conclusive for a final evaluation of the treatment, as its effect is expected to depend on individuals' (heterogeneous) prior beliefs. This point will be closely analyzed in what follows (see Section 5.5). For the interpretation of the moral appeal's ineffectiveness, it is important to recall that our mailings targeted a selected sample of individuals who deviate from the law. It is unlikely that the mailing recipients share the moral values expressed in the treatment. Hence, the null-result does not imply that moral standards are irrelevant for the compliance of the overall population. In fact, there is some evidence that those who violate legal norms "develop subnorms that may be antithetical to those of the law-abiding world" (Meares, Katyal, and Kahan 2004, p.1184). Appealing to a conflicting norm might further reduce these people's willingness to register. This might explain why we find a negative effect of the appeal in several subsamples of our study (see, e.g., Section 5.5). In any case, our data clearly show that moral appeals are not an attractive policy to enforce compliance among cheaters.

5.4.2 Contract Updates. Next, we consider treatment effects on update responses. We regress the dummy variable P_i^{upd} , that indicates whether individual i updated her contract details, on our treatment variables. As we cannot measure contract updates among the no-mailing group, we focus on the mailing sample and exclude data on non-delivered mailings. Estimation results are shown in column (III) and (IV) of Table 2.

The estimates indicate a positive effect of the threat. However, once we account for the interactions between our treatments, the effect vanishes. While an F -test does not reject the joint hypothesis of both interaction terms being zero ($p = 0.121$), we can clearly reject the null of the *Threat* and *Threat* \times *Moral* being zero ($p = 0.002$). Hence, the threat per se has no robust effect on contract updates; it only works in the interaction with the moral appeal. Given that the threat also increases the law-abiding individuals' perceived inspection risk, they do not seem to be driven by the desire to avoid an interaction with a field inspector. Instead, the threats' ineffectiveness among the law-abiding part of the population is consistent with the significantly positive response of cheaters to the threat: evaders are aware of possible sanctions (see footnote 5) and are thus sensitive to threats. Complying individuals (rationally) expect that they face no real consequences and therefore do not respond to a higher inspection risk. Section 6.1 provides further evidence in support of this interpretation.

In line with the nonparametric analysis, the estimates from Table 2 also show that the moral appeal and the social information both have a significantly negative effect on contract updates. The observation that "social norms marketing" can backfire is not new in the literature (see, e.g., Beshears et al. 2010; Costa and Kahn 2010).

In our case, both treatments have in common that they point to non-compliance. In addition, the moral framing emphasizes that complying individuals are harmed by cheaters. Stressing the fact that some people cheat them might undermine the law-abiding individuals' willingness to respond to the mailing. The threat may offset this effect, as it documents the authority's efforts to impose sanctions on those who do not comply with their legal duties (resulting in the positive effect of the threat in the interaction with the moral appeal).

5.4.3 Overall Response. In columns (V) and (VI) of Table 2, we estimate the treatment effects on the overall response. The dependent variable is now an indicator that captures any reaction of individual i in response to a mailing. Hence, the estimates combine the treatment effects on registrations, contract updates with their impact on C- and D-responses. The outcome shows a significant effect of the threat, which increases the overall response rate by roughly two percentage points. The negative effect from the moral appeal and the social information found for update responses also prevail for the overall response rate.

5.5. Interaction Effects

As discussed in Section 4, we expect the social information to increase registrations only among those who initially expect compliance to be lower than communicated. As GIS permitted neither to vary the communicated compliance level experimentally (as, e.g., in Frey and Meier 2004) nor to elicit prior beliefs in our sample, we evaluate this prediction by exploiting a link between individual beliefs and the local evasion rate.

Our approach is motivated by a large, representative survey which asked households to state their belief about the frequency of license fee evasion in Austria. The survey documents a strong correlation of beliefs with the local evasion rate. In regions with widespread evasion, the overall evasion rate was expected to be high, too. Vice versa, in regions with high compliance rates, people expected evasion to be rare.²⁰ Given that beliefs and local evasion rates are similarly correlated for the individuals in our sample, we expect a positive effect of the social information in municipalities with a high evasion rate. To test this hypothesis, we estimate an equation that interacts our treatment variables with the local evasion level (as measured by GIS),

$$\begin{aligned}
 P_i^{reg} = & \alpha + \beta_1 Threat_i + \beta_2 Moral_i + \beta_3 Info_i + \gamma_0 Evasion_i^D \\
 & + \gamma_1 Threat_i \times Evasion_i^D + \gamma_2 Moral_i \times Evasion_i^D \\
 & + \gamma_3 Info_i \times Evasion_i^D + X_i \delta + \varepsilon_i,
 \end{aligned} \tag{2}$$

where X_i controls for a large set of municipality characteristics and $Evasion_i^D$ is a dummy indicating whether recipient i lives in a municipality with a high evasion rate. We consider municipalities in the top quartile and in the top tercile of the local evasion rate, respectively. The coefficients β_1 , β_2 , and β_3 measure the main treatment

20. Evidence on this correlation is discussed in the Online Appendix.

TABLE 3. Treatment effects on registrations: local evasion.

Dependent variable: Registrations (within 50 days)		
	(I)	(II)
Threat	0.014***	0.013***
(β_1)	(0.003)	(0.003)
Moral	-0.009**	-0.009**
(β_2)	(0.004)	(0.004)
Info	-0.007	-0.007
(β_3)	(0.004)	(0.004)
Threat \times Evasion ^D	-0.004	-0.004
(γ_1)	(0.006)	(0.006)
Moral \times Evasion ^D	0.012	0.011
(γ_2)	(0.007)	(0.007)
Info \times Evasion ^D	0.014*	0.013*
(γ_3)	(0.007)	(0.007)
Evasion ^D	0.011*	0.010
(γ_0)	(0.006)	(0.006)
Observations	41,007	41,007

Notes: All specifications are estimated with linear probability models and include a constant term and a vector of control variables. Evasion^D is a dummy indicating a municipality in the top quartile (I) or top tercile (II) of the (pre-experimental) evasion rate. Robust standard errors are in parentheses.

***Significant at the 1% level; **significant at the 5% level; *significant at the 10% level.

effects. Any additional effects in high-evasion municipalities are taken up by γ_1 , γ_2 , and γ_3 ; the non-interacted difference in registrations is captured by γ_0 . According to our hypothesis, γ_3 as well as $\beta_3 + \gamma_3$, the overall effect of the social information treatment in high evasion municipalities, should be positive.

Column (I) in Table 3 displays the results from estimating equation (2) with interactions for municipalities in the top quartile of the local evasion rate. The results show that γ_3 is positive and significant at a good 10%-level ($p = 0.053$). Specification (II), which considers interactions for municipalities in the top tercile, yields virtually identical results. The estimates demonstrate a significantly different effect of the social information treatment in municipalities with widespread non-compliance compared to municipalities with low evasion rates. In the former, providing information has a weak positive, in the latter a weak negative effect. However, the treatments' overall impact in high-evader municipalities is not significantly different from the baseline—an F -test does not reject the null hypothesis $\beta_3 + \gamma_3 = 0$ ($p = 0.226$ and $p = 0.246$ for (I) and (II), respectively). Moreover, we do not observe a significant interaction effect once we estimate a model with a continuous measure of the estimated evasion rate. Hence, the effect is not very robust. Finally, it is also worth noting that the coefficient from the moral appeal is significantly negative in both specifications. The moral appeal exerts a significantly negative effect on registrations in a sizable subsample of our study.

While the evidence on the heterogenous effect of the social information is not very strong (potentially because we have to rely on the local evasion rate as a proxy for individual beliefs), our data indicate that individuals condition their compliance

on the (perceived) compliance of others. This observation contributes to the literature on conditional cooperation (Gächter 2007) and closely relates to research on social influence (Cialdini 1998), in particular to Schultz et al. (2007) and Costa and Kahn (2010). The latter study documents that providing households with feedback on their energy usage has different effects for liberals and conservatives. This result provides a motivation to explore the role of political preferences (captured by vote shares) and other municipality characteristics in moderating the impact of our treatment effects.

We studied further interaction effects by running regressions for several subgroups that split our sample according to the median of different variables. The analysis reveals several heterogeneous treatment effects.²¹ The threat turns out to be more effective in municipalities with many center-left voters and has a considerably weaker effect in more right-leaning municipalities. Thus, leftist cheaters seem to be more sensitive to the threat than rightish evaders. While it is hard to identify a straightforward explanation for this observation, it is interesting to note that at the time of our study, the Austrian Public Broadcasting was influenced by the center-right government. In this context, it might be that some leftist voters evade unless they are confronted with a threat.

The threat is also more successful in larger, urban municipalities as well as in richer municipalities. We also see that the negative effect from the moral appeal becomes significant in smaller, less densely populated regions and in municipalities with a high share of right voters. Beyond these effects, we considered interactions with several additional variables. For instance, one would expect the population inflow into municipalities to be positively correlated with the frequency of evaders who recently moved, but did not yet register for license fees at the new place (in contrast to those who have already evaded for a longer time). The estimates did not indicate any heterogeneous treatment effects.

6. Discussion

6.1. Perception Survey

Our experiment establishes controlled field evidence on the causal impact of different interventions on behavior. To understand better the mechanisms behind these results, we conducted an anonymous online survey among more than 3,000 students from the University of Innsbruck that evaluates the treatments' impact on subjective perceptions.²² Survey participants were randomly confronted with hypothetical "vignette" persons who experienced one of the treatments from the field experiment. In the mailing conditions, for instance, participants were informed that the vignette person received a mailing from GIS and one of our six cover letters was shown. Survey

21. Estimation results are provided in the online appendix. See also Table 8 in Fellner, Sausgruber, and Traxler (2009).

22. Details on the survey design are provided in the Online Appendix. Note that cheating on license fees is quite common among university students. Hence, the subject pool should contain a significant fraction of evaders.

participants were then asked to evaluate, among others, the person's risk of a field inspection, possible sanctions, and the person's inclination to respond to the treatment.

The survey data complement the findings from the field experiment. First, we find a large impact of the mailings on risk perceptions. As compared to the no-mailing condition, a person's perceived inspection risk is significantly higher after being confronted with a mailing. The mailings seem to signal surveillance and thereby increase the risk perception. Second, among the different mailing treatments, only the threat shows a significant effect on the perceived detection risk. Quantitatively, however, the basic mailing effect on risk perceptions is substantially larger than the additional effect of a threat mailing. This is reassuring to see, as it mirrors the pronounced effect of sending mailings and the relatively modest additional increase in compliance from the threat observed in the experiment. Finally, survey participants' expectations regarding the inclination to respond to the treatments are consistent with the treatment effects on risk perceptions and again in line with the results from the field: the mailing is expected to have a major impact on responses and the threat is expected to have a small additional effect (see the Online Appendix for details).²³

6.2. Indirect Treatment Effects

A growing literature on randomized field experiments highlights the importance of indirect treatment effects (see Miguel and Kremer 2004). In our setup, one might argue that communication among recipients of different mailings could either mitigate or aggravate treatment effects.²⁴ To address this point, we studied whether our results are affected by a municipality's mailing coverage (i.e., the fraction of mailings relative to the population in the municipality). If we exclude all observations from municipalities in the top quartile regarding mailing coverage, our estimates indicate a slightly stronger effect of the threat. Moreover, running the regressions from Table 3 on the restricted sample, one also obtains a stronger effect of the social information treatment.

The stronger treatment effects, however, may be driven by changes in the sample's type composition rather than by indirect treatment effects. To see this point, note that a high mailing coverage indicates that GIS has little information from prior mailing campaigns in that area—in particular, little information on individuals who live in households where someone else already pays fees. As a consequence, the mailings are less precisely targeted to the non-compliant part of the population. A high mailing coverage is therefore strongly correlated with the fraction of already

23. The data further reveal that complying individuals are expected to face significantly lower fines than evaders. This is consistent with the fact that GIS does not impose fines on law-abiding individuals and supports our interpretation of the heterogenous impact of the threat on registrations and update responses observed in the field experiment (see Section 5.2): the threat is successful in enforcing registrations because it increases the perceived inspection risk *and* because cheaters expect significant sanctions. While complying individuals also perceive a higher chance of a field inspector, the inspection is rationally expected to have little consequences. Thus, the threat treatment does not increase update responses.

24. If we had increased the actual level of enforcement in the threat treatments, a different problem would have emerged: as shown by Rincke and Traxler (2011), the enforcement of the fees through field inspections causes systematic spillovers on undetected cheaters. These spillovers would have affected the measured treatment response.

complying individuals in the sample.²⁵ When we exclude municipalities with a high coverage, we focus on a sample with a higher frequency of evaders. Therefore, we mechanically estimate stronger treatment effects on registrations (see footnote 17).

7. Conclusions

This study tested different strategies to enforce compliance with license fees in a natural field experiment. The experiment manipulated the text of personalized mailings that were sent to potential evaders of the fee. We observe a strong effect of mailings. The fraction that started paying the fee in response to a mailing was significantly higher than in an untreated control group. Survey evidence indicates that the effect might be explained by the mailings signaling surveillance and thus increasing the perceived sanction risk. In addition, the mailings reduce the transaction costs of registering, may serve as a “nudge” and remind cheaters of their legal duty. Note, however, that evaders experience frequent reminders from daily spots in countrywide broadcasted channels. It is therefore unclear whether the mailings’ reminder function is quantitatively important for our result.

Among the different mailing treatments, a moral appeal and social information had no effect on aggregate, but the information treatment had a weak positive (negative) effect when evasion was initially believed to be common (rare). In contrast, the threat mailings produced an unambiguous increase in compliance. Quantitatively, this effect was modest compared to the basic effect of receiving a mailing. This might be due to the fact that all mailings included information on possible legal and financial sanctions. The threat only increased the salience of these consequences. Regarding the longevity of the effect, we found that some evaders who were initially deterred by the threat, started cheating again within six months. However, this had only a statistically but not an economically significant impact on the post-treatment compliance rate. After all, the threat was still the most successful mailing strategy.

In speculating how our results generalize beyond the specific setup of our study—a domain with a high level of compliance—it is important to recall that our mailings were sent to the noncompliant part of the population. Note that this is a common policy in mail audits of tax authorities, the deterrence of internet piracy, and other areas where mailings are targeted specifically at those who are suspected of law violations. It appears plausible that the basic mailing effect and the effect of the threat also prevail for the cheating population in these areas. By the same argument, our study suggests that moral appeals are not an attractive policy to enforce compliance among the least honest part of the population. The heterogenous treatment effect of social information is also consistent with evidence on social influence in other domains (e.g. Schultz et al. 2007; Beshears et al. 2010). Similar as in Costa and Kahn (2010), our data show that it is not optimal to communicate one piece of information to a diverse population. Belief

25. This is well reflected in our data: the rate of update responses (which, per definition, can only stem from law-abiding individuals) exceeds 44% in the top quartile regarding mailing coverage, as compared to 24% in the remaining sample.

management might only be an attractive policy if the information is accurately tailored to people's prior beliefs.

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Appendix A: Text of cover letters.

Appendix B: Evidences on the perceived level of non-compliance.

Appendix C: Heterogenous Treatment Effects.

Appendix D: Perception Survey.