

Fellner, Gerlinde; Sausgruber, Rupert; Traxler, Christian

**Working Paper**

## Testing enforcement strategies in the field : legal threat, moral appeal and social information

CESifo Working Paper, No. 2787

**Provided in Cooperation with:**

Ifo Institute – Leibniz Institute for Economic Research at the University of Munich

*Suggested Citation:* Fellner, Gerlinde; Sausgruber, Rupert; Traxler, Christian (2009) : Testing enforcement strategies in the field : legal threat, moral appeal and social information, CESifo Working Paper, No. 2787, Center for Economic Studies and ifo Institute (CESifo), Munich

This Version is available at:

<https://hdl.handle.net/10419/30572>

**Standard-Nutzungsbedingungen:**

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

**Terms of use:**

*Documents in EconStor may be saved and copied for your personal and scholarly purposes.*

*You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.*

*If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.*

# Testing Enforcement Strategies in the Field: Legal Threat, Moral Appeal and Social Information

Gerlinde Fellner  
Rupert Sausgruber  
Christian Traxler

CESIFO WORKING PAPER NO. 2787  
CATEGORY 1: PUBLIC FINANCE  
SEPTEMBER 2009

*An electronic version of the paper may be downloaded*

- *from the SSRN website:* [www.SSRN.com](http://www.SSRN.com)
- *from the RePEc website:* [www.RePEc.org](http://www.RePEc.org)
- *from the CESifo website:* [www.CESifo-group.org/wp](http://www.CESifo-group.org/wp)

# Testing Enforcement Strategies in the Field: Legal Threat, Moral Appeal and Social Information

## 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 alert effect of mailings, leading to a substantial increase in compliance. Among different mailing conditions a legal threat that stresses a high detection risk has a significant and highly robust deterrent effect. Neither appealing to morals nor imparting information about others' behavior enhances compliance. However, the information condition has a positive effect in municipalities where evasion is believed to be common. Overall, the economic model of crime performs remarkably well in explaining our data.

JEL Code: K42, C93.

Keywords: field experiments, law enforcement, compliance, deterrence.

*Gerlinde Fellner*  
*Department of Economics*  
*Vienna University of Economics and*  
*Business*  
*Austria - Vienna*  
*gfellner@wu.ac.at*

*Rupert Sausgruber*  
*Department of Public Economics*  
*University of Innsbruck*  
*Austria - Innsbruck*  
*rupert.sausgruber@uibk.ac.at*

*Christian Traxler*  
*Max Planck Institute for Research on Collective Goods*  
*Kurt-Schumacher-Strasse 10*  
*Germany – 53113 Bonn*  
*traxler@coll.mpg.de*

September 1, 2009

We thank Christoph Engel, Martin Hellwig, Peter Kooreman, Jo Rincke, Joel Slemrod, Adriaan Soetevent and participants in seminars at the Universities of Copenhagen, Göteborg, Michigan, Munich, Paris Nanterre, Tokyo (Waseda), Toulouse, Vienna, Zurich, as well as at the Enable Workshop (UvA, Amsterdam), the LEaF Conference (UCL, London) and the ESA Meetings in Rome, Tucson and Shanghai for helpful comments and suggestions. Carina Woodage provided excellent research assistance. The invaluable support of Annette Chemnitz, Herbert Denk and Gabriela Jerome at FIS and the financial support by the Austrian National Bank (*OeNB Jubiläumsfonds*, Grant No. 12301) and the Austrian Science Fund (FWF, Project No. P17029) is gratefully acknowledged.

# 1 Introduction

The economic analysis of crime gives clear advice on how to achieve compliance with the law: it requires the detection and punishment of law violations to deter crime (Becker, 1968). Ever since Becker's seminal work, the deterrence hypothesis has been subject to controversy. On the one hand, scepticism is fueled by the difficulties in identifying deterrence. Despite substantial advances of the empirical literature (for a survey, see Levitt and Miles, 2007), there are only a few studies that solve the identification problem (e.g., Corman and Mocan, 2000; Levitt, 2002; DiTella and Schargrotsky, 2004; Drago *et al.*, 2009). On the other hand, it has been argued that formal law enforcement explains only little of the variation in compliance (e.g., Dills *et al.*, 2009) and that informal institutions, like social norms, might play a more important role in shaping adherence to the law.<sup>1</sup> The question then arises whether law enforcement should be based on the classical economic approach to deterrence or whether one can draw upon different behavioral motives.

The present paper offers novel insights to this question by testing alternative enforcement strategies in a large-scale natural field experiment. In our setup, people are required by law to pay an annual fee for receiving public broadcasting. An enforcement problem exists because public broadcasting channels can be received without paying the fee. Those who deviate from the law, however, face a non-negligible detection risk and the threat of sizeable fines. The enforcement authority granted us access to a unique sample of more than 50,000 individuals who were identified as potential evaders. In cooperation with the authority, we send mailings to 95% of this sample. In addition, a 5% randomly selected subsample did not receive mailings. Mailing recipients were randomly assigned to treatment conditions that differed in the wording of the cover letter. Using the authority's standard letter as baseline, we varied the text along three dimensions: the letter may contain a *legal threat*, stressing a high detection risk and possible legal and financial sanctions, a *moral appeal*, emphasizing that compliance is a matter of fairness, and *social information*, highlighting the overall level of compliance. The different treatments were evaluated by comparing the mailing response to that in the baseline treatment and the

---

<sup>1</sup>See, among many others, Kahan (1997); Posner (2000); McAdams and Rasmusen (2007) and the conference volumes on *Social Norms, Social Meaning and the Economic Analysis of Law* (Journal of Legal Studies 27(3), 1998) as well as on *The Legal Construction of Norms* (Virginia Law Review 86(8), 2000).

behavior in the no-mailing group. Our main focus is thereby on the number of evaders who quit deviating from the law and start to pay the fee.

The setup of the experiment allows us to overcome the identification problem faced by the law enforcement literature. First, and most importantly, the random treatment assignment assures that enforcement strategies are exogenous. Second, our key dependent variable – whether an evader starts to pay the fee – can neither be affected by incapacitation (Levitt, 1998; Kessler and Levitt, 1999) nor by spatial displacement effects (i.e., the displacement of criminals in response to a local increase in law enforcement; see, e.g., Jacob *et al.*, 2007). Finally, the measurement of our dependent variable is independent of the experimental conditions. To ensure this property, the treatments did not intervene with the effectively implemented enforcement level: the legal threat just stresses elements of the current enforcement practice, but the objective sanction risk remained constant over all treatments.<sup>2</sup> Any observed response to the threat (as well as to the other mailing treatments) must therefore be driven by changes in the mailing recipients' subjective perceptions regarding, e.g., the detection risk and the magnitude of fines. To explore this point further, we conducted a follow-up survey to the experiment which exposed participants from a different sample to the letters used in the field experiment. In this vein, we elicited the treatments' impact on different perception domains.

The paper provides two sets of results. The first concerns the effectiveness of deterrence. First of all, we find a striking impact of the mailings. In the no-mailing condition, the fraction of individuals who start to comply with the law within 50 days of the experiment is very low (0.8%). Sending mailings increases this fraction to 7.7%. Hence, there are almost ten times more cheaters who start to pay license fees in the mailing conditions. This corresponds to a net-revenue per mailing of €15. While the effect could also be attributed to a decline in transaction costs, our survey indicates that the mailings trigger a strong alert effect: receiving a mailing causes a substantial increase in the perceived detection risk. The mailings signal surveillance and thus – jointly with a transaction cost effect – create a pronounced effect on compliance behavior.

---

<sup>2</sup>There are several reasons why we decided not to change actual enforcement. First, the number of individuals who start paying the fee is mechanically linked to the number of detections from field enforcement. Similar to an increase in the level of police, one would have to disentangle a deterrence effect from the mere impact on the measured crime rate (Levitt, 1998). Second, as shown in Rincke and Traxler (2009), the enforcement of license fees through field inspections causes systematic spillovers on undetected cheaters. These spillovers would blur the measured treatment response to the legal threat.

The comparison across the different mailing treatments identifies a strong deterrent effect of the legal threat. Compared to the baseline mailing, the threat increases the share of individuals who start complying by an additional percentage point. This effect corresponds to 15% increase in compliance. The survey reveals that the treatments' impact on behavior can be traced back to an increase in the perceived costs of non-compliance. Our evidence therefore documents that the legal threat shapes perceptions and that individuals rationally adjust their compliance behavior to these perceptions. By exploiting information on different types of mailing respondents we find further support for this conclusion. While the legal threat has a strong effect on evaders who face potentially severe sanctions in case of detection, it has no effect on types who are not exposed to an actual sanction risk. In summary, this first set of results lends strong support to the deterrence hypothesis. In our experiment, deterrent threats unambiguously work as enforcement strategies.

Our second set of results clarifies whether compliance can be enforced by moral suasion or by providing social information – i.e., without resorting to legal threats. The moral framing, which appeals to the fairness aspect of compliance, calls upon people's conscience and aims at making a specific moral concept more salient. Social psychology research suggests that the framing should increase compliance if the value judgment is shared by the recipients (Cialdini, 1998). In contrast to this conjecture, but in line with other studies in economics (Blumenthal *et al.*, 2001; Wenzel and Taylor, 2004), we do not find an effect of the appeal. It is important to note, however, that our mailings are targeted to a specific sample of those who deviate from the law. Hence, the null-result does not imply that moral standards are irrelevant for the compliance of the overall population. As those who violate the law often develop anti-social norms (Meares *et al.*, 2004), individuals in our sample might be more likely to disagree with the moral statement in the letter. In line with this interpretation, we observe that the appeal has a negative effect on compliance in several subsamples of the study. Our experiment therefore suggests that moral appeals are no attractive policy to enforce compliance among those who purposely deviate from the law.

Legal scholars emphasize that compliance is also driven by “*the informal enforcement of social mores by acquaintances, bystanders, trading partners, and others*” (Ellickson,

1998, p. 540) – a view that is also reflected in economic research documenting the importance of informal sanctions (see, e.g., Falk *et al.*, 2005). Norm-enforcing sanctions are thereby considered to be stronger the more people follow a norm (Elster, 1989).<sup>3</sup> The social information treatment, which communicates the actual compliance rate of 94%, may thus shape perceptions regarding the strength of informal law enforcement. Equivalently, the ‘broken windows theory’ (Wilson and Kelling, 1982) suggests that the provided information may change perceptions about formal law enforcement.<sup>4</sup> By altering perceptions about both formal and informal sanctions, the social information can therefore affect compliance behavior.

In our experiment, we do not observe a significant effect of the information treatment. Consistent with the theoretical considerations, however, we find that imparting social information has significantly different effects in municipalities where evasion is believed to be common compared to municipalities where evasion is believed to be rare. In the former, the treatment has a positive, in the latter a weakly negative effect on compliance. This outcome complements existing evidence on social interaction effects in crime (e.g., Glaeser *et al.*, 1996). Moreover, it relates to the literature on conditional cooperation in the private provision of public goods (Frey and Meier, 2004; Shang and Croson, 2007; Fischbacher and Gächter, 2009). Our study delivers a first piece of evidence on policies that build upon ‘conditional compliance’ with the law – i.e., individuals conditioning their compliance on the (perceived) compliance of others. At the same time, our results demonstrate the limits of applying ‘belief management’ as an enforcement tool (Fehr and Falk, 2002). With non-common priors, one and the same information can trigger opposing behavioral responses. Hence, even if individuals are conditionally compliant, belief management might fail to increase aggregate compliance.

Our results contribute to several strands of the literature. First and most importantly, we provide experimental field evidence for the deterrence hypothesis in a setup that avoids displacement and incapacitation effects. Second, we offer novel results on the effectiveness of alternative enforcement strategies within a given institutional framework. Our data thus allow for a direct comparison of classical and ‘behavioral’ approaches to law

---

<sup>3</sup>Supporting evidence for this relationship for the specific domain of TV license fee evasion is provided by survey evidence discussed in Traxler and Winter (2009).

<sup>4</sup>For recent contributions on the broken windows effect, see Corman and Mocan (2005); Harcourt and Ludwig (2006); Keizer *et al.* (2008).

enforcement and identify several impediments for strategies that go ‘beyond punishment’ (Frey, 2009). Finally, linking the results from the experiment to our perception survey, we not only evaluate the effectiveness of different enforcement strategies but also contribute to an understanding of the causal links between policies, subjective perceptions, and behavior (Nagin, 1998; Lochner, 2007; Hjalmarsson, 2009).

In terms of methodology, the paper is closely related to field experiments in the domain of tax enforcement.<sup>5</sup> The most prominent is the Minnesota Income Tax Compliance Experiment, where the Internal Revenue Service approached taxpayers with different mailing types (Blumenthal *et al.*, 2001; Slemrod *et al.*, 2001). A treatment similar to our legal threat studied the effects of communicating a high audit probability. The results were mixed. While low and middle-income taxpayers responded with a modest increase in reported income, the audit threat led to lower reported tax liabilities from high income taxpayers (Slemrod *et al.*, 2001). Appealing to taxpayers’ conscience did not show any significant effects (Blumenthal *et al.*, 2001).<sup>6</sup> In an experiment with the Australian tax authorities, Wenzel and Taylor (2004) studied a number of interventions on taxpayers’ property tax deductions. A deterrent letter did not have a significant effect in their study. More recently, Kleven *et al.* (2009) ran a large-scale experiment in collaboration with the Danish Inland Revenue. While they find that individuals update their declared income in response to audit threats, they also observe that many taxpayers reported lower taxable incomes (similar to Slemrod *et al.*).

One crucial aspect that distinguishes our study from all these important contributions concerns the measurement of the treatment response. Tax enforcement experiments typically rely on the evaluation of differences in reported income before and after a specific manipulation. The immense heterogeneity among taxpayers (in terms of their characteristics and the availability of different tax-minimizing strategies) renders such a comparison troublesome. In contrast, we evaluate our treatments based on a simple indicator variable – whether or not an evader starts to comply with the law. In addition, the set of possible responses to our treatments is well-defined and leaves less scope for the interpretation of

---

<sup>5</sup>For an overview of randomized experiments in the criminology literature, see the meta-study by Farrington and Welsh (2006).

<sup>6</sup>In a similar setup, Schwartz and Orleans (1967) found that normative appeals were slightly more effective than sanction threats. In a replication of the study, McGraw and Scholz (1991) neither found an effect of threats nor of appeals. The power of both contributions is, however, limited due to small sample sizes.



our results. Finally, none of the mentioned studies addresses the role of perceptions for the link between policy manipulations and behavioral responses.

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 studies complementary evidence on perceptions from a survey. The paper concludes with a discussion of our findings.

## 2 Institutional Background

A significant share of radio and television broadcasting around the world is provided by public broadcasters (the US being a notable exemption) that are mainly financed through TV and radio *license fees*: two thirds of all European, one half of African and Asian, and about 10% of countries in the Americas employ license fees. In Europe, the total amount of fees collected added up to roughly €20 billion in 2005.<sup>7</sup> A typical license fee system is in place in Austria. According to the Austrian *Broadcasting License Fee Act*, households must file a registration and pay license fees if they own a TV or a radio. The size of the annual fee is substantial. In 2005, it ranged from €206 to €263 (the fee varies between federal states). The amount is due per household, regardless of the number of household members, TVs and radios.<sup>8</sup> An enforcement problem exists since public broadcasting programs can be received without paying the annual fees.<sup>9</sup>

The license fee system is managed by ‘Fee Information Service’ (henceforth FIS), a subsidiary of the Austrian Public Broadcasting Company. FIS is responsible for collecting and enforcing the license fee and takes actions to sustain compliance. To identify potential evaders, FIS compares residence data (i.e., a federal database in which everyone must enroll with her place of residence) and data from direct mailing companies with their own data on license fee registrations. If an individual lives at a certain address without having

---

<sup>7</sup>Own computation based on Open Society Institute (2005) and information provided by *BFA – Broadcasting Fee Association* (see [www.broadcastingfee.com](http://www.broadcastingfee.com)). Further information on license fees is provided by Head (1985) and Newcomb (2004). For a welfare analysis of private versus public broadcasting, see Anderson and Coate (2005).

<sup>8</sup>We use the term household in a broad sense, including apartment-sharing communities, etc. In addition to the fee that covers TV and radio, there exists a reduced fee (between €60 to €76) for radios only.

<sup>9</sup>As typical for Europe, most households receive TV via a simple rooftop antenna or a satellite dish. Cable companies have no information about whether a household pays the license fee for public broadcasting. These companies mind their own fees and do not exclude customers from viewing the public broadcasting channels.

registered any TV or radio – and if no one else in the household pays fees – FIS sends a mailing to this person. The license Fee Act requires mailing recipients to respond and to provide correct information. Data on those who do not respond are handed over to FIS’ enforcement division. This division employs field inspectors who check potential evaders at their homes (see Rincke and Traxler, 2009). In fact, detections by field inspectors are quite frequent. In 2004, door-to-door inspections resulted in a clearance rate of one third. A person who is detected evading the fee must register and licensing inspectors typically date back the registration for several months, i.e., a detected evader has to make supplementary payments.<sup>10</sup> In addition, field inspectors may report a case to the authorities who can then impose a fine of up to €2,180. If someone does not comply with the payment duty after an official report, criminal proceedings will be initiated. Austrians seem to be aware of the possible sanctions. A national survey conducted in 2000 finds that 55% of respondents expect ‘severe’ or ‘very severe’ sanctions if they are detected evading TV license fees.<sup>11</sup>

FIS’ enforcement activities are reflected in a high compliance rate: in July 2005, 94% of all Austrian households were registered for license fees and payed a total of €644 million (0.3% of GDP). The 94% give a reasonable proxy for the overall compliance level, as only one percent of households own neither a TV nor a radio (ORF Medienforschung, 2006). Note, however, that compliance is in constant flux due to, e.g., changes in the household composition or de-registrations of households that move or simply state that they no longer operate any TV or radio. Thus, FIS constantly seeks to improve its enforcement practice.

### 3 Experiment Design and Data

In collaboration with FIS, we experimentally manipulated mailings that were sent to potential evaders of license fees. Following the standard procedures, a mailing contained a cover letter, an information sheet and a response form with a postpaid envelope. In

---

<sup>10</sup>At the time of detection past savings from evasion are sunk. The retrospective payments thus create similar incentives as a fine.

<sup>11</sup>For tax evasion via black labor market participation [absenteeism from work], the corresponding figure is 60% [38%]. Details on this survey ( $N > 1000$ ), which is further discussed in Traxler and Winter (2009), are available from the authors.

the letter, FIS explains that – according to their data – the mailing recipient has not registered any TV/radio and that the recipient is required by law to clarify the facts by returning the response form within 14 days (see Appendix I for the full text of the cover letter). 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 as well as the maximum fine that can be imposed in case one is detected evading the fee (see section 2).

### 3.1 Treatments

The experiment varied the text of the cover letter. Everything else, the response form and the information sheet, and thus the information on the possible fine, was kept the same across all treatments. We used the standard FIS letter as control and varied the text along three dimensions: we introduced a *legal threat*, *social information*, and a *moral appeal*. In addition, we interacted the threat with the two other dimensions, resulting in six different mailing treatments in total. Table 1 illustrates these treatments.<sup>12</sup> Finally, we also implemented a *no-mailing* condition (T0). Individuals in this untreated control group did not receive a mailing.

Table 1: Mailing Treatments

	Standard Letter	Social Information	Moral Appeal
without Legal Threat	T1	T3	T5
	Control	Info	Moral
with Legal Threat	T2	T4	T6
	Threat	Threat×Info	Threat×Moral

In the legal-threat treatments (T2, T4, and T6), the cover letter contained a paragraph that communicates a significant detection probability and emphasizes potential consequences of non-compliance (see Appendix I for details on all letters). We used the following wording:

<sup>12</sup>Our approach can be interpreted as an incomplete  $2 \times 2 \times 2$ -design. At the benefit of a larger number of observations in each treatment cell, we decided not to implement the interactions between the social information and the moral appeal.

*“If you do not respond to this letter, a staff member of FIS 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, FIS 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.”*

In treatments T3 and T4, the letter imparted social information, i.e., information about the actual level of compliance with the law (as estimated by FIS). The relevant paragraph was formulated as follows:

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

Treatments T5 and T6 extended the standard cover letter by the following moral appeal:

*“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**

FIS provided us with 50,498 addresses of potential evaders. From these data we first took a 5% random subsample and assigned it to the no-mailing group (T0). We then randomly allocated the remaining data to the six mailing treatments T1 – T6. Table 2 provides summary statistics and demonstrates that the treatment assignment was orthogonal to observable individual and municipality characteristics.<sup>13</sup> Further descriptive statistics on the 1120 (out of 2300) Austrian municipalities covered by our sample are provided in Table A.1 in the Appendix.

There is no deliberate selection of communities into the experiment. Following standard procedures, FIS collects updated residence data alternately from different municipalities as well as new data from direct mailing companies. Our sample is based on the most recent data that were available to FIS in August 2005 and that were not used in prior mailing campaigns. Hence, there is no ‘randomization bias’ (Levitt and List, 2009). From these data, potential evaders were identified by FIS according to the procedures

---

<sup>13</sup>Kruskal Wallis tests, as well as variance analyses, confirm the null hypothesis of equality of means across treatments of all variables in table 2.

Table 2: Individual and municipality characteristics per treatment

		T0	T1	T2	T3	T4	T5	T6	Total
		No-Mailing	Control	Threat	Info	Info&Threat	Moral	Moral&Threat	
Number of Observations		2,586	7,984	7,821	7,998	8,101	8,084	7,924	50,498
<i>Individual Characteristics: Sample Mean</i>									
Gender (% of males)		64.6 (47.8)	63.4 (48.2)	63.7 (48.1)	62.6 (48.4)	63.3 (48.2)	62.5 (48.4)	64.3 (47.9)	63.3 (48.2)
Age (years)		36.9 (12.1)	36.5 (11.8)	36.3 (11.9)	36.5 (11.8)	36.1 (11.8)	36.4 (12.1)	36.6 (11.8)	36.4 (11.8)
<i>Municipality Characteristics</i>									
Population size	Median	3,954	3,954	3,917	3,883	3,917	3,917	3,917	3,917
	Mean	45,815 (77,148)	43,377 (75,306)	44,543 (76,469)	43,903 (76,001)	43,319 (75,326)	44,301 (75,938)	43,610 (75,289)	43,941 (75,792)
Population density (inh./km <sup>2</sup> )	Median	134	126	128	126	126	131	126	126
	Mean	817 (2,140)	856 (2,417)	811 (2,188)	796 (2,175)	835 (2,307)	848 (2,368)	805 (2,243)	825 (2,278)
Compliance rate (in %)	Median	94.1	94.1	94.1	94.3	94.3	94.0	94.1	94.2
	Mean	93.5 (5.8)	93.5 (5.7)	93.5 (5.8)	93.5 (5.8)	93.5 (5.7)	93.4 (5.7)	93.4 (5.8)	93.5 (5.8)

*Notes:* Age is only available for a subsample of 16,281 recipients. Population density is measured as the number of inhabitants per square kilometer. Similar to the overall rate, FIS approximates the local compliance rate by the share of households who are registered for license fees relative to the total number of households living in a municipality. Standard deviation is in parentheses.

described in the previous section. Thus, the final sample is not representative of the overall population. Moreover, it covers individuals beyond the subpopulation of license fee evaders.

To illustrate this point and to facilitate the following analysis, we classify the different types of individuals in the sample. First, there are evaders (*A-types*), individuals from households that have not registered their TVs/radios.<sup>14</sup> Second, the sample contains individuals who comply with their payment duty or live in a household that does so (*B-types*). Nevertheless, these types are in the sample, either because of spelling errors in the residence data or because they changed their address or name (e.g., after marriage) without reporting these changes to FIS. Finally, the sample includes individuals who neither own a TV nor a radio (*C-types*). As we will discuss below, these different types face different response options and different incentives to respond.

The mailings were sent by FIS in two waves between September 19 and October 17, 2005. In order to avoid confounding time effects, each wave contained all treatments in equal proportions. Between September and December 2005, FIS did not change the in-

<sup>14</sup>It typically takes several months until data on newly formed households or households that recently moved appear in the residence data available to FIS. Those who have not yet registered their TV/radio by the time they receive a mailing can therefore be called evaders rather than procrastinators.

tensity of field inspections, nor did they inform anybody about our study. The mailing recipients were not aware of participating in an experiment. Following the taxonomy by Harrison and List (2004), this feature distinguishes our study as a natural field experiment.<sup>15</sup>

Mailing responses and unsolicited registrations in the no-mailing group were measured by FIS' computer system.<sup>16</sup> This system allowed a highly accurate measurement of behavior: it tracked responses via the reply forms that were sent along with the mailings, it recorded registrations that were made online or by phone, and it detected responses from another individual living in the same household as the recipient of the mailing (e.g., the spouse). Note further that the measurement of the experimental outcome was the same for all mailing treatments. This is an important detail as it assures that the measurement is orthogonal to the experimental conditions.

The mailing responses were classified into four categories:

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

These response categories can be directly linked to the type classification introduced above. A-types, who are liable to pay fees, can either respond to the mailing by registering (A), by asserting that they have no TV or radio in the household (C), or they can ignore the mailing and not respond. B-types, who are already law-abiding, can either clarify their status by a B-response or they can ignore the mailing. B-responses require a valid registration number (e.g., from the registration of another household member). If FIS

---

<sup>15</sup>Harrison and List emphasize that the knowledge about being observed can induce a Hawthorne effect on its own. Such an effect would be highly undesirable in empirical research that studies compliance with the law. See Levitt and List (2007) for a general discussion of this issue.

<sup>16</sup>Individuals may register online, by calling FIS' service hotline, or by sending a filled hard-copy registration form that is available at banks, postal offices, etc.

cannot match the stated number with their data, the response is classified as category D. Finally, C-types may respond in category C or not at all.<sup>17</sup>

Note that one can unambiguously link A- and B-responses to particular types. Only those who do not comply with the payment duty (A-types) can register for license fees (A-response) and only law-abiding individuals (B-types) can send an update of contract details (B-response). The primary focus of FIS is to maximize A-responses, i.e., revenues. B-responses are their secondary target. Updates about existing registrations allow FIS to improve the targeting of their enforcement measures (mailings and field inspections) on the non-compliant population. In the following, we will analyze both A- and B-responses. This allows us to study whether strategies that are successful in enforcing compliance (a high rate of A-responses) cause any undesirable spillovers on law-abiding individuals (on B-responses). A type-specific response analysis is also interesting from a theoretical point of view. As A- and B-types face different incentives to respond, the comparison of A- and B-responses enables us to infer the causal effects driving the response to our interventions.

## 4 Discussion of Treatments

### 4.1 A-Responses

Consider an A-type who chooses between registering – i.e., paying license fees – 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, supplemental payments and maybe a fine. Rational decision makers trade off these potential costs with the benefits from (not) registering (Becker, 1968). The decision depends on their risk preferences and – with imperfect information on enforcement (Sah, 1991) – the perceived sanction risk.

*No-mailing Treatment.* Comparing the no-mailing group, T0, with the mailing treatments, T1–T6, the mere fact of receiving a mailing signals that FIS suspects the recipient

---

<sup>17</sup>One could think of several other possibilities: an A-type could assert incorrectly that someone else in the household pays fees. However, based on the registration number, this would be discovered and the response would be coded as unclassified feedback. B-types might make a mistake and register a second time. Such errors would be discovered by FIS, which is obliged to cancel the double registration. C-types could start to pay license fees even if they do not have any broadcasting receivers – a scenario that is highly unlikely. All three types can produce a response of class D.

of violating the law. The A-types in the no-mailing group do not receive this surveillance signal. Hence, we conjecture that mailing recipients perceive a higher sanction risk and are therefore more likely to register than individuals in the no-mailing condition. This effect should be intensified by the mailings, lowering the transaction cost of a registration.<sup>18</sup>

Turning to the mailing treatments, it is important to recall that our six treatments neither changed the ‘true’ economic incentives for compliance – the actual detection risk and the magnitude of the sanctions were constant across all treatments – nor did they alter the transaction costs for registering. It follows that any behavioral change in response to our mailing treatments must be brought about by changes in the recipients’ perceptions.

*Legal Threat.* The legal threat aims at enforcing compliance through intervening directly with the perceived sanction risk. For the treatment to work, two conditions must hold: first, the threat has to affect risk perceptions and, second, individuals who perceive a higher sanction risk must be more likely to stop breaching the law. If these conditions are met, the legal threat will increase the number of registrations. Regarding the first condition, there should be room for manipulation if individuals have subjective expectations.<sup>19</sup> While this finds ample empirical support (e.g., Lochner, 2007), there is still no clear-cut evidence to which extent policy changes – e.g., an increase in enforcement efforts – translate into changes of risk perceptions (Nagin, 1998). For instance, mailing recipients might not take the threat to be credible (Graetz *et al.*, 1986). Even if they do, it is an open question whether the treatment shocks perceptions beyond the level that is induced by the mere fact of receiving any mailing.

Given that our manipulation successfully increases risk perceptions, it is still not certain that the threat will trigger more registrations. This point is made by the motivation crowding literature that considers the possibility of increased external incentives – in the form of stricter enforcement – crowding out the intrinsic motives for compliance with the law (Frey, 1997; Benabou and Tirole, 2006). Evidence in support of this case is provided by Gneezy and Rustichini (2000), who showed that introducing a fine reinforced

---

<sup>18</sup>Recall that individuals in the no-mailing group can make an unsolicited registration by picking up, filling and sending in a hard-copy registration form, or by registering online or by phone. In the context of charitable giving, Huck and Rasul (2009) find strong transaction cost effects from mailing letters. One might further argue that mailings simply remind ‘unintentional evaders’ of their legal duty. This effects is very unlikely to happen, as FIS runs intensive media campaigns that clarify the obligations emerging from the License Fee Act. During the time of the experiment, for instance, FIS placed on average three spots per day in countrywide broadcasted channels.

<sup>19</sup>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).



the sanctioned behavior. Slemrod *et al.* (2001) observed an equally counterintuitive effect. Threatening taxpayers with an increased audit probability resulted in a decline of reported income among high-income groups.<sup>20</sup>

*Social Information.* There are two alternative channels over which the social information treatment can affect the decision to register. The first builds upon standard deterrence incentives and derives from the ‘broken windows theory’ (Wilson and Kelling, 1982). According to this approach, signs of non-compliance (‘broken windows’) may signal lax enforcement and thereby trigger further deviations from the law. The information about a 94% compliance rate should thus have a positive effect on registrations from individuals who initially expect lower levels of compliance. The opposite holds for those who initially consider compliance to be higher than communicated.

Social norms and conformity motives are the second channel for the information treatment to influence behavior (Elster, 1989; Bernheim, 1994). This is pointed out by legal scholars, who argue that compliance is enforced not only by formal, but also by informal social sanctions (see the references in footnote 1). As norm enforcement is considered to be stronger the more people adhere to a social norm, individuals might condition their compliance on others’ norm-adherence (similar to conditional cooperation; see, e.g., Fischbacher and Gächter, 2009). The case for conditional compliance with the law is made by Traxler and Winter (2009), who find that half of the participants in a national survey in Austria are willing to impose sanctions on license fee evaders. They further show that the inclination to sanction declines with the belief about the pervasiveness of non-compliance. If this pattern is correctly anticipated, we arrive at the same prediction as above: among those who initially expect low compliance rates, the social information will signal that social (rather than legal) sanctions are stronger than expected. In turn, this should increase registrations.<sup>21</sup>

*Moral Appeal.* The moral appeal treatment stresses that evading license fees harms honest households, which violates not only the legal, but also a fairness norm. Psychology

---

<sup>20</sup>These findings relate to a growing number of studies that demonstrate detrimental effects of economic incentives (Fehr and List, 2004; Falk and Kosfeld, 2006; Ariely *et al.*, 2009).

<sup>21</sup>Similar caveats apply to this hypothesis as for the legal threat. In particular, it is not clear whether the nationwide compliance rate (rather than a local rate) shapes perceptions about legal or social sanctions. Moreover, the importance of social interaction in evasion behavior is still elusive, as shown by Fortin *et al.* (2007), who do not find any social interaction in a lab experiment, and by Galbiati and Zanella (2008), who find a large social multiplier in evasion behavior.

research has illustrated the importance of personal norms and moral concepts – as well as their salience – as driving forces of behavior (Cialdini, 1998). Individuals’ judgements seem to be guided by moral shortcuts that are sensitive to framing effects (Sunstein, 2004). Provided that our framing makes a relevant moral concept more salient, one might expect the moral appeal to have a positive impact on registrations.

This prediction is at odds with several studies in the domain of tax enforcement that hardly find any evidence on the effectiveness of moral suasion (Blumenthal *et al.*, 2001; Wenzel and Taylor, 2004). From an economic perspective this is not surprising: the appeal has no informational content and preferences, even when they reflect social motives, are considered to be stable. Quite on the contrary, the treatment might 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).

## 4.2 B-Responses

Remember that B-types are equally obliged to respond to the mailings as A-types. However, FIS never imposes sanctions on complying individuals when they do not respond. Hence, there are zero costs for not responding. At the same time, responding entails only minor transaction costs since the return mailing is postpaid. From an economic perspective, it is therefore difficult to assess the treatments’ impact – in particular, the effect of the moral appeal and the social information – on B-types’ response behavior. Of course, B-types might not be aware that they face no sanction risk or they 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. Moreover, signalling models in the fashion of Benabou and Tirole (2006) suggest that B-types have a desire to signal that they are ‘good guys’ who comply with their legal duty. All these motives might interact with the legal threat.

According to signaling models, the legal threat could have a detrimental effect on B-responses, if the treatment reduces the value of the ‘good type’-signal for B-types. If the expected embarrassment from a field inspection drives the response, or if B-types expect economic sanctions, the threat should increase the frequency of B-responses, as long as the treatment increases the perceived chance of an inspection. The impact of the threat

should be smaller, however, the lower the expected costs from an inspection are. This has a straightforward implication for the comparison of response between A- and B-types. Given that the threat does increase the perceived inspection risk of both types, B-types should be less sensitive to the threat than A-types as long as the former 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 complements the analysis by estimation results. Section 5.4 offers evidence on interaction effects of the information as well as the threat treatment with municipality characteristics. A detailed discussion of our findings is deferred to section 7.

### 5.1 Overall effect of sending mailings

Table 3 compares the frequency of registrations (A-responses) in treatments T1–T6 to the unsolicited registrations in the control group (T0) in intervals of 25 days.<sup>22</sup> Within 25 days after sending the first mailings, only 8 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 group 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 10 times higher. Assuming that newly registered households pay license fees for at least one year, the expected revenue per mailing (net of the marginal cost for printing, postage, etc.)

---

<sup>22</sup>A similar picture emerges if biweekly or weekly intervals are considered.

Table 3: Registrations per 25-days intervals

Registrations	T0		T1–T6	
days 1–25*	8	0.31%	2,794	5.83%
days 26–50*	13	0.50%	877	1.83%
days 51–75	9	0.35%	166	0.35%
days 76–100	5	0.19%	95	0.20%
Sample	2,586		47,912	

*Notes:* Percentages are registration rates relative to the *total* number of observations in T0 and T1–T6, respectively. For intervals marked with \*, the difference in registration rates is significant at a 1%<sub>o</sub>-level.

amounts to €15.<sup>23</sup> We conclude that the mailing campaign is highly effective in raising revenues. As discussed above, the huge effect of the mailings can be due to reduced transaction costs, the mailings’ impact on the perceived sanction risk, or to a combination of both effects. In section 6, we will present further evidence that allows to assess the relevance of these possible causes.

## 5.2 Effects of the mailing treatments

Table 4 summarizes the results for the mailing treatments T1–T6. On average, 14.41% of the mailings could not be delivered because of erroneous addresses resulting from typos in individual and street names in the FIS data base. Due to the random assignment, however, the share of non-delivered mailings does not statistically differ between treatments ( $\chi^2$ -test:  $p = .793$ ). In the following, we report responses relative to mailings delivered rather than mailings sent. Motivated by the results from above, we focus on the response within 50 days. All our results are robust to extending this observation period.

Consider first A-responses. Recall that only evaders (A-types) can register. The share of registrations thus measures the treatments’ success in enforcing compliance. Table 4 shows that, on average, 8.95% of recipients responded with a registration of their TV or radio.<sup>24</sup> A comparison of the registration rates reveals a significant positive effect of the

<sup>23</sup>According to FIS, one year is a very conservative assumption on the average registration spell. The calculation further accounts for the fact that, in our sample, 95% registered TV plus radio and 5% registered only a radio, resulting in a reduced fee (see footnote 8).

<sup>24</sup>As the precise distribution of A-, B- and C-types in our sample is not identified, one cannot judge whether the 8.95% corresponds to a particularly low or high *type-specific* response rate. For a closer discussion, see Appendix IV.

Table 4: Mailing Response within 50 Days

	T1 Control		T2 Threat		T3 Info		T4 Info&Threat		T5 Moral		T6 Moral&Threat		Total	
Mailings sent	7,984		7,821		7,998		8,101		8,084		7,924		47,912	
Non-Deliverable	1,126	14.10%	1,127	14.41%	1,173	14.67%	1,141	14.08%	1,164	14.40%	1,174	14.82%	6,905	14.41%
Mailings delivered	6,858		6,694		6,825		6,960		6,920		6,750		41,007	
A. Registrations	591	8.62%	647	9.67%	562	8.23%	675	9.70%	567	8.19%	629	9.32%	3,671	8.95%
B. Updates	1,998	29.13%	1,991	29.74%	1,875	27.47%	1,938	27.84%	1,790	25.87%	1,908	28.27%	11,500	28.04%
C. No Equipment	172	2.51%	203	3.03%	162	2.37%	179	2.57%	163	2.36%	179	2.65%	1,058	2.58%
D. Unclassified	194	2.83%	172	2.57%	179	2.62%	185	2.66%	166	2.40%	174	2.58%	1,070	2.61%
Overall Response	2,955	43.09%	3,013	45.01%	2,778	40.70%	2,977	42.77%	2,686	38.82%	2,890	42.81%	17,299	42.19%

*Notes:* Percentage of non-delivered mailings relative to the *total* number of mailings. Percentages for (A–D and overall) response rates are relative to the number of *delivered* mailings.

legal threat between T1 and T2 ( $p = 0.034$ ), T3 and T4 ( $p = 0.003$ ), and between T5 and T6 ( $p = 0.020$ ).<sup>25</sup> In contrast, neither the social information nor the moral appeal have any significant effects (T1 vs. T3:  $p = 0.320$ , T1 vs. T5:  $p = 0.369$ ). This finding does not change when the two treatments are interacted with the threat (T2 vs. T4:  $p = 0.948$ ; T2 vs. T6:  $p = 0.493$ ).

Second, we turn to B-responses. Table 4 indicates that 28.04% updated their information on a valid registration. The legal threat shows no effect between the treatments T1 and T2 ( $p = 0.437$ ), nor between T3 and T4 ( $p = 0.625$ ). It is significant only between T5 and T6 ( $p = 0.002$ ), when it interacts with the moral appeal treatment. However, the moral framing per se has a significantly negative impact on update responses (T1 vs. T5:  $p = 0.000$ ). In treatment T6, the threat only partially counterbalances this negative effect, so that the response rate is still below the one in treatment T2 (T2 vs. T6:  $p = 0.059$ ). The social information treatment also triggers a significant decline in B-responses (T1 vs. T3:  $p = 0.031$ ; T2 vs. T4:  $p = 0.014$ ).

Finally, for category C responses – individuals who declare not to have any broadcasting receiver – the legal threat has a slightly positive impact that is significant at a 10%-margin between treatment T1 and T2 ( $p = 0.063$ ). Unlike for A- and B-responses, this effect cannot be attributed to a single type: it could be driven either by A-types who pretend not to hold a broadcasting receiver or by honestly-responding C-types. Concerning category D (unclassified response), there are no significant treatment effects.

<sup>25</sup>The treatments also affect how quickly mailing recipients respond with a registration. The time pattern of A-responses is discussed in Appendix III.

### 5.3 Regression analysis of treatments

*A-Responses.* We complement the analysis by estimating regressions on A-responses. As dependent variable we use a dummy  $P_i^A$  that indicates whether individual  $i$  registered (A-response) within 50 days.<sup>26</sup> We consider the model

$$P_i^A = \alpha + \beta \text{Mailing}_i + \gamma_1 \text{Threat}_i + \gamma_2 \text{Moral}_i + \gamma_3 \text{Info}_i + \varepsilon_i. \quad (1)$$

The variable  $\text{Mailing}_i$  captures the basic effect of receiving a mailing, taking registrations in the no-mailing condition as benchmark.  $\text{Threat}_i$ ,  $\text{Info}_i$  and  $\text{Moral}_i$  are treatment dummies equal to one if individual  $i$  received a mailing that included the legal threat (T2, T4, and T6), social information (T3 and T4) or a moral appeal (T5 and T6), respectively. The  $\gamma$ -coefficients thus measure the treatments' impact on registrations, relative to the baseline mailing T1. To account for potential interactions between the treatments, we step-by-step include  $\text{Threat} \times \text{Moral}_i$  and  $\text{Threat} \times \text{Info}_i$  in equation (1). All equations are estimated using the linear probability model.<sup>27</sup> The results are reported in table 5.

The regressions confirm our findings from above. Receiving a mailing has a significant and strongly positive effect on registrations. Among the different mailing treatments, the threat significantly increases individuals' propensity to register by one percentage point. 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 columns (II) to (IV) is statistically significant.<sup>28</sup> While the first set of estimations is based on the full sample of 50,498 observations, we drop the no-mailing sample and exclude all observations with non-deliverable mailings in columns

---

<sup>26</sup>As discussed in Appendix IV, this variable – which is insensitive to responses from B- and C-types – is determined by the share of A-types in the sample and their response behavior. The accurate frequency of A-types is not identified. Moreover, one cannot simply make use of the bounds on the type distribution that are implied by the frequency of B-responses, since these responses are also sensitive to the treatments. Our estimations assume that each mailing recipient could produce an A-response. This yields consistent estimates of the treatment effects on the observed registration frequency for our sample. However, as there is a non-negligible share of B-types, we will underestimate the type specific treatment effects (see Appendix IV).

<sup>27</sup>In the following, we estimate equations with many interaction terms as explanatory variables. Computing correct interaction effects in non-linear 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 tables 5 and 6 are basically identical to those from probit estimations (not reported).

<sup>28</sup>Based on the estimations in columns (IV) and (VIII), an F-test indicates that one cannot reject the null that both interaction terms equal zero ( $p = 0.728$  and  $p = 0.814$ , respectively).

Table 5: Treatment effects on registrations (A-responses)

Dependent variable: Registrations (within 50 days)								
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Mailing	0.065*** (0.003)	0.065*** (0.003)	0.066*** (0.003)	0.066*** (0.003)				
Threat	0.010*** (0.002)	0.011*** (0.003)	0.009*** (0.003)	0.009** (0.004)	0.012*** (0.003)	0.013*** (0.003)	0.011*** (0.003)	0.010** (0.005)
Moral	-0.004 (0.003)	-0.003 (0.004)	-0.004 (0.003)	-0.004 (0.004)	-0.004 (0.003)	-0.003 (0.004)	-0.004 (0.003)	-0.004 (0.005)
Info	-0.002 (0.003)	-0.002 (0.003)	-0.004 (0.004)	-0.004 (0.004)	-0.002 (0.003)	-0.002 (0.003)	-0.004 (0.004)	-0.004 (0.005)
Threat × Moral		-0.002 (0.005)		0.001 (0.006)		-0.001 (0.006)		0.001 (0.007)
Threat × Info			0.004 (0.005)	0.004 (0.006)			0.004 (0.006)	0.004 (0.007)
Constant	0.008*** (0.002)	0.008*** (0.002)	0.008*** (0.002)	0.008*** (0.002)	0.085*** (0.003)	0.085*** (0.003)	0.086*** (0.003)	0.086*** (0.003)
Observations	50,498	50,498	50,498	50,498	41,007	41,007	41,007	41,007

*Notes:* All specifications are estimated with a linear probability model. (I)–(IV) are based on the full sample, (V)–(VIII) drops the no-mailing sample and observations where a mailing was not delivered. Robust standard errors are in parentheses. \*\*\* and \*\* indicate significance at a 1% and 5%-level, respectively.

(V)–(VIII).<sup>29</sup> The second set of estimations reveals that the point estimates of the treatment effects hardly change. As a further robustness check we include additional control variables, like recipients' gender and several municipality characteristics in the estimations (see table A.1 in Appendix II). Adding these variables leaves the estimates basically unaffected (not reported), demonstrating that the controls are orthogonal to our treatments due to the successful randomization.

The results provided above corroborate the economic model of crime. Threatening evaders with inspections clearly works as an enforcement strategy, whereas the moral appeal and the social information fail. The legal threat increases the registration rate by slightly more than one percentage point as compared to an estimated 8.5 percentage points for the baseline mailing. This suggests that the threat successfully altered individuals' risk perceptions. (We will discuss additional evidence that supports this interpretation below.) The registration rates in the legal threat treatments are about 15% above the rates in the mailing treatments without the threat paragraph. Under conservative assumptions (see footnote 23), our change in the wording of the cover letter thus produced an increase in

<sup>29</sup>Information on erroneous addresses, i.e., non-delivered mailings, is not available for the no-mailing sample.

expected revenues per delivered mailing of €2.50. For the 100,000 mailings delivered by FIS per year, this would correspond to an additional revenue of €250,000.

In contrast to the legal threat, the strategies based on moral appeals and the provision of social information (further discussed below) fail to increase compliance. The ineffectiveness of the moral appeal, however, does not necessarily indicate that moral standards are irrelevant for compliance with the law. As our mailings were targeted to a specific sample of individuals who deviate from the law, it is likely that they do not share the activated moral value (Meares *et al.*, 2004). This interpretation is supported by evidence of Pruckner and Sausgruber (2008), who report that people respond to a moral appeal only if they have internalized a norm against cheating.

*B-Responses.* Next, we consider the treatments impact on update responses. We regress the dummy variable  $P_i^B$  that indicates whether individual  $i$  updated her contract detail, on our treatment variables.<sup>30</sup> As we cannot measure contract updates among the no-mailing group, we focus on the mailing sample and exclude data on non-delivered mailings.

While column (I) in table 6 indicates a positive effect of the legal threat, columns (II) and (IV) show that the effect is not robust when we account for the interaction between the threat and the moral appeal. The interaction term is significant in specification (II), but not in (IV).<sup>31</sup> For all specifications presented in table 6, the main treatment effects of the moral appeal and the social information are significantly negative. Like above, adding additional control variables does not affect the estimation results.

The estimations, in particular the full model in specification (IV), show no robust effect of the legal threat. This suggests that B-types are not driven by the desire to avoid an interaction with a field inspector. Given that law-abiding individuals are aware that they face no real sanction threat, the evidence is consistent with the deterrence hypothesis and, at the same time, with the significantly positive response of A-types to the threat: A-types fear sanctions and are thus sensitive to legal threats. B-types do not expect any fines and are thus insensitive. Section 6 provides further evidence on A- and B-types' perceptions, which supports this interpretation.

---

<sup>30</sup>Regarding type identification, similar caveats apply as above (see footnote 26 and Appendix IV).

<sup>31</sup>An F-test does not reject the joint hypothesis of both interaction terms being zero ( $p = 0.121$  for specification (IV)). However, the F-test clearly rejects the null of the *Threat* and *Threat*  $\times$  *Moral* being both zero ( $p = 0.005$  for (IV)).



Table 6: Treatment effects on contract updates (B-responses)

Dependent variable: Update responses (within 50 days)				
	(I)	(II)	(III)	(IV)
Threat	0.011** (0.004)	0.005 (0.005)	0.015*** (0.005)	0.006 (0.008)
Moral	-0.024*** (0.005)	-0.033*** (0.007)	-0.024*** (0.005)	-0.033*** (0.008)
Info	-0.018*** (0.005)	-0.018*** (0.005)	-0.012* (0.007)	-0.017** (0.008)
Threat $\times$ Moral		0.019** (0.009)		0.018 (0.011)
Threat $\times$ Info			-0.011 (0.009)	-0.002 (0.011)
Constant	0.289*** (0.004)	0.287*** (0.005)	0.292*** (0.005)	0.291*** (0.005)
Observations	41,007	41,007	41,007	41,007

*Notes:* All specifications are estimated with a linear probability model. Robust standard errors are in parentheses. \*\*\*, \*\*, \* indicate significance at a 1%, 5%, 10%-level, respectively.

It is not straightforward to explain the negative effect observed for the moral appeal and the social information treatment. Both treatments have in common that they point to non-compliance. The moral framing, for instance, explicitly reminds B-types of being cheated (*'...harm all honest households'*). Stressing the fact that some people cheat them might undermine the law-abiding individuals' propensity 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, thus resulting in the positive interaction found in specification (II).

## 5.4 Interaction Effects with Municipality Characteristics

### 5.4.1 Social Information

The fact that the social information treatment did not increase A-responses on aggregate is not conclusive for a final evaluation of this treatment. As discussed in section 4, providing social information should increase registrations only among those who initially expect compliance to be lower than communicated. FIS did neither allow us to experimentally vary the communicated compliance level (similar to Frey and Meier, 2004) nor to elicit

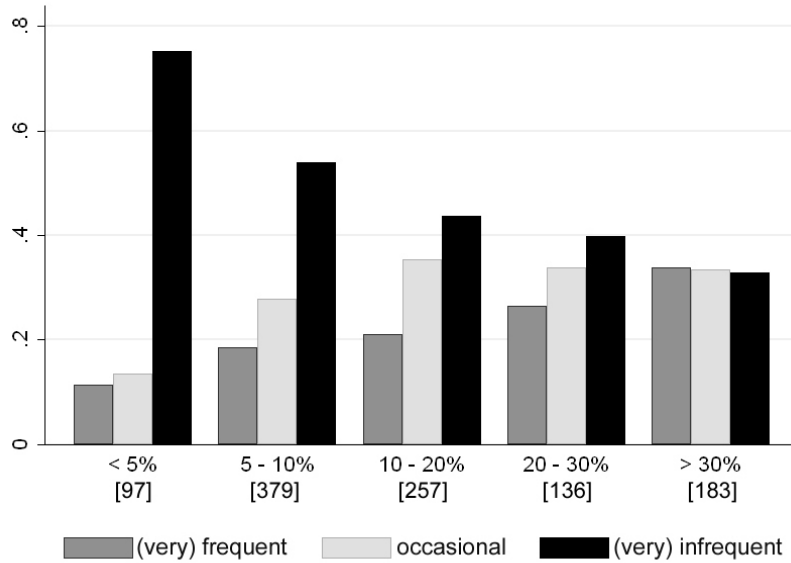


Figure 1: Beliefs about TV license fee evasion

*Notes:* The figure plots the response distribution on beliefs about the frequency of license fee evasion in Austria for regions with an estimated local evasion rate of 0-5%, 5-10%, 10-20%, 20-30% and above 30%. The number of observations in the five categories is displayed in squared brackets.

prior beliefs in our sample. We therefore evaluate the prediction by exploiting a link between individual beliefs and local evasion levels estimated by FIS.

In a national survey conducted in the year 2000, more than 1000 Austrian households were asked to state their belief about the frequency of license fee evasion (see Traxler and Winter, 2009). Figure 1 plots the distribution of the response (measured on a five-point Likert scale ranging from ‘very frequent’ to ‘very infrequent’) against FIS’ estimate of the local evasion rate (in the categories 0-5%, 5-10%, 10-20%, 20-30% and above 30%) in the respondent’s home district at the time of the survey.<sup>32</sup> The figure reveals a strong correlation of beliefs with the local evasion levels. In regions with an evasion rate below 5%, more than 75% believe that dodging the fee is infrequent or very infrequent. In districts with evasion levels above 30%, this number drops to 33%. Vice versa, the share of respondents who believe that evasion is frequent or very frequent rises from 11% in low evasion regions to 34% in districts with high levels of non-compliance. Estimating

<sup>32</sup>The estimate for local evasion corresponds to one minus the share of households who are registered for license fees relative to the total number of households living in a jurisdiction (see table 2).

ordered probit models that control for individual characteristics confirms this correlation (not reported).

Provided that beliefs and local evasion levels 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. This is tested by estimating an equation that interacts our treatment variables with the local evasion level,

$$\begin{aligned}
 P_i^A = & \alpha + \beta_1 Threat_i + \beta_2 Moral_i + \beta_3 Info_i + \gamma_0 Evasion_i^{Q1} & (2) \\
 & + \gamma_1 Threat_i \times Evasion_i^{Q1} + \gamma_2 Moral_i \times Evasion_i^{Q1} + \gamma_3 Info_i \times Evasion_i^{Q1} \\
 & + X_i \delta + \varepsilon_i,
 \end{aligned}$$

where  $Evasion_i^{Q1}$  is a dummy indicating whether recipient  $i$  lives in a municipality that ranges in the top quartile of the local evasion rates. The vector of control variables  $X_i$  includes the recipient's gender and several municipality characteristics (see Table A.1 in the Appendix).  $\beta_1$ ,  $\beta_2$  and  $\beta_3$  measure the main treatment effects. Any additional effects in high-evasion municipalities are taken up by  $\gamma_1$ ,  $\gamma_2$  and  $\gamma_3$ ; the non-interacted difference in registrations is captured by  $\gamma_0$ . According to our hypothesis,  $\gamma_3$  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 7 displays the results from estimating equation (2). In column (II), we introduce a full set of municipality dummies. In both specifications,  $\gamma_3$  is positive and significant at a 10% level. In specifications (III) and (IV), where we replace  $Evasion_i^{Q1}$  with  $Evasion_i^{T1}$ , a dummy for the top tercile regarding the local evasion rates, we obtain virtually identical results. The estimations document 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 positive, in the latter a weakly 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  $p = 0.212$  for specification (I) and (II), respectively). Moreover, we do not observe any significant interaction effect once we estimate a model with linear treatment interactions with the evasion rate  $Evasion_i$  (estimations not reported). Hence, the observed effect is not very robust. Finally, it

Table 7: Treatment effects on registrations: local evasion

Dependent variable: Registrations (within 50 days)				
	(I)	(II)	(III)	(IV)
Threat	0.014***	0.013***	0.013***	0.012***
( $\beta_1$ )	(0.003)	(0.003)	(0.003)	(0.003)
Moral	-0.009**	-0.007	-0.009**	-0.006
( $\beta_2$ )	(0.004)	(0.004)	(0.004)	(0.004)
Info	-0.007	-0.005	-0.007	-0.005
( $\beta_3$ )	(0.004)	(0.004)	(0.004)	(0.004)
Threat $\times$ Evasion <sup>Q1</sup>	-0.004	-0.004		
( $\gamma_1$ )	(0.006)	(0.006)		
Moral $\times$ Evasion <sup>Q1</sup>	0.012	0.011		
( $\gamma_2$ )	(0.007)	(0.007)		
Info $\times$ Evasion <sup>Q1</sup>	0.014*	0.012*		
( $\gamma_3$ )	(0.007)	(0.007)		
Evasion <sup>Q1</sup>	0.011*			
( $\gamma_0$ )	(0.006)			
Threat $\times$ Evasion <sup>T1</sup>			-0.004	-0.003
			(0.006)	(0.006)
Moral $\times$ Evasion <sup>T1</sup>			0.011	0.011
			(0.007)	(0.007)
Info $\times$ Evasion <sup>T1</sup>			0.013*	0.012*
			(0.007)	(0.007)
Evasion <sup>T1</sup>			0.010	
			(0.006)	
Municipality Controls ( <i>MC</i> ) or Dummies ( <i>FE</i> )	<i>MC</i>	<i>FE</i>	<i>MC</i>	<i>FE</i>
Observations	41,007	41,007	41,007	41,007

*Notes:* All specifications are estimated with linear probability models and include a constant term. Robust standard errors are in parentheses. \*\*\*, \*\*, \* indicate significance at a 1%, 5%, 10%-level, respectively.

is worth noting that the coefficient from the moral appeal is significantly negative in specification (I) and (III). The moral framing tends to exert a significantly negative effect on registrations in a sizable subsample of our study. This effect also appears in the following analysis.

#### 5.4.2 Legal Threat

There is substantial heterogeneity across municipalities in our sample. The municipalities' population size, for instance, ranges from a mere 100 to 240,000 individuals for Vienna's largest district. This heterogeneity allows us to explore the role of municipality characteristics in altering the impact of our treatments. We focus on A-responses and estimate

interaction models similar to those from above, either including the full set of control variables or municipality dummies. The results from these estimations are reported in table 8.

The first column of the table reveals that the effectiveness of mailings in raising registrations decreases with the population size. Relative to the other mailings, however, the threat is more successful in larger municipalities. The main effect of the threat remains significant in both specifications (I) and (II). This is in line with the results from columns (III) and (IV), which display a stronger effect of the threat in municipalities in the top quartile of population density. The legal threat seems to have a larger effect in urban areas. Like in table 7, we find a significantly negative effect of the moral appeal in specifications (I) and (III). The latter estimation also indicates a positive interaction with the population density. However, both effects of the moral appeal vanish once we include municipality dummies.

Columns (V) and (VI) further show that the threat is slightly more effective in municipalities with an average income in the top end of the distribution. However, the interaction is only significant at the 10%-level in the FE-specification. Column (VII) and (VIII) report the interaction with the political orientation. The legal threat is most effective in municipalities with many center-left voters and has a weaker effect in more right-leaning municipalities. We considered several additional treatment interactions, for instance with the population inflow into municipalities. This variable should, *ceteris paribus*, positively correlate with the frequency of evaders who recently moved but did not yet register at the new place (in contrast to those who already evade for a longer time). Estimations did not indicate any significant interaction effects.

Finally, we studied the sensitivity of our results with respect to the municipalities' mailing coverage, i.e., the number of mailings relative to the municipality population. One might suspect that communication between recipients of different mailings could undermine the treatment effects. In fact, we estimate a slightly stronger effect of the legal threat if we exclude the top quartile of municipalities regarding mailing coverage (not reported). Running the estimations from table 7 on the restricted sample, one also obtains a stronger effect of the social information. These stronger treatment effects, however, seem to be driven by type composition rather than by treatment spillover effects. To see this

Table 8: Treatment interactions with municipality characteristics

Dependent variable: Registrations (within 50 days)								
Interactions	Population Size		High Pop.Density		High Income		Share of Right Voters	
	(I)	(II)	(III)	(IV)	(V)	(VI)	(VII)	(VIII)
Threat	0.008*** (0.003)	0.007** (0.003)	0.008** (0.003)	0.006* (0.003)	0.009*** (0.003)	0.008** (0.003)	0.042*** (0.013)	0.042*** (0.013)
Moral	-0.008** (0.004)	-0.005 (0.004)	-0.009** (0.004)	-0.006 (0.004)	-0.006 (0.004)	-0.002 (0.004)	0.002 (0.015)	0.000 (0.016)
Info	-0.002 (0.004)	-0.000 (0.004)	-0.002 (0.004)	0.000 (0.004)	-0.002 (0.004)	-0.001 (0.004)	0.005 (0.015)	0.005 (0.016)
Threat × PopSize	0.009** (0.004)	0.010** (0.004)						
Moral × PopSize	0.006 (0.005)	0.005 (0.005)						
Info × PopSize	-0.000 (0.005)	-0.001 (0.005)						
PopSize	-0.022*** (0.008)							
Threat × PopDens <sup>Q1</sup>			0.018*** (0.007)	0.020*** (0.007)				
Moral × PopDens <sup>Q1</sup>			0.015* (0.008)	0.011 (0.009)				
Info × PopDens <sup>Q1</sup>			-0.001 (0.008)	-0.004 (0.008)				
PopDens <sup>Q1</sup>			-0.016 (0.010)					
Threat × Income <sup>Q1</sup>					0.010 (0.007)	0.012* (0.007)		
Moral × Income <sup>Q1</sup>					0.002 (0.008)	-0.002 (0.008)		
Info × Income <sup>Q1</sup>					0.000 (0.008)	-0.001 (0.009)		
Income <sup>Q1</sup>					0.000 (0.013)			
Threat × RightVoters							-0.049*** (0.019)	-0.051*** (0.019)
Moral × RightVoters							-0.011 (0.023)	-0.005 (0.023)
Info × RightVoters							-0.013 (0.023)	-0.010 (0.023)
RightVoters							-0.018 (0.037)	
Municipality Controls ( <i>MC</i> ) or Dummies ( <i>FE</i> )	<i>MC</i>	<i>FE</i>	<i>MC</i>	<i>FE</i>	<i>MC</i>	<i>FE</i>	<i>MC</i>	<i>FE</i>
Observations	41,007	41,007	41,007	41,007	41,007	41,007	41,007	41,007

*Notes:* All specifications are estimated with a linear probability model and include a constant term. Robust standard errors are in parentheses. \*\*\*, \*\*, \* indicate significance at a 1%, 5%, 10%-level, respectively. *PopSize* measures the population in 1,000 inhabitants; *PopDens<sup>Q1</sup>* and *Income<sup>Q1</sup>* are dummies for municipalities in the top quartile of the population density and the gross average incomes, respectively; *RightVoters* is the share of votes casted to rightist parties (*Volkspartei*, *Freiheitliche* and *Bündnis Zukunft Österreich*) in the 2006 parliamentary elections.

point, note that a high mailing coverage indicates that FIS has little information from prior mailing campaigns in these municipalities and thus addresses many B-types. This is well reflected in our data: the rate of B-responses exceeds 44% in the top quartile regarding mailing coverage, as compared to 24% in the remaining sample. Excluding high-coverage municipalities, we thus obtain stronger treatment effects on A-responses simply because the frequency of B-types is lower (see Appendix IV). In any case, our findings are highly robust when we account for heterogenous mailing coverage.

## 6 Perception Survey

Our experiment establishes controlled field evidence on the causal impact of different interventions on compliance behavior. To understand better the mechanisms behind these results, we conducted an anonymous survey that evaluates the treatments' impact on subjective perceptions. The web-based survey was implemented among a sample of students from the University of Innsbruck. 3,233 participants completed the survey. Details on the procedure are described in Appendix V.

Survey participants were randomly confronted with one of two scenarios. One scenario described an A-type vignette (a person who moved to a new place six months ago and evades the fee since then), the other one a B-type vignette (someone who moved six months ago, paid the fee, but did not inform FIS about the change in address). After participants read the scenario description, the web page randomly linked them to a situation that mimicked one of the treatments from the field experiment. A random subsample formed the control (T0). All others were instructed that the vignette person received a mailing from FIS. Thereafter a cover letter, which corresponds to one of our six mailing treatments (T1–T6), was displayed on the web page. Survey participants were then asked to evaluate the situation of the person. In this way, we elicited the treatments' impact on perceptions regarding, e.g., the risk of a field inspection, the size of fines, and potential social sanctions.

Consider first how the treatments affect the perceived inspection risk. Table 9 shows the outcome of regressing the stated risk perception (ranging from 0 and 100) on the treatments and the scenario type. By far the largest impact on perceptions is induced by the mailing conditions. After being confronted with a mailing, survey participants

Table 9: Survey results: Treatment effects on risk perceptions

Dependent variable: Expected inspection risk		
	(I)	(II)
Mailing	22.923*** (2.179)	24.333*** (2.183)
Mailing $\times$ Threat	2.921*** (1.104)	3.193*** (1.107)
Mailing $\times$ Moral	0.768 (1.332)	-0.056 (1.338)
Mailing $\times$ Info	-0.306 (1.360)	-1.064 (1.362)
A-type	0.673 (1.103)	0.862 (1.104)
Constant	38.388*** (2.106)	56.801*** (5.354)
Additional control	No	Yes
Observations	3,213	3,098

*Notes:* Results from OLS regressions with robust standard errors in parentheses. \*\*\* indicates significance at a 1%-level.

evaluate the household's inspection risk to be roughly 60% higher than in the no-mailing group.<sup>33</sup> This suggests that the huge overall impact of the mailings observed in the field experiment is also driven by an alert effect. Next to lowering transaction costs, the mailings signal surveillance and thereby increase the risk perception. Among the mailing treatments, only the legal threat shows a significant effect. Compared to the baseline mailing (T1), the threat increases the expected inspection risk by another 5%. Specification (II) includes control variables on the respondents' personal characteristics. This hardly changes the point estimates of the treatment effects. The same holds true when we estimate Tobit instead of OLS regressions.

Turning to the expected size of fines, we do not find any impact of the legal threat nor any mailing effect. The survey reveals, however, that participants expect significantly lower fines for B- than for A-types ( $p = 0.003$ , according to a Wilcoxon rank-sum test). Equivalently, B-types are also expected to face significantly weaker social sanctions in case

<sup>33</sup>The percentage is based on the estimates from specification (I) in table 9, putting the coefficient for the mailing dummy relative to the constant.



of an inspection ( $p = 0.003$ ). These findings are consistent with the fact that FIS does not impose fines on B-types and supports our interpretation of the heterogenous impact of the legal threat on A- and B-types established in the field experiment (see section 5.2): the legal threat is successful in enforcing registrations because it increases the perceived inspection risk *and* because A-types expect significant sanctions. While the threat also induces B-types to perceive a higher chance of facing a field inspector, the inspection is rationally expected to have few consequences. Thus, the threat treatment does not increase B-responses.

## 7 Conclusions

We tested different strategies to enforce compliance with the law in a large-scale natural field experiment. The experiment manipulated the text of personalized mailings that were sent to potential evaders of TV license fees. Treatment effects were evaluated by comparing the response of recipients, in particular whether evaders start to comply with the payment duty. We gathered supplementary data from a survey that studied the treatments' impact on subjective perceptions. These data enabled us to analyze further the mechanism linking different policy manipulations, perceptions, and behavior. The main contributions of our study are twofold. First, we provide strong and conceptually consistent support for the economic model of crime. Second, we show that alternative enforcement strategies are, by and large, unsuccessful and discuss the reasons for the failure.

Regarding the first contribution, we demonstrate that the authority can successfully deter law violations by manipulating individuals' perceived costs of non-compliance. The study contains three pieces of evidence in support of this conclusion. First, comparing those who received a mailing with an untreated control group, we observe a striking impact of the mailings. The fraction of evaders who start to comply with the payment duty is nearly 10 times higher in the mailing condition. The huge effect is equally reflected in the survey, which reveals that mailings alert individuals to the risk of being detected as violators. In combination with a reduction in transaction costs, this alert effect creates the pronounced change in behavior. Second, adding a legal threat to a neutral letter induces

a further, considerable increase in compliance. Because the setup of the experiment excludes incapacitation and displacement effects, the result provides clean, experimental evidence on deterrence in the field. The survey data corroborate the economic model of crime, indicating that the deterrent effect of the threat is caused by increasing individuals' expectations about the price of non-compliance. Finally, we take advantage of the fact that our sample consists of heterogeneous types: evaders who face potentially severe sanctions and others, who are not exposed to economic sanctions. In line with rational behavior, the legal threat produces a strong response only among the former. Taken together, this first set of results lends compelling support to the economic model of crime.

A further property of the legal threat is that it has no negative effect on the behavior of individuals who abide by the law. This observation is of great importance from a policy perspective: in most natural situations, law-abiding and law-violating individuals are exposed to the same enforcement strategies, just like in our experiment. For an effective enforcement policy, it is therefore essential not to cause detrimental spillovers on the compliant population. Regarding non-compliant individuals, our results show an unrestrictedly positive effect of the threat on compliance. The treatment's success is robust for all strata within our sample and insensitive to interactions with additional treatments. Note, however, that our study only measures short-run responses. In the long run, the effectiveness of legal threats presumably relies on the actual enforcement activities. The costs of maintaining the credibility of deterrent threats stimulate the search for alternative enforcement strategies, leading to our second set of results.

Our second contribution consists of testing moral appeals and the provision of social information as instruments of enforcement. Compared to a neutral mailing, both strategies failed to increase aggregate compliance. To evaluate the ineffectiveness of the moral suasion, it is important to note that our mailings were targeted at a sample of individuals who deviate from the law. Thus, it is likely that the activated moral value is not shared by the recipients. Appealing to a conflicting norm might further reduce the willingness to comply with the law. In line with this interpretation, we find a negative effect of the appeal in large subsamples of our study. Hence, moral appeals are no attractive strategy to enforce compliance among those who deliberately violate the law.

While the social information treatment is ineffective on aggregate, the treatments' impact on behavior crucially relies on individuals' prior beliefs about the compliance of others: with non-common priors, the information that 94% of all Austrians follow the law can affect beliefs – and thus compliance – in either direction. Our empirical assessment of this hypothesis makes use of supplementary evidence that documents a positive correlation between the perceived and the actual local compliance rate. Exploiting this correlation, we uncover significantly different treatment effects in municipalities with low and high compliance rates. In the former, where evasion is believed to be common, the social information has a positive, in the latter a weakly negative impact. This conditional compliance effect, however, is not very strong. To test social information policies comprehensively, future research should experimentally vary the provided information. Already our findings demonstrate that 'belief management', i.e., policy interventions that aim at affecting subjective beliefs, may not be easily applicable to change behavior into the desired direction. With non-common priors, one and the same piece of information can convey different messages and cause contrary reactions. To render belief management successful, it is necessary to communicate the 'right' piece of information to the 'right' target group – a precondition that might be very costly to meet in practice.

To sum up, our study provides clear-cut evidence on deterrence. The experiment documents that legal threats unambiguously work as an enforcement strategy. Neither moral appeals nor imparting social information turn out to increase compliance. Overall, the economic model of crime performs remarkably well in explaining our data. Designing successful enforcement policies that rely on behavioral motives beyond the incentives considered in the Beckerian analysis remains a challenging task for future research.

## References

- AI, C. and NORTON, E. C. (2003). Interaction terms in logit and probit models. *Economics Letters*, **80** (1), 123–129.
- ANDERSON, S. P. and COATE, S. (2005). Market provision of broadcasting: A welfare analysis. *Review of Economic Studies*, **72** (4), 947–972.
- ARIELY, D., BRACHA, A. and MEIER, S. (2009). Doing Good or Doing Well? Image Motivation and Monetary Incentives in Behaving Prosocially. *American Economic Review*, **99** (1), 544–555.
- BARDACH, E. (1989). Moral suasion and taxpayer compliance. *Law and Policy*, **11** (1), 49–69.
- BECKER, G. (1968). Crime and punishment: An economic approach. *Journal of Political Economy*, **76** (2), 169–217.
- BENABOU, R. and TIROLE, J. (2006). Incentives and prosocial behavior. *American Economic Review*, **95** (5), 1652–1678.
- BERNHEIM, D. B. (1994). A theory of conformity. *Journal of Political Economy*, **102** (5), 841–877.
- BLUMENTHAL, M., CHRISTIAN, C. and SLEMROD, J. (2001). Do normative appeals affect tax compliance? evidence from a controlled experiment in minnesota. *National Tax Journal*, **54** (1), 125–138.
- CIALDINI, R. (1998). *Influence: The Psychology of Persuasion (Revised Version)*. New York: Collins.
- CORMAN, H. and MOCAN, N. (2000). A time-series analysis of crime, deterrence, and drug abuse in new york city. *American Economic Review*, **90** (3), 584–604.
- and — (2005). Carrots, sticks, and broken windows. *Journal of Law & Economics*, **48** (1), 235–66.
- DILLS, A. K., MIRON, J. A. and SUMMERS, G. (2009). What do economists know about crime? In S. Edwards, R. D. Tella and E. Schargrodsky (eds.), *Crime, Institutions, and Policies*, Chicago: University of Chicago Press, forthcoming.
- DiTELLA, R. and SCHARGRODSKY, E. (2004). Do police reduce crime? estimates using the allocation of police forces after a terrorist attack. *American Economic Review*, **94** (1), 115–133.
- DRAGO, F., GALBIATI, R. and VERTOVA, P. (2009). The deterrent effects of prison: Evidence from a natural experiment. *Journal of Political Economy*, **117** (2), 257–280.
- ELICKSON, R. C. (1998). Law and economics discovers social norms. *The Journal of Legal Studies*, **27** (2), 537–552.
- ELSTER, J. (1989). *The Cement of Society: A study of social order*. Cambridge (MA): Cambridge University Press.

- FALK, A., FEHR, E. and FISCHBACHER, U. (2005). Driving forces behind informal sanctions. *Econometrica*, **73** (6), 2017–2030.
- and KOSFELD, M. (2006). The hidden cost of control. *American Economic Review*, **96** (5), 1611–1630.
- FARRINGTON, D. P. and WELSH, B. (2006). Randomized experiments in criminology: What have we learned in the last two decades? *Journal of Experimental Criminology*, **1** (1), 9–38.
- FEHR, E. and FALK, A. (2002). Psychological foundations of incentives. *European Economic Review*, **46** (4-5), 687–724.
- and LIST, J. (2004). The hidden costs and returns of incentives – trust and trustworthiness among CEOs. *Journal of the European Economic Association*, **2** (5), 743–771.
- FISCHBACHER, U. and GÄCHTER, S. (2009). Social preferences, beliefs, and the dynamics of free riding in public good experiments. *American Economic Review*, forthcoming.
- FORTIN, B., LACROIX, G. and VILLEVAL, M.-C. (2007). Tax evasion and social interactions. *Journal of Public Economics*, **91** (11-12), 2089–2112.
- FREY, B. (1997). *Not Just for Money. An Economic Theory of Personal Motivation*. Cheltenham, UK: Edward Elgar Publishing.
- (2009). Punishment – and beyond, CESifo Working Paper No.2706.
- and MEIER, S. (2004). Social comparisons and pro-social behavior: Testing ‘conditional cooperation’ in a field experiment. *American Economic Review*, **94** (5), 1717–1722.
- GALBIATI, R. and ZANELLA, G. (2008). The social multiplier of tax evasion: Evidence from Italian audit data, Department of Economics, University of Siena, Working Paper 539.
- GLAESER, E., SACERDOTE, B. and SCHEINKMAN, J. (1996). Crime and social interactions. *Quarterly Journal of Economics*, **111** (2), 507–548.
- GNEEZY, U. and RUSTICHINI, A. (2000). A fine is a price. *Journal of Legal Studies*, **29** (1), 1–17.
- GRAETZ, M. J., REINGANUM, J. F. and WILDE, L. L. (1986). The tax compliance game: Toward an interactive theory of law enforcement. *Journal of Law, Economics and Organization*, **2**, 1–32.
- HARCOURT, B. E. and LUDWIG, J. (2006). Broken Windows: New Evidence from New York City and a Five-City Social Experiment. *University of Chicago Law Review*, **73** (1), 271–320.
- HARRISON, G. and LIST, J. (2004). Field experiments. *Journal of Economic Literature*, **42** (4), 1009–1055.
- HEAD, S. W. (1985). *World Broadcasting Systems: A Comparative Analysis*. Belmont, California: Wadsworth.

- HJALMARSSON, R. (2009). Crime and expected punishment: Changes in perceptions at the age of criminal majority. *American Law and Economics Review*, forthcoming.
- HUCK, S. and RASUL, I. (2009). Transaction costs in charitable giving: Evidence from two field experiments, Mimeo, University College London.
- JACOB, B., LEFGREN, L. and MORETTI, E. (2007). The dynamics of criminal behavior: Evidence from weather shocks. *Journal of Human Resources*, **42** (3), 489–527.
- KAHAN, D. M. (1997). Social influence, social meaning, and deterrence. *Virginia Law Review*, **83** (2), 349–395.
- KEIZER, K., LINDENBERG, S. and STEG, L. (2008). The spreading of disorder. *Science*, **322** (5908), 1681–1685.
- KESSLER, D. and LEVITT, S. D. (1999). Using sentence enhancements to distinguish between deterrence and incapacitation. *Journal of Law and Economics*, **42**, 343–363.
- KLEVEN, H. J., KNUDSEN, M., KREINER, C. T., PEDERSEN, S. and SAEZ, E. (2009). An experimental evaluation of the tax evasion and tax enforcement in Denmark, Mimeo, Department of Economics, University of Copenhagen.
- LEVITT, S. D. (1998). Why do increased arrest rates appear to reduce crime: Deterrence, incapacitation, or measurement error? *Economic Inquiry*, **36** (3), 353–72.
- (2002). Using electoral cycles in police hiring to estimate the effects of police on crime: Reply. *American Economic Review*, **92** (4), 1244–1250.
- and LIST, J. A. (2007). What do laboratory experiments tell us about the real world? *Journal of Economic Perspectives*, **21** (2), 153–174.
- and — (2009). Field experiments in economics: The past, the present, and the future. *European Economic Review*, **53** (1), 1–18.
- and MILES, T. J. (2007). Empirical study of criminal punishment. pp. 455–495.
- LOCHNER, L. (2007). Individual perceptions of the criminal justice system. *American Economic Review*, **97** (1), 444–460.
- MANSKI, C. F. (2004). Measuring expectations. *Econometrica*, **72** (5), 1329–1376.
- MCADAMS, R. H. and RASMUSEN, E. B. (2007). Norms and the law. In A. M. Polinsky and S. Shavell (eds.), *Handbook of Law and Economics, Volume 2*, Amsterdam: North Holland, pp. 1573–1618.
- MCGRAW, K. M. and SCHOLZ, J. T. (1991). Appeals to civic virtue versus attention to self-interest: Effects on tax compliance. *Law and Society Review*, **25** (3), 471–498.
- MEARES, T. L., KATYAL, N. and KAHAN, D. M. (2004). Updating the study of punishment. *Stanford Law Review*, **56**, 1171–1210.
- NAGIN, D. S. (1998). Criminal deterrence research at the outset of the twenty-first century. *Crime and Justice*, **23**, 1–42.

- NEWCOMB, H. (2004). *The Museum of Broadcast Communications Encyclopedia of Television*. New York: Routledge, 2nd edn.
- OPEN SOCIETY INSTITUTE (2005). Television across europe: Regulation, policy and independence. eu monitoring and advocacy program (eumap).
- ORF MEDIENFORSCHUNG (ed.) (2006). *Ausstattung der Haushalte 1986-2006*. Vienna.
- POSNER, E. A. (2000). *Law and Social Norms*. Cambridge: Harvard University Press.
- PRUCKNER, G. J. and SAUSGRUBER, R. (2008). Honesty on the Streets - A Natural Field Experiment on Newspaper Purchasing, Working Paper, University of Innsbruck.
- RINCKE, J. and TRAXLER, C. (2009). Deterrence through word of mouth, Max Planck Institute for Research on Collective Goods, Working Paper 2009-04.
- SAH, R. K. (1991). Social osmosis and patterns of crime. *Journal of Political Economy*, **99** (6), 1272–1295.
- SCHWARTZ, R. D. and ORLEANS, S. (1967). On legal sanctions. *University of Chicago Law Review*, **34** (2), 274–300.
- SHANG, J. and CROSON, R. (2007). Field experiments in charitable contribution: The impact of social influence on the voluntary provision of public goods. *The Economic Journal*, forthcoming.
- SLEMROD, J., BLUMENTHAL, M. and CHRISTIAN, C. (2001). Taxpayer response to an increased probability of audit: evidence from a controlled experiment in minnesota. *Journal of Public Economics*, **79** (3), 455–483.
- SUNSTEIN, C. R. (2004). Moral heuristics and moral framing. *Minnesota Law Review*, **88** (6), 1556–1597.
- TRAXLER, C. and WINTER, J. (2009). Survey evidence on conditional norm enforcement, Max Planck Institute for Research on Collective Goods, Working Paper 2009-03.
- WENZEL, M. and TAYLOR, N. (2004). An experimental evaluation of tax-reporting schedules: a case of evidence-based tax administration. *Journal of Public Economics*, **88** (12), 2785–2799.
- WILSON, J. Q. and KELLING, G. L. (1982). Broken windows. *The Atlantic Monthly*, **249** (3), 29–38.

## Appendix I: Text and Layout of Mailings

Note that the texts of all treatment manipulations were approved by FIS' legal department. Below we report the translated mailing texts and an original copy of a mailing that illustrates the layout.

### I.1 Standard Cover Letter (Control Treatment T1)

*Dear Mr. X,*

*You listen to radio, you watch TV? Then you are aware of the program variety offered by Austrian Public Broadcasting. The provision of these services, however, requires funding. Therefore, everybody who owns a radio or a TV has to pay license fees. It is the task of FIS Gebühren Info Service GmbH to ensure that all TV and radio consumers pay these fees.*

[1]

***Our data base does not show a registration of TV or radio equipment at your address. This can have several reasons:***

- We may have made a mistake in our data base and you are already registered at FIS. **In this case, we apologize in advance.***
- Your registration data may have changed, e.g., due to a move or a name change (marriage), and our computer system cannot match the data with your registration.*
- You may not hold a radio or a TV at this address and therefore do not have to register anything.*
- Maybe you have just forgotten to register your TV or radio.*

*We are legally obliged to clarify this issue and kindly ask you to answer our questions – even if you have already registered at FIS. On the back of this letter you find a response form. **Please fill in this form and send it back within the next 14 days.***

[2]

*We thank you for your cooperation. If you require further information, please call our service hotline at 0810 00 10 80 (Monday to Friday, 8.00 am to 9.00 pm, Saturday from 9.00 am to 5.00 pm) or visit our web page at [www.orf-gis.at](http://www.orf-gis.at). Kind regards, your FIS-Team.*



## I.2 Legal Threat

In the legal threat treatments (T2, T4 and T6), the cover letter includes the following paragraph at position [2] of the standard letter:

***If you do not respond to this letter, a staff member of FIS 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, FIS 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.***

## I.3 Social Information

In the social information mailings (treatments T3 and T4), we add the following paragraph at position [1] of the standard letter:

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

## I.4 Moral Appeal

The treatments with a moral appeal (T5 and T6), included the following paragraph at position [1]:

***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.***

## I.5 Original Copy



GIS GMBH, 1051 Wien, Postfach 1999, DMMD211 60125200

Frau

Personenstand:   
Geburtsdatum:   
Matrikel-Nr.:



GEBÜHREN INFO SERVICE

60125200  
DMMD211

Wien, 3. Oktober 2005

### SIE HÖREN RADIO, SIE SEHEN FERN?

Sehr geehrte Frau *Staudl*,

Sie hören Radio, Sie sehen fern? Dann kennen Sie die vielfältigen Programme, die Ihnen der ORF täglich bietet. Diese Leistungen kosten Geld. Aufgabe der GIS Gebühren Info Service GmbH ist es, dafür zu sorgen, dass alle Konsumenten von Radio und Fernsehen Gebühren zahlen. Denn sobald man ein Rundfunkempfangsgerät besitzt, sind Gebühren zu bezahlen.

### Wissen Sie übrigens, dass beinahe alle Bürger dieser gesetzlichen Verpflichtung nachkommen?

Es sind **94 Prozent**, also eine überwältigende Mehrheit aller Haushalte, die ihre Rundfunkempfangsgeräte gemeldet haben.

### Unter Ihrer Anschrift finden wir keine Meldung von Rundfunkempfangsgeräten in unserer Datenbank.

Das kann verschiedene Gründe haben:

- Vielleicht ist uns ein Fehler in der Datenverwaltung unterlaufen und wir dürfen Sie bereits zu unseren Kunden zählen. **In diesem Fall bitten wir schon jetzt um Entschuldigung!**
- Vielleicht haben sich Ihre Kundendaten geändert, etwa durch Übersiedlung oder Namensänderung (Heirat), und unsere EDV kann Ihre Daten nicht zuordnen.
- Vielleicht haben Sie keine Rundfunkempfangseinrichtungen unter dieser Adresse und sind daher auch nicht meldepflichtig.
- Vielleicht haben Sie aber einfach nur vergessen, Ihre Geräte anzumelden.

Wir sind von Gesetz wegen verpflichtet nachzufragen und bitten Sie deshalb, unsere Fragen zu beantworten – in jedem Fall, auch wenn Sie bereits Kunde sind. Eine Rückantwort ist auf der Rückseite dieses Schreibens vorbereitet. **Bitte füllen Sie diese aus und schicken Sie den Bogen innerhalb von 14 Tagen im Antwortkuvert an uns zurück.**

**Sollten Sie auf dieses Schreiben nicht reagieren, wird Sie demnächst ein Mitarbeiter der GIS kontaktieren um eine persönliche Auskunft zu erhalten.** Verweigern Sie diese oder besteht der begründete Verdacht, dass Sie eine falsche Auskunft erteilt haben, hat die GIS eine Überprüfung der Gebührenpflicht seitens der zuständigen Bundesverwaltungsbehörde zu veranlassen. Bedenken Sie bitte, dass Ihnen in diesem Fall rechtliche Konsequenzen sowie erhebliche finanzielle Kosten entstehen können.

Wir danken Ihnen für Ihre Mithilfe. Wenn Sie weitere Informationen wünschen, können Sie uns unter der Service-Hotline **0810 00 10 80** (Montag bis Freitag von 8.00 bis 21.00 Uhr, Samstag von 9.00 bis 17.00 Uhr) anrufen oder uns im Internet besuchen: [www.orf-gis.at](http://www.orf-gis.at).

Mit freundlichen Grüßen

*Jhr*  
*GIS Team*

GIS GEBÜHREN INFO SERVICE GMBH

1051 Wien, Postfach 1999 • Service-Hotline: **0810 00 10 80** • E-Mail: [gis.office@orf-gis.at](mailto:gis.office@orf-gis.at) • Internet: [www.orf-gis.at](http://www.orf-gis.at)  
DVR 0997285 • Sitz: Wien, Österreich • PSK Konto Nr. 7 503 146, BLZ 60 000 • Fbg: Handelsgericht Wien • Firmenbuch Nr. 174 754 t

Figure A.1: Original mailing for treatment T4 (Info × Threat)

## Appendix II: Summary Statistics on Municipality Characteristics

Table A.1: Municipality control variables

Variable	Description	Mean	SD	Min	Max
PopSize	population size	43,085.37	74,649.49	62.00	240,278.00
PopDensity	inhabitants/ha	8.52	23.84	0.01	256.30
Compliance	compliance rate in 2005	93.51	5.87	58.24	143.54*
Mailing Coverage	mailings relative to PopSize	0.06	0.06	0.00	0.20
AvIncome	average income/year in €	20,863.45	2150.70	17,497.13	36,105.19
AvEducation	average years of education	11.17	0.47	9.72	13.23
AvAge	average age of inhabitants in years	48.03	1.18	42.81	54.18
Unemployed	unemployment rate	3.14	1.67	0	16.96
Migration	% of population inflow from moving	5.26	3.14	0.39	31.70
Foreign	% of non-Austrian nationalities	9.37	5.44	0	50.32
VoteLeft	% of votes for left parties**	11.80	5.23	0	37.23
VoteCenterLeft	% of votes for center-left party	27.07	12.33	2.00	78.41
VoteRight	% of votes for right parties	18.53	5.56	4.00	46.00
StudentShare	% of students	1.84	1.16	0	6.67
HomeOwner	% of home owners	57.85	19.80	10.06	98.27
FamMarried	% of married households	42.78	2.73	31.67	54.16
FamDiv	% of divorced and widowed households	11.55	4.10	3.78	20.42
CathShare	% of Roman Catholics	80.83	14.49	20.06	99.90
OrthShare	% of Catholic Orthodox	1.48	1.75	0	11.37
Sample	41,007 (delivered mailings)				

Notes: \* Compliance rates above 100% are due to municipalities where households have registered their TVs but no longer live there (many households do not unregister license fees after moving, but continue to pay fees under their old address). \*\* Vote shares are in percentage of the electorate rather than casted votes.

## Appendix III: Response Time

Figure A.2 compares the relative as well as the cumulated frequencies of registrations (A-responses) per week between the baseline treatment (T1) and the threat (T2), social-information (T3), and the moral-appeal (T5) treatments, respectively. The figure shows that the threat increases registrations particularly in the first two weeks after sending the mailings (upper left panel); within this period, the other two treatments show hardly any difference to the baseline. The average response time in the threat treatments is 22.1 days (23.7 in the treatments without a threat). In the moral appeal, we observe an average of 24.4 days.

Recall that the mailings request recipients to respond within two weeks. We therefore estimated the treatments' impact on A-responses within this period (using estimations equivalent to those from section 5.2.1). The findings are similar to the results presented in the main text. The legal threat increases registrations within the first two weeks by nearly 20%. While the social information has no significant effect, we find a highly significant effect of the moral appeal. The treatment *reduces* response by 18% as compared to the neutrally framed mailing. As indicated in the lower panel of figure A.2, the gap to the

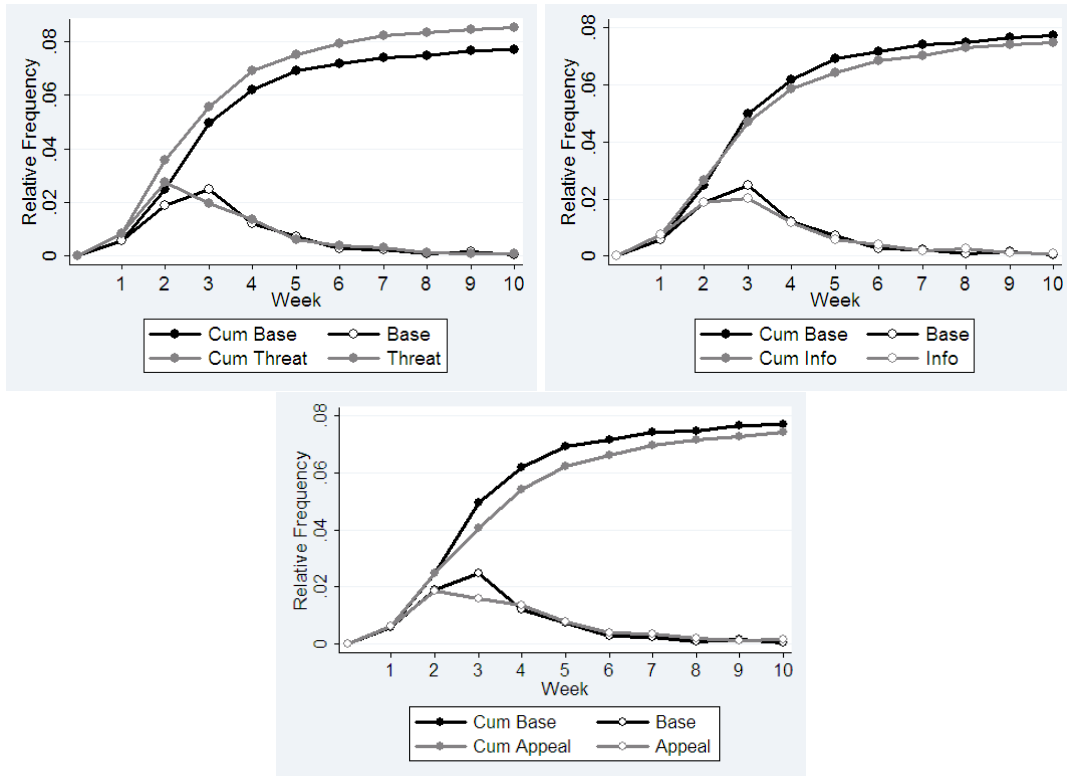


Figure A.2: Time pattern of A-Responses (registrations): base vs. legal threat (top left), base vs. social information (top right), and base vs. moral appeal

base treatment starts to decline with week four. Therefore, we do not find a significantly negative effect of the moral appeal when we focus on the response within 50 days (or more).

## Appendix IV: Type Heterogeneity

Let us denote the share of evaders (A-types) and law-abiding persons (B-types) in our sample by  $s^A$  and  $s^B$ , respectively. The fraction of those without TV or radio (C-types) is  $1 - s^A - s^B$ . Given the type specific response options discussed in Section 3.2, we know that response in category A and B reveals the respondents' type.<sup>34</sup> The observed frequency of registrations in treatment  $j \in \{1, \dots, 6\}$ ,  $p_j^A$ , can be expressed as

$$p_j^A = s_a r_j(A|a),$$

the registration frequency among A-types,  $r_j(A|a)$ , weighted with the share of evaders (A-types) in the sample. Equivalently, the observed frequency of update-responses in treatment  $j$  corresponds to

$$p_j^B = s_b r_j(B|b),$$

the frequency of B-responses among B-types,  $r_j(B|b)$ , weighted with the share of these types in the sample.

The precise type distribution is unobserved and cannot be identified from our data. Consequently, we cannot obtain point estimates of the type-specific treatment response,  $r_j(A|a)$  and  $r_j(B|b)$ . Due to randomization, however, the types' frequencies will not systematically differ between the treatments. This is why we have implicitly assumed  $s_{aj} = s_a$  and  $s_{bj} = s_b$  for all  $j$ . Any significant differences in our observed response rates  $p_j^A$  and  $p_j^B$  must therefore reflect differences in the treatments' impact on  $r_j(A|a)$  and  $r_j(B|b)$ , respectively.

The implications of the type heterogeneity for the quantitative interpretation of the treatment differences are straightforward. The data clearly reveal that  $s_a < 1$  and  $s_b < 1$ . The comparison of  $p_j^A$  (respectively  $p_j^B$ ) between treatments will therefore underestimate the differences in the type-specific response rates. To illustrate this point, consider the outcome of the experiment as illustrated in table 4. Comparing T1 and T2, we find that the legal threat raises the registration rate by about one percentage point, from  $p_1^A = 0.0862$  to  $p_2^A = 0.0967$ . On average, however, 28% of the individuals identify themselves as B-types (see table 4). It follows that  $s_a \leq 0.72$ , which implies that the difference between  $r_1^A$  and  $r_2^A$  will be at least 1.5 percentage points ( $(p_2^A - p_1^A) / 0.72$ ). Note also that the estimations presented in sections 5 neglects  $s_a < 1$  and  $s_b < 1$ . The estimated coefficients provide lower bounds for the treatment effects on the type-specific response.

---

<sup>34</sup>That is, only A-types can make an A-response (registration) and only B-types can produce a B-response (updating of information with a verified registration ID).

## Appendix V: Details on the Manipulation Check

The manipulation check was implemented November 24–28, 2008, using the online survey tool *unipark.de*. Survey participants were invited via email over the University of Innsbruck mailing list. To provide an incentive for participating, everybody who finished the survey had a 1/25 chance of winning an Amazon voucher worth €25. 4,165 individuals clicked on the link to the survey and 3,233 completed the survey within an average of 8 minutes.

The question regarding the perceived risk of an inspection was formulated as follows:

*“How large do you think is the risk – after having received this mailing [omitted for the control group (T0)] – that the person will receive a ‘visit’ by a licensing inspector within the next 4 weeks? Please tick a number on the scale between 0 (very unlikely) to 100 (very likely).”*

We also asked for the expected fines in case of a detection:

*“Assume that the person is indeed detected by a field inspector. Which consequences do you expect? He has to pay...”*

- (1) no fine.*
- (2) a fine of less than €100.*
- (3) a fine of €100 – 500.*
- (4) a fine of €500 – 1000.*
- (5) a fine of €1000 – 2000.*
- (6) a fine of €2000 – 4000.*
- (7) a fine of more than €4000.”*

Expectations regarding social sanctions were elicited in the following way:

*“An acquaintance of the person learns that he has not payed TV license fees for the past 6 months. [in the B-type vignette: ...that he has not informed FIS about his change in the address.] How will the acquaintance react?”*

- (1) will strongly approve the behavior, and support him not to register for license fees [not to update the information].*
- (2) will approve the behavior.*
- (3) will not react at all.*
- (4) will disapprove the behavior.*
- (5) will strongly disapprove the behavior, and cool down the contact to the person.*

# CESifo Working Paper Series

for full list see [www.cesifo-group.org/wp](http://www.cesifo-group.org/wp)

(address: Poschingerstr. 5, 81679 Munich, Germany, [office@cesifo.de](mailto:office@cesifo.de))

---

- 2725 Ernesto Reuben and Arno Riedl, Enforcement of Contribution Norms in Public Good Games with Heterogeneous Populations, July 2009
- 2726 Kurt Schmidheiny and Marius Brülhart, On the Equivalence of Location Choice Models: Conditional Logit, Nested Logit and Poisson, July 2009
- 2727 Bruno S. Frey, A Multiplicity of Approaches to Institutional Analysis. Applications to the Government and the Arts, July 2009
- 2728 Giovanni Villani, A Strategic R&D Investment with Flexible Development Time in Real Option Game Analysis, July 2009
- 2729 Luca Di Corato and Michele Moretto, Investing in Biogas: Timing, Technological Choice and the Value of Flexibility from Inputs Mix, July 2009
- 2730 Gilad D. Aharonovitz, Nathan Skuza and Faysal Fahs, Can Integrity Replace Institutions? Theory and Evidence, July 2009
- 2731 Michele Moretto and Sergio Vergalli, Managing Migration through Conflicting Policies: an Option-theory Perspective, July 2009
- 2732 Volker Nitsch, Fly or Cry: Is Airport Noise Costly?, July 2009
- 2733 Francesco Cinnirella and Joachim Winter, Size Matters! Body Height and Labor Market Discrimination: A Cross-European Analysis, July 2009
- 2734 Samuel Bowles and Sandra Polanía Reyes, Economic Incentives and Social Preferences: A Preference-based Lucas Critique of Public Policy, July 2009
- 2735 Gary Burtless, Lessons of the Financial Crisis for the Design of National Pension Systems, July 2009
- 2736 Helmuth Cremer, Firouz Gahvari and Pierre Pestieau, Fertility, Human Capital Accumulation, and the Pension System, July 2009
- 2737 Hans Jarle Kind and Frank Stähler, Market Shares in Two-Sided Media Industries, July 2009
- 2738 Pamela Campa, Alessandra Casarico and Paola Profeta, Gender Culture and Gender Gap in Employment, August 2009
- 2739 Sebastian Gechert, Supplementary Private Health Insurance in Selected Countries: Lessons for EU Governments?, August 2009

- 2740 Leif Danziger, Endogenous Monopsony and the Perverse Effect of the Minimum Wage in Small Firms, August 2009
- 2741 Yan Dong and John Whalley, A Third Benefit of Joint Non-OPEC Carbon Taxes: Transferring OPEC Monopoly Rent, August 2009
- 2742 Valentina Bosetti, Carlo Carraro and Massimo Tavoni, Climate Change Mitigation Strategies in Fast-Growing Countries: The Benefits of Early Action, August 2009
- 2743 Christina Felfe, The Willingness to Pay for Job Amenities: Evidence from Mothers' Return to Work, August 2009
- 2744 Jörg Franke, Christian Kanzow, Wolfgang Leininger and Alexandra Väh, Effort Maximization in Asymmetric N-Person Contest Games, August 2009
- 2745 Bruno S. Frey and Paolo Pamini, Making World Heritage Truly Global: The Culture Certificate Scheme, August 2009
- 2746 Frank N. Caliendo, Is Social Security behind the Collapse of Personal Saving?, August 2009
- 2747 Caterina Liesegang and Marco Runkel, Corporate Income Taxation of Multinationals and Fiscal Equalization, August 2009
- 2748 Chrysovalantou Milliou and Apostolis Pavlou, Upstream Horizontal Mergers and Efficiency Gains, August 2009
- 2749 Rüdiger Pethig and Christian Wittlich, Interaction of Carbon Reduction and Green Energy Promotion in a Small Fossil-Fuel Importing Economy, August 2009
- 2750 Kai Carstensen, Oliver Hülsewig and Timo Wollmershäuser, Monetary Policy Transmission and House Prices: European Cross-country Evidence, August 2009
- 2751 Olaf Posch, Explaining Output Volatility: The Case of Taxation, August 2009
- 2752 Beatrice Scheubel, Daniel Schunk and Joachim Winter, Don't Raise the Retirement Age! An Experiment on Opposition to Pension Reforms and East-West Differences in Germany, August 2009
- 2753 Daniel G. Arce, Dan Kovenock and Brian Roberson, Suicide Terrorism and the Weakest Link, August 2009
- 2754 Mario Larch and Wolfgang Lechthaler, Comparative Advantage and Skill-Specific Unemployment, August 2009
- 2755 Horst Raff and Nicolas Schmitt, Buyer Power in International Markets, August 2009
- 2756 Seppo Kari, Hanna Karikallio and Jukka Pirttilä, The Impact of Dividend Taxation on Dividends and Investment: New Evidence Based on a Natural Experiment, August 2009



- 2757 Mirco Tonin and Michael Vlassopoulos, Disentangling the Sources of Pro-social Behavior in the Workplace: A Field Experiment, August 2009
- 2758 Nicole Grunewald and Inmaculada Martínez-Zarzoso, Driving Factors of Carbon Dioxide Emissions and the Impact from Kyoto Protocol, August 2009
- 2759 Yu-Fu Chen and Michael Funke, Booms, Recessions and Financial Turmoil: A Fresh Look at Investment Decisions under Cyclical Uncertainty, August 2009
- 2760 Jan-Egbert Sturm and Jakob de Haan, Does Central Bank Communication really Lead to better Forecasts of Policy Decisions? New Evidence Based on a Taylor Rule Model for the ECB, August 2009
- 2761 Larry Karp, Sacrifice, Discounting and Climate Policy: Five Questions, August 2009
- 2762 Marianna Belloc and Samuel Bowles, International Trade, Factor Mobility and the Persistence of Cultural-Institutional Diversity, August 2009
- 2763 Charles Noussair and Fangfang Tan, Voting on Punishment Systems within a Heterogeneous Group, August 2009
- 2764 Birgit Bednar-Friedl and Karl Farmer, Internationally Coordinated Emission Permit Policies: An Option for Withdrawers from the Kyoto Protocol?, August 2009
- 2765 Pierre M. Picard and David E. Wildasin, Labor Market Pooling, Outsourcing and Labor Contracts, August 2009
- 2766 Stefan Voigt and Lorenz Blume, The Economic Effects of Federalism and Decentralization – A Cross-Country Assessment, August 2009
- 2767 David S. Jacks, Christopher M. Meissner and Dennis Novy, Trade Booms, Trade Busts, and Trade Costs, August 2009
- 2768 Mario Jametti and Thomas von Ungern-Sternberg, Hurricane Insurance in Florida, August 2009
- 2769 Alessandro Balestrino, Kind of Black: The Musicians' Labour Market in Italy, August 2009
- 2770 Yosr Abid Fourati and Cathal O'Donoghue, Eliciting Individual Preferences for Pension Reform, August 2009
- 2771 Christian Breuer and Chang Woon Nam, VAT on Intra-Community Trade and Bilateral Micro Revenue Clearing in the EU, August 2009
- 2772 Choudhry Tanveer Shehzad, Jakob De Haan and Bert Scholtens, Growth and Earnings Persistence in Banking Firms: A Dynamic Panel Investigation, August 2009
- 2773 Erdal Yalcin, Uncertain Productivity Growth and the Choice between FDI and Export, August 2009

- 2774 Klaus Abberger, Wolfgang Nierhaus and Shynar Shaikh, Findings of the Signal Approach for Financial Monitoring in Kazakhstan, September 2009
- 2775 Sascha O. Becker, Francesco Cinnirella and Ludger Woessmann, The Trade-off between Fertility and Education: Evidence from before the Demographic Transition, September 2009
- 2776 Thomas Aronsson and Erkki Koskela, Optimal Income Taxation, Outsourcing and Policy Cooperation in a Dynamic Economy, September 2009
- 2777 Joel Slemrod, Old George Orwell Got it Backward: Some Thoughts on Behavioral Tax Economics, September 2009
- 2778 Cagri Seda Kumru and Athanasios C. Thanopoulos, Social Security Reform and Temptation, September 2009
- 2779 Alessandro Buccioli and Roel M. W. J. Beetsma, Inter- and Intra-generational Consequences of Pension Buffer Policy under Demographic, Financial and Economic Shocks, September 2009
- 2780 Eduardo Strube and Marcelo Resende, Complementarity of Innovation Policies in the Brazilian Industry: An Econometric Study, September 2009
- 2781 Henry Tulkens and Vincent van Steenberghe, "Mitigation, Adaptation, Suffering": In Search of the Right Mix in the Face of Climate Change, September 2009
- 2782 Maria L. Loureiro, Anna Sanz-de-Galdeano and Daniela Vuri, Smoking Habits: Like Father, Like Son, Like Mother, Like Daughter, September 2009
- 2783 Momi Dahan, Tehila Kogut and Moshe Shalem, Do Economic Policymakers Practice what they Preach? The Case of Pension Decisions, September 2009
- 2784 Eytan Sheshinski, Uncertain Longevity and Investment in Education, September 2009
- 2785 Nannette Lindenberg and Frank Westermann, How Strong is the Case for Dollarization in Costa Rica? A Note on the Business Cycle Comovements with the United States, September 2009
- 2786 Leif Danziger, Noncompliance and the Effects of the Minimum Wage on Hours and Welfare in Competitive Labor Markets, September 2009
- 2787 Gerlinde Fellner, Rupert Sausgruber and Christian Traxler, Testing Enforcement Strategies in the Field: Legal Threat, Moral Appeal and Social Information, September 2009