ENFORCEMENT SPILLOVERS

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Abstract—This paper identifies spillovers from law enforcement. Our approach makes use of microdata on compliance with TV license fees that allow us to distinguish between households that were subject to enforcement and those that were not. Using snowfall as an instrument for local inspections, we find a striking response of households to increased enforcement in their vicinity: on average, three detections make one additional household comply with the law. As compliance rises significantly among those who had no exposure to field inspections, our findings establish a sizable externality in enforcement.

I. Introduction

OVER the past decade, a growing number of studies have provided evidence in support of Becker's (1968) model of crime. While several contributions have identified a deterrent impact of sanctions (Kessler & Levitt, 1999; Drago, Galbiati, & Vertova, 2009) and a negative effect of police on crime (Corman & Mocan, 2000; Levitt, 2002; Di Tella & Schargrodsky, 2004), the way enforcement actually works is less clear. In particular, we know little about the mechanisms through which enforcement ultimately affects individual behavior.

This paper builds on recent literature stressing the role of individual perceptions for understanding the impact of enforcement.[1] Relaxing Becker's assumption that the detection probability is common knowledge, Sah (1991) has analyzed the evolution of individual risk perceptions and the corresponding coevolution of crime. In his model, the agents update their beliefs about the detection risk based on information obtained from sampling in their vicinity. Beyond this model, information on enforcement activities might also shape perceptions about the strength of legal and social sanctions, such as stigmatization (Rasmusen, 1996). In either case, the updating of individual beliefs establishes a positive link between actual enforcement and the perceived costs of noncompliance.

Our study focuses on one important implication of such a linkage: enforcement spillovers. Such spillovers arise if detections of law violations have an impact on the perceptions of individuals who themselves have not been subject to any enforcement and if these individuals adjust their behavior in terms of compliance with a legal norm. To identify enforcement spillovers empirically, we exploit unique microdata on the enforcement of TV license fees and compliance behavior in Austrian households. In this setting, enforcement is targeted at individual households. Moreover, enforcement activities are not publicly observable: the presence of so-called licensing inspectors in a community is not announced, they are not uniformed, and they do not use any police-like cars. These distinctive features enable us to distinguish between households that have been subject to enforcement and those that have not. The empirical investigation then tracks the response of "untreated" households to the level of enforcement targeted at other households in a municipality. We find evidence of a behavioral response to increased enforcement, with compliance increasing significantly among untreated households. Hence, our results establish the presence of enforcement externalities.

While previous contributions have shown that individual risk perceptions are indeed responsive to personal experience with the criminal justice system (see Lochner, 2007, and the references therein), evidence on the link of enforcement, perceptions, and individual compliance behavior is extremely scarce. The only paper that touches on this issue is Alm, Jackson, and McKee (2009), who study tax compliance in a lab experiment. In a treatment without any official information on auditing risks, they find that income reporting is sensitive to information obtained from other subjects. To the best of our knowledge, this paper is the first to provide field evidence on externalities in law enforcement.

We estimate the enforcement spillover based on a complete record of household-level data on registrations for TV license fees and enforcement by inspectors for the period from November 2005 to March 2006. By aggregating to the level of municipalities, we construct a panel of monthly enforcement and registration rates that allows us to estimate the impact of enforcement on unsolicited registrations. To deal with the likely endogeneity of inspectors' enforcement activities, we follow an instrumental variables strategy (as in Levitt, 1997, 2002; Jacob, Lefgren, & Moretti, 2007). As instrumental variables, we use snowfall and the frequency of car accidents as descriptors of local weather and driving conditions. These instruments are motivated by a number of facts. First, licensing inspectors work under piece-rate contracts without being reimbursed for the time spent on traveling, and they independently choose when and where to become active. Furthermore, the time period under consideration was characterized by record levels of snowfall. Hence, for licensing inspectors, the decision where to go became an important one in terms of the opportunity costs of traveling. Finally,

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[1] See Lochner (2007), Hjalmarsson (2008), as well as the evidence discussed in Nagin (1998). A more general treatment of subjective expectations is provided by Manski (2004).

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Austria is an alpine country with strong regional variation in terms of accessibility during periods of heavy snowfall, providing most inspectors with an option to trim their traveling plans toward areas with reasonable driving conditions. It turns out that our instruments are strong predictors for actual enforcement levels. As conjectured, inspectors respond to their piece-rate incentives, avoiding regions that are more difficult to access if driving conditions are poor.

Exploiting the exogenous variation in enforcement induced by the instrumental variables, we find that the activity of licensing inspectors involves a significant externality: on average, three directly enforced registrations trigger one additional unsolicited registration. Given that the scope for the externality is limited by a relatively high overall level of compliance, the estimated effect is sizable. Since the implications of our findings for the design of optimal law enforcement policies in other domains (as, for instance, white-collar crime or tax evasion) depend on the information channels through which individuals learn about enforcement, we run several sensitivity tests to assess the relative importance of different channels. Our results suggest that the enforcement externality is mainly driven by interpersonal communication between treated and untreated households. We therefore conclude that enforcement spillovers might be important even in cases where enforcement activities are naturally unobservable to the general public.

The remainder of the paper is organized as follows. We next describe the institutional background. Section III motivates our empirical approach and discusses estimation methods and data. Section IV presents the results, and section V concludes.

II. Institutional Background

In most countries of the world, a significant share of broadcasting is provided by public broadcasters funded mostly by broadcasting license fees (Newcomb, 2004). A typical license fee system is in place in Austria. According to the Austrian Broadcasting Licence Fee Act (BLFA), every household must register its operational TVs and radios. Regardless of the number of household members, only one license fee has to be paid per household. Technically, however, public broadcasting programs can also be received without paying the annual fee, which ranged from 206 to 263 euros in 2005–2006.[2] License fees are managed by the Fee Information Service (FIS), a subsidiary of the Austrian Public Broadcasting Company. In 2005, 94% of all Austrian households were registered and paid a total of 650 million euros (0.3% of GDP).[3]

The number of registered households is in permanent flux. In principle, households can always deregister, stating that they no longer operate any broadcasting receiver. Those who do so, however, will be thoroughly checked by the FIS enforcement division. An easier way to escape fees emerges for moving households. Broadcasting registrations are attached to the place of residence, and the law requires moving households to update their registration details with FIS. Deregistering at the old place without registering at the new place gives households an opportunity to start evading without the need to state explicitly the absence of TV and radio receivers.

Table 1.—Number of New Registrations By Type, November 2005–March 2006

Type of Registration	Count	Percentage
Response to mailing	12,327	13.77
Field inspection	28,193	31.48
Unsolicited registration	49,028	54.75
Hard-copy	31,164	34.80
Online and phone	17,864	19.95
Total number of registrations	89,548	100.00

FIS tracks evasion of fees by comparing residence data with its own database. In principle, all residents who have not registered a TV or radio are treated as potential evaders. Of course, this mechanism does not perfectly identify those who do not comply with the BLFA, but it provides FIS with well-defined targets for specific enforcement measures: potential evaders are first addressed in a mailing, which asks them to clarify their status and register for license fees.[4] The data of those who do not respond to a mailing are then handed over to the FIS enforcement division. Members of this division, so-called licensing inspectors, enforce the BLFA in the field by personally approaching target households. FIS can impose a fine of up to 2,180 euros on detected evaders. In addition, a detected household eventually has to pay evaded fees for several past months. Note that the maximum level of fines is also communicated in the roughly 100,000 mailings that FIS sends to households every year. The availability of this penalty is reflected in a recent national survey, finding that 55% of Austrian households expect "severe" or "very severe" sanctions if they are found to be cheating on license fees.[5]

From November 2005 to March 2006, mailings resulted in 12,327 registrations, while field inspections contributed a total of 28,193 new registrations. However, as shown in table 1, the bulk of registrations came from unsolicited registrations. Such registrations originate from households that either send in a hard-copy registration form, which is available at municipal and post offices as well as at branches of banks. Alternatively, households may register online or by phone. In the five months covered by our data, hard-copy forms accounted for 31,164 unsolicited registrations, and 17,864 households registered online or by phone.

[2] License fees are typically paid by direct debit. There also exists a reduced fee that covers only radios (60–76 euros annually). All federal states have their own fees.

[3] Less than 1% of households hold neither a radio nor a TV (Statistics Austria, 2006). The figure of 94% therefore gives a reasonable proxy for the overall compliance rate.

[4] See Fellner, Sausgruber, and Traxler (forthcoming) for a field experiment testing different strategies in these mailings.

[5] For black labor market participation (skiving off work), the corresponding figure is 60% (38%). See Traxler and Winter (2009) for details.

For what follows, it will be useful to highlight several features of the FIS enforcement system. During the period under consideration, 207 inspectors were active, most of them working part time. As noted above, inspectors are not uniformed and do not use any official, police-like cars but their private vehicles. In contrast to police on the streets, the presence of licensing inspectors is therefore not visible to the general public. Each inspector is assigned a limited number of municipalities (eighteen on average, often overlapping with municipalities covered by other inspectors). They are paid according to a simple piece-rate contract (without any fixed income component), earning a premium of 20 euros for every new registration they deliver. Equipped with information on target households, inspectors independently choose their effort. In particular, they independently decide when and where to become active. There are no public announcements about which municipalities are going to be inspected or any coordination among inspectors. The sole FIS requirement is that inspectors cover every municipality within their domain at least once a year. However, because their travel expenses are not reimbursed, inspectors seem to have little incentive to regularly cover remote and sparsely populated areas.

Inspectors are credited for two types of registrations. First, they get a piece-rate for each registration they generate by face-to-face interaction with a target household. When nobody is at home when an inspector visits, he or she leaves an informational brochure with a registration form. If the form is returned later, FIS identifies the respective inspector from a code printed on the form and credits the inspector with the registration. Eight percent of all registrations credited with field inspectors emerge in this way.

III. Identifying Enforcement Spillovers

Our analysis focuses on the choice to register for license fees and the impact of enforcement on this decision. Before turning to the estimation approach and the data, we briefly discuss the relationship of registering, self-reporting, and enforcement.

A. *Registering, Self-Reporting, and Enforcement*

Consider an agent's decision to register or not to register in a simple one-period setting. If she registers, she has to pay license fees. If she does not register, she evades paying the fee. However, she might be detected by a licensing inspector and potentially incur fines, supplementary payments, and possibly social sanctions, such as stigmatization. Our empirical analysis focuses on agents who, given their assessment of the detection and sanction risk, initially prefer to evade the fees. How will local enforcement activities (that is, detection of evaders) affect the decision of these agents? There are at least three mechanisms through which enforcement can produce an externality on their inclination to register: via (a) the formation of the agents' risk perceptions, (b) an impact on a social norm for compliance, or (c) a preference for conformity.[6]

A formal analysis of the first mechanism is provided by Sah (1991), who studies a model of agents with subjective beliefs about the detection risk. Agents learn about the detection probability from sampling in their vicinity, with an increase in the number of detections within this sample driving up the perceived risk. In turn, the propensity to commit a crime decreases. Applying Sah's reasoning to our setup, we expect a rise in the number of detections within a municipality to cause an increase in local registrations: the more households are detected by licensing inspectors, the more likely an evader will be confronted with a higher number of detections among his neighbors, friends, and acquaintances. The perceived risk of detection rises, and so does the inclination to register. Enforcement can thus produce a spillover on compliance.[7]

The second mechanism that could mediate an enforcement spillover builds on social norms. Survey evidence suggests that a norm for license fee compliance is supported by social disapproval and other norm-enforcing sanctions (Traxler & Winter, 2009). The literature on social norms (Lindbeck, Nyberg, & Weibull, 1999) has argued that the severity of these sanctions increases the pervasiveness of norm compliance: the more people comply with a norm, the stronger are the potential social sanctions for an individual who is violating the norm. When agents learn about detections in our context, they might update their beliefs regarding the (local) level of norm compliance. Accordingly, they would adjust their expectation regarding the severity of social sanctions, for example, the possible stigmatization from a detection (Rasmusen, 1996). Again, this would increase the propensity of evaders to register in response to local enforcement activities.

Note that the enforcement spillover via mechanisms a and b stems from deterrence effects. Evaders learn about an increase in local detection and update their perceptions regarding the risk of formal or social sanctions. If noncomplying agents perceive a sufficiently high detection risk or sufficiently strong social sanctions, they will quit evading and register. Such registrations can be interpreted as an implicit form of self-reporting: whenever these agents register, they do so because they "fear more severe treatment if they do not" (Kaplow & Shavell, 1994, p. 583). In our case, they avoid fines, supplementary payments of fees, social sanctions, and an embarrassing interaction with a field inspector.

In addition to deterrence motives, one might think of social interaction effects as shaping the pattern of compliance

[6] One might further argue that detections work as reminders for unintentional evaders. Note that FIS runs intensive campaigns to support registration. For instance, during the time considered in this study, it placed about three spots a day in countrywide broadcast TV and radio channels. We therefore do not consider unintentional evasion a problem of any practical importance.

[7] Enforcement spillover might be further mediated by the updating of agents' perceptions of legal and economic sanctions. As our data cover information on detection, but not on fines, we follow Sah (1991) and focus on risk perceptions.

in a community (see, Glaeser, Sacerdote, & Scheinkman, 1996; Bayer, Hjalmarsson, & Pozen, 2009). For instance, some agents might have a preference for conformity, as in Bernheim (1994). As detections increase compliance, conformity could result in additional registrations if evaders want to imitate nonevaders once the latter group becomes sufficiently large. This provides for a possible social interaction mechanism, suggesting the presence of an enforcement spillover.

B. Empirical Approach

The primary aim of our empirical analysis is to identify a possible externality in enforcement: the causal effect of enforcement on self-reporting. In particular, we assess to what extent license fee evaders register their broadcasting receivers in response to an increased number of detections within their municipality.

We employ monthly data on enforcement and unsolicited registrations at the municipality level. Our two key variables are the effective enforcement rate ($Enforcement_{it}$) and the registration rate ($Registration_{it}$). The former measures monthly detections within a municipality—the number of registrations credited with field inspectors—per 1,000 households, whereas the registration rate is the corresponding rate of unsolicited registrations. The latter rate is taken to be determined by

$$Registration_{it} = \alpha + \beta Enforcement_{it} + \gamma Mobility_{it} + \theta_i + \eta_t + \epsilon_{it}, \qquad (1)$$

where the subscripts i and t denote municipalities and months, respectively. $Mobility_{it}$ captures individuals moving into or within the municipality relative to the total number of households,[8] α is a constant, θ_i and η_t account for unobserved municipality and period-specific effects, respectively, and ϵ_{it} is a residual. The parameter of interest, β, measures the impact of enforcement on registrations.[9]

Conceptually, our approach differs from other work on law enforcement as we study the link between enforcement and "quit decisions" of those who violate the law (the decisions of households that currently evade license fees and stop doing so) rather than the link with crime. By focusing on quit decisions, we avoid typical problems with the measurement of crime (MacDonald, 2002), as well as the possibility of crime displacement (Jacob et al., 2007; Yang, 2008): if a local increase in enforcement has a positive spillover on quit rates, this captures an increase in compliance that cannot be due to the relocation of unlawful activities to other places.

The estimation of the model parameters is complicated by the likely endogeneity of the enforcement rate. The problem is akin to the simultaneity problem in studies addressing the impact of police on crime (Di Tella & Schargrodsky, 2004; Levitt, 1997, 2002). In our context, the endogeneity comes from the fact that the level of enforcement is not randomly assigned to communities but most likely depends on the level of compliance. If field inspectors are more likely to be present in communities with lower compliance levels (which have lower rates of unsolicited registrations), we would expect the covariance between $Enforcement_{it}$ and the residual ϵ_{it} to be negative. As a consequence, the OLS estimate of β would be biased downward. Hence, similar to estimations of the deterrent impact of police on crime, we need to account for the simultaneity between enforcement and compliance to obtain consistent parameter estimates.

To cope with the endogeneity of the enforcement rate, we make use of instrumental variables (IVs). Our selection of IVs builds on the incentives that field inspectors face. Recall that inspectors independently decide on their effort level (hours worked per month, number of target households approached, and so on), as well as on which municipalities within their domain to inspect in a given month. Remember further that licensing inspectors are paid a constant piece rate for each registration they enforce, regardless of the time and effort they spend on driving to target households. Given these incentives, we presume the inspectors' overall effort as well as their choice of target households to be sensitive to the costs of traveling to different areas.

In search of instrumental variables, we draw on descriptors of local weather and driving conditions. These instruments appear promising for two reasons. First, our data cover the winter of 2005–2006, a long winter with extraordinarily heavy snowfall. In the southeastern regions of Austria, for instance, the snowfall during November 2005 alone amounted to more than 70% of the total snowfall in an average winter. In December, weather stations in the north and east registered record-breaking snow levels, while the average amount of fresh snow measured at 241 stations located over all Austria was 73 centimeters (with a median of 59 and a maximum of 235 centimeters). In January (February) 2006, the average depth of snow was 50% (37%) above the long-term average. In March, the corresponding figure was 120%. These extraordinary weather conditions are also reflected in the statistics on car accidents: the number of accidents that occurred on snow-covered streets was 90% above the level recorded for the mild winter of 2007.

Second, due to its location in the eastern Alps, Austria is a mountainous country with substantial variation in altitude. While the lowermost parts of Austria are around 100 meters above sea level, only 32% of the municipalities are located below 500 meters; 25% of all municipalities are located at altitudes higher than 675 meters and 10% at altitudes above 900 meters. In the lowermost parts of Austria, reasonable driving conditions are typically restored rather quickly, even after a heavy snowfall. In the more mountainous

[8] The reason for including mobility is that moving will generate registrations whenever households deregister at the old and reregister for license fees at the new place. Recall, however, that FIS asks households to just update their address in case of moving instead of de- and reregistering. If most households comply with this procedure, the effect of mobility on the registration rate should be modest.

[9] We do not consider asymmetries in the response to increased versus decreased levels of enforcement. For a discussion of asymmetric responses, see Mocan and Bali (2010).

TABLE 2.—DESCRIPTIVE STATISTICS

Variable	Mean	S. D.	Minimum	Maximum
Enforcement rate	1.13	5.10	0	130
Registration rate	2.60	2.43	0	46.4
Registration rate, hard-copy forms only	1.80	2.13	0	46.4
Snowfall	0.42	0.34	0.01	2.60
Accident rate	0.34	0.64	0	9.46
Mobility rate	15.5	13.4	0	548
Altitude	456	222	117	1,444
Number of households	2,359	7,278	500	115,245
Population density	360	1,622	5.15	25,629
Municipality size	39.5	38.0	1.09	285

Number of observations: 6375 ($N = 1275$, $T = 5$). All rates defined as total number of monthly incidents per 1,000 households. Snowfall and altitude are measured in meters, municipality size in square kilometers, population density in inhabitants per square kilometer. Data sources: FIS (enforcement and registrations); Institute for Meteorology and Geodynamics, Vienna (snowfall); National Council on Traffic Safety (accidents); Austrian Bureau of Statistics (mobility, altitude, number of households, municipality size, population density).

areas, however, conditions often remain critical for many days, in particular during periods of persistent snowfall. Driving to more remote alpine municipalities may require special equipment like snow chains.

Based on these considerations, we selected three variables as instruments for the enforcement rate: the amount of fresh snow (*Snowfall*), the interaction with the average altitude of a municipality (*Snowfall* × *Altitude*), and, as an additional measure capturing variation in driving conditions, the frequency of car accidents (*Accidents*). While the main effect of altitude is captured by the municipality fixed effects, the interaction of snowfall with the local altitude allows a given level of snowfall to have a different, presumably stronger, impact in more mountainous areas. The first-stage regression thus reads

$$\begin{aligned} Enforcement_{it} = {} & \vartheta + \lambda_1\, Snowfall_{it} \\ & + \lambda_2\, (Snowfall \times Altitude)_{it} \\ & + \lambda_3\, Accidents_{it} + \mu Mobility_{it} \\ & + \psi_i + \chi_t + \omega_{it}, \end{aligned} \tag{2}$$

where ψ_i and χ_t denote municipality and period effects, respectively, and ω_{it} is the first-stage residual.

C. Data

Our data set is based on a record of all license fee registrations by individual households between November 2005 and March 2006. FIS provided us with comprehensive microdata comprising roughly 90,000 observations. For each individual registration, we observe detailed information on the households' place of residence (complete street address and postal code), as well as the type of registration as listed in table 1. For unsolicited registrations, we know the date of the registration. For enforced registrations, we have information on the month in which the household was detected.

In a first step, we geocoded the microdata by matching geographic coordinates to each individual registration. We then assigned each observation to one of the 2,380 Austrian municipalities (larger cities are split into postal code areas) and aggregated the microdata to municipality-month cells, using the five-month period starting with November 2005. Effectively, this procedure provided us with a municipality-by-month panel data set with enforcement rates as well as registration rates for the different registration types. All of these rates give the number of incidents relative to the number of households within a municipality.

Our main results are based on a sample that essentially cuts the lower tail of the municipality distribution in terms of population size. First, we exclude municipalities with fewer than 500 households. The main reason for doing so is that these municipalities rarely see any enforcement. As a consequence, our key explanatory variable does not show significant variation in very small municipalities: almost 80% of the dropped municipalities do not contain a single enforced registration over all five months. Second, we exclude municipalities where we could not assign sufficiently precise geographic coordinates to the individual observations. This restriction ensures a high accuracy in the assignment of registrations and incidents of enforcement to municipalities as well as in measures of physical distance between detections and unsolicited registrations. Third, we eliminate municipalities without any location (bank or postal office, for example) offering hard-copy forms for the registration of broadcasting receivers. This removes municipalities where the lack of hard-copy registration forms could potentially induce a correlation between the registration rate and our weather-related instruments, which would question the validity of the exclusion restrictions (see discussion below). Effectively, the three restrictions left us with 1,275 municipalities observed over five months.[10]

Summary statistics on our key variables and several municipality characteristics are listed in table 2. On average, we observe 1.13 enforced registrations per 1,000 households.[11] With a mean registration rate of 2.6, unsolicited registrations are more frequent. Our empirical analysis also makes use of the rate of unsolicited registrations from hard-copy forms.

[10] Our parameter estimates are robust to relaxing these sample restrictions. In particular, the estimated effect of enforcement is virtually the same if we include all municipalities. However, using the full sample weakens the IVs in the first-stage regression. A similar finding is reported by Levitt (1997).

[11] The maximum of the enforcement rate comes from a municipality with 740 households where 13% of them were detected evading. In the same month, the rate of unsolicited registrations jumped to 1.5%—ten times the municipality's mean registration rate in the four remaining months.

TABLE 3.— FIRST-STAGE REGRESSION

Dependent variable: Enforcement rate	
Snowfall	3.761***
	(1.044)
Snowfall × Altitude	−0.711***
	(0.213)
Accidents	−0.135
	(0.089)
Sample size ($N \times T$)	6,375
F-statistic for excluded IVs	9.60

Standard errors (robust to heteroskedasticity and clustering on municipalities) in parentheses. F-statistic valid for i.i.d. errors. Additional regressors: mobility rate and a full series of period as well as municipality effects. Snowfall is measured in meters and altitude in 100 meters. Significance level: *** 1%.

With a mean of 1.8, this rate is considerably below the overall rate of unsolicited registrations.

Turning to our instruments, we use data from two additional sources. From the Institute for Meteorology and Geodynamics in Vienna, we obtained the monthly records of 241 weather stations located over all Austria. To each municipality, we assigned the records from the closest weather station (in terms of the Great Circle distance between each station and the centroid of the respective municipality). For the snowfall variable, we used the accumulated amount of fresh snow for each month. Together with the municipalities' altitude above sea level, this provides us with our first two instruments, *Snowfall* and *Snowfall × Altitude*. The National Council on Traffic Safety provided us with data on monthly accidents at the municipality level. We used the number of daytime accidents as a descriptor of driving conditions.[12] The corresponding accident rate (relative to 1,000 households) was our third instrument.

The measure for mobility was based on data provided by the Austrian Bureau of Statistics. We computed the mobility rate as the number of individuals who moved into or within a municipality relative to the number of households.[13] Finally, note that our sample covers municipalities from an altitude of 117 to more than 1,400 meters, with an average number of 2,359 households (median: 905), an average size of about 40 square kilometers, and a mean population density of 360 inhabitants per square kilometer (median: 96).

IV. Results

This section describes our empirical findings. We show that enforcement has a sizable spillover on registrations and provide evidence suggesting that the effect is driven by interpersonal communication rather than personal observation or experience.

A. First-Stage Regression

Before turning to the main results, we briefly discuss the performance of the IVs in the first-stage regression. Table 3 reports the fixed-effects regression of the enforcement rate on the instruments—snowfall, snowfall interacted with altitude, and the accident rate—as well as the mobility rate and a full series of period effects. We find that the snowfall-related instruments are strongly partially correlated with the enforcement rate. Interestingly, the coefficient of snowfall is positive, whereas the one for the interaction is negative. The first-stage coefficients imply that snowfall raises the enforcement rate in municipalities with an altitude below 530 meters but lowers enforcement in municipalities located higher. The accident rate as a direct measure of driving conditions shows the expected negative sign and is almost significant at the 10% level: for given weather conditions, inspectors are less active in municipalities where the accident rate is high.

Taken together, the results of the first-stage regression indicate that the behavior of the licensing inspectors is significantly affected by local weather and driving conditions. More precisely, the first-stage regression suggests that the activity of inspectors is driven by a sort of substitution effect: they seem to avoid driving to more mountainous areas in periods of heavy snowfall. Instead of reducing their overall effort, however, they just shift their focus and enforce more registrations in more easily accessible municipalities.[14] Note also that the F-statistic of 9.6 indicates that our IVs have substantial predictive power in the first-stage regression, making us confident that we have identified instrumental variables sufficiently strongly correlated with the enforcement rate to solve our identification problem.[15]

With respect to the validity of the instruments, one might be concerned about a direct impact of local weather conditions on households' registration behavior. Note, however, that we excluded municipalities without locations offering FIS hard-copy forms from our analysis, since an impact of weather conditions on registrations (if it is present) appears most likely to occur in such municipalities. Note further that online and phone registrations, in contrast to hard-copy registrations, should (if anything) be positively affected by harsh weather conditions. One might therefore conjecture that our IVs have a negative impact on the share of hard copy to all unsolicited registrations. To test this conjecture, we regressed the share of hard-copy registrations on our instruments. The results (presented in the appendix) indicate that all our IVs are far from being statistically significant predictors of the ratio of hard-copy registrations. Further evidence on the validity of the

[12] Driving under the influence of alcohol is the main cause for nighttime accidents. We focus on daytime accidents to avoid variation in the instrument that is driven by this type of accidents.

[13] The mobility rate attains unusually high values in some small communities in Austria's main tourist areas, where seasonal workers are officially registered as new residents. Excluding these or any other outliers does not change our results.

[14] This finding seems to be in line with the notion of income targeting; compare the discussion in Camerer et al. (1997) and Farber (2008).

[15] Using a Stock-Yogo test on weak instruments (Stock & Yogo, 2005), we can reject the null of the 2SLS bias exceeding 10% of the OLS bias. We also replicated all of our estimations using limited-information maximum likelihood (LIML) and its modification by Fuller (1977) (compare Andrews & Stock, 2005, and Hahn, Hausman, & Kuersteiner, 2004). All of these alternative estimators provided point estimators as well as standard errors that are almost identical to those reported below.

TABLE 4.— ENFORCEMENT EXTERNALITIES: BASELINE ESTIMATIONS

Dependent Variable: Registration Rate	(1) FE OLS	(2) FE IV
Enforcement rate	0.134***	0.362***
	(0.015)	(0.115)
Mobility rate	0.007***	0.003
	(0.003)	(0.003)
Sample size ($N \times T$)	6,375	6,375
R^2 (within)	0.142	–
Hansen test (p-value)	–	0.287

Dependent variable is overall registration rate (total number of unsolicited registrations per 1,000 households). Standard errors (robust to heteroskedasticity and clustering on municipalities) in parentheses. All estimations include a full series of period effects. Significance level: *** 1%.

exclusion restrictions (also reported in the appendix) is based on the fact that a substantial share of all municipalities does not see any enforcement. In this sample, enforcement cannot have any explanatory power, and we can run a reduced-form regression that tests for a direct impact of the IVs on registration behavior. Again, our IVs pass the test without any difficulty, making us confident that we have identified a set of valid instruments.

B. *Enforcement Spillovers*

Table 4 reports our main estimation results on the enforcement externality. Column (1) displays the fixed effects OLS estimation of equation (1), using the overall registration rate as the dependent variable and ignoring the likely endogeneity of enforcement. The coefficient of the enforcement rate is estimated to be 0.134, and it is highly significant. Moreover, we find a positive impact of mobility on registrations, but the effect is far from being economically significant. This is in line with our conjecture that most moving households just update the address in their FIS account instead of deregistering at the old and reregistering at the new place of residence. It is also worth noting that after netting out the municipality fixed effects, the OLS within-estimation explains 14.2% of the overall variation in unsolicited registrations.

As outlined above, we expect the OLS estimate of β to be biased downward. This expectation is confirmed if we turn to a fixed-effects IV estimation. Column (2) reports results derived from estimating equation (1) by two-stage least squares (2SLS) while instrumenting enforcement by snowfall, snowfall interacted with altitude, and the accident rate. The effect of enforcement is now estimated to be 0.361, suggesting that the bias in the OLS estimate is substantial. The effect for household mobility is not significantly different from 0.

The coefficient of the enforcement rate indicates an effect that is remarkably strong in relation to the direct effect of enforcement on registrations.[16] According to the estimates in column (2), one additional detection leads to about 0.36 additional unsolicited registrations. Hence, on average, three additional detections trigger more than one additional registration. Taking into account that the scope for any spillover of detections is limited by the high compliance rate—nationwide only about 6% of all households evade license fees—the estimated effect is sizable. To illustrate this point, consider a municipality with 1,000 households and an initial enforcement rate of 0. On average, there will be 60 households that have not registered for license fees. If now a field inspector enforces twenty registrations, we predict another seven unsolicited registrations (assuming a linear effect). In this case, the enforcement spillover would turn 17.5% of the remaining evaders into compliant households.

Regarding the figures from table 1, our estimates suggest that the roughly 30,000 enforced registrations during the period from November 2005 to March 2006 triggered about 10,000 unsolicited registrations (assuming again a linear relationship). The spillovers from local field inspections would thus account for about 20% of all unsolicited registrations.

C. *Channels of Information Transmission*

For many important domains of law enforcement, such as fighting white-collar crime or tax evasion, enforcement activities are typically unobservable to the public. In order to assess the validity of our findings for these domains, it is important to know the channels of information transmission underlying our findings. If the enforcement externality is driven by individuals' own experience and observation of inspections, one might question the external validity of our results. If, however, spillovers are mainly triggered by interpersonal communication, the evidence would carry strong implications for the optimal policy design in the noted areas of law enforcement. In the following, we discuss evidence that clearly points to the last case.

Recall that the presence of field inspectors is generally unobservable in our setup. One could nevertheless think of cases where we consider households as untreated although they actually were approached by field inspectors. Think of people who were absent while a field inspector was at their door. Since inspectors leave an information brochure together with a registration form at the door, registrations that are made using these forms are identified in our data as enforced registrations. Thus, they do not spoil the count of unsolicited registrations. However, households could ignore the registration form, pick up another registration form at the municipal or post office (which is basically identical to the form distributed by inspectors), and register with this form. Alternatively, they could register by phone or online. We consider the first case as extremely unlikely. In contrast, the scenario of phone or online registrations of households approached by inspectors seems to be quite realistic. Fortunately, our data allow us to identify registrations of this type. In particular, it is straightforward to compute the rate of unsolicited registrations made by hard-copy forms only (thereby

[16] Throughout the paper, we focus on the contemporary effect of enforcement. OLS estimations incorporating lagged values of enforcement indicate that this approach is justified: magnitude and significance of the impact of enforcement are hardly affected, whereas the lagged enforcement rate is neither economically nor statistically significant.

TABLE 5.—ENFORCEMENT EXTERNALITIES: CHANNELS OF INFORMATION TRANSMISSION

	Dependent variable: Hard-copy registration rate			
	(1)	(2)	(3)	(4)
Sample	More than 500 HHs	500–1,000 HHs	500–1,000 HHs, No Registrations in Same House	500–1,000 HHs, No Registrations in 50 m Circle
Enforcement rate	0.322***	0.287***	0.331**	0.358**
	(0.105)	(0.081)	(0.151)	(0.181)
Mobility rate	0.003	0.003	0.002	0.003
	(0.003)	(0.003)	(0.005)	(0.004)
Sample size ($N \times T$)	6,375	3,670	3,445	3,405
Hansen test (p-value)	0.200	0.945	0.898	0.884

Fixed-effects IV estimations with standard errors (robust to heteroskedasticity and clustering on municipalities) in parentheses. Dependent variable is hard-copy registration rate (number of hard-copy registrations per 1,000 households). All estimations include a full series of period effects. Significance levels: *** 1%, ** 5%. HH = households.

excluding those which were made by phone or online) and check the robustness of our findings.

Results based on the hard-copy registration rate are reported in table 5, column (1). As with the overall registration rate, we find a highly significant enforcement spillover. Restricting attention to registrations by hard-copy forms excludes roughly 40% of all unsolicited registrations and thereby reduces the registration rate from 2.6 to 1.8 (see table 2). Despite this fact, the point estimate for the enforcement spillover is only slightly below the one from table 4, column 2. Intuitively, when regressing the overall registration rate on enforcement, we potentially count some directly enforced registrations as unsolicited, while using the hard-copy registration rate ignores all true spillover registrations made online or by phone and thereby leads to underestimating the externality. Hence, the estimates from table 4, column 2 and table 5, column 1 should provided us with an upper and a lower bound on the spillover, respectively.[17] It is reassuring to see that the point estimates neatly fit this intuition, marking narrow bounds on the effect.

A scenario where individuals' own experience could contribute to the spillover is the direct observation of inspections by immediate neighbors. If bystanding evaders respond to their observation with a registration,[18] this would still reflect an externality in enforcement, but it would rest on a channel that might not be present in other areas of law enforcement. Again, the microstructure of our data allows us to study the extent to which personal observations contribute to the overall spillover. Knowing the exact location of all households with enforced or unsolicited registrations, we compute the distance of each household with an unsolicited registration to the closest household that was detected by a field inspector in the same month. In the sample used for estimations reported so far, 11.20% of all (hard-copy) registrations emerged within a 50 meter diameter circle around the location of households detected by inspectors. Moreover, 9.55% of all (hard-copy) registrations stem from multiunit dwellings (apartment blocks or multifamily houses) where a registration was enforced by a field inspector in the same month.

As a first step to check the sensitivity of our results with respect to close-by registrations, we restrict the sample to municipalities with 500 to 1,000 households while maintaining all other sample restrictions discussed above. Because these municipalities are relatively sparsely populated, we conjecture that immediate observations of neighbors should be less likely. In line with this conjecture, the frequency of hard-copy registrations within a 50-meter diameter circle (within the same multi-unit dwelling) of a detection drops to 2.47% (1.77%) in the restricted sample. Admittedly, the sample restriction not only reduces the scope for immediate observation of detections by neighbors, but might also lead to a sample with different patterns of social interaction in general and more frequent interpersonal communication in particular. Despite these possible differences, the 2SLS regression of hard-copy registrations on enforcement for the sample of small municipalities confirms our previous results. The estimates displayed in column (2) of table 5 indicate a spillover that is only slightly lower than the one found for the sample including municipalities above 500 households. Moreover, the effect is still estimated with good precision.[19]

To investigate further whether direct observation of enforcement affects our estimates, we exclude municipalities from the sample where at least one unsolicited registration was made in a multiunit dwelling with a field inspector being present in the same month. Finally, we also eliminate municipalities from the sample where registrations were made within a 50 meter diameter circle around enforced registrations. The corresponding 2SLS estimations are reported in columns 3 and 4 of table 5. Again, the externality of enforcement turns out to be robust. Note, however, that each additional sample restriction increases the standard errors

[17] To show formally that our approach gives us an upper- and a lower-bound estimate, it is sufficient to assume that registrations using hard-copy forms other than those distributed by inspectors are of no practical importance among households approached by an inspector and that the overall registration rate according to equation (1) is additive in all components (in hard-copy and in online or phone registrations).

[18] Note that such cases can emerge only if the fact that the bystander evades license fees is unknown to the field inspector. Otherwise the inspector would directly approach the bystander to enforce a registration.

[19] Note that we do not claim that the enforcement spillover is independent from average municipality characteristics (beyond the scope of observability) that changes with the sample restriction. However, we claim that the finding demonstrates that the spillover is present in municipalities with a very limited scope for direct observability of inspections.

of our point estimates, leaving us with an estimate for the enforcement externality significant at only the 5% level.[20]

To wrap up, our evidence documents the robustness of the enforcement spillover when we exclude cases that could potentially be driven by own experience and observation. The nature of the application precludes a conclusive evaluation of the relative importance of the different information channels through which the enforcement spillover might work. However, our data strongly suggest that interpersonal communication is the key factor shaping the spillover.[21]

V. Conclusion

This paper studies externalities in law enforcement. Using data on TV license fee registrations, we ask whether households that have not been subject to enforcement by licensing inspectors react to changes at the local level of enforcement. Since the actual level of enforcement is likely to be endogenous to registration behavior, our estimations rely on instrumental variables capturing variation in local weather and driving conditions.

We find a spillover from enforcement that is strong relative to the direct effect of enforcement. If enforcement increases such that three unregistered households are forced to register, this induces, on average, the registration of one additional household. This corroborates the existing evidence on the deterrent impact of police and constitutes an important result in its own right, since the specific features of our setting ensure that our findings cannot be attributed to crime displacement effects or erroneous measurement of crime rates.

In a next step, we assess the channels of information transmission underlying our results. It is self-evident that evaders who observe an inspection and respond by registering could, in principle, contribute to the enforcement spillover. However, since enforcement activities are generally not publicly observable, there seems to be little scope for direct observation in our context. In addition, the microstructure of our data allows us to identify specific cases where own experience could potentially induce spillover registrations. An extensive analysis of these cases suggests that the externality in enforcement is mainly driven by interpersonal communication between treated and untreated households. This relates our study to recent contributions demonstrating the importance of communication for decision making in other contexts as, for instance, job search (Ioannides & Loury, 2004) and investment decisions (Hong, Kubik, & Stein, 2005).

Our study carries important implications for the design of optimal enforcement policies. First, enforcement externalities clearly boost the marginal benefits from enforcement measures. The sizable effect found in our data indicates that it is crucial to consider these spillovers in cost-benefit evaluations and the analysis on the optimal level of law enforcement. Second, if externalities in enforcement are triggered by communication among neighbors, friends, and coworkers, a single detection of a law violation can produce a nonnegligible externality in basically all domains of law enforcement—even when enforcement activities and detections are unobservable to the general public.

Several points are left for future research. First, we do not identify the mechanism mediating the spillover. The externality might stem from individuals who respond to detections in their vicinity by updating perceptions regarding expected formal (fines, legal consequences) or informal (disapproval, stigmatization) sanctions. Next to this deterrence mechanism, the externality could, in principle, also be driven by preferences for conformity. Recall, however, that our analysis focuses on month-to-month changes in enforcement and compliance. Given that we identify the spillover from such short-term fluctuations, we do not consider it plausible that preferences for conformity with enforced compliance give rise to a significant externality on unsolicited registrations. While we consider it more reasonable to think of deterrence as the key mechanism behind the spillover, it is left to future research to disentangle empirically conformity from deterrence motives.

A further important aspect concerns the potential heterogeneity in the spillover. It would be interesting to explore how the transmission of information on enforcement depends on municipality or individual characteristics. One might conjecture that detections of individuals linked to large social networks should trigger a more pronounced externality. The identification of this heterogeneity is complicated by additional layers of endogeneity, as well as unobserved cross-sectional variation (for example, in terms of risk aversion or the level of noncompliance), which all interact in shaping enforcement spillovers.

[20] It is also worth noting that the performance of our IVs in the first-stage regression (not reported) is not adversely affected by the sample restriction. We obtain first-stage coefficients of the excluded IVs well in line with those shown in table 3. Moreover, with the sample restrictions, the F-statistic for the excluded IVs takes values between 7.1 and 11.8. Again all our findings are confirmed if we replicate them using limited-information maximum likelihood estimations (see note 15).

[21] Further evidence supporting this claim is discussed in an earlier version of this paper (see Rincke & Traxler, 2009).

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APPENDIX

Validity of Instruments

A concern regarding our IV strategy is that unsolicited registrations, in particular those made by hard-copy forms, could be directly influenced by local weather conditions. This would be the case if the willingness to go to one of the locations that provide registration forms were affected by snowfall. The forms are available at almost 6,400 municipal and post offices, as well as branches of banks. On average, there are 4.5 such locations per 1,000 households. Moreover, these locations are typically in central places within a community and in shopping areas. In general, it is hard to think of people who are, for an entire month, prevented from passing these areas due to weather conditions.

Let us nevertheless address the concern in more detail. Note first that we have excluded municipalities that do not have any location offering FIS forms from our sample of municipalities, because in such municipalities a dependence of hard-copy registrations on snowfall is most likely. A straightforward approach to test the validity of our IVs is to check for a potential impact of local weather conditions on registration behavior by means of regressions. Obviously, harsh weather conditions could decrease the willingness of households to register by hard-copy forms, as this implies some sort of outdoor mobility, suggesting that the share of hard-copy registrations among all unsolicited registrations should go down (and, vice versa, the share of online and phone registrations should go up) in months with heavy snowfalls.[22] We test this hypothesis by regressing the share of hard-copy registrations on our instruments. The results (see table A1) indicate that snowfall is far from being a statistically significant predictor of the share of hard-copy registrations, lending further support to the view that our identification relies on a set of valid instruments.

A second test exploits the fact that a substantial share of all municipalities does not see any enforcement over all five months considered. If we restrict our attention to the sample of these municipalities, enforcement cannot have any explanatory power. With no need to account for enforcement and, thus, to impose an exclusion restriction with respect to the IVs, we can estimate a reduced form,

$$\begin{aligned} Registration_{it} = \alpha &+ \rho_1\, Snowfall_{it} \\ &+ \rho_2\, (Snowfall \times Altitude)_{it} \\ &+ \rho_3\, Accidents_{it} + \gamma Mobility_{it} \\ &+ \theta_i + \eta_t + \epsilon_{it}. \end{aligned} \tag{A1}$$

In this regression, we have to evaluate the t-statistics of ρ_1, ρ_2, and ρ_3 to test for a direct impact of our IVs on registrations. Table A2 reports two different reduced-form regressions.

Column 1 uses the overall registration rate as the dependent variable and derives the direct effect of the IVs on registrations. The estimation is based on a balanced panel of municipalities with no enforcement over the whole period considered (note that this is a subsample of the communities used to derive our main results). It turns out that for all IVs—Snowfall, Snowfall × Altitude, and Accidents—the null of no impact on the registration rate cannot be rejected at any reasonable level of significance. Since a potential effect of weather and driving conditions on registrations is most likely for hard-copy registrations, we report in column (2) the reduced-form regression using the hard-copy registration rate (excluding phone and online registrations). The results closely match those obtained in the first column. Moreover, the IVs remain insignificant if we regress the registration rate on each instrument separately (results not reported). Hence, estimations of the reduced form for the subsample of municipalities with zero overall enforcement provide strong support for the validity of the exclusion restriction on our IVs.

[22] Telecommunications networks in Austria are typically unaffected by severe winter conditions. Hence, there is no reason to think that online and phone registrations will be negatively affected by snowfall.

TABLE A1.— FIXED EFFECTS ESTIMATION: SHARE OF HARD-COPY FORM REGISTRATIONS

	(1)	(2)	(3)	(4)	(5)
Snowfall	−0.024	−0.001	−0.025	–	−0.002
	(0.025)	(0.063)	(0.025)		(0.063)
Snowfall × Altitude	–	−0.004	–	–	−0.004
		(0.010)			(0.010)
Accidents	–	–	−0.004	−0.004	−0.004
			(0.009)	(0.009)	(0.009)
Sample size	5,412	5,412	5,412	5,412	5,412

Dependent variable is the share of hard-copy form registrations on all registrations. Municipality-month cells with a registration rate of 0 have been dropped. Standard errors (robust to heteroskedasticity and clustering on municipalities) in parentheses. All estimations include a full series of period effects.

TABLE A2.— REDUCED-FORM ESTIMATION FOR MUNICIPALITIES WITH ZERO ENFORCEMENT

Dependent Variable	(1) Overall Registration Rate	(2) Hard-Copy Registration Rate
Snowfall	0.224	0.159
	(0.661)	(0.629)
Snowfall × Altitude	−0.063	−0.069
	(0.112)	(0.109)
Accidents	−0.042	−0.035
	(0.054)	(0.046)
Mobility rate	0.002	0.004
	(0.006)	(0.005)
Sample size	2,690	2,690

Fixed effects estimation including a full series of period effects. Sample consists of balanced panel of municipalities with zero enforcement over all five months. Standard errors (robust to heteroskedasticity and clustering on municipalities) in parentheses.