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Compliance Behavior in Networks: Evidence from a Field Experiment

Francesco Drago[*], Friederike Mengel[**] and Christian Traxler[***]

Abstract
This paper studies the spread of compliance behavior in neighborhood networks involving over 500,000 households in Austria. We exploit random variation from a field experiment which varied the content of mailings sent to potential evaders of TV license fees. Our data reveal a strong treatment spillover: 'untreated' households, who were not part of the experimental sample, are more likely to switch from evasion to compliance in response to the mailings received by their network neighbors. We analyze the spillover within a model of communication in networks based on DeGroot (1974). Consistent with the model, we find that (i) the spillover increases with the treated households' eigenvector centrality and that (ii) local concentration of equally treated households produces a lower spillover. These findings carry important implications for enforcement policies.

Keywords: neighborhood networks; social learning; spillover; evasion; field experiment.

JEL Cassification: D8, H26, Z13

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1 Introduction

Research across various fields shows that social learning affects many important outcomes: the adoption of new products and technologies (Conley and Udry, 2010; Banerjee et al., 2013), political opinions (Baldassari and Bearman, 2007; Algan et al., 2015), formal and informal insurance (Ambrus et al., 2014; Cai et al., 2015) as well as financial decisions (Hong et al., 2005; Bursztyn et al., 2014) are all influenced by social learning (for a survey see Möbius and Rosenblat, 2014).

An important strand of research highlights the role of networks and their characteristics in shaping the outcomes of social learning (Jackson, 2008). Theoretical studies have analyzed the role of network-level measures (Jackson and Rogers, 2007; Golub and Jackson, 2012) and properties of individual nodes – such as centrality, clustering, or homophily – in the diffusion of information (Jackson et al., 2012; Currarini et al., 2009; DeMarzo et al., 2003; Banerjee et al., 2014; BenYishay and Mobarak, 2015). Empirically testing the predictions from these models is crucial as they have significant implications, e.g., for the optimal targeting of policy interventions (Alatas et al., 2012; Beaman et al., 2015). However, causal evidence on how individual network positions influence the spread of information is still scarce – primarily, because credible identification requires not only an experimental design but also a large data set which ideally covers many different networks.[1] Using detailed micro data on more than 500,000 Austrian households and a large-scale field experiment, the present paper exploits such a research design.

We analyze social learning in neighborhood networks in the context of legal compliance. Our analysis builds upon a randomized control trial that tested different strategies to enforce compliance with TV license fees. The experiment introduced exogenous variation in the treatment of 50,000 potential license fee evaders. In a *baseline* treatment, households received a letter that asked them why they were not paying fees. In a *threat* treatment, the letter communicated an imminent inspection and emphasized possible financial and legal consequences from non-compliance. Relative to a *control* group that did not receive any mailing, the two letter treatments significantly increased compliance. Mediated by a higher perceived risk, the threat triggered the largest effect (Fellner et al., 2013).

In this paper we study the treatments' impact on the compliance behavior of the *untreated* population. Since neither receiving a letter nor compliance is observable, behavior can only

[1] The existing evidence comes mostly from a development context. For example, in a pioneering study on microfinance in Indian villages, Banerjee et al. (2013) find that a measure of 'diffusion centrality' explains the importance of a node in information aggregation, albeit without experimental variation. The challenges in the identification of network effects are detailed in Jackson (2015).

spread via treatment-induced communication. We explore communication patterns in a large online-survey. The survey documents, among others, a high communication frequency among neighbors especially in rural areas, where communication intensity declines with the geographic distance to the next neighbor. The evidence further documents people's willingness to share information on TV license fee enforcement with their neighbors.

Using precise micro data and geo-coded information on the full population of small Austrian municipalities, we compute neighborhood networks based on geographic distance. Motivated by the survey evidence, we assume two households to be linked if they live within a given distance, for instance, 50 meters.[2] A network is composed by all households that are directly and indirectly linked. The networks thus reflect population density and the way settlements are spread over the municipalities' areas. Identification of the treatment effects on the untreated neighbors is achieved by the fact that, conditional on the number of households covered by the experiment, the treatment of these 'experimental households' varies exogenously. Consistent with this idea, we find no correlations of our treatment variables with observable network and municipality characteristics. The empirical design thus allows us to overcome the identification problems associated with social learning (Manski, 1993).

Our basic results document a pronounced spillover effect: untreated households, who were not part of the experimental sample, are more likely to switch from evasion to compliance in response to letters received by neighbors in the same network. Our estimates suggest that sending one additional threat [baseline] letter into a network increases each untreated evader's propensity to comply by 7 [5] percentage points. A back of the envelope calculation implies that 1,000 additional threat [baseline] mailings spread over 3,764 neighborhood networks would induce 230 [150] untreated households to start complying. While the comparison between direct and indirect treatment effects is complicated by different sample compositions, it is worth stressing that the overall spillover appears to be (at least) similar in magnitude to the direct treatment impact.

Several pieces of evidence indicate that neighborhood networks are crucial in shaping the social learning process behind the effect. For instance, if we increase the distance threshold defining a network link to above 500 meters or if we estimate at the level of (quite small) municipalities, the spillover effect vanishes. The same holds for placebo tests, which allocate households from the *same* municipality to randomly generated networks: again, we find a null effect. We also exploit the fact that many mailings were mistakenly targeted to households who

[2] We document that all our results are qualitatively robust to distance assumptions up to 500 meters.

were already complying. The analysis reveals larger spillovers when there is scope for a direct treatment effect on compliance (for a comparable result, see Banerjee et al., 2013). However, even letters sent to compliant households (who cannot stop evading by definition) trigger a small but significant spillover. Behavioral changes among targeted households are therefore not necessary to induce the indirect treatment effects.

To further investigate the role of networks in mediating the spillovers, we introduce a model of communication and learning in networks. Using an updating process in the spirit of DeGroot (1974) (as applied in, e.g, DeMarzo et al., 2003; Golub and Jackson, 2010, 2012), we analyze the treatments' impact on the spread of compliance. We focus on two testable predictions which have received significant attention in the literature. The first is that 'eigenvector centrality', a particular measure of a node's centrality within a network, determines its influence on social learning outcomes (DeMarzo et al., 2003). The second prediction suggests that a higher level of homophily, i.e., a higher likelihood of households to be linked to others who are similar to themselves, hampers social learning (Golub and Jackson, 2012). To test this prediction we explore the local concentration of treatments, which considers two neighbors as similar if they are in the same treatment. Conditional on two neighbors being in the experiment, random treatment assignment ensures that this layer of similarity varies exogenously. Just as other dimensions of homophily, local treatment concentration captures important dimensions of similarity of neighbors (e.g., their post-treatment beliefs and propensities to comply). Local concentration is also measured with an index commonly used to measure homophile. Crucially, however, unlike the inherently endogenous concept of homophily, local concentration is exogenous to other household characteristics. It therefore allows us to isolate the network effects of similarity in shaping the spillover.

Our data support the first prediction: the higher the eigenvector centrality of the injection points, i.e., the households targeted by letters, the larger the treatments' indirect effects on the untreated neighbors in the network.[3] From a policy perspective, this means that targeting a network's most 'central' households will maximize the intervention's indirect effects on compliance. In fact, our estimates suggest that the gains from targeting might be substantial, boosting spillovers by 25 to 50%.

[3]This effect is robust to the inclusion of the injection points' degree, clustering coefficient, betweenness centrality as well as the interactions of these characteristics with the share of treated households. None of these interactions are statistically significant nor are there any significant interactions with average network-level characteristics.

Consistent with the second prediction, we find that local treatment concentration within a network – similarity of neighbors in terms of receiving the same treatment – tends to decrease the spillover: the higher the local concentration of mailings in a network, the lower is their indirect effect on untreated households. Since, unlike typical homophily measures, our measure of local concentration is exogenous, we offer a first, causal evidence on the negative effects of network neighbors' similarity on social learning (Golub and Jackson, 2012). The finding has again a clear policy implication: mailing campaigns should avoid local concentration. To achieve a maximum spillover, mailings should be spread broadly within a given network.

Finally, we also show that the spillovers from baseline letters are limited to the first-order neighbors of treated households but reach further in the case of threat mailings, showing yet another dimension in which the threat treatment has a bigger impact. Overall, the findings from our refined analysis of network and node characteristics point out key properties of networks which mediate spillovers and further corroborate that the structure of our geographic networks is useful to capture patterns of social learning among neighbors, beyond what mere spatial distance can achieve.

This study contributes to several important strands of literature. We document that social learning can shape evasion and avoidance decisions not only within firm- (Pomeranz, 2015) or family- (Alstadsæter et al., 2014), but also in neighborhood networks. Our findings point out how geographic information, which is readily available (via address data) in many applications, could be incorporated in the design of interventions that account for enforcement spillovers (Rincke and Traxler, 2011; Kaufmann et al., 2012; Pomeranz, 2015). Broadly speaking, the targeting of audits or inspections should not only be based on individual-specific indicators, but also on an individual's position within a network. In doing so, geographic information appears particularly relevant when enforcement activities are 'geographically correlated', as it is the case with most door-to-door inspections at households or firms (e.g., Olken, 2007). Beyond enforcement, our results speak to a much broader set of applications that might exploit neighborhood communication, e.g., to effectively seed (fundraising) programs (Landry et al., 2006; Bruhin et al., 2014), to provide information on tax incentives (Chetty et al., 2013) or public health programs (Miguel and Kremer, 2004), or in marketing campaigns more generally (Goldenberg et al., 2009). Given the growing role played by geographic-proximity based social networks (such as Nextdoor.com), we expect neighborhood applications to gain further momentum in the future.

Our study also provides experimental evidence supporting the predictions from an important model of communication and learning in networks (DeMarzo et al., 2003; Golub and Jackson, 2012). To the best of our knowledge, we are the first to empirically identify a negative effect from a targeted node's homophily on a social learning outcome. Our evidence also highlights the importance of weak ties in passing information in the tradition of Granovetter (1973). While neighborhood networks represent, in Granovetter's terminology, weak rather than strong ties, we nevertheless identify sizeable treatment spillovers that derive from social learning among neighbors. However, our results also indicate that it is essential to consider details of geographic proximity and network structure in order to capture social interaction effects. In this respect, our paper differs from other studies that rely on agents' geographic proximity but do not exploit any geographic network structure (e.g., Bayer et al., 2008; Kuhn et al., 2011).

We also contribute to the growing literature on how networks shape agents' decisions in many important domains, such as insurance (Ambrus et al., 2014; Cai et al., 2015), job referrals (Beaman and Magruder, 2012; Dustmann et al., 2015) or microfinance (Banerjee et al., 2013), among others. Many of these studies elicit social networks via surveys (or in some cases by extracting information from social media). In this regard, our approach also presents a methodological innovation in that we rely on geographic neighborhood networks, which can be important vehicles of information transmission. Using geographic networks, we avoid the sampling issues described in Chandrasekhar and Lewis (2014). While geographic networks can be easily obtained and call for further research in other settings, the usefulness of a geographic approach will certainly depend on the type of communities (geographic proximity tends to be important in smaller municipalities but not in major cities) and the types of issues considered (whether the issue is a relevant topic of conversation among geographic neighbors).[4]

The remainder of the paper is organized as follows. Section 2 provides further information on the institutional background and the field experiment. Section 3 reports survey results on communication patterns among neighbors. Our main data are described in Section 4, and Section 5 presents our basic results. Section 6 introduces a simple theoretical model of communication and tests several comparative statics predictions from that model. Section 7 concludes.

[4]Geographic networks have been shown to matter in quite diverse domains such as households' energy consumption (Allcott, 2011), blood donations (Bruhin et al., 2014) or the diffusion of knowledge of the tax code (Chetty et al., 2013). Beaman et al. (2015), who study technology adoption, show that seeding based on geographic networks works fairly well. While seeding based on a complex model of elicited social networks increases spillovers, the geographic network approach is much cheaper and easier to implement.

2 Background of the Field Experiment

2.1 License Fees

Obligatory radio and television license fees are a common tool to fund public service broadcasters. A typical license fee system is operated by Fee Info Service (henceforth FIS), a subsidiary of the Austrian public broadcasting company. In Austria, the Broadcasting License Fee Act prescribes that all 'households' (including apartment sharing communities, etc.) owning a TV or a radio must register their broadcasting equipment with FIS. The authority then collects an annual license fee of roughly 230 euro per household.[5] Households face an incentive to evade the fee because public broadcasting programs can be received without paying.

FIS takes several actions to enforce compliance. Using official data from residents' registration offices, they match the universe of residents with data on those paying license fees. Taking into account that 99% of all Austrian households are equipped with a radio or a TV (ORF Medienforschung, 2006), each resident who is not paying fees is flagged as a potential evader (unless another household member has been identified as paying). Potential evaders are then contacted by mail and asked to clarify why they have not registered any broadcasting equipment. Data on those who do not respond are handed over to FIS' enforcement division. Members from this division personally approach households and make door-to-door inspections (see Rincke and Traxler, 2011). A detected evader is registered and typically has to pay the evaded fees for up to several past months. In addition, FIS can impose a fine of up to 2,180 euro. If someone does not comply with the payment duty, legal proceedings will be initiated.

The enforcement efforts are reflected in the compliance rate: in 2005, around 90% of all Austrian households had registered a broadcasting equipment and paid a total of 650 million euro (0.3% of GDP).[6] The number of registered households is in constant flux. New registrations emerge from mailing campaigns, door-to-door inspections as well as from unsolicited registrations. The latter originate from households who register, for instance, using a web form or by calling a hotline.[7]

[5]The fee is independent of the number of household members and varies between states. In 2005, the year covered by our data, the fee ranged between 206 and 263 euro.

[6]FIS' 'official' estimate for the compliance rate in 2005 was 94%. This estimate, however, may vary quite a bit depending on several assumptions (see Berger et al., 2015).

[7]Households can also deregister from license fees by stating that they no longer possess any broadcasting equipment. In practice, however, this is hardly observed as such households are thoroughly inspected by FIS' enforcement division.

2.2 Field Experiment

Fellner et al. (2013) tested different enforcement strategies in a field experiment. In cooperation with FIS they randomly assigned more than 50,000 potential evaders, who were selected following the procedures described above, to an untreated *control* group or to different mailing treatments. All mailings, which were sent out during September and October 2005, included a cover letter and a response form with a prepaid envelope. The experiment varied whether or not the cover letter included a threat. The cover letter in the *baseline* mailing treatment simply clarified the legal nature of the interaction and asked why there was no registered broadcasting equipment at this household. In the *threat* treatment, the letter included an additional paragraph which communicated a significant risk of detection and emphasized possible financial and legal consequences from non-compliance (see the Supplementary Appendix for the cover letters' text).

Fellner et al. (2013) found that the mailings had a significant impact on compliance. Most of the treatment responses occurred during the first weeks: within the first 50 days of the experiment, only 0.8% registered their broadcasting equipment in the control group. In the baseline mailing treatment, the fraction was 6.5pp higher. The threat treatment raised the registration rate by one additional percentage point. Beyond 50 days, there were no observable differences in registration rates. Complementary survey evidence suggested that, in comparison to the control group, all mailings had a strong positive impact on the expected detection risk. Relative to the baseline, the threat mailing further increased the expected sanction risk. This pattern is consistent with the larger effect of the threat treatment.

The present paper studies whether the treatments triggered any spillover effects on the untreated population that was not covered by the experiment. More specifically, we exploit the experimental variation to analyze if the mailing interventions affected untreated neighbors of those that were targeted. Given that neither the intervention itself (receiving a mailing[8]) nor the behavioral response (registering with FIS and starting to pay license fees) is observable, communication among neighbors is necessary for any spillover from treated households to untreated neighbors. In a first step, we will therefore discuss survey evidence on communication patterns.

[8]Similar as in other countries, the privacy of correspondence is a constitutional right in Austria. Violations are punished according to the penal code (§118).

3 Communication among Neighbors

To study communication between neighbors we ran a survey with a professional online survey provider. The company maintains a sample that is representative for Austria's adult population. From this pool we surveyed a subsample of almost 2,000 individuals. Participants were asked about the geographic distance to and the communication frequency with their first, second and third closest neighbors in terms of geographical (door-to-door) distance. We also elicited the relevance of TV license fees in the communication among neighbors. Details of the survey are relegated to Supplementary Appendix.

The survey's main results are the following: First, the survey indicates that the average communication intensity among neighbors is fairly high, averaging about 60% of the intensity of communication with a respondent's best friends from work/school. This finding is consistent with other evidence which suggests that neighbors form an important part of people's social capital.[9]

Second, the intensity of communication declines with geographic distance. This pattern, which is consistent with other evidence documenting that geographic proximity is an important determinant of social interaction (e.g., Marmaros and Sacerdote, 2006), is observed when we compare the communication with the first-, second- and third-closest neighbors: moving from closer to more distant neighbors, we see a strong drop in communication frequencies. A similar correlation is obtained when we explore variation in the door-to-door distance to the closest neighbor: the further away this neighbor, the lower is the reported communication frequency. (Below we will return to the fact that communication levels drop strongly once we move beyond a distance of 200 meters.)

Third, the survey indicates that the positive link between geographic proximity and communication intensity is systematically violated in larger, more urban municipalities. This is due to households living in apartment buildings. By definition, these households live very close to each other but, at the same time, communicate fairly infrequently with their neighbors.[10] This problem does not seem to occur in more rural regions: The survey data show that in small mu-

[9] The International Social Survey Programme's 2001 survey, for instance, shows that 11.2% of Austrians would turn to their neighbors as first or second choice to ask for help in case they had the flu and had to stay in bed for a few days. Similar rates are observed for other central and north European countries (e.g., Switzerland: 16.0%, Germany: 9.4%, Great Britain: 10.6%). For southern European countries (e.g., Italy: 4.7%) and the US (6.3%) the data document lower rates.

[10] It is worth noting that our evidence supports arguments made by Jacobs (1961), who criticized the urban planning policy of the 1950s/60s with its emphasis on large apartment blocks – precisely because it prevents many types of social interaction common in smaller municipalities.

nicipalities – where apartment buildings tend to be smaller and less anonymous – the 'closeness' of neighbors in apartment buildings is not aligned with lower communication frequencies (see Supplementary Appendix). As further discussed below, this motivates our focus on networks from small municipalities.

Fourth, concerning the content of communication among neighbors, we observe that, in general, TV license fees are a relatively uncommon topic (similar to neighbors talking about job offers or financial opportunities). However, the survey reveals that people are willing to pass on license fee related information to their neighbors, once some relevant news arrives: for a scenario where a household receives a FIS mailing which indicates a possible inspection, almost two out of three respondents say that they would share this information with their neighbor and 'warn' them. This seems reasonable, as inspections are strongly locally correlated. Overall, the evidence thus suggests that households are willing to initiate communication with their neighbors after receiving a mailing.

4 Data

To evaluate the impact of the experiment on the non-experimental population we build on several unique sets of data provided by FIS. The first data cover the universe of all Austrian households and includes precise address information from official residency data (zip code, street name and number, floor, apartment number) together with FIS' assessment of the households' compliance before the implementation of the field experiment. FIS derives this information – compliant or not (i.e., potentially evading) – from their data on all households paying license fees, data on past mailing campaigns and field inspections as well as data from the residents' registration office.

A second dataset covers the population from the field experiment (a subset of the first data) and indicates which households were in which treatment. The third dataset contains information on all incoming registrations – unsolicited registrations, responses to mailings, and detections in door-to-door inspections – after the experiment. Using these data we can observe behavioral changes in compliance. In particular, we can observe registrations among the population from the field experiment and unsolicited registrations among those that were not covered by the field experiment. Our analysis will focus on the latter population.

4.1 Sample

The survey documents that geographic proximity is positively correlated with communication frequencies among neighbors in *small* but not necessarily in large municipalities (see Section 3). In line with this finding, we focus on municipalities with less than 2,000 households (corresponding to a population size of approximately 5,000 – the cutoff for small municipalities in the survey). The restriction is further motivated by the fact that these jurisdictions are predominantly characterized by detached, single-family houses. Less than 20% [5%] of households in these municipalities live in buildings with three [ten] or more apartment units.[11] For the geographical network approach introduced below, this is an important attribute.

Full sample. The sample restriction leaves us with 2,112 municipalities (out of 2,380) with an average of 1,700 inhabitants and a population density of 99 inhabitants per square kilometer. We geocoded the location of each single household from these municipalities.[12] In a few cases we failed to assign sufficiently precise geographic coordinates; we then excluded the affected parish ('*Zählsprengel*'). With this procedure we arrive at a sample of 576,373 households. Among this sample, we distinguish three types: (I) potential evaders from the experimental sample, (II) potential evaders that were not covered by the experiment, and (III) compliant households (not part of the experiment).

Type I: Experimental participants. Our sample includes 23,626 households that were part of the field experiment. Summary statistics for these type I households, which will serve as 'injection points' in our analysis of indirect treatment effects, are provided in the first three columns of Table 1. The table splits the experimental sample according to the different treatment groups: 1,371 households were in the control group, 11,117 in the baseline mailing and 11,078 in the threat mailing treatment. Consistent with Fellner et al. (2013), we observe three patterns: (i) The observables are balanced across the treatments; this holds for age, gender, and several network characteristics introduced below.[13] (ii) The registration rates for the mailing treatments is significantly higher than in the untreated group. After the first 50 days of the experiment, 1.09% of all households in the control group registered for license fees. For the baseline mailing

[11] Among municipalities with 2,000 – 3,000 households, the share jumps to 39% [15%].

[12] The geocoding was implemented with software from a commercial provider of GIS tools (WIGeoGIS).

[13] Table 1 does not include any point estimates for the between treatment-group difference. However, as it is clear from the summary statistics, no variable turns out to be statistically different across the three groups. Note further that the high share of males is due to FIS' procedure treating male individuals as household heads.

treatment it was 7.01%. (iii) The threat mailing has a stronger effect: Table 1 indicates a registration rate of 7.65%.

Table 1 about here.

Type II: Potential evaders not covered by the experiment. In addition to the experimental participants, the sample includes 131,884 type II households who where classified as potential evaders at the time of the experiment. There are at least three reasons why these households were not part of the experimental sample. First, FIS excludes those who were 'unsuccessfully' contacted with mailings in the past from future mailing campaigns. Second, all households that first appeared in the official residents' registration record during the experiment's setup time could not be included in the experiment (e.g., recently formed households). Hence, some type II households might be long-time, others short-term evaders. Third, the classification of potential evaders is also based on information that was not available to FIS during the experiment's setup phase (see below).

It is worth noting that type I and II households together account for a fourth of our total sample. This high fraction, which is well above the overall rate of non-compliance, reflects the fact that FIS' method to identify potential evaders is imperfect and delivers many 'false positives' – i.e., compliant households that are wrongly flagged as evaders. This point is also reflected in Table 1 which shows that the ex-ante compliance rate (*before* the experimental intervention) among type I households was roughly 36%. A non-negligible fraction of the mailing targets could therefore not respond by switching from evasion to compliance – a fact that we will exploit in our analysis. Finally, note that the classification of potential evaders in the non-experimental sample makes use of ex-post information (e.g., from enforcement activities and behavioral responses after the experiment). This allows eliminating many false positives and yields a more accurate measurement of (non-)compliance. As a consequence, the ex-ante compliance rate in the type II sample should be considerably lower than in the type I sample.

4.2 Geographical Networks

Our analysis studies if potential evaders who were *not* covered by the experiment (type II households) start to comply with license fees in response to experimental interventions (the treatment of type I households) in their geographical network of neighbors. We therefore focus on networks that cover at least one type I and at least one type II household. We call these *relevant* networks. To derive geographical networks we first compute Euclidean distances between all households in each municipality. Whenever the distance between two households i and j is below an exogenous threshold z, we say there is a link between i and j. A network then consists of all households that are either directly or indirectly linked. Households that are directly linked to i are referred to as i's first-order neighbors (FONs), households one link further away as second order neighbors (SONs). Figure 1 illustrates this approach and shows how it produces disjoint networks.

Figure 1 about here.

A reasonable choice for the threshold z can be motivated by the survey evidence which suggests that communication frequencies with FONs decline sharply once the geographical distance exceeds 200 meters (see Supplementary Appendix, Figure S.2). This suggests $z \leq 200$ meters. Note further that larger thresholds leave us with fewer but larger networks. This point is illustrated in Table A.1 in the Appendix. The table displays the number of relevant networks as well as the number of different household types per networks for different thresholds z. For $z = 50$ we observe the largest number of relevant networks. Since this will facilitate any between-network analysis, we will use a threshold of 50 meters as a benchmark for our analysis. To assess the robustness of our findings with respect to z, we rerun all our main estimations for networks based on thresholds between 25 and 2000 meters.

With a 50-meter threshold we arrive at 3,764 relevant networks that were covered by the experiment. The networks come from 771 different municipalities and include about 68,000 households (type I, II and III; see also Table A.1). Among these, there are 14,787 type II households. Summary statistics for this group are provided in Panel A of Table 2. The variable degree shows that the median [mean] type II household is linked to 6 [11] FONs that live within 50 meters distance. Average Eigenvector centrality is 0.19 and the mean clustering coefficient equals 0.73, indicating that about three out of four FONs are directly linked among themselves.[14] The table further shows that 54% [12%] of type II households have at least one FON [SON, second-order neighbor] that is covered by the experiment. The mean Euclidean distance to the closest type I household is 97 meters. Finally, the variable registration rate indicates that 8% of type II households unsolicitedly registered within 50 days after the experiment. Note that this number is twice as high as the average registration rate among *all* non-experimental potential

[14]The different network characteristics are discussed in detail below as well as in Appendix B.2.

evaders (i.e., type II households inside and outside of networks covered by the experiment; see Table 1, column (4)).[15] Below we will show that the higher registration rate can be explained by the presence of spillover effects from experimental to non-experimental households in these networks.

Table 2 about here.

Panel B reports descriptive statistics at the network level. The network size, in the following denoted by N_k, has a median [mean] of 6 [18] households. For each network k, we computed variables that measure the treatment coverage: $Total_k$ captures the rate of other households in the experiment sample divided by $N_k - 1$. Similarly, $Base_k$, $Threat_k$ and $Control_k$ indicate the ratios of other households targeted with a baseline, a threat mailing and untreated experimental households, respectively. Using $(N_k - 1)$ as denominator assures that the treatment rates vary between zero and one.[16] Table 2 shows that, from the perspective of a type II household in an average network, 45% of the other households in a network were covered by the experiment; 21, 22 and 2% of the other network members were in the baseline, threat or control treatment, respectively.[17] The table further reports several measures of local treatment concentration and 'homophily' measures which are further discussed in Section 6.

Panel C of Table 2 presents summary statistics for census data at the municipality level. An average municipality (hosting a relevant network) is populated by 1,790 inhabitants with a mean labor income (wages and salaries) of 27,250 euro. 82% households live in single-family and two-family homes. The household heads are on average 54 years old. The fraction of non-Austrians citizens is low (5%) and a large majority of the population is Catholic (88%). We also observe a high voter turnout at the 2006 national elections (77% on average).

5 Spillover Effects

5.1 Identifying Indirect Effects from the Experiment

Instead of studying the treatment responses of treated (type I) households, we focus on compliance responses among the non-experimental population. We want to identify if and how a

[15] One cannot directly compare these registration rates to those observed among type I households. As pointed out in Section 4.1, the latter sample contains a high fraction of households who were already complying before the experiment.

[16] Below we will focus on the responses of type II households. Computing treatment rates relative to N_k would impose an upper bound (at $(N_k - 1)/N_k$) which mechanically varies with the network size.

[17] The high treatment ratios reflect our focus on (relevant) networks with at least one experimental household.

type II household's probability to register for license fees changes in response to the experimental interventions. To estimate these spillover effects we consider the following model:

$$y_{ik} = \alpha + \beta_0 Total_k + \beta_1 Base_k + \beta_2 Threat_k + \epsilon_{ik}, \tag{1}$$

where y_{ik} indicates if a type II household i from network k starts to comply with license fees within 50 days after the experiment.[18] The regressors measure the treatment rates at the network level, i.e., the fraction of other households in the network that were part of the experimental sample ($Total_k$), in the baseline ($Base_k$) or in the threat treatment ($Threat_k$), with $Total_k = Control_k + Base_k + Threat_k$ (see Section 4.2).[19]

The coefficients of interest, β_1 and β_2, measure the spillover effects on type II households' propensity to start paying fees in response to a *cet. par.* increase in the network's rate of baseline and threat treatments, i.e., keeping constant $Total_k$. Put differently, these are the effects from moving experimental households from the control group to one of the mailing treatments. Alternatively, the effect of sending more mailings (while keeping constant the number of households in the control group) is given by $\beta_0 + \beta_1$ [$\beta_0 + \beta_2$] for the baseline [threat] treatment. The coefficient β_0 captures possible correlations between the experimental coverage and the average probability of observing unsolicited registrations in a network; α depicts the baseline rate at which type II households start to comply with license fees.

Identification. For a given experimental coverage, identification of β_1 and β_2 is obtained from network-level variation in the treatment rates. The identifying assumption is that, conditional on $Total_k$, variation in $Base_k$ and $Threat_k$ is as good as random: between networks with the same coverage ($Total_k$), the assignment of the experimental households to the different treatments varies exogenously. This is equivalent to a conditional independence assumption. Networks that differ in experimental coverage are allowed to be different in unobservables which might affect the propensity to comply (as reflected in β_0). Controlling for $Total_k$, however, the variation in the treatment ratios should be orthogonal to unobservables because the experiment randomly assigned households to different treatments. The basic idea is simple: with random treatment assignment in a given sample (i.e., among type I households), treatments should vary randomly

[18] The dummy only captures unsolicited registrations. Enforced registrations do not enter y_{ik}.

[19] Below we show that specification (1) can be interpreted within a model of communication and learning in networks (see Section 6). Alternative specifications in levels yield almost identical results as those reported below. However, estimations in levels turn out to be more sensitive to outliers related to a few very large networks.

in its sub-samples identified by our networks.[20] The identifying assumption is credible because – as underpinned by the balancing test in Fellner et al. (2013) and our Table 1 – randomization in the experiment was successful.

To provide network-level evidence in support of our identifying assumption we estimate models of the following structure: $Base_k = \mu_0^{base} + \mu_1^{base} Total_k + \mu_2^{base} x_k + \epsilon_k^{base}$ and $Threat_k = \mu_0^{threat} + \mu_1^{threat} Total_k + \mu_2^{threat} x_k + \epsilon_k^{threat}$, where x_k is an observable characteristic that varies at the network level. Our conditional independence assumption implies that, controlling for $Total_k$, we should not find any correlation between observable network characteristics and our key regressors: neither μ_2^{base} nor μ_2^{threat} should be statistically different from zero. Table 3 presents the results. Each entry in the table reports an estimated μ_2-coefficient for a different regressor x_{ik} from a separate regression. For columns (1) and (3), each estimation is based on the sample of 3,764 relevant networks. None of the coefficients is statistically significant. In columns (2) and (4), we repeat the exercise using different municipality characteristics. The municipality-level estimates again yield no significant coefficients. The results thus support our identifying assumption suggesting that, after controlling for experimental coverage, there is no selection on observables.

Table 3 about here.

5.2 Basic Results

Using a linear probability model we estimate model (1) for all potential evaders from the non-experimental population (type II households) in relevant networks with $Total_k > 0$.[21] The results, together with standard errors clustered at the network level, are reported in column (1) of Table 4. The coefficients of interest are both positive and precisely estimated. The estimates hardly change when we control for the networks' experimental coverage linearly (as in column 1) or non-parametrically, i.e., by including a set of 471 dummies for each value of $Total_k$ (column 2). The point estimates imply that a one percentage-point increase in the rate of the baseline [threat] treatment increases the likelihood that an untreated potential evader registers by 0.25pp [0.35pp], respectively. An F-test on the equality of β_1 and β_2 rejects the null that the two effects are equal. The coefficient on $Total_k$ is small and only weakly significant. The negative sign indicates that a

[20] It is worth noting that the partitioning of households into different networks is driven by the way settlements are spread over the municipalities' area. Effectively, the variation in our treatment variables thus comes from the experiment in combination with characteristics such as population density and 'gaps' between settlements.

[21] Including networks with $Total_k = 0$ does not change the results since the other regressors in (1) also equal zero for networks with $Total_k = 0$.

higher experimental coverage of the network is correlated with a lower probability of unsolicited registration among type II households.

To illustrate the size of the effect, consider the thought experiment where we move one experimental household from the control to the threat treatment. For a median network with $N = 6$, the additional mailing implies a 20pp increase in the threat treatment rate ($\frac{1}{6-1} = 0.2$). Our estimates imply that the additional threat mailing would increase the type II households' probability to register by 7pp ($0.2 \times 0.35 = 0.07$). On average, there will be just one type II household in such a network (20% of $N - 1 = 5$; see Table A.1). We would therefore expect a total spillover of 0.07×1 unsolicited registrations for license fees.[22] Keeping constant the experimental coverage, one additional threat [baseline] mailing thus increases the probability of observing one additional registration among the untreated evaders in the network by 7pp [5pp]. While the comparison of registration rates between the treated and the untreated sample is problematic (see footnote 15), it is nevertheless worth noting that the total spillover effect seems to be of a similar magnitude as the direct treatment effects on type I households (6.0pp and 6.6pp for the baseline and threat treatment, respectively; see Table 1).

Table 4 about here.

Having detected a significant spillover from the experiment onto the non-experimental population, it is important to note that we would miss these indirect treatment effects if we estimated model (1) at the municipality rather than the network level. This point is documented in column (3) of Table 4. For this regression we have assigned the sample from columns (1) and (2) (i.e., all potential evaders from geographic networks with $Total_k > 0$) into one network per municipality. In line with this alternative network definition, we accordingly computed new treatment rates for all 771 municipalities. Despite the fact that these municipalities are still fairly small observational units (with a population of 1,790 individuals on average), we obtain estimates for β_1 and β_2 that are very small and statistically insignificant.

In column (4) we enlarge the sample and include all type II households from all municipalities with at least one experimental household. In this way, we obtain a sample of 62,064 non-experimental potential evaders spread over 982 municipalities. Despite the larger sample, we still obtain imprecisely estimated coefficients that are close to zero. Hence, the municipality

[22] For larger networks, the effect of one additional mailing on the treatment rates would of be smaller but the spillovers would spread to a larger number of potential evaders. It is straightforward to show that the model from (1) implies that the total expected spillover from sending one additional mailing into a network is independent of the network size.

seems to be an observational unit that does not allow us to capture the indirect treatment effects that we detected at the level of geographical networks.

To sum up, Table 4 provides two first insights. First, there is a sizable spillover from the experimental treatments on households that were not part of the experiment. Second, analyzing this effect at the level of geographic networks (rather than municipalities) is important in order to detect the spillover. Taken together, this suggests that geographical networks may be crucial in shaping relevant parameters that affect compliance decisions. We will further explore this case below. Before, we will discuss the robustness and some refinements of the spillover effect.

5.3 Robustness and Refinements

Alternative specifications. Table 5 reports the results from several exercises which assess the robustness of our basic result. We first estimate model (1) using a probit model. Column (1) reports the marginal effects, computed at the mean of the independent variables. The spillover effects are similar to those indicated by the LPM estimates and the effect size differs again significantly between the two mailing treatments. In column (2) we return to the LPM estimations and augment the basic specification by adding fixed effects at the municipality level. As expected, this leaves our results unchanged.

Table 5 about here

Next we consider possible interferences with field inspections. To account for spillovers from local enforcement activities (see Rincke and Traxler, 2011), the specification from column (3) controls for the enforcement rate at the network level. Consistent with Rincke and Traxler (2011), enforcement has a positive effect on the propensity to register. However, the point estimates for our coefficients of interest are essentially identical to those from column (1) in Table 4. In an additional step, we run our basic model excluding all networks in which at least one household was targeted by a field inspector. This drops roughly 100 networks from the sample.[23] The results reported in column (4) show that the estimated coefficients remain again unchanged. Finally, we test for potentially heterogenous spillover effects with respect to municipality characteristics. The analysis reveals significant interaction effects for two variables: the dwelling structure and the voter turnout. The magnitude of the spillover increases with the

[23]The number of non-experimental households in the sample drops by a larger share as the excluded networks are the larger ones. This is due to the fact that, *cet. par.*, the probability to have at least one household targeted by a field inspector is increasing with the network size.

fraction of people living in single- and two-family dwellings (as compared to multi-family homes) as well as with the turnout (see Table A.2 in the Appendix). The latter interaction might be related to a higher social capital.

Different network assumptions. To understand the sensitivity of our findings with respect to z, we computed geographical networks based on distance thresholds that vary between 25 and 2000 meters.[24] We then replicate our estimates for the different samples of relevant networks. Panel A in Table 6 reports the results for the basic model from (1) for different thresholds z. The estimated coefficients turn out to be fairly stable for $z < 500$ meters. For larger values of z, the coefficient on the baseline treatment starts to decline whereas the one on the threat treatment remains large, however, with large standard errors.

In comparing the different estimation results, one has to take into account that a change in z varies the number of relevant networks, the average network size, as well as the number of type II households (see Table A.1). A first attempt to provide a meaningful comparison across samples is provided in Panel B of Table 6. Here we normalize the coefficients on the two treatment rates relative to the median network size (minus one). With this normalization, we get the effect from sending *one* additional mailing to *each* relevant network on the register probability of a type II household (in a median-sized network). Panel B shows that the effect from one mailing monotonically declines with z. Given the results from Panel A, this decline is induced by the fact that the median network size increases monotonically with z.

Table 6 about here

Panel C reports the results from a different thought experiment, which considers sending a fixed amount of 1,000 additional baseline or threat mailings to relevant networks. Based on our estimates and the network properties we then compute the total number of additional registrations that we expect to be induced by the spillover effects from these mailings.[25] For $z = 50$, for instance, the number of expected registrations that emerge from the indirect treatment effect is 150 [232] for 1,000 additional baseline [threat] mailings, respectively. Panel C indicates that the overall spillover starts to decline with $z > 100$. This observation fits the survey evidence which showed that communication frequencies among FONs sharply declines in this range.

[24] Employing within-municipality distance matrices, we exclude links between networks from different municipalities. This restriction becomes relevant for $z \geq 500$ but affects only a small part of the sample.

[25] For the baseline mailings, this number is computed as follows: Number of Observations $\times \frac{1{,}000}{\text{Number of Networks}} \times \frac{\hat{\beta}_0+\hat{\beta}_1}{N-1}$. Note that the effect is weighted with the total number of observations to account for the fact that the spillover applies to all type II households in the relevant networks.

Permutation test on networks. In principle, the results from Table 4 could be interpreted in support of the idea that geographical networks of neighbors are a key unit for the information transmission which shapes the spillover effects. A concern with this interpretation is that we might simply have too little variation to detect any spillover when we estimate the regressions at the municipality level. To address this concern and to provide further evidence that geographical networks are crucial in determining the spillovers, we perform the following permutation test.

Within each of the 771 municipalities covered by the sample from our main specification (Table 4, column 1), we randomly allocate all (type I, II and III) households into networks of size $N = 10$.[26,27] With this procedure, all households remain in their true municipalities but they are randomly grouped in different networks, independently of their geographic location within the municipality. For such a randomly generated network we then compute our regressors and estimate model (1) on the sample from Table 4 (the 14,767 type II households). The results from 1,000 iterations of this exercise are illustrated in Figure 2.

Figure 2 about here.

The figure displays the cumulative distribution functions of the estimated coefficients for the baseline (β_1, left panel) and the threat mailing (β_2, right panel) as well as the true point estimates. In less than the 5% of the cases (4.00% for β_1; 1.60% for β_2) the coefficients from the permutation test are larger than the estimates from Table 4. This suggests that the results from Table 4 are not simply driven by partitioning municipalities into smaller units. Instead, the networks based on geographical distance seem to pick up a systematic spillover effect that is shaped by interaction within these networks.

Spillovers within the experimental sample. Given our main results from above, it seems natural to ask whether there are also spillovers *within* the experimental sample. If a type I household's treatment response depended on the treatment of other households in the network, this would imply a violation of the stable unit treatment value assumption (SUTVA; see Imbens and Wooldridge, 2009) for evaluating the direct effect of the experiment. To explore this case, we focus on type I households and analyze whether the behavior of a treated household depends, in addition to its own treatment, on the treatment rates in its network. Our analysis does not yield any compelling evidence that treatment responses of type I households are influenced by

[26]For a municipality with, say, 1,017 households, we would randomly form 100 networks with 10 and one network with the remaining 17 households.

[27]We obtain very similar results when we use $N = 5$ (close to the median network size, see Table 2) or $N = 15$ (close to the 3rd quartile).

the treatment of their neighbors: controlling for the baseline and threat mailing rates does not alter the estimates for the direct treatment effects of the mailings. Results are presented in Table A.3 in the Appendix and suggest that, for the experimental sample, the direct treatment effect dominates any indirect effect from the experiment.

5.4 Linking Direct and Indirect Treatment Effects

Receiving a mailing or starting to comply with licence fees is unobservable to other network members. The spillover effect must therefore stem from the dispersion of information via communication. Understanding the underlying patterns of communication – i.e., who passes on which information – is important for optimal policy design that accounts for indirect treatment effects (e.g. Banerjee et al., 2013; Beaman et al., 2015).

In our context one could imagine that type I households who do not start to comply in response to a mailing might not talk about their treatment. Alternatively, if they communicated with their neighbors, the information content might not trigger any effect on the neighbors' compliance. The latter prediction can be derived from theories of conformity, imitation and social norms (e.g. Akerlof, 1980; Bernheim, 1994) that all focus on behavioral interdependencies: a change in the compliance of the treated household would be necessary to induce any indirect treatment effect. In this case, we should not find any spillover from mailings that did not have any direct treatment effect on compliance. If the indirect treatment effects were indeed contingent on compliance responses of treated households, the spillover should be closely aligned with the direct treatment effects. Our data do not support this case. Table 1 shows that the threat produces roughly 10% more registrations than the baseline mailing (7.65/7.01). In contrast, the estimates from Table 4 indicate that the indirect effect from a threat mailing is 40% larger than the one from a baseline mailing ($\beta_2/\beta_1 = 0.35/0.25$).

To directly assess the role of direct treatment responses for the emergence of spillovers we make use of the fact that more than a third of the type I households were actually complying with license fees at the time of the experiment (see Section 4.1). Hence, these treated households could, by definition, not switch from evasion to compliance. If behavioral changes were necessary to induce an indirect effect, the mailings sent to compliant households should not produce any spillover. To test this hypothesis, we re-run our basic regression model on the sample of networks where *all* mailing targets were already complying with license fees before the treatment. Column (1) in Table 7 reports the results.

The estimates indicate highly significant, positive spillovers from both mailing treatments. The threat treatment triggers again a larger indirect effect than the baseline mailing (the equality of coefficients is clearly rejected, $F = 8.51$). Note that this difference cannot be due to any differential in the direct treatment effects. The model introduced in the following section shows that the observation can be due to a treatment variation in the treated households' likelihood of passing on information or in the 'relevance' of the communicated information (or both).

Table 7 about here.

Column (2) replicates the estimation for the sample where the ex-ante compliance among mailing targets was below 100%. When we account for the change in the estimated coefficient on $Total_k$, the point estimates for the indirect effects are larger for the latter sample.[28] This observation is supported by the specification from column (3), which interacts the treatment ratios with the average ex-ante compliance rate among the mailing targets in each network. The negative coefficients on the two interaction terms suggest that the spillover, in particular the one from the baseline mailing, decreases with the ex-ante compliance rate of the treated households.[29]

To sum up, we find larger spillovers when there is scope for the treatments to directly affect compliance behavior of the injection points. This mirrors a result from Banerjee et al. (2013), who find that adopters of new technologies are crucial in the diffusion of the technology. However, we detect sizable spillovers even when the mailings cannot alter the treated households' compliance behavior. Behavioral changes among the targeted households are therefore not necessary to induce the spillover. This means that the indirect treatment effects are not solely shaped by behavioral interdependencies (as, e.g., in pure models of conformity). Building on these findings, we now introduce a theoretical model of communication within networks that further studies the dispersion of information and compliance.

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[28]In column (1), for networks where all mailing recipients were already complying, we get $\beta_0 + \beta_1 = 0.103$ [$\beta_0 + \beta_2 = 0.272$]. In these networks, a one percentage point increase in the rate of baseline [threat] mailings – that also increases the experimental coverage – increases a potential evader's probability to register by 0.103pp [0.272pp]. In column (2), for networks where the mailings can directly alter the compliance behavior of the treated households, the corresponding effect size is 0.205pp [0.297pp].

[29]We obtain very similar results when we interact the treatment rates with treatment-specific ex-ante compliance rates (analogously to, e.g., equation 5).

6 Communication in Networks

To analyze the spillover's micro structure we introduce a theoretical model of communication in networks. A network is formally defined as a collection of nodes $\mathcal{N} = \{1, ..., N\}$ and a set of edges (links between the nodes) defined as $\Xi \subseteq \{(i,j)|i \neq j \in \mathcal{N}\}$, where an element (i,j) indicates that i and j are linked. A network can be described by its adjacency matrix $\mathbf{A}$ with entries $a_{ij} \in \{0,1\}$ where $a_{ij} = 1$ indicates that i and j are linked. Networks in our setting are undirected (i.e. if $(i,j) \in \Xi$, then also $(j,i) \in \Xi$), which means that the adjacency matrix $\mathbf{A}$ is symmetric. The set of FONs of i is denoted by $\mathcal{N}_i = \{j \in \mathcal{N}|(i,j) \in \Xi\}$.[30]

The objective of our analysis is to understand how the different treatments and different network structures interact in producing spillovers. In doing so, we distinguish four categories of households, indicated by $\tau \in \{a,b,c,d\}$: experimental (type I) households in the threat (a), baseline (b) or control (c) treatment, as well as untreated (type II and III) households (d). Each household has a 'propensity' to comply, p_i. Given our objective, we are not tying ourselves to any specific interpretations of p. The propensity might be shaped by extrinsic incentives (e.g., related to subjective beliefs about the sanction risk) or intrinsic motives (e.g., the strength of a social norm which is malleable by neighbors' views and decisions). If we denote the mean propensities of all households of category τ in network k *after* receiving a possible treatment by $p^0_{\tau k}$, we can make the following assumption:

Assumption A1: Before communication propensities satisfy $p^0_{ak} > p^0_{bk} > p^0_{ck}, \forall k$.

Essentially, A1 amounts to saying that the treatments have a direct effect. After the treatment (but before communication) households in the threat treatment have a higher propensity to comply than those in the baseline treatment who in turn have a higher propensity to comply than those of the control group (see Table 1 and Fellner et al., 2013).

Our model allows households to communicate for multiple rounds r. Specifically, in each round, households communicate with their FONs about p^r_i and update their compliance propensities as follows:

$$p_i^{r+1} = \frac{\sum_{j \in \mathcal{N}_i} \lambda_{ij}\, p_j^r + \lambda_{ii}\, p_i^r}{\sum_{j \in \mathcal{N}_i} \lambda_{ij} + \lambda_{ii}}, \tag{2}$$

[30]Since the networks are undirected, 'being a FON' is a symmetric binary relation: if i is a FON of j, then j is also a FON of i.

where $\lambda_{ij} \in \mathbb{R}$ is the weight that i places on j's opinion.[31] Hence, a household's updated propensity is a weighted average of their own and their neighbors' past propensities. This updating process dates back to DeGroot (1974) and has been widely used in recent literature (see, among others, DeMarzo et al., 2003; Acemoglu et al., 2010; Golub and Jackson, 2010, 2012; Jadbabaiea et al., 2012).[32]

Updating of p will, by definition, affect compliance: households will stop evading whenever their propensity p_i exceeds a given threshold $\widehat{p_i}$. The threshold is allowed to differ across households, reflecting heterogeneity in (risk) preferences, income, etc. The model then suggests that treatment differences in spillover effects can be driven by two factors: differential treatment effects on p and treatment specific communication frequencies captured by different weights λ. To illustrate this point, consider the result discussed in Section 5.4: relative to the baseline mailings, the threat produces a 10% larger direct treatment effect, but a 40% larger spillover effect. The larger direct effect can be related to a differential increase in p. The larger indirect effect, by contrast, suggests that households place higher weights on neighbors who were targeted with a threat (instead of a basline) mailing (compare fn. 31). We will return to this point below.

Using matrix notation one can express the updating rule as $p^{r+1} = \mathbf{T}p^r$, where $\mathbf{T}$ is a 'communication matrix'. The ij-th entry of $\mathbf{T}$ is given by $\lambda_{ij}a_{ij}$, i.e., it corresponds to the weights λ_{ij} from equation (2) if there is a link between i and j, otherwise it is zero. After 'sufficiently many' rounds of communication the updating process converges to a consensus in which all agents in a network have the same p (DeGroot, 1974; DeMarzo et al., 2003). (This does not imply that all households must display the same behavior, since preferences, and thus the threshold $\widehat{p_i}$, may differ across households.) The consensus propensity p^* can be expressed as a weighted average of all types' initial propensities, i.e.,

$$p^* = \sum_{j \in \mathcal{N}} \omega_j p_j^0, \tag{3}$$

where ω_j are weights which are further discussed below. In Appendix B we document that our basic estimation model from (1) can be interpreted in terms of the consensus propensity from equation (3). In particular, the coefficients β_1 and β_2 from model (1) will reflect the joint effect

[31] We allow for these weights to differ across neighbors. This might reflect any unobserved heterogeneity in the relationship between i and j. One might also assume that weights are treatment specific. A high weight placed on neighbors in the threat treatment (relative to the baseline treatment) could, for instance, capture the fact that these neighbors might be more likely to initiate communication.

[32] Empirical support for this form of learning has been found in lab experiments in some contexts (Chandrasekhar et al., 2015; Grimm and Mengel, 2014), though in others the support has been weaker (Möbius et al., 2015).

of $\bar{\omega}_\tau \times p^0_\tau$, where $\bar{\omega}_\tau$ is the average weight placed on agents of category a or b, respectively.[33] These weights depend on the weights λ_{ij} from equation (2), but also on the network positions the different types occupy. How network position can affect spillovers will be the topic of the next subsection.

6.1 Network Characteristics of Targeted Nodes

It can be shown (e.g., in Theorem 1 in DeMarzo et al., 2003), that the weights $\omega = (\omega_1, ..., \omega_N)$ solve the row eigenvector equation

$$\omega \mathbf{T} = \omega. \tag{4}$$

Since the communication matrix $\mathbf{T}$ can be interpreted as the transition matrix of a finite, irreducible, and aperiodic Markov chain, it has a unique eigenvalue equal to one and all other eigenvalues with modulus smaller than one. The vector of weights ω is given by the row eigenvector corresponding to the largest eigenvalue of $\mathbf{T}$. Network positions matter for ω, since they matter for $\mathbf{T}$ (the ij-th entry of $\mathbf{T}$ is non-zero if and only if i and j are linked). In particular, a household's weight in the final consensus ω_i will be closely related to i's eigenvector centrality.[34] In the special case where $\lambda_{ij} = \lambda, \forall i, j$ (and hence $\mathbf{T}$ is proportional to $\mathbf{A}$) an agent's weight in the final consensus ω_i exactly coincides with her eigenvector centrality.

Under assumption A1, mailing-treated households with a higher eigenvector centrality should then trigger a higher spillover and, *cet. par.*, increase the impact of the intervention. To assess this prediction, we analyze how the network position of mailing-treated households – the injection points – affects the size of the spillovers. Three network characteristics are of particular interest in our context: (i) degree, (ii) clustering and (iii) eigenvector centrality.[35] The *degree* of a household simply reflects how many FONs this household has. As we assume that FONs communicate with each other, the degree of household i measures to how many other households i directly talks (and listens) to. The *clustering coefficient* reflects how 'tightly knit' communities are: it shows how many of household i's neighbors are neighbors themselves. While degree

[33] In Appendix B we discuss more specific assumptions on the updating process which would lead to specific functional forms of $\bar{\omega}_\tau$ and, in some cases, allow for identification of p^0_τ.

[34] Loosely speaking, eigenvector centrality measures with how many others a household connects, where connections to other 'important' households contribute more to the influence of the household in question than equal connections to 'unimportant' households. Hence it is a recursively defined measure (see Appendix B.2 for the precise definition). Google's PageRank is a variant of the eigenvector centrality measure.

[35] A formal definition and further discussion of these characteristics is provided in Appendix B.2.

and clustering are important characteristics, our model suggests that it is the targeted node's *eigenvector centrality* that should be crucial in determining spillovers.

To confront these predictions with our data, we estimate models of the following structure:

$$\begin{aligned} y_{ik} &= \alpha + \beta_0\, Total_k + \beta_1\, Base_k + \beta_2\, Threat_k + \gamma_{1c}\, \overline{X}_{ck}^{base} + \gamma_{2c}\, \overline{X}_{ck}^{threat} \\ &\quad + \gamma_{3c} \left(\overline{X}_{ck}^{base} \times Base_k \right) + \gamma_{4c} \left(\overline{X}_{ck}^{threat} \times Threat_k \right) + \epsilon_{ik}, \end{aligned} \tag{5}$$

where $\overline{X}_{ck}^{base}$ and $\overline{X}_{ck}^{threat}$ are the average values of the respective household characteristic c (degree, clustering, or eigenvector centrality) among all injection points in the baseline or the threat treatment in network k, respectively. We are primarily interested in the coefficients on the interaction terms, γ_{3c} and γ_{4c}, which capture the impact of the injection points' properties on the spillover. Results from LPM estimates of equation (5) are presented in Table 8. Columns (1) to (3) present interactions with the injection point's degree, clustering and eigenvector centrality. Column (4) presents a specification which includes interactions for all three characteristics.

Table 8 about here.

The estimates do neither indicate significant interactions with the degree nor with the clustering coefficient of the injection points: in columns (1), (2) and (4), the estimated coefficients on the interaction terms are small and imprecisely estimated. Consistent with theoretical predictions, however, columns (3) and (4) reveal significant interactions with the eigenvector centrality. Mailings targeted at 'more central' households within a network produce larger spillovers. A one standard deviation increase in the eigenvector centrality of the injection points *cet.par.* increases the spillover by about 25%.[36] Hence, consistent with the model's predictions, there is scope to increase spillovers by targeting households with a higher eigenvector centrality within a network. This result echoes previous findings by Banerjee et al. (2013) who found that the centrality of the injection points in sampled networks constitutes a strong and significant predictor of micro-finance adoption in Indian villages.

In the Appendix we further explore this result. We consider interactions with betweenness centrality, which is positively correlated with eigenvector centrality ($\rho = 0.4638$) but clearly a distinct measure. When we augment the specification from column (4) in Table 8 by including

[36] Recall first our average treatment effect from Section 5: a one percent higher baseline [threat] treatment rate increases an evader's likelihood to comply by 0.25pp [0.35pp]. The point estimates from column (4) indicate that, with a one standard deviation higher eigenvector centrality of the injection points (approximately 0.24), the effect size would be 0.32pp [0.43pp].

betweenness centrality, we continue to find a positive interaction with the eigenvector centrality (see Table A.4). We also examined interactions with average characteristics of *all* households in a network (rather than the characteristics of injection points). Our results indicate that, by and large, average network characteristics do *not* play a major role in shaping the spillovers (see Table A.5). For instance, the *average* eigenvector centrality – in contrast to the centrality of the injection points – does not significantly affect the indirect effects from the mailings. Average network characteristics do not seem to provide useful information for the optimal targeting of interventions. In our context, targeting should be based on the characteristics of potential injection points.

6.2 Local Concentration of Interventions

Another important question for the optimal design of an enforcement intervention concerns the effect of treatment concentration: is it more effective to locally concentrate mailings or to spread treatments broadly within a network? The problem is illustrated in Figure 3, which displays a case with high local treatment concentration, i.e., where treated households are FONs (left panel), and another example where concentration is low (right panel). Where should we expect larger spillovers? Intuition suggests that both targeting strategies could be reasonable. On the one hand, a low concentration *cet.par.* means that more households will hear about the treatment. Each of these households, however, is likely to hear only about *one* mailing and to talk to many other untreated households. Hence, while many households will hear about a mailing, the effect on each of their propensities may be too small to induce compliance. High concentration resolves the latter issue but it will typically mean that less people hear about a mailing.

Figure 3 about here.

The notion of local concentration is closely linked to the concept of homophily, which has received a lot of attention in recent years across a variety of fields, including economics (Benhabib et al., 2010), sociology (McPherson et al., 2001) or management (Borgatti and Foster, 2003). Homophily refers to the empirical regularity that people tend to interact with 'similar' others. This fact has been documented across many different dimensions of similarity (e.g., ethnicity, gender, political opinions) and typologies of social ties (from intimate relations of friendship and marriage to business collaborations and everyday interactions). Our notion of local treatment concentration can be viewed as a homophily concept, where two neighbors are similar – in terms of their (post-treatment) compliance propensity p^0_τ – if they are in the same treatment. The random assignment of treatments assures that, conditional on being experimental participants, local concentration will not reflect other household properties. While the exogeneity distinguishes local concentration from the inherently endogenous concept of homophily, it allows us to isolate the network effects of similarity on information diffusion described in the model of Golub and Jackson (2012).

Golub and Jackson (2012) employ the model from above to study whether convergence of beliefs to the consensus p^* is helped or hindered by homophily. According to their results, convergence is hindered by (spectral) homophily implying that it is *not* optimal to locally concentrate interventions.[37] Under assumption A1 this means that spillovers decrease with the degree of local concentration of the intervention. To asses this prediction empirically we consider three different measures: (i) *l*ocal *c*oncentration *d*ummies, LCD_k^{base} and LCD_k^{threat}, which equal one if there are at least two injection points who are FONs and receive the same treatment (baseline or threat); (ii) the inbreeding homophily index (IH-index) due to Coleman (1958) and employed by Currarini et al. (2009), which is further discussed in Appendix B.3; (iii) a dummy indicating whether the IH-index is positive.[38] All of these measures vary at the network level. Table 2 (Panel b) suggests that in 17% of all networks there is at least one pair of FONs in the baseline treatment. The summary statistics further reveal a lot of variation in the inbreeding homophily index, with a majority of networks having a negative inbreeding homophily for the two mailing treatments, but also a substantial share of networks having a positive IH index.

Following the structure of equation (5), we then estimate the model

$$\begin{aligned} y_{ik} = \alpha &+ \beta_0\, Total_k + \beta_1 Base_k + \beta_2 Threat_k + \delta_1\, LCM_k^{base} + \delta_2\, LCM_k^{threat} \\ &+ \delta_3 \left(LCM_k^{base} \times Base_k\right) + \delta_4 \left(LCM_k^{threat} \times Threat_k\right) + \epsilon_{ik}, \end{aligned} \tag{6}$$

where LCM_k^{base} and LCM_k^{threat} capture one of the three *l*ocal *c*oncentration *m*easures. To address possible concern regarding the exogeneity of the local concentration of mailing targets within a network, we will augment equation (6). More specifically, we control for the local concentration of *all* experimental (type I) households, independently of their treatment. Conditional on this 'overall' concentration of possible injection points, random treatment assignment

[37]See Theorem 1 in Golub and Jackson (2012) and our discussion in Appendix B.3.

[38]The IH dummy avoids one downside of the IH index, which is that it is slightly biased downwards in small networks (see Appendix B.3).

implies that the variation in the treatment specific concentration measures will be exogenous.[39] The estimation results are presented in Table 9.

Table 9 about here.

The estimates suggest that local treatment concentration is associated with *smaller* spillovers. A negative interaction is observed for all measures, for both mailing treatments, as well as for the basic and the augmented version of equation (6). The interaction effects are only significant for the IH-index (for the threat treatment) and the dummies capturing positive IH-indices (both treatments). For the latter measures, we obtain the strongest results, both in terms of effect size and precision: the point estimates from columns (5) and (6) indicate that the size of the spillover in networks with a positive IH-index shrinks between 60% (threat) and 90% (baseline).[40] To assess the sensitivity of these interaction effects, we run estimations with local concentration measures that pool the two mailing treatments (see Table A.6 in the Appendix). The exercise increases statistical power and confirms the negative interaction effects: with a higher treatment concentration, we get significantly smaller spillovers.

6.3 How 'local' are spillovers?

In this section we ask how far the mailing-induced communication content 'travels' in the network. If new information does not travel very far in the network, then households with a closer network distance to the injection points will tend to have higher (post communication) propensities than those far away.[41] Spillovers may therefore be limited to households 'closer' to a treated household.

Figure 4 about here.

Figure 4, which presents a hypothetical network, illustrates a case of local spillovers. In this network, only household k receives a mailing. Since only k can start to spread the word, it will always first reach k's FONs (panel (b) in Figure 4). If the FONs do *not* pass on the information, then any spillover will be limited to the treated households' FONs. If they do pass it on, then

[39] The argument is similar to the conditional independence assumption discussed in Section 5.

[40] The estimates from column (6) suggest that the average treatment effects for the baseline [threat] treatment – a 0.25pp [0.35pp] increase in the probability to stop cheating – is 0.22pp [0.22pp] smaller in networks with a positive IH-index for the baseline [threat] treatment.

[41] Network distance, often also referred to as geodesic distance, refers to the length of the shortest path between two agents (nodes).

the news will reach k's SONs (panel (c) in Figure 4). Roughly speaking, if a treatment induces more intensive communication, then more distant households (in terms of network distance) will be reached via the spillover.

To analyze how far-reaching spillovers are in terms of network distance, we compute treatment rates that distinguish between the treatments of a households' first-, second- or a higher-order neighbors (HONs). For the baseline treatment, for instance, we count the number of i's FONs, SONs, and HONs who received this treatment. Dividing these numbers by $N_k - 1$, we obtain the rates $Base_{ik}^h$ for $h \in \{FON, SON, HON\}$. This approach assures the identity $Base_k = \sum_h Base_{ik}^h$ (which analogously holds for the threat treatment). Hence, we estimate a model variant to the one from equation (1), which now exploits variation between and within networks:

$$y_{ik} = \alpha + \beta_0 Total_k + \sum_h \beta_1^h Base_{ik}^h + \sum_h \beta_2^h Threat_{ik}^h + \epsilon_{ik}. \quad (7)$$

The discussion from above suggests that the estimated coefficients for β_1^h and β_2^h eventually do not increase as we move from $h = FON$ to $h = SON$ and to $h = HON$.

Table 10 about here.

The results from LPM estimations of equation (7) are presented in column (1) of Table 10. In general, the estimates support the hypothesis that a given mailing triggers a larger spillover on household i if the mailing is targeted to 'closer' rather than more 'distant' neighbors of i. The baseline and threat mailings both trigger sizable spillovers on the treated household's FONs. The point estimates indicate that the spillovers are around 50 to 60% lower among SONs (with significantly different coefficients, F-statistics of 3.95 and 12.38 for the baseline respectively the threat mailing). For the baseline treatment, the estimates do not indicate any significant spillovers to higher-order neighbors. The spillover from this treatment is therefore concentrated among FONs and SONs. For the threat treatment, this seems to be different. Here we observe significant spillovers beyond SONs. In fact, the estimate for β_2^{HON} is even larger (but less precisely estimated) than β_2^{SON}. The results are robust when we control for the geographic distances to the nearest household treated with a baseline mailing or a threat mailing (see column 2).[42]

[42] Results remain essentially unchanged if we also decompose the total rate of experimental participants ($Total_k$) by h.

The estimate indicate that the spillovers from the threat treatment have a larger scope than those from the baseline treatment. One possible interpretation of this observation is that the impact of the threat 'lives on longer', i.e., the treat induces more rounds of communication than the baseline mailing. This would mean that households further away from the targeted nodes will eventually hear about the threat but not about the baseline mailing. Another possible interpretation is that the threat mailing simply leads to higher pre-communication propensities of targeted nodes which can then lead to higher post-communication propensities by first-, second-, and higher-order neighbors. Note that the first explanation is consistent with a higher weight λ being placed on threat treated injection points – an idea that we discussed in Section 5.4, in light of the stark differential in direct and indirect treatment effects between the baseline and the threat treatment.

Naturally these results are sensitive to varying the distance threshold z: whether SONs or HONs can be reached will depend on who is defined to be a SON or HON. What we robustly see across all specifications for z, though, is that the threat letter reaches more distant neighbors than the baseline treatment (see Table A.7 in the Appendix).

7 Conclusions

This paper studied how a randomized direct mailing campaign spills over onto compliance decisions of untreated neighbors in geographic networks. Our research design builds on three important blocks. First, by exploiting a large-scale field experiment which randomly assigns targeted nodes to different treatments, we identify networks effects. Second, by applying a geographic network approach on comprehensive data that cover the universe of households from rural Austria, we arrive at a large amount of networks with substantial variation in their characteristics. Third, we formulate a model of communication in networks which captures how the treatments' impact is transmitted from treated to untreated nodes in a network. The theoretical framework then guides our empirical analysis on how the network positions of targeted households affect social learning and, ultimately, the compliance spillover.

Our analysis identifies strong and precisely estimated spillover effects. In addition, the data support two key predictions from the model. First, the spillover increases considerably with the treated nodes' eigenvector centrality (Banerjee et al., 2013, 2014). By contrast, neither the nodes' degree, the clustering coefficient nor any average network-level characteristics seem to influence the effect size. Second, consistent with the idea that homophily impedes social learning, we document that a higher local treatment concentration produces a significantly smaller spillover (Golub and Jackson, 2012). Together with several null results from various placebo network exercises, these findings indicate that the structure of our geographic networks is useful to capture patterns of social learning among neighbors.

Our results carry important implications for the optimal targeting of enforcement policies and all other interventions that play on word-of-mouth diffusion among neighbors. Our findings suggest that one could, *cet. par.*, greatly improve the intervention's impact by targeting central households and by spreading the treatment broadly within a network. The easy availability of geographic information thereby facilitates the computation of neighborhood networks and allows to implement this strategy at fairly low costs.

While our results clearly highlight the potential of a geographic network approach, more research is needed to better understand when and how geographic proximity affects social learning. Future work needs to identify in which areas geographic networks play a more, less or an equally important role as other types of networks. A first step in this direction is presented in Beaman et al. (2015), who compare seeding based on geographic and elicited social networks in the context of technology adoption in a developing country. Understanding how information transmission in geographic networks compares to other networks in different contexts will also help to clarify the effectiveness of geographic approaches in other contexts and policy domains.

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Tables

Table 1: Summary statistics for type I and type II households (raw data)

	Type I *Experimental Sample* Control	Base	Threat	*Type II* *Non-Experimental Sample* Potential Evaders
Registration rate	0.0109	0.0701	0.0765	0.0425
	(0.1041)	(0.2552)	(0.2659)	(0.2018)
Ex-ante compliance	0.3625	0.3627	0.3752	–
	(0.4809)	(0.4808)	(0.4842)	–
Male	0.8363	0.8375	0.8379	0.8156
	(0.0418)	(0.0423)	(0.0423)	(0.0430)
Age	40.04	39.67	39.28	
	(13.00)	(12.47)	(12.11)	
Degree	19.87	21.66	21.42	
	(52.09)	(55.39)	(55.83)	
Clustering	0.67	0.68	0.67	
	(0.40)	(0.39)	(0.69)	
Eigenvector centrality	0.31	0.31	0.31	
	(0.24)	(0.24)	(0.24)	
Number of households	1,371	11,177	11,078	131,884

Notes: Summary statistics (mean and standard deviation in parenthesis) for potential evaders in the experimental (type I) and non-experimental sample (type II households). The first three columns present sample means for the three treatment arms of the experiment (Control, Base and Threat), the last column includes type II households. Information on age (reported in columns 1–3) is only available for a subset of 2,778 households.

Table 2: Summary statistics for relevant networks, $z = 50$ meters

(A) Household level (14,787 type II households)	mean	sd	median	1^{st} quart	3^{rd} quart
Degree	**11.26**	22.35	6	3	11
Clustering	**0.73**	0.29	0.80	0.60	1
Eigenvector Centrality	**0.19**	0.00	0.34	0.13	0.41
FON in experiment	**0.54**	0.50	1	0	1
FON w/ Base	**0.27**	0.44	0	0	1
FON w/ Threat	**0.26**	0.44	0	0	1
FON w/ Control	**0.07**	0.26	0	0	0
SON in experiment	**0.12**	0.33	0	0	0
SON w/ Base	**0.05**	0.21	0	0	0
SON w/ Threat	**0.05**	0.23	0	0	0
SON w/ Control	**0.01**	0.10	0	0	0
Spatial distance to closest neighbor...					
... in experiment	**96.73**	144.54	45.36	2.87	115.03
... w/ Base	**294.00**	738.15	86.13	35.53	243.84
... w/ Threat	**317.27**	741.38	91.37	34.28	273.04
... w/ Control	**681.05**	1079.82	115.73	297.84	737.43
Registration rate	**0.08**	0.27	0	0	0
(B) Network level (3,764 networks)					
Network size (N_k)	**17.98**	45.48	6	3	14
Treatment Rates:					
Total$_k$	**0.45**	0.33	0.40	0.39	0.67
Base$_k$	**0.21**	0.28	0.10	0.00	0.33
Threat$_k$	**0.22**	0.29	0.10	0.00	0.33
Control$_k$	**0.03**	0.11	0.00	0.00	0.00
Local Treatment Concentration:					
LCD^{base}	**0.17**	0.38	0	0	0
LCD^{threat}	**0.17**	0.38	0	0	0
LCD^{all} (any treatment)	**0.25**	0.43	0	0	1
IH^{base}	**-0.17**	0.28	-0.04	-2.00	0.41
IH^{threat}	**-0.17**	0.28	-0.04	-1.00	0.69
IH^{all} (any treatment)	**-0.32**	0.35	-0.20	-2.00	0.79
(C) Municipality level (771 municipalities)					
Population	**1,790**	1,006	1,570	1,060	2,365
Labor Income	**27,250**	2,172	26,936	25,977	28,332
Average Age	**53.73**	20.10	51.5	36.75	69.25
Non-Austrian Citizens	**0.05**	0.04	0.04	0.02	0.07
Catholic	**0.88**	0.10	0.91	0.86	0.95
1- or 2-Family Dwellings	**0.82**	0.14	0.84	0.75	0.92
Voter Turnout	**0.76**	0.07	0.77	0.72	0.81

Notes: Panel A presents summary statistics at the household level for all potential evaders in the non-experimental sample (type II households) that are located in networks with at least one experimental participant (type I household). Panel B reports (unweighted) network level statistics for these relevant networks. Panel C considers several municipality characteristics (unweighted, at municipality level). FON and SON abbreviate First and Second Order Neighbors, respectively. LCD's are Local Concentration Dummies and IH denotes inbreeding homophily (see Section 6). Spatial distance is measured in meters.

Table 3: Balancing Tests

	(1) Base_k	(2) Base_k	(3) Threat_k	(4) Threat_k
Network Size	-0.0000		-0.0000	
	(0.0000)		(0.0000)	
Clustering	0.0056		-0.0095	
	(0.0136)		(0.0135)	
Degree	0.0001		-0.0001	
	(0.0001)		(0.0001)	
Eigenvector Centrality	0.0102		-0.0071	
	(0.0179)		(0.0178)	
Population		0.0000		-0.0000
		(0.0000)		(0.0000)
Labor Income		0.0000		-0.0000
		(0.0000)		(0.0000)
Average Age		-0.0003		0.0004
		(0.0009)		(0.0009)
Catholic		0.0015		0.0035
		(0.0087)		(0.0092)
Non-Austrian Citizens		-0.0252		0.0476
		(0.0315)		(0.0334)
1- or 2-family dwellings		-0.0006		0.0050
		(0.0107)		(0.0116)
Voter Turnout		0.0120		-0.0122
		(0.0289)		(0.0301)
Observations	3,764	771	3,764	771

Notes: The table reports estimates for different μ_2 coefficients from regressions of $Base_k = \mu_0^{base} + \mu_1^{base} Total_k + \mu_2^{base} x_k + \epsilon_k^{base}$ and $Threat_k = \mu_0^{threat} + \mu_1^{threat} Total_k + \mu_2^{threat} x_k + \epsilon_k^{threat}$, where x_k is a network level variable in columns (1) and (3) and a municipal level variable in columns (2) and (4). Each estimate in the table is obtained from a different regression based on 3,764 (network-level) or 771 observations (municipality-level). Robust standard errors are in parentheses. None of the coefficients is significant at conventional levels.

Table 4: Basic results

	(1)	(2)	(3)	(4)
	Networks with $z = 50$m		Municipality level	
Base$_k$	0.2491***	0.2450***	0.0073	0.0221
	(0.0386)	(0.0385)	(0.3038)	(0.1378)
Threat$_k$	0.3533***	0.3491***	-0.0495	-0.0061
	(0.0394)	(0.0393)	(0.3419)	(0.1541)
Total$_k$	-0.0581*		0.2736	0.2123
	(0.0335)		(0.3277)	(0.1487)
Constant	0.0313***	0.0199***	0.0463***	0.0228***
	(0.0027)	(0.0069)	(0.0039)	(0.0010)
F-test for $H_0 : \beta_1 = \beta_2$	13.47	12.27		
Total$_k$ Fixed Effects	No	Yes	No	No
Observations	14,787	14,787	14,787	62,064
Networks	3,764	3,764	771	982
R^2	0.0560	0.0971	0.0150	0.0176

Notes: The table presents the results from LPM estimations of equation (1). In column (2) the variable Total$_k$ enters non-parametrically by including a set of dummies for each value of Total$_k$. Column (3) uses the sample from the first two specifications but assumes that the network is defined by the municipality. Column (4) maintains this assumption but enlarges the sample including all potential evaders with at least one experimental participant in their municipality. Standard errors, clustered at the network (columns 1 and 2) or municipality level (3 and 4), are reported in parentheses. ***/**/* indicates significance at the 1%/5%/10%-level, respectively.

Table 5: Robustness of basic results

	(1)	(2)	(3)	(4)
	Probit	Municipality Fixed Effects	Field Inspections	
Base$_k$	0.1973***	0.2212***	0.2495***	0.2531***
	(0.0551)	(0.0443)	(0.0386)	(0.0390)
Threat$_k$	0.2419***	0.3341***	0.3532***	0.3593***
	(0.0550)	(0.0454)	(0.0394)	(0.0398)
Total$_k$	-0.0548	-0.0769*	-0.0582*	-0.0615*
	(0.0542)	(0.0415)	(0.0335)	(0.0339)
Enforcement Rate	–	–	0.0040***	–
			(0.0005)	
Constant	-0.0639	0.0400***	0.0304***	0.0294***
	(0.0589)	(0.0042)	(0.0027)	(0.0030)
Observations	14,787	14,787	14,787	12,891
Networks	3,764	3,764	3,764	3,657
R^2	0.0774	0.1101	0.0570	0.0600

Notes: Column (1) is based on a Probit estimation of equation (1). It reports marginal effects evaluated at the mean of the indepedent variables. Columns (2)–(4) are estimated using a linear probability model. Column (2) includes fixed effects at the municipality level. Column (3) controls for the rate of enforcement from field inspections. Column (4) excludes all networks with an enforcement rate greater than zero. Standard errors, clustered at the network level, are reported in parentheses. ***/**/* indicates significance at the 1%/5%/10%-level, respectively.

Table 6: Different distance (network) assumptions

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Threshold $z =$	25	50	75	100	250	500	1000	1500	2000
Panel A. Estimation Results									
Base_k	0.2454***	0.2491***	0.2687***	0.2775***	0.2730***	0.2668**	0.2271	0.0759	-0.0673
	(0.0336)	(0.0386)	(0.0573)	(0.0545)	(0.0798)	(0.1359)	(0.2594)	(0.3699)	(0.4851)
Threat_k	0.3106***	0.3533***	0.3940***	0.3910***	0.3569***	0.5063***	0.4910*	0.4342	0.2788
	(0.0341)	(0.0394)	(0.0590)	(0.0559)	(0.0823)	(0.1362)	(0.2588)	(0.3582)	(0.4559)
Total_k	0.0198	-0.0581*	-0.1056**	-0.1207**	-0.1207*	-0.2039*	-0.1903	-0.0943	0.0485
	(0.0313)	(0.0335)	(0.0524)	(0.0476)	(0.0716)	(0.1230)	(0.2379)	(0.3349)	(0.4357)
Constant	0.0187**	0.0314***	0.0348***	0.0386***	0.0421***	0.0430***	0.0446***	0.0447***	0.0444***
	(0.0078)	(0.0027)	(0.0021)	(0.0020)	(0.0018)	(0.0016)	(0.0015)	(0.0015)	(0.0015)
Observations	5,236	14,787	23,473	29,012	41,347	50,488	58,298	60,320	61,351
Networks	3,198	3,764	3,319	2,990	2,113	1,554	1,169	1,073	1,020
R^2	0.0782	0.056	0.0369	0.0264	0.0151	0.0114	0.0081	0.0077	0.0078
Panel B. Average individual effect from one additional mailing into each network:									
Base	0.1231	0.0498	0.0336	0.0277	0.0105	0.0039	0.0014	0.0004	-0.0003
Threat	0.1550	0.0707	0.0492	0.0391	0.0137	0.0075	0.0031	0.0023	0.0013
Panel C. Total spillover from 1,000 additional mailing, spread over all networks:									
Base	217.15	150.01	144.21	152.08	114.54	30.09	11.53	-5.40	-5.42
Threat	270.55	231.91	254.91	262.18	177.74	144.49	94.30	99.49	94.20

Notes: Panel A reports the results from LPM estimations of equation (1) for the different samples of type II households that emerge for different levels of the distance threshold z. Standard errors, clustered at the network level, are in parentheses. ***/**/* indicates significance at the 1%/5%/10%-level, respectively. Panel B presents the effect of sending one additional baseline [threat] mailing into each network on a type II household's probability to register. The effect is derived from the point estimates from Panel A and given by $\frac{\hat{\beta}_1}{N-1}$ $[\frac{\hat{\beta}_2}{N-1}]$, where we use the respective median network size for N. Panel C computes the expected spillover from sending a fixed number of 1,000 additional baseline [threat] mailings into the networks covered by the respective sample. For the baseline mailings, the total number of expected registrations is computed as follows: Number of Observations$\times \frac{1{,}000}{\text{Number of Networks}} \times \frac{\hat{\beta}_0+\hat{\beta}_1}{N-1}$. The effect is weighted with the total number of observations to account for the fact that the spillover applies to all type II households in these networks.

Table 7: Spillover effects and ex-ante compliance rate of experimental participants

	(1) Comp= 1	(2) Comp< 1	(3) Full Sample
Base_k	0.3031***	0.2573***	0.2927***
	(0.0984)	(0.0406)	(0.0408)
Threat_k	0.4723***	0.3496***	0.3756***
	(0.1068)	(0.0414)	(0.0414)
Total_k	-0.2003**	-0.0525	-0.0532
	(0.0826)	(0.0347)	(0.0334)
Ex-ante Comp Rate			-0.0057
			(0.0075)
Base_k × Ex-ante Comp			-0.1794***
			(0.0454)
Threat_k × Ex-ante Comp			-0.0760
			(0.0508)
Constant	0.0356***	0.0306***	0.0326***
	(0.0074)	(0.0029)	(0.0032)
Obs	1,564	13,223	14,787
Networks	556	3,208	3,764
R^2	0.0567	0.0590	0.0594

Notes: The table presents results from LPM estimations of variations of equation (1). In the first two columns we report the results of the specification of column 1 of Table 4 restricted to networks with an ex-ante of the households targeted by the experiment equal to 1 and less than 1, respectively. In column (3) we estimate (1) by adding as controls the ex-ante compliance rate and its interactions with the baseline and threat mailings rates. Standard errors, clustered at the network level, are in parentheses. ***/**/* indicates significance at the 1%/5%/10%-level, respectively.

Table 8: Spillover effects and injection points

	(1) Degree	(2) Clustering	(3) Centrality	(4) All Measures
Base_k	0.2533***	0.0997	0.2526***	0.0507
	(0.0390)	(0.0641)	(0.0417)	(0.0789)
Threat_k	0.3531***	0.2152***	0.3594***	0.1304
	(0.0399)	(0.0694)	(0.0426)	(0.0841)
Total_k	-0.0616*	-0.0368	-0.0753**	-0.0435
	(0.0334)	(0.0326)	(0.0342)	(0.0335)
$\text{Base}_k \times \text{Degree}^{base}$	-0.0009			-0.0002
	(0.0022)			(0.0023)
$\text{Threat}_k \times \text{Degree}^{threat}$	0.0024			0.0024
	(0.0024)			(0.0026)
Degree^{base}	-0.0002			-0.0002
	(0.0003)			(0.0004)
Degree^{threat}	-0.0007***			-0.0006
	(0.0002)			(0.0004)
$\text{Base}_k \times \text{Clustering}^{base}$		0.0236		0.0705
		(0.0430)		(0.0499)
$\text{Threat}_k \times \text{Clustering}^{threat}$		0.0323		0.0831
		(0.0461)		(0.0542)
Clustering^{base}		-0.0136*		0.0030
		(0.0073)		(0.0085)
$\text{Clustering}^{threat}$		-0.0209***		0.0052
		(0.0075)		(0.0092)
$\text{Base}_k \times \text{Centrality}^{base}$			0.1896**	0.2714**
			(0.0895)	(0.1096)
$\text{Threat}_k \times \text{Centrality}^{threat}$			0.2157**	0.3426***
			(0.0974)	(0.1179)
Centrality^{base}			0.0442*	0.0160
			(0.0255)	(0.0296)
$\text{Centrality}^{threat}$			0.0094	-0.0192
			(0.0255)	(0.0290)
Obs	14,787	14,431	14,787	14,431
Networks	3,764	3,764	3,755	3,755
R^2	0.0574	0.0567	0.0607	0.0621

Notes: The table presents results from LPM estimations of variations of equation (5). In the first, second and third column the network characteristic of injection points included in the model is the degree, the clustering coefficient and the eigenvector centrality. In the last column all three measures are included. Standard errors, clustered at the network level, are in parentheses. ***/**/* indicates significance at the 1%/5%/10%-level, respectively.

Table 9: Spillover effects and local treatment concentration

	LCDummies		IH-Index		IH-Dummies	
	(1)	(2)	(3)	(4)	(5)	(6)
Base_k	0.2617***	0.2582***	0.1648***	0.1712***	0.2865***	0.2871***
	(0.0394)	(0.0392)	(0.0476)	(0.0527)	(0.0406)	(0.0407)
Threat_k	0.3717***	0.3684***	0.2446***	0.2510***	0.4075***	0.4080***
	(0.0404)	(0.0403)	(0.0493)	(0.0555)	(0.0416)	(0.0417)
Total_k	-0.0417	-0.0375	-0.0396	-0.0454	-0.0838**	-0.0836**
	(0.0329)	(0.0328)	(0.0328)	(0.0401)	(0.0340)	(0.0340)
$\text{Base}_k \times \text{LCD}_k^{base}$	-0.0502	-0.0488				
	(0.0508)	(0.0509)				
$\text{Threat}_k \times \text{LCD}_k^{threat}$	-0.0629	-0.0614				
	(0.0525)	(0.0529)				
LCD_k^{base}	-0.0059	-0.0000				
	(0.0118)	(0.0125)				
LCD_k^{threat}	-0.0186*	-0.0124				
	(0.0112)	(0.0121)				
LCD_k^{all}		-0.0129*				
		(0.0074)				
$\text{Base}_k \times \text{IH}_k^{base}$			-0.0045	-0.0012		
			(0.0555)	(0.0571)		
$\text{Threat}_k \times \text{IH}_k^{threat}$			-0.1112*	-0.1080*		
			(0.0612)	(0.0625)		
IH_k^{base}			-0.0924**	-0.0876*		
			(0.0439)	(0.0470)		
IH_k^{threat}			-0.0316	-0.0266		
			(0.0502)	(0.0540)		
IH_k^{all}				-0.0076		
				(0.0299)		
$\text{Base}_k \times \text{IHD}_k^{base}$					-0.2061***	-0.2161***
					(0.0576)	(0.0597)
$\text{Threat}_k \times \text{IHD}_k^{threat}$					-0.2068***	-0.2143***
					(0.0594)	(0.0617)
IHD_k^{threat}					0.0304***	0.0306***
					(0.0061)	(0.0061)
IHD_k^{base}					0.0188***	0.0190***
					(0.0060)	(0.0061)
IHD_k^{all}						0.0045
						(0.0076)
R^2	0.0596	0.0598	0.0596	0.0596	0.0602	0.0602

Notes: The table presents results from LPM estimations of variations of model equation (6). In columns 1-2, the local concentration measures are the local concentration dummies (LCD). In columns 3-4, the local concentration measures are inbreeding homophily indexes (IH-index) and in columns 5-6, the inbreeding homophily dummies (IH-dummies). All estimates are based on 14,787 observations from 3,764 networks. Standard errors, clustered at the network level, are in parentheses. ***/**/* indicates significance at the 1%/5%/10%-level, respectively.

Table 10: How local are spillovers?

	(1)	(2)
Base^{FON}	0.2643***	0.2620***
	(0.0388)	(0.0328)
Base^{SON}	0.1424**	0.1372**
	(0.0668)	(0.0670)
Base^{HON}	0.0141	0.0022
	(0.0699)	(0.0783)
Threat^{FON}	0.3665***	0.3670***
	(0.0396)	(0.0400)
Threat^{SON}	0.1439**	0.1427**
	(0.0678)	(0.0680)
Threat^{HON}	0.3204***	0.3197**
	(0.0796)	(0.0800)
Total	-0.0475	-0.0483
	(0.0329)	(0.0329)
Distance to nearest Base	-	-0.0000***
		(0.0000)
Distance to nearest Threat	-	-0.0000
		(0.0000)
F-tests:		
$\text{Base}^{FON}=\text{Base}^{SON}$	3.95	4.13
$\text{Base}^{FON}=\text{Base}^{HON}$	15.39	15.94
$\text{Base}^{SON}=\text{Base}^{HON}$	2.39	2.49
$\text{Threat}^{FON}=\text{Threat}^{SON}$	12.38	12.54
$\text{Threat}^{FON}=\text{Threat}^{HON}$	0.38	0.40
$\text{Threat}^{SON}=\text{Threat}^{HON}$	3.41	3.42
Obs	14,787	14,787
Networks	3,764	3,764

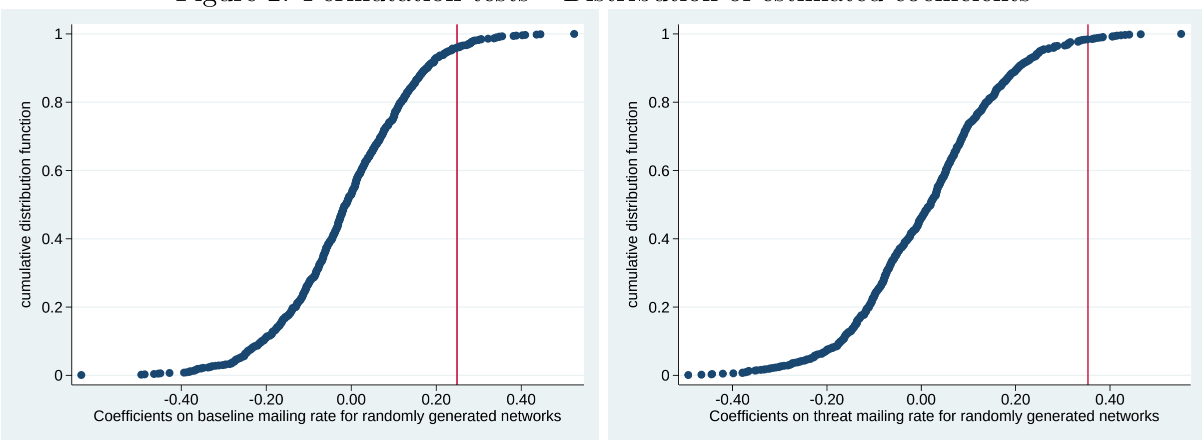
Notes: The table presents results from LPM estimations of equation (7). Column (2) controls for distances to the nearest household treated with a baseline and a threat mailing, respectively. Standard errors, clustered at the network level, are in parentheses. ***/**/* indicates significance at the 1%/5%/10%-level, respectively.

Figures

Figure 1: Illustration of geographical networks

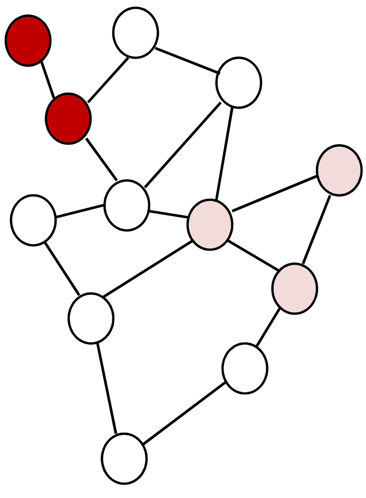
Notes: The figure presents an example of two disjoint networks for the case $z = 50$ meters.

Figure 2: Permutation tests – Distribution of estimated coefficients



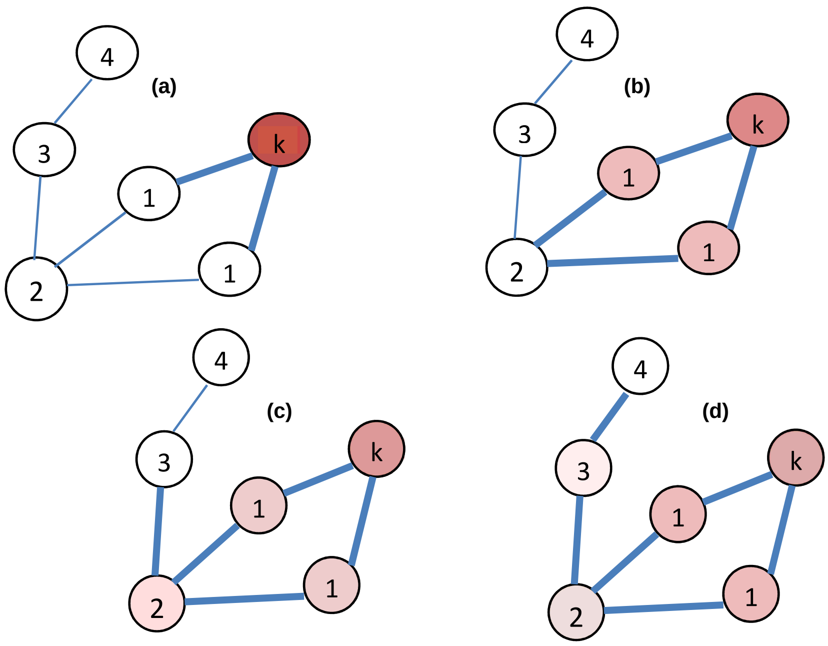
Notes: The figures present the cumulative distribution function of the coefficients on the baseline (left panel) and threat (right) mailing rates, obtained from the permutation test described in Section 5.3. The red vertical lines represent the coefficients on the baseline and the threat mailing rates obtained from estimating model (1).

Figure 3: Local treatment concentration



Notes: The figure shows two networks with the same structure but different degrees of local treatment concentration. There are three types represented in this picture. The dark nodes could represent type A (threat), the lighter nodes type B (base) and the empty nodes untreated, non-experimental agents (type D). In the left panel, households of the same type tend to be neighbors, while households in the right panel tend to be neighbors with households of a different type.

Figure 4: How local are spillovers?



Notes: The figure shows targeted household k with its first-, second-, third-, and fourth-order neighbors (indicated by numbers 1,2,3,4) in a hypothetical network. Panel (a) represents the time before communication starts where only k knows about its letter, in (b) the news has spread to k's FONs, in (c) to her SONs, etc. Note that each time the message spreads its impact becomes smaller: in (c) for instance, the SON of k learns about the letters but also about the fact that k's FONs – with who she communicates – did not receive a letter.

Appendix A Additional Figures and Tables

Table A.1: Properties of relevant networks for different distance thresholds z.

Network threshold $z =$	25	50	75	100	250	500	1000	1500	2000
Networks	3,198	3,764	3,319	2,990	2,113	1,554	1,169	1,073	1,020
Mean Network Size	5.94	17.98	32.93	45.13	90.40	151.95	232.91	261.66	279.54
Type I HHs	4,481	11,794	15,343	17,172	20,699	22,332	23,097	23,227	23,296
Type II HHs	5,236	14,787	23,473	29,012	41,347	50,488	58,298	60,320	61,351
Type III HHs	9,277	41,107	70,482	88,742	128,959	163,311	190,882	197,214	200,483

Notes: A network becomes relevant for studying spillovers if there is at least one type I household and at least one type II household.

Table A.2: Interactions with municipality characteristics

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Variable $M =$	Income	Catholic	Population	Age	Non-Austrians	Turnout	Dwellings
Total_k	-0.0571*	-0.0561*	-0.0586*	-0.0604*	-0.0569*	-0.0457	-0.0544
	(0.0335)	(0.0336)	(0.0335)	(0.0334)	(0.0335)	(0.0333)	(0.0334)
Base_k	0.1607	0.6840***	0.2494***	1.4187**	0.2123	-0.4590**	0.0354
	(0.2029)	(0.2616)	(0.0496)	(0.6700)	(0.3288)	(0.2096)	(0.1060)
Threat_k	0.3223	0.5068**	0.3525***	0.1773	-0.2460	-0.1770	0.0443
	(0.2370)	(0.2455)	(0.0537)	(0.6986)	(0.3085)	(0.2262)	(0.1206)
$\text{Base}_k \times M$	-0.0000	-0.4823*	0.0000	-0.0244*	0.0392	0.9881***	0.2787**
	(0.0000)	(0.2859)	(0.0000)	(0.0139)	(0.3526)	(0.2924)	(0.1319)
$\text{Threat}_k \times M$	-0.0000	-0.1695	0.0000	0.0037	0.6427*	0.7402**	0.4042***
	(0.0000)	(0.2690)	(0.0000)	(0.0146)	(0.3305)	(0.4855)	(0.1517)
M	0.0000	-0.0070	-0.0000	-0.0018	0.0508	0.0185	-0.0089
	(0.0000)	(0.0269)	(0.0000)	(0.0018)	(0.0525)	(0.0428)	(0.0151)
R^2	0.0521	0.0568	0.0560	0.0569	0.0571	0.0605	0.0585

Notes: The table reports results from LPM estimates of extension of equation (1) where the treatment rates are interacted with a municipality variable M (see Panel (C) in Table 2). All estimates are based on 14,787 observations from 3,764 networks. A constant term is included but estimates are not reported. Standard errors, clustered at the network level, are reported in parentheses. ***/**/* indicates significance at the 1%/5%/10%-level, respectively.

Table A.3: Spillover effects within the experiment

	(1)	(2)	(3)	(4)
Base_i	0.0591***	0.0597***	0.0475***	0.0487***
	(0.0037)	(0.0038)	(0.0050)	(0.0051)
Threat_i	0.0656***	0.0660***	0.0536***	0.0546***
	(0.0037)	(0.0039)	(0.0051)	(0.0052)
Base_k	-	0.0086	-	0.0199
		(0.0251)		(0.0622)
Threat_k	-	0.0118	-	0.0072
		(0.0245)		(0.0603)
Total_k	-	-0.0580**	-	-0.0418
		(0.0230)		(0.0577)
Constant	0.0109***	0.0293***	0.0103***	0.0360***
	(0.0028)	(0.0034)	(0.0038)	(0.0060)
Observations	23,626	23,626	11,794	11,794
Networks	–	10,535	–	1,530
R^2	0.0030	0.009	0.003	0.007

Notes: The table reports LPM estimates for the *direct treatment effects* in the experimental sample (type I households). Baselinet_i and Threat_i indicate a dummy equal to 1 if type I houeshold i was in the baseline or threat treatment, respectively. In columns (1) and (2) we run regressions on all type I households included in our raw data, in column (3) and (4) we consider only type I households from relevant networks with $z = 50$ as defined above (see Section 4.2). Columns (2) and (4) add controls for the treatment rates at the network level. In column (1) and (3) robust standard errors are in parentheses, in column (2) and (4) standard errors are clustered at the network level. ***/** indicates significance at the 1%/5%-level, respectively.

Table A.4: Spillover effects and injection points: Betweenness centrality

	(1) Betweenness	(2) Basic specification	(3) (3) All measures
Base_k	0.2535***	0.0507	0.0343
	(0.0392)	(0.0790)	(0.0821)
Threat_k	0.3651***	0.1304	0.1607*
	(0.0392)	(0.0841)	(0.0821)
Total_k	-0.0566	-0.0435	-0.0427
	(0.0336)	(0.0335)	(0.0336)
$\text{Base}_k \times \text{Betweenness}^{base}$	-0.1552		-0.0768
	(0.1291)		(0.1329)
$\text{Threat}_k \times \text{Betweenness}^{threat}$	-0.1923*		-0.1431
	(0.1169)		(0.1194)
$\text{Base}_k \times \text{Degree}^{base}$	-0.0002	-0.0003	
		(0.0023)	(0.0023)
$\text{Threat}_k \times \text{Degree}^{threat}$		0.0024	0.0025
		(0.0026)	(0.0026)
$\text{Base}_k \times \text{Centrality}^{base}$		0.2714**	0.3151***
		(0.1096)	(0.1156)
$\text{Threat}_k \times \text{Centrality}^{threat}$		0.3426***	0.2852**
		(0.1179)	(0.1226)
$\text{Base}_k \times \text{Clustering}^{base}$		0.0705	0.0862*
		(0.0499)	(0.0515)
$\text{Threat}_k \times \text{Clustering}^{threat}$		0.0831	0.0739
		(0.0542)	(0.0557)
Observations	14,787	14,431	14,431
R^2	0.0571	0.0621	0.0627
Networks	3,764	3,755	3,755

Notes: The table replicates Table 8 by including betweenness centrality in equation (5). (Betweenness centrality of an injection point i is defined as the share of shortest paths between any two households within the network passing through i.) Standard errors, clustered at the network level, are in parentheses. ***/**/* indicates significance at the 1%/5%/10%-level, respectively.

Table A.5: Spillover effects and *average* network characteristics

Variable A_k =	Degree (1)	Centrality (2)	Clustering (3)	Diameter (4)	Charpathlength (5)	Assortativity (6)
Base_k	0.2526***	0.2253***	0.2872***	0.2571***	0.2581***	0.1971**
	(0.0390)	(0.0659)	(0.0459)	(0.0404)	(0.0403)	(0.0936)
Threat_k	0.3498***	0.2548***	0.3953***	0.3710***	0.3765***	0.2799***
	(0.0399)	(0.0696)	(0.0467)	(0.0419)	(0.0417)	(0.0921)
Total_k	-0.0631*	-0.1073***	-0.0353	-0.0652*	-0.0621*	-0.1208
	(0.0334)	(0.0375)	(0.0340)	(0.0339)	(0.0338)	(0.0854)
$\text{Base}_k \times A_k$	-0.0011	0.0430	-0.1015**	-0.0037	-0.0003	0.0719
	(0.0022)	(0.0977)	(0.0506)	(0.0069)	(0.0004)	(0.0569)
$\text{Threat}_k \times A_k$	0.0025	0.1644	-0.1153**	-0.0109	-0.0010**	0.0935
	(0.0025)	(0.1061)	(0.0528)	(0.0078)	(0.0005)	(0.0665)
A_k	-0.0008***	0.1084***	0.0663***	-0.0004**	-0.0000*	-0.0531***
	(0.0002)	(0.0258)	(0.0212)	(0.0002)	(0.0000)	(0.0104)
R^2	0.0575	0.0616	0.0569	0.0590	0.0600	0.0212

Notes: The table reports results from LPM estimates of extension of model (1) where the treatment rates are interacted with a network-level variable A_k. Each variable A_k is a network-level average that is computed based on characteristics from *all* households in a network. All estimates are based on 14,787 observations from 3,764 networks. A constant term is included but estimates are not reported. Standard errors, clustered at the network level, are in parentheses. ***/**/* indicates significance at the 1%/5%/10%-level, respectively.

Table A.6: Local treatment concentration – Pooled mailing treatments

	LCDummies		IH-Index		IH-Dummies	
	(1)	(2)	(3)	(4)	(5)	(6)
Mailing_k	0.3208***	0.3149***	0.2264***	0.2445***	0.3233***	0.3300***
	(0.0368)	(0.0366)	(0.0379)	(0.0596)	(0.0390)	(0.0417)
Total_k	-0.0387	-0.0311	-0.0413	-0.0585	-0.0656*	-0.0722*
	(0.0329)	(0.0328)	(0.0329)	(0.0561)	(0.0352)	(0.0379)
$\text{Mailing}_k \times \text{LCD}_k^{mail}$	-0.0840***	-0.0859***				
	(0.0299)	(0.0299)				
LCD_k^{mail}	-0.0080	0.0097				
	(0.0088)	(0.0112)				
LCD_k^{exp}		-0.0200***				
		(0.0077)				
$\text{Mailing}_k \times \text{IH}_k^{mail}$			-0.0372	-0.0359		
			(0.0434)	(0.0438)		
IH_k^{mail}			-0.0579*	-0.0360		
			(0.0329)	(0.0619)		
IH_k^{exp}				-0.0223		
				(0.0584)		
$\text{Mailing}_k \times \text{IHD}_k^{mail}$					-0.1347***	-0.1327***
					(0.0388)	(0.0391)
IHD_k^{mail}					0.0134	0.0186
					(0.0088)	(0.0126)
IHD_k^{exp}						-0.0064
						(0.0129)
R^2	0.0572	0.0577	0.0563	0.0564	0.0554	0.0554

Notes: The table replicates Table 9 by pooling the two mailing treatment rates into one (i.e., $\text{Mailing}_k = \text{Base}_k + \text{Threat}_k$). All estimates are based on 14,787 observations from 3,764 networks. A constant term is included but estimates are not reported. Standard errors, clustered at the network level, are in parentheses. ***/**/* indicates significance at the 1%/5%/10%-level, respectively.

Table A.7: How local are spillovers? Threshold distance z for 25m, 75m and 100m

	25 meters		75 meters		100 meters	
	(1)	(2)	(3)	(4)	(5)	(6)
Base^{FON}	0.2479***	0.2530***	0.2849***	0.2824***	0.2905***	0.2886***
	(0.0337)	(0.0342)	(0.0565)	(0.0566)	(0.0547)	(0.0548)
Base^{SON}	0.0850	0.0934	0.0768	0.0717	0.2241**	0.2198**
	(0.0951)	(0.0956)	(0.0807)	(0.0809)	(0.0895)	(0.0896)
Base^{HON}	0.2981*	0.3079*	0.1101	0.1015	0.1475*	0.1417*
	(0.1726)	(0.1730)	(0.0831)	(0.0832)	(0.0828)	(0.0831)
Threat^{FON}	0.3145***	0.3191***	0.3924***	0.3935***	0.3979***	0.3997***
	(0.0343)	(0.0349)	(0.0583)	(0.0585)	(0.0562)	(0.0563)
Threat^{SON}	0.1932*	0.2036*	0.3588***	0.3598***	0.3347***	0.3382***
	(0.1053)	(0.1059)	(0.0937)	(0.0939)	(0.0955)	(0.0957)
Threat^{HON}	0.0384	0.0526	0.3321***	0.3348***	0.3479***	0.3523***
	(0.1531)	(0.1536)	(0.0918)	(0.0923)	(0.0893)	(0.0897)
Total	0.0146	0.0121	-0.0786	-0.0793	-0.0993**	-0.0996**
	(0.0314)	(0.0316)	(0.0509)	(0.0509)	(0.0463)	(0.0463)
Distance to nearest Base	-	0.0000	-	-0.0000*	-	-0.0000
		(0.0000)		(0.0000)		(0.0000)
Distance to nearest Threat	-	0.0000	-	0.0000	-	0.0000
		(0.0000)		(0.0000)		(0.0000)
F-tests:						
Base^{FON}=Base^{SON}	3.15	3.01	10.37	10.61	0.69	0.75
Base^{FON}=Base^{HON}	0.09	0.10	6.78	7.25	3.90	4.10
Base^{SON}=Base^{HON}	1.23	1.25	0.14	0.11	0.55	0.58
Threat^{FON}=Threat^{SON}	1.35	1.22	0.58	0.17	0.53	0.50
Threat^{FON}=Threat^{HON}	3.36	3.12	0.17	0.55	0.41	0.37
Threat^{SON}=Threat^{HON}	0.70	0.66	0.06	0.05	0.01	0.02
Observations	5,236	5,236	23,473	23,473	29,012	29,012
Networks	3,198	3,198	3,319	3,319	2,990	2,990
R^2	0.0796	0.0799	0.0394	0.0395	0.0275	0.0276

Notes: The table replicates Table 10 for threshold distances z equal to 25m, 75m and 100m. Standard errors, clustered at the network level, are in parentheses. ***/**/* indicates significance at the 1%/5%/10%-level, respectively.

Appendix B Theoretical Background

In this section we provide more background on the theoretical model from Section 6. We first highlight how our basic estimation equation (1) can be understood in terms of the consensus belief p^* (section Appendix B.1). We then discuss the different network characteristics (Appendix B.2) and local concentration measures in more detail (Appendix B.3). Finally we discuss how local we should expect spillovers to be (Appendix B.4).

Appendix B.1 Long Run Consensus

Remember the updating process described in Section 6, equation (2). DeGroot (1974) and DeMarzo et al. (2003) have shown that, under mild conditions on the network structure, updating processes as the one described in equation (2) converge to a consensus propensity $p_i^* = p^*, \forall i \in \mathcal{N}$ which is shared by all agents in the network. The consensus propensity will be a weighted average of all households pre-communication propensity p^0, i.e.,

$$p^* = \sum_{j \in \mathcal{N}} \omega_j p_j^0, \tag{B.1}$$

where the weights ω_j are determined by (i) the initial weights λ_{ij} in updating rule (2), and (ii) the network position of household j and all other households.

Can our estimation equation from (1) be understood in terms of the long-run consensus described in equation (B.1)? To answer this question, let us focus on untreated non-compliant households (type II). We are interested in understanding their compliance decision

$$y_i = \begin{cases} 1 & \text{if } p_i^* \geq \widehat{p_i} \\ 0 & \text{else} \end{cases} \tag{B.2}$$

If we assume that households of the same category $\tau \in \{a, b, c, d\}$ share the same prior, then we can rewrite equation (B.1) as follows

$$p_i^* = \sum_{j \neq i : \tau(j) = a} \omega_j \frac{p_a^0}{N} + \sum_{j \neq i : \tau(j) = b} \omega_j \frac{p_b^0}{N} + \sum_{j \neq i : \tau(j) = c} \omega_j \frac{p_c^0}{N} + \sum_{j \neq i : \tau(j) = d} \omega_j \frac{p_d^0}{N}, \tag{B.3}$$

where $p_\tau^0, \tau = a, b, c, d$ is the prior of households in category τ and $\tau(j)$ is the category household j falls into. This can be rewritten as

$$p_i^* = \overline{\omega_a} p_a^0 \frac{\sum_{j \neq i : \tau_j = a}}{N} + \overline{\omega_b} p_b^0 \frac{\sum_{j \neq i : \tau_j = b}}{N} + \overline{\omega_c} p_c^0 \frac{\sum_{j \neq i : \tau_j = c}}{N} + \overline{\omega_d} p_d^0 \left(1 - \frac{\sum_{j \neq i : \tau_j \in \{a,b,c\}}}{N}\right), \tag{B.4}$$

where $\overline{\omega_\tau} = \sum_{j \neq i : \tau(j) = \tau} \omega_j$. The coefficients from our basic estimation equation from Section 5,

$$y_i = \alpha + \beta_0 Total + \beta_1 Base + \beta_2 Threat + \epsilon_i,$$

with $Threat = \frac{\sum_a}{N-1}$, $Base = \frac{\sum_b}{N-1}$ and $Total = \frac{\sum_{a,b,c}}{N-1}$, then reflect a joint effect of the weights and pre-communication propensities of the different categories of households. Note that the theoretical model uses rates defined over N. However, these rates would not be suitable for our empirical analysis (as pointed out above, e.g., in footnote 16). To see this, recall that we model the decision of an untreated, non-compliant households i. As such a type II household is never treated by definition, treatment-ratios defined over the full network size N (rather than $N-1$) would vary between 0 and $\frac{N-1}{N}$. Hence, these alternative rates would be mechanically correlated with the network size. We avoid this problem by using variables that capture the treatment rates among other households, i.e., we use $N-1$ as a denominator.

If one is willing to make further assumptions on the weights λ_{ij} that households place on their neighbors' (and their own) propensities in the communication process, then more can be said about the weights ω in the long run consensus. For example, if we assume that $\lambda_{ij} = \lambda_j, \forall i, j$, i.e., if all households i (who are neighbors of j) place the same weight on j, then $\frac{\omega_i}{\omega_j} = \frac{\lambda_i \sum_{h \in \mathcal{N}_i} \lambda_h}{\lambda_j \sum_{h \in \mathcal{N}_j} \lambda_h}$. If weights are symmetric, i.e., if $\lambda_{ij} = \lambda_{ji}, \forall i, j$, then $\frac{\omega_i}{\omega_j} = \frac{\sum_{h \in \mathcal{N}_i} \lambda_{hi}}{\sum_{h \in \mathcal{N}_j} \lambda_{hj}}$. Finally, if all weights are equal, i.e., if $\lambda_{ij} = \lambda, \forall i, j$, then $\frac{\omega_i}{\omega_j} = \frac{\text{card}\,\mathcal{N}_i}{\text{card}\,\mathcal{N}_j}$, where card $\mathcal{N}_i$ measures household i's degree, i.e., the number of her FONs. These results can be found in Theorem 7 in DeMarzo et al. (2003). In the latter case, for example, we could interpret the ratio $\frac{\beta_1}{\beta_2}$ as a product of the relative degree of base- and threat- treated households in the network multiplied by the relative strength of priors $\frac{p_b^0}{p_c^0}$. Since the degrees can be observed and the ratio of pre-communication beliefs inferred from direct effects, making such assumptions would allow identification of λ.

Appendix B.2 Network Characteristics

This section defines and explains the network characteristics referred to in Section 6. Recall that we refer to $\mathbf{A} = [\mathbf{a_{ij}}]$ as the adjacency matrix of a network, where $a_{ij} = 1$ if there is a link between households i and j (i.e., if $(i, j) \in \Xi$) and zero otherwise.

(i) **Degree.** The degree of household i is given by the number of its first-order neighbors (FONs), i.e., by the cardinality of the set $\mathcal{N}_i$.

(ii) **Clustering.** The clustering coefficient is the fraction of neighbors of i who are neighbors themselves. The clustering coefficient c_i of household i is defined as follows:

$$c_i = \frac{\sum_{j<k} a_{ij} a_{ik} a_{jk}}{\sum_{j<k} a_{ij} a_{ik}}.$$

(iii) **Eigenvector Centrality.** Eigenvector centrality (EC) is one of several measures that determine the relative importance of a node within a network. The measure assigns relative scores to all nodes in the network, assuming that connections to high-scoring nodes contribute more to the score of the node in question than equal connections to low-scoring nodes. Eigenvector centrality is defined as

$$EC_i = \frac{1}{\lambda} \sum_{j \in N_i} EC_j = \frac{1}{\lambda} \sum_{j \in \mathcal{N}} a_{ij} EC_j.$$

The equality can be rewritten as the eigenvector equation $\mathbf{A}\, EC = \lambda EC$. Newman (2006) shows that only the highest λ satisfies the requirement of entirely positive entries of the vector EC and thus, eigenvector centrality of agent i is uniquely determined as the i^{th} entry of the respective eigenvector EC.

How should these characteristics affect spillovers? In the main text we have argued that eigenvector centrality is of particular relevance in determining the weights of households in the long run consensus and hence the size of spillovers. To see this, recall from equation (4) and our discussion in Section 6.1 that the weights of agents in the final consensus can be expressed by solving the row eigenvector equation $\omega\mathbf{T} = \omega$. Since $\mathbf{T}$ can be interpreted as the transition matrix of a finite, irreducible, and aperiodic Markov chain, it has a unique eigenvalue equal to one and all other eigenvalues with modulus smaller than one. The row eigenvector corresponding to the largest eigenvalue coincides with the vector of weights $\omega = (\omega_1, ..., \omega_N)$. This implies that an agent's weight in the final consensus ω_i is closely related to her eigenvector centrality. In the special case where $\lambda_{ij} = \lambda, \forall i, j$ (see Appendix B.1), an agent's weight in ω_i exactly coincides with her eigenvector centrality.

Appendix B.3 Local Concentration of Intervention

We use three different measures of local concentration. To define these measures, we need to introduce some notation. Define by in_k^τ the average number of neighbors of type-τ agents in network k, who are also of type τ. Define by out_k^τ the average number of neighbors of type-τ agents in network k, who are *not* of type τ. (Obviously, $in_k^\tau + out_k^\tau$ coincides with the average degree of type-τ households in network k, i.e., with their average number of FONs.) We can then define the index $H_k^\tau = \frac{in_k^\tau}{in_k^\tau + out_k^\tau}$ (compare Currarini et al., 2009). Based on this, we can now introduce:

(i) **LC Dummy**: LCD_k^τ equals one if there are at least two households who received a given mailing treatment (baseline or threat) and are also FONs, i.e., if $in_k^\tau > 0$.

(ii) **IH-Index**: The inbreeding homophily index for type τ is given by $IH_k^\tau = \frac{H_k^\tau - \frac{N-1}{N}(\tau - rate_k)}{1 - \frac{N-1}{N}(\tau - rate_k)}$.

(iii) **IH Dummy**: $IHD_k^\tau = 1 \leftrightarrow IH_k^\tau \geq 0$.

The IH-Index is positive if there is homophily and negative if there is heterophily. Note that in small networks the IH-index is slightly biased downwards in the sense that in the case of purely random linking, the expected value of the index would be $\frac{-1}{N-1}$. This expression converges to zero as N becomes large, but it is clearly different from zero (and negative) for small network sizes. The latter fact motivates us to consider the IH Dummy, which indicates simply whether there is homophily in the network or not.

Golub and Jackson (2012) have asked whether local concentration helps or hinders convergence in the deGroot model (DeGroot, 1974). They find that local concentration is detrimental (Theorem 1 in Golub and Jackson, 2012). Several differences between the assumptions underlying their result and our empirical approach should be noted. First, they use a spectral measure of homophily while we work with the inbreeding-homophily index. The latter is more common in empirical work (e.g., Currarini et al., 2009). Second, they need a number of assumptions to show their result. One of these ('comparable densities') requires that the interaction probabilities of different types do not diverge. We cannot ensure this, since in some of our networks some types do not occur (if, e.g., no one was treated with a baseline-letter).

Appendix B.4 How local are spillovers?

Let us now derive some comparative statics for cases where communication ends *before* a consensus is reached. We have assumed that in each round r agents update their beliefs according to equation (2). After one round of communication, household i's compliance propensity is then given by

$$p_i^1 = \frac{\sum_{j\in\mathcal{N}_i} \lambda_{ij}\, p_j^0 + \lambda_{ii}\, p_i^0}{\sum_{j\in\mathcal{N}_i} \lambda_{ij} + \lambda_{ii}}, \tag{B.5}$$

Hence, after one round of communication, the marginal effect from a treated household ℓ's propensity on the propensity of her FON i is given by

$$\frac{\partial p_i^1}{\partial p_\ell^0} = \frac{\lambda_{i\ell}}{\sum_{j\in\mathcal{N}_i} \lambda_{ij} + \lambda_{ii}}. \tag{B.6}$$

After two rounds of communication, i's beliefs will have the following shape:

$$\begin{aligned} p_i^2 \;&=\; \sum_{j\in\mathcal{N}_i} \frac{\lambda_{ij}}{\sum_{j\in\mathcal{N}_i} \lambda_{ij} + \lambda_{ii}} \left(\frac{\sum_{\ell\in\mathcal{N}_j} \lambda_{j\ell}\, p_\ell^0 + \lambda_{jj}\, p_j^0}{\sum_{\ell\in\mathcal{N}_j} \lambda_{j\ell} + \lambda_{jj}} \right) \\ &+\; \frac{\lambda_{ii}}{\sum_{j\in\mathcal{N}_i} \lambda_{ij} + \lambda_{ii}} \left(\frac{\sum_{j\in\mathcal{N}_i} \lambda_{ij}\, p_j^r + \lambda_{ii}\, p_i^r}{\sum_{j\in\mathcal{N}_i} \lambda_{ij} + \lambda_{ii}} \right). \end{aligned} \tag{B.7}$$

After two rounds of communication, the marginal effect of increasing the treated household k's propensity on the propensity of her SON i is thus given by

$$\frac{\partial p_i^2}{\partial p_k^0} = \frac{\sum_{j\in(\mathcal{N}_i\cap\mathcal{N}_k)} \lambda_{ij} \frac{\lambda_{jk}}{\sum_{h\in\mathcal{N}_j} \lambda_{jh} + \lambda_{jj}}}{\sum_{j\in\mathcal{N}_i} \lambda_{ij} + \lambda_{ii}}. \tag{B.8}$$

This derivative is composed of two effects: first, the weight that household i places on those FONs who are also linked to the targeted node k (the agents in $\mathcal{N}_i \cap \mathcal{N}_k$) and, second, the weight that these FONs place on k. Hence the size of the effect will depend on how many agents j connect i to k (indirectly) and how much weight these agents place on k. Since $\frac{\sum_{j\in(\mathcal{N}_i\cap\mathcal{N}_k)} \lambda_{ij}}{\sum_{j\in\mathcal{N}_i} \lambda_{ij}+\lambda_{ii}} < 1$, it follows that – as long as $\lambda_{ij} = \lambda_{ik}, \forall j, k \neq i$, i.e., as long as agents place the same weight on all their neighbors – the marginal effect on any given SON is smaller than the marginal effect on FONs. The probability that a non-treated household starts to comply should therefore be higher if it is a FON of a treated household compared to when it is a SON of a treated household.[43] By the same logic, the probability that a SON of a treated household starts to comply should *cet. par.* be higher than the probability that a third-order neighbor switches to compliance, etc.

[43]Note that while the marginal effect on any given FONs is always higher than that on a SON, this does not necessarily mean that the spillover among FONs is higher. This will depend on how many FONs and SONs neighbors the treated household has. This again supports estimating a model in rates, such as in equation (1).