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## Compliance and accountability: Evidence from a field experiment in Argentina

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IDB WORKING PAPER SERIES N° IDB-WP-1476

# Compliance and Accountability:

Evidence from a Field Experiment in Argentina

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## Compliance and Accountability:

### Evidence from a Field Experiment in Argentina

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## Abstract<sup>\*</sup>

Does compliance with low-cost civic duties increase demand for social accountability? We address this question by conducting a field experiment at train stations in Buenos Aires. We create exogenous variation in compliance with paying the public transportation fare by i) highlighting sanctions for non-compliance and ii) appealing to compliance norms whereby 90 percent of passengers pay the fare. We find that both sanctions and norms treatments raise compliance. However, only appeals to compliance norms make treated passengers more willing to sign a petition demanding quality public transportation service—our measure of demand for social accountability. To probe the mechanisms explaining these patterns, we show that compliance invoked by adherence to norms makes subjects feel more entitled to demand accountability and trust the government to respect this right to a greater extent. Our findings suggest that raising compliance through appeals to social norms may thus have wider benefits for civic behaviors.

**JEL classifications:** D91, H26, O12

**Keywords:** Compliance, Accountability, Norms, Sanctions, Argentina

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## 1. Introduction

A common paradox of many developing states is that they need revenues more badly than developed states yet do a much poorer job of collecting taxes (Akitoby et al., 2020). One obvious answer to this puzzle is that developing states lack capacity to enforce compliance, which might perpetuate the vicious circle of underdevelopment. However, this explanation is not consistent with the observation that many developing states are fairly capable of exercising tight authoritarian control through repression and surveillance (Hager and Krakowski, 2022), while others voluntarily refrain from collecting (some) taxes by offering a broad range of goods and services for free (Holland, 2015; Chaudhry, 1997; Rawlings and Rubio, 2005).

Another answer to the puzzle points to deliberate choice of not enforcing compliance by some developing states. In doing so, these states could undermine citizens' right to demand social accountability. Consequently, their governments may go unpunished for delivering bad governance and (possibly) engaging in corruption (Paler, 2013; Weigel, 2020). Political historians have long argued that European monarchs who needed tax revenue had to cede political control in exchange for tax compliance (North and Weingast, 1989; Tilly et al., 1992). Conversely, in rentier states, leaders are known to provide goods to citizens for free in exchange for political quiescence (Holland, 2015; Chaudhry, 1997; Waterbury, 1997; Beblawi and Luciani, 1987).

Are non-compliant citizens less likely to demand accountability? We address this question through a novel field experiment that exogenously raises compliance with low-cost civic duties: payment for a train ticket (see Dai et al., 2018). We conduct our experiment at metro train stations located in the Buenos Aires metropolitan area. To raise compliance with fare payment, we appeal to the fear of sanctions and invoke adherence to compliance norms, which have been documented as the most common reasons for people to comply with their basic civic duties around the world.<sup>1</sup> We measure demand for social accountability by eliciting citizens' willingness to sign a petition demanding quality public transportation service. The petition requests the enforcement of the obligation that the public transportation administration has to provide a minimum of services even during strikes, as stipulated by Article 24 of Argentine Law 25877.

Why could compliance breed demand for accountability? We hypothesize that the process follows the “taxation-produces-representation” logic (Peruzzotti and Smulovitz, 2006). The cost-

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<sup>1</sup> See, e.g., Alm et al. (2017), Hallsworth (2014), Bursztyn et al. (2019), Saulitis (2023). Appendix A.1. provides a detailed discussion on the compliance literature.

benefit model (Bates and Donald Lien, 1985), in which citizens care about the price they pay for the government services they receive, predicts that an exogenous increase in the propensity to pay would produce a higher demand for quality public services. Put differently, compliant citizens should be more strongly motivated to invest resources in monitoring the provision of public services and thus hold service providers accountable.

However, the “taxation-produces-representation” logic hinges on the underlying reciprocity mechanism and assumes that actors trust each other to do their part by providing rights in exchange for duties, or vice versa (see Ortega et al., 2016). Based on this premise, we expect that demand for accountability should only increase in response to compliance motivated by reciprocity concerns—embodied in the belief that everyone should do their part, as indirectly invoked by our norms treatment (Lindenberg et al., 2021). The sanctions treatment, by contrast, could signal that if a sanction needs to be highlighted, then perhaps others are not following the norm, thus undermining the expectations of reciprocity and related accountability. We test this hypothesis by creating exogenous variation in compliance through sanctions and reciprocity channels independently. We do so thanks to a novel experimental protocol, to which we turn next.

## **2. Design**

### ***2.1 Setting***

We conducted a field experiment at metro train stations in the Buenos Aires Metropolitan Area between October 4 and December 17, 2021. We exposed metro commuters traveling towards the periphery of the city to messages meant to induce them to pay for the metro ticket. To make readership of the message high, we placed a research assistant at the entry of the train station wearing a T-shirt with the treatment message and handing out flyers to passengers with the same message (see the lower panel of Figure 1 below).

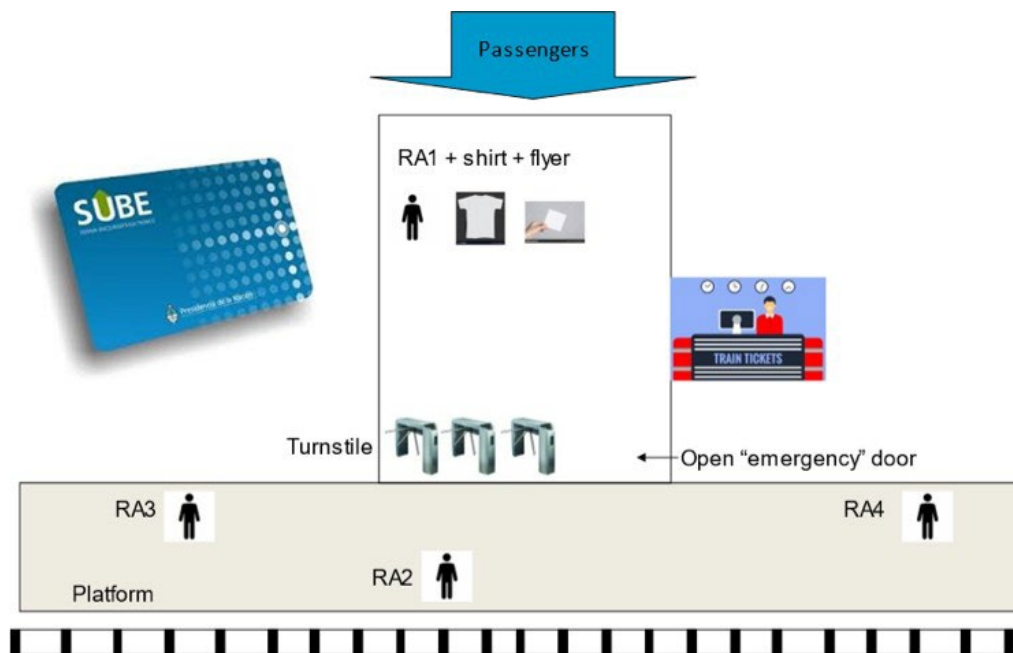
Figure 1 represents a typical train station and shows where each of the research assistants (RA) is located. RA1 is located at the entrance, several meters before passengers decide whether to pay or dodge the fare by entering the platform through the “emergency” door. RA1 delivers the treatment message combining a flyer and T-shirt. (Figure A1 in the Appendix shows some real train stations.) RA2 counts how many people entered the platform through the turnstile (pay fare) and how many people entered through the “emergency” door (dodge the fare). RAs 3 and 4 are in

the platform and conduct a follow-up survey with the population of people who entered the platform.<sup>2</sup>

## 2.2 Types of Treatment

We increase compliance through different channels (fear of sanctions vs. reciprocity concerns) by using two treatment messages which remind passengers that: i) there is a fine in case of evading the ticket (Evite Multas) and ii) most passengers buy the ticket (90% de los Pasajeros Pagan Boleto).<sup>3</sup> The control group is exposed to a research assistant without any message. Each treatment (RA with T-shirt + flyer) lasts for one hour and 15 minutes and is then replaced by another treatment. We assign treatments to station-time units on a rotation basis, using different sequences on different dates (more details on our treatments schedule are available in Appendix A.2).

**Figure 1. Experimental Set-up**



<sup>2</sup> Throughout the fieldwork, we vary the gender of RAs in specific roles. RAs 3 and 4 interview every third passenger entering the platform except for people who looked unambiguously older than 65 years, younger than 16 years, handicapped or wear a police uniform. We excluded these groups because people younger than 16 cannot sign petitions and may be exempt from fare payment (e.g., school children). Likewise, most people older than 65 are pensioners who receive the minimum benefit and do not have to pay the transportation fare. Handicapped individuals and police personnel on duty are also exempt.

<sup>3</sup> This is an estimation of the share of total passengers of public transportation that pay their tickets in the Buenos Aires Metropolitan Area. It includes subway and bus passengers (where fare dodging is almost impossible), and metro train passengers travelling to Buenos Aires downtown, where it is also very difficult to leave the terminal station without paying the fare.



**Figure 1, continued**



*Notes:* The upper panel shows the experimental set-up, outlining the position of our research assistants, those delivering the treatment and those collecting compliance and survey data. The lower panel shows one of our research assistant while he delivers Treatment 1 that appeals to norms (on the left) and Treatment 2 that appeals to the fear of sanctions (on the right).

### **2.3 Sample**

There are more than 150 stations on the seven train lines in the Buenos Aires Metropolitan Area, as shown in Figure A2 in the Appendix. We randomly select train stations, and assign them to each date, from the population of stations that ex ante meet the conditions for conducting the experiment. Each week we cover a different train line. Our original plan was to visit one station per day over 11 weeks (i.e., 55 train stations). In some cases, however, a revenue protection officer arrived at the train station while we were conducting the experiment, closed the emergency door, and actively enforced fare payment. Under these circumstances, passengers did not have the option of dodging the fare. The conditions for conducting the experiment were therefore not fulfilled, and we thus immediately ended the experiment and moved to a different train station. In sum, we collect data on 62 different train stations (Appendix A.3 provides a full list and a map).

## **2.4 Outcomes**

Inside the station, a research assistant checks whether the entering passengers bought a ticket (RA2), and two additional researchers (RA3 and RA4) ask passengers to complete a very short survey while waiting on the platform for their train. The survey includes questions about age, sex, and, crucially, whether the passenger is willing to sign a petition demanding the enforcement of a law which stipulates that a minimum of public transportation service should be provided even during strikes (for the exact wording of the question, see Appendix A.5). Signing the petition is our behavioral measure of respondents' demand for social accountability. RA3 and RA4 also recorded whether a given interviewee paid the fare. We compare their compliance estimates based on the individual-level data, with aggregate-level counts collected by RA2 (see Appendix A.7).

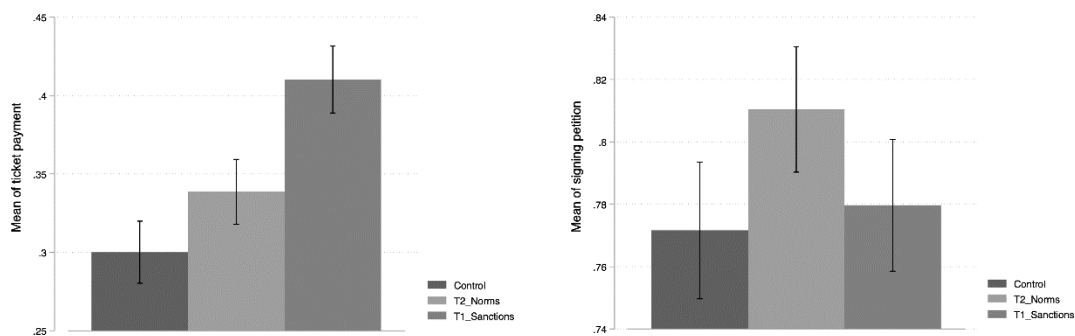
In the course of the fieldwork, some respondents refused to answer our survey and we were thus unable to measure their outcomes (beyond independently observable compliance with fare payment and basic demographics). We address this issue in three steps. First, we investigate whether response rates differ by treatment categories, detecting evidence of differential attrition (Figure A4). Second, we address the problem of differential attrition by estimating a series of selection models, following Heckman (1976) and related models with endogenous treatment and sample selection (see columns 4–6 of Table 1). Third, we impute extreme values on our petition variable for the non-respondents and examine how sensitive our estimates are to these imputations (Figure A5). Reassuringly, we find consistent results across these tests.

## **3. Results**

We divide the discussion of our empirical results in two parts. First, we analyze how the propensity to pay the fare varies by treatment groups. Second, we exploit the exogenous change in the propensity to pay the fare to study its effects on demand for social accountability. In all the forthcoming analyses, we restrict our sample to people 16 to 65 years of age ( $N=7,627$ ). We do so because people younger than 16 cannot sign petitions and most people older than 65 do not have to pay the transportation fare (see footnote 2). Yet, our results are robust to including these people ( $N=168$ ) in the analytical sample. Figure 2 (left panel) compares the means of fare payment by treatment conditions.

Only 31.5 percent of passengers in the control group bought tickets. During Treatment 1 (“avoid fines,” the sanctions message), the share paying was 43.7 percent; and during Treatment 2 (“90% passengers pay their tickets,” the norms message), the share paying was 36.9 percent. Differences between these treatment groups and the control group are statistically significant at the 1 percent level (see Table A2 in the Appendix). In sum, the descriptive analysis suggests that our interventions raised compliance, as expected. These patterns are confirmed in the regression framework in which we control for respondent and enumerator-level covariates and include station and schedule fixed effects (Table A3).<sup>4</sup>

**Figure 2. Treatment, Compliance, and Demand for Social Accountability**



*Notes:* The figure shows the means and the accompanying 95 percent confidence intervals of the indicated outcomes by treatment assignment status.

In the next step, we examine the effects of treatment on our measure of demand for social accountability: signing the petition (SignPetition, the mean of 0.787). Figure 2 (right panel) shows that passengers exposed to our norms treatment are more willing to sign the petition compared to the control condition. In line with our expectations, the sanctions treatment does not have the same effect. We confirm these descriptive patterns by estimating a two-least square (2SLS) instrumental variable (IV) regression. We use the two different treatments as instruments of compliance in separate IV models. The models control for age, age squared, sex, sex of the interviewer, and include train station and schedule fixed effects. In addition, we estimate a reduced form model in which we regress the petition outcome directly on the exogenous treatment assignment (and covariates).

<sup>4</sup> We use these controls to increase the precision of our estimates. Table A2 shows that the covariates are balance with respect to the treatment assignment, including the age and sex of the passenger, and the sex of the interviewer.

Columns 1-3 of Table 1 present the results of these analyses. Paying the fare has a positive and significant effect on signing the petition when we instrument compliance with the treatment appealing to norms (column 3). It does not have the same effect when we instrument compliance with the treatment appealing to the fear of sanctions (column 2). These results are confirmed in the reduced-form model (column 1).<sup>5</sup>

**Table 1. Treatment, Compliance, and Signing the Petition**

	(1) Reduced form	(2) IV = T1 Sanctions	(3) IV = T2 Norms	(4) Reduced form (Heckman)	(5) IV = T1 Sanctions (-eprob-)	(6) IV = T2 Norms (-eprob-)
T1_Sanctions	0.009 (0.016)			0.080 (0.054)		
T2_Norms	0.037** (0.016)			0.168*** (0.047)		
Paid_ticket		0.099 (0.129)	0.600** (0.284)		0.365* (0.188)	0.053*** (0.012)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	4360	2894	2876	6126	4106	4083

*Notes:* Robust standard errors in parenthesis. Significance level shown below \*p<0.10, \*\* p<0.05, \*\*\*p<0.01.

#### 4. Falsification Tests

The validity of our IV estimates hinges on the five assumptions we discuss in Appendix A.10. A key concern is the excludability assumption, which posits that our treatments affect outcomes only through the hypothesized channel of increased compliance. Yet, the treatment messages could have a direct effect on signing the petition, independent of buying the metro ticket. Perhaps, the information about high levels of civic compliance (“90% of passengers pay the ticket”) updates people’s beliefs of collective efficacy in general, thus making “conditional cooperators” more willing to act prosocially (see Appendix A.1 and Bicchieri, 2005).

To address these concerns, we conducted two falsification tests. First, during the last three weeks of our fieldwork, we modified the second part of the experiment. Instead of asking

<sup>5</sup> In addition, columns 3-6 of Table 1 address the above-mentioned problem of sample selection. Details about these models are available in Appendix A.9.

passengers whether they would like to sign a petition demanding quality public transportation service, we asked whether they would like to sign a petition demanding the prohibition of using animals for experimental purposes (see Appendix A.5). According to the social accountability logic outlined above, ticket payment should be unrelated to signing the petition requesting a ban on animal testing if the excludability assumption is met. Simply put, the ban on animal testing petition is unrelated to the quality of public services. If, however, the excludability assumption is violated, we could observe an effect of ticket payment on signing the animal testing petition, for instance, due to the fact that our treatment raises expectations of collective efficacy more broadly.

Table A5 replicates the models from Table 1 (columns 1-3), but using data from the final three weeks of the fieldwork. The dependent variable is an indicator equal to 1 if the person signs the petition to ban animal testing, and 0 if they explicitly choose not to sign (SignPetitionFT, the mean of 0.777). The table shows no effect on signing the animal testing petition. This result builds confidence in the validity of the excludability assumptions.

Our second falsification test makes use of a new sample of university students from the University of Buenos Aires. These students are exposed to our treatment messages during an online survey. We incentivize the participation in this survey by offering the students a chance to win a T-shirt in the lottery. The T-shirts are the same as we used in the main experiment: they contain i) the sanctions message, ii) the compliance norms message, and iii) no message at all (see Figure A7). We randomly show one of these T-shirts to the survey participants as a preview their potential prize. After showing them these T-shirts, we ask them whether they are willing to sign the petition demanding the enforcement of the law which stipulates that a minimum of public transportation service should be provided even during strikes.

The crucial difference between the online and station-based experiments is that the online treatments cannot affect actual compliance behavior: there is no fare to pay. Therefore, if our treatments have any effect on propensity to sign the petition online, this would indicate that the exclusion restriction is likely to be violated. Figure A6 shows that there are no differences in signing the petition across treatment groups in the online experiment. This finding thus underscores the plausibility of our preferred interpretation of the offline findings; namely, that the effect of treatment passes through actual compliance.

## 5. Mechanisms

How can we explain the findings thus far? We argued that compliance motivated by reciprocity concerns—which we exogenously varied thanks to the norms treatment—should strengthen citizens’ perception of having the right to demand accountability and make citizens trust the government to respect this right more. We measure the former outcome in the final three weeks of our experiment, while trust in the government is measured in the initial eight weeks.<sup>6</sup> Table 2 shows that the norms treatment increases perceptions of one’s right to demand accountability (columns 1 and 3) and make compliant citizens more trustful in the government in general (columns 4 and 6). This pattern holds true in the full IV regressions as well as in the reduced-form models. The sanctions treatment does not have the same effects.

Next, we explore two additional implications of the above mechanism. First, if our mechanism is correct, we should expect that the reported effects of compliance on demand for accountability are stronger in wealthier neighborhoods. The relative wealth of those areas could signal to the residents that the government is capable of providing well-being to the citizens, thus making it warranted to trust the government. We measure neighborhood’s wealth with indicators of night-time luminosity and population density in the area surrounding a given metro-train station. The results are presented in Figures A8 and A9, confirming that the reported effects are concentrated in wealthier neighborhoods.

Second, we expect that the reciprocity channel (rights in exchange for duties) should be stronger among women than men. Women have been consistently shown to be more egalitarian and more worried about fairness than men (Croson and Gneezy, 2009; Mutz and Lee, 2020), and they should thus respond to the “taxation produces representation” logic more promptly. In Figure A10, we find that compliance induced by the invocation of norms increases demand for accountability among women, but not among men. This pattern is therefore consistent with the proposed mechanisms.

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<sup>6</sup> We could not measure all outcomes at the same time because our enumerators had extremely little time to interview passengers waiting on the platform for their train.

Table 2. Treatment, Compliance, and the Perceived Right to the quality public service

	(1)	(2)	(3)	(4)	(5)	(6)
	<b>Right to quality public service</b>			<b>Trust in the government</b>		
	Reduced form	IV = T1 Sanctions	IV = T2 Norms	Reduced form	IV = T1 Sanctions	IV = T2 Norms
T1_Sanctions	0.122** (0.050)			0.066 (0.101)		
T2_Norms	0.160*** (0.050)			0.231** (0.100)		
Paid_ticket		3.090 (3.095)	1.899* (1.109)		0.400 (0.828)	4.077* (1.951)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	673	435	418	4268	2831	2822

Notes: Robust standard errors in parenthesis. Significance level shown below \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\* $p < 0.01$ .

## 6. Discussion

In this study, we conducted a field experiment at train stations in Buenos Aires to estimate the effect of civic compliance on demand for social accountability. We found that appealing to the fear of sanctions and reciprocity norms increases compliance with paying the public transportation fare. However, only appeals to norms also make passengers more willing to sign the petition demanding quality public services—an important indication of their propensity to hold their officials accountable. Our findings suggest that raising compliance through appeals to reciprocity norms may have wider societal benefits, compared to interventions inducing compliance through the threat of sanctions.

These patterns point to important heterogeneities in the documented effects, and we speculate that different types of individuals might respond to each of our treatments. We suspect that individuals who fit the “homo economicus” type are likely to change their propensity to pay the fare when exposed to the sanctions treatment (even if it signals that others may not be paying). They do so because of their rational self-interested behavior, and thus paying the fare does not affect their likelihood of engaging in a cooperative effort such as the provision of the public good of accountability. By contrast, individuals who react to the invocation of compliance norms are likely to be the “homo reciprocans” type (Fehr and Gächter, 1998). They demand quality public

services after paying the transportation fare because they plausibly recognize the reciprocal logic of this exchange: rights in return for duties, and vice versa.

Our key contribution is micro evidence on the “taxation produces representation” hypothesis and its underlying mechanism (Peruzzotti and Smulovitz, 2006). To the best of our knowledge, our study provides rare causal evidence supporting this proposition with reference to a domain of everyday compliance.<sup>7</sup> We are aware that compliance with civic duties and social accountability includes many actions that are certainly costlier than paying a train ticket or signing a petition. However, we focused on this outcome because it is malleable to weak informational treatments. Our study thus provides evidence-based policy recommendation on cheap and potentially scalable ways of improving everyday forms of compliance and their effect on public-spiritedness at large.

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<sup>7</sup> A variant of this hypothesis that focuses on voting behavior have been more widely studied with the use of (quasi-) experimental methods (see, e.g., Ross, 2004; Paler, 2013; Weigel, 2020).



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## Appendix

### *A.1 Review of the Literature on Compliance*

The literature on compliance has identified two broad categories of explanations of why people comply with their civic duties.<sup>8</sup> These explanations relate to i) sanctions and ii) reciprocity and normative concerns. The first explanation proposes that people comply with civic duties if their non-compliance could be easily detected and punished by legal authorities (Alm et al., 2012; Alm, 2012; Kirchler et al., 2008; Ritsatos, 2014; Torgler and Werner, 2005; Katz and Owen, 2013; Riahi-Belkaoui, 2004).

The second explanation posits that people comply with civic duties because they trust the government to convert their compliance into a basis for a well-functioning society that provides high-quality services to its citizenry (Nurkholis et al., 2020; Andriani, 2016; Chan et al., 2017; Daude et al., 2012; Ortega et al., 2016; Torgler, 2003; Levi and Stoker, 2000; Hosseini Kondelaji et al., 2016; Ibrahim et al., 2015; Leonardo, 2011). Another version of this explanations suggests that people comply with civic duties because they are influenced by social and moral norms of their communities that stigmatize non-compliance and other forms of uncivic behavior (Andreoni et al., 1998; Falk and Fischbacher, 2006; Fergusson et al., 2019; Alm et al., 2017; Ritsatos, 2014; Kirchler et al., 2010; Frey and Torgler, 2007; Pruckner and Sausgruber, 2013).

Based on the above findings, a series of field experiments has tested the effectiveness of low-cost “nudging” interventions that authorities could use to increase civic compliance through the above channels. Most commonly, these interventions focus on activating the fear of sanctions or appealing to cooperative social norms (Hallsworth, 2014). Castro and Scartascini (2015), for example, find that authorities can increase tax compliance by 5 percentage points by simply emphasizing possible fines related to tax evasion. Hallsworth et al. (2017), in turn, find that appeals to social norms of compliance can allow tax authorities to improve overdue tax collection by a margin of 2-5 percentage points.

Other interventions aimed at triggering the reciprocity mechanism directly (e.g., Coleman, 1996; Slemrod et al., 2001; Dickson et al., 2017; González-Navarro and Quintana-Domeque, 2015). They did so by altering citizens’ beliefs about the (in)capacity of the state to transform civic compliance into a basis of a well-functioning society. In some lab experiments, for example,

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<sup>8</sup> For a recent review, see Mascagni (2018).

participants were informed that all tax revenues a central authority were about to collect were going to be destroyed (Andrighetto et al., 2016; Steinmo and D’Attoma, 2021). In other (field) experiments, public service provision was improved through incentives or additional funding (Armand et al., 2021).

The key difference between appeals to sanctions and social norms lies in the source of norms these interventions refer to. The sanctions approach relies on formal, legal norms, threatening people with audits and fines in the case of non-compliance. The social norms approach, in turn, draws on informal norms, creating peer pressure to conform with civic behaviors of the majority of citizens. The sanctions approach is thus based on threatening messages, evoking fear as a key emotion. The social norms approach, by contrast, relies on more positive messages that induce emotions of guilt or shame in the case of non-compliance (see Elster, 1999).

The literature has compared the relative effectiveness of sanctions and social norms approaches, finding the former to work better (see Hallsworth, 2014; Horodnic, 2018; Dularif and Rustiarini, 2021). Yet, these comparisons focused on the effects of nudging interventions on inducing compliance in specific domains for which these interventions were designed. Nonetheless, the compliance-inducing interventions can have spillover effects in other domains as well (Altmann et al., 2021). Nudging citizens to repay hospital debts at time  $t$  can potentially affect their repayment of other debts or the repayment of similar debts at time  $t + 1$ . To the best of our knowledge, these spillover effects have not yet been tested—a task we undertake in the present study.

One important mechanism through which compliance-inducing interventions in one domain may have spillover effects on other domains of civic life is the accountability logic. According to the “taxation-produces-representation” hypothesis, an exogenous increase in the propensity to pay taxes would produce a higher demand for quality services (Ross, 2004; Peruzzotti and Smulovitz, 2006). Simply put, citizens who comply with civic duties do not want their efforts to be wasted. They thus assume larger responsibility in their societies by engaging in other types of civic behaviors and monitoring public-spiritedness of fellow citizens and their governors (see also Ronconi, 2019).

Importantly, if the accountability mechanism drives the hypothesized spillover effects, one may expect the social norms approach should be more effective in increasing civic behaviors across multiple domains, compared to the sanctions approach. For one, the social norms approach

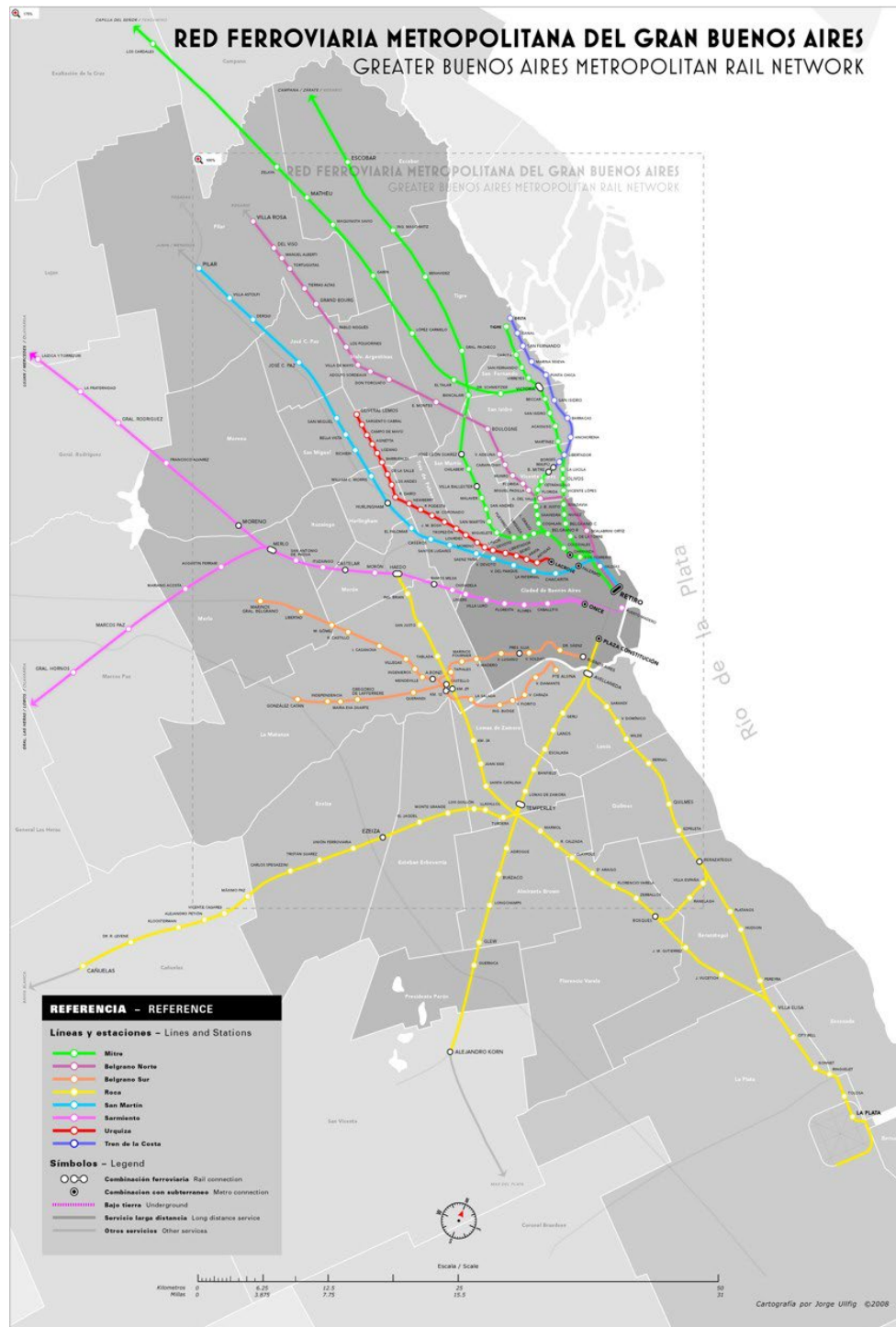
highlights the collective commitment to comply with civic duties, thus raising the expected efficacy of individual contributions. Note that civic engagement is a collective action in which the value of a single contribution increases with the volume of others' contributions. As a result, people tend to cooperate conditionally on others' cooperation (Bicchieri, 2005). The sanctions approach, by contrast, could signal to citizens that if a sanction needs to be highlighted, then perhaps this is because others are not following the norm, thus undermining the expectations of reciprocity and related accountability.

**Figure A1. Beccar (upper panel) and Morón (lower panel) Train Stations**



*Notes:* The pictures show two metro train stations located in Greater Buenos Aires (Beccar and Morón) where the experiment was conducted. Note the turnstiles and the open door nearby.

Figure A2. Map of the Buenos Aires Commuter Rail Network



## ***A.2 Schedule***

We developed the following schedule. On the first day, we began the morning shift (9:00 AM to 1:00 PM) with T1, then T2, and finally C; and repeated the same sequence during the second shift (2:00 PM to 6:00 PM). The sequence for the second day was T2, C, T1; for the third day C, T1, T2; and so on. Table A1 shows the treatments during the first week of work.

**Table A1. Types of Treatments Included in Our Vignettes**

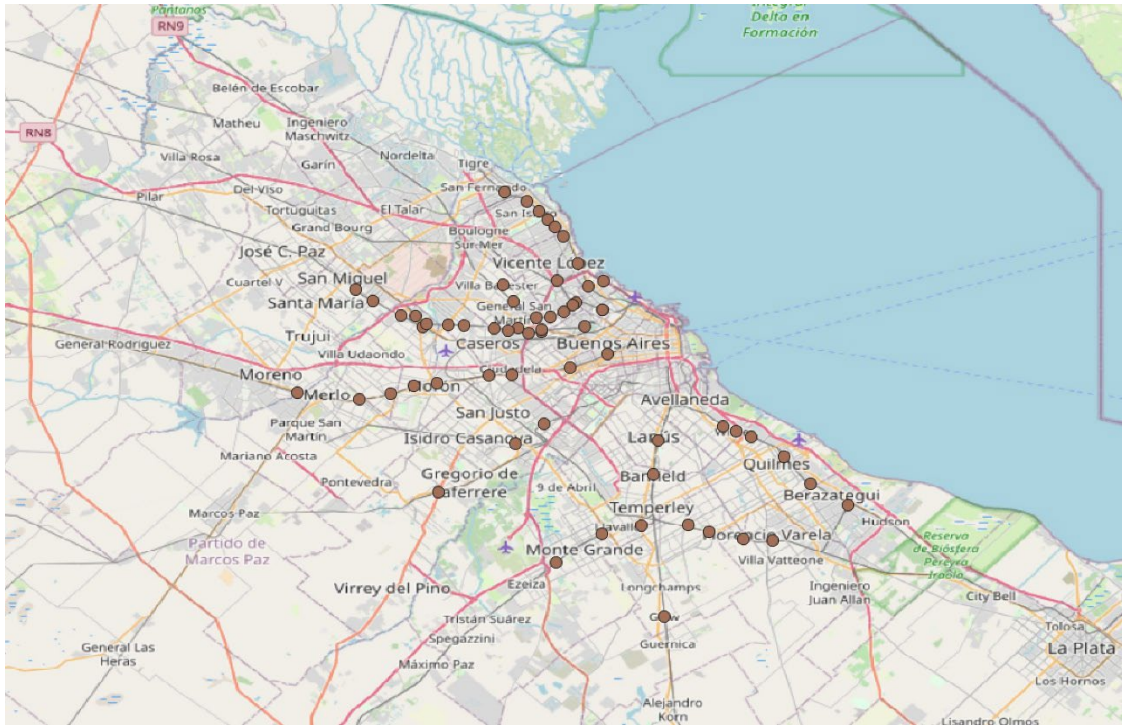
Schedule	Monday Station 1	Tuesday Station 2	Wednesday Station 3	Thursday Station 4	Friday Station 5
9:00 - 10:15 AM	T1	T2	C	T1	T2
10:20 - 11:35 AM	T2	C	T1	T2	C
11:40 - 12:55 AM	C	T1	T2	C	T1
Lunch					
2:00 - 3:15 PM	T1	T2	C	T1	T2
3:20 - 4:35 PM	T2	C	T1	T2	C
4:40 - 5:55 PM	C	T1	T2	C	T1

## ***A.3 Experimental Venues***

We conducted our experiment at the following train stations: A. Devoto, Acassuso, Ardigo, Artigas, Ballester, Banfield, Beccar, Bella Vista, C. Univ., Caballito, Calzada, Carranza, Castelar, Ciudadela, Claypole, Coghlan, Colegiales, Don Bosco, Drago, Ejército Andes, El Jagüel, Ezpeleta, F. Beiro, F. Moreno, F. Varela, Floresta, Fournier, Glew, Guillón, Hurlingham, Independencia, Ingenieros, Ituzaingo, Juan B. Justo, La Lucila, Lanús, Lourdes, Lynch, M. Coronado, Martínez, Miguelete, Morón, Muñiz, Núñez, Padua, Paso del Rey, Podestá, Pueyrredón, Quilmes, Ramos Mejía, Rubén Darío, San Andrés, San Isidro, Tropezón, Turdera, Urquiza, V. Dominico, Vicente López, Villa España, Virreyes, W. Morris, and Wilde.



### Figure A3. Map of Train Stations Where We Conducted the Experiment



*Notes:* Brown circles indicate locations of our intervention.

#### A.4 Ethics and Pre-registration

While the project was approved by the ethics committee's review at the Universidad de San Martín (Buenos Aires, Argentina), it was not formally pre-registered. We did not do so because we were working under very strict time constraints in the implementation of the experiment related to the Covid-19 pandemic and could not find time to pre-register the study before the roll-out of our intervention.

We believe that the lack of pre-registration in this case does not undermine the transparency and validity of our findings because of two main reasons. First, a research proposal (that is, a file similar to a pre-analysis plan) was made available to the public on the funding agency's website (Latin American and Caribbean Research Network) in 2019, and the file is still available. Our hypotheses, treatments, and measurement strategies are all described there and have not changed in the current manuscript.

Second, the main objective of the project is to contribute to the design of public policies on a topic where little is known; thus, combining a deductive and inductive approach was deemed



convenient and justifiable. In this spirit, we made one change to the ongoing experiment, which we transparently report in the main text and describe in detail below.

The research proposal was written in late 2019 (before the pandemic), but the field experiment started in October 2021 in Argentina, immediately after the reduction in Covid cases made it possible. In the spirit of inductive learning, we made one important adjustment during the data collection process in the spirit of inductive learning. Instead of running the proposed experiment during all of the 11 weeks, we decided to change one question during the last three weeks of the data collection in order to conduct a falsification test. During the initial eight weeks we asked whether the passenger was willing to sign a petition demanding the enforcement of a law which stipulates that a minimum of public transportation service should be provided even during strikes; while during the last three weeks we asked the passengers whether they were willing to sign a petition demanding the prohibition of using animals for experimental purposes (details in Appendix A.5). We made the adjustment to gain confidence about the appropriateness of the excludability restriction. Together with this modification, we also replaced the question on trust in the government that we had in our survey instrument during the initial eight weeks with another item that captures our proposed mechanism: perceived entitlement to demand quality public service (which we implemented in the final three weeks). We report these changes transparently in the main body of the paper.

#### ***A.5 Outcome Questions Wording (in Spanish)***

*Main outcome:*

“Buen día. Soy estudiante de la Universidad de Buenos Aires, ¿está dispuesto o no a firmar esta petición solicitando que el Estado garantice el funcionamiento mínimo de transporte público aun en caso de huelga?” [referring to Argentine Law 25877, article 24]

*Falsification outcome:*

“Buen día. Soy estudiante de la Universidad de Buenos Aires, ¿está dispuesto o no a firmar esta petición solicitando que el Estado prohíba los experimentos con animales para el desarrollo de productos domésticos y medicamentos?”

## A.6 Descriptive Statistics

**Table A2. Descriptive Statistics**

Group	Observations	Paid ticket	Age	Female	Female interviewer
Control	2579	0.315	35.879	0.522	0.772
Treatment_1	2525	0.437	36.337	0.531	0.790
Treatment_2	2523	0.369	35.794	0.543	0.770
Diff C - T		-0.088***	-0.188	-0.0167	-0.008
Std. Err.		(0.012)	(0.297)	(0.012)	(0.010)

Notes: Significance level shown below \*p<0.10, \*\* p<0.05, \*\*\*p<0.01.

## A.7 Aggregate Compliance Data

In the design section in the main text, we mentioned that one of our research assistants (RA2 on Figure 1, upper panel) collected compliance data at the aggregate level (station-treatment). The total number of people we interviewed individually was 7,795; however, the number of people who entered the station during our experiment was about three times higher: 23,896. As mentioned before, at rush hours, we only interviewed part of people who entered the station, contacting every third passenger. What is more, some people enter the train station less than two minutes before the train arrives, and we did not interview two minutes before the train arrival. In addition, some people who enter the station are part of groups that are exempt from paying the fare, and we did not interview people who appeared to be members of those groups: pensioners, public school students, police, and the handicapped.

The share paying for the ticket is smaller in the aggregate data compared to the interviewed sample (27 percent versus 35 percent, respectively). Importantly, however, we observe the same discrepancy between the aggregate and individual-level data across all three experimental conditions: 24.8 percent paid in the control condition, 30.8 percent in the sanctions condition, and 27.1 percent in the norms condition according to the aggregate data (compare with the individual-level data reported in Table A2). We believe that the most plausible explanation for the lower paying share in the aggregate data is related to the fact that people who enter the station in the last two minutes (and whom we could not interview) usually do not pay. Likewise, RA2 did not exclude from their compliance counts people who appeared to be members of social groups that are exempt from paying the ticket (pensioners, public school students, police, handicapped). The former

reason could lead us to overestimate compliance in the individual-level data; while the latter could make us overestimate non-compliance in the aggregate data—together plausibly accounting for the observed discrepancy.

#### ***A.8 Treatment and Compliance: Detail***

We investigate the effects of treatment on fare dodging by estimating a linear model:

$$P_{ijs} = \alpha_j + \gamma_s + \beta T_{js} + \beta X_i + \epsilon_{ij} \quad (1)$$

where  $P$  is an indicator for paying the train fare of an individual  $i$  in a station  $j$  during a schedule  $s$ ;  $T$  indicates whether the passenger entered the station when there was a treatment message. We distinguish between two types of messages: T1 sanctions message, and T2 social norms message. We evaluate the effects of these treatments with reference to the control group.  $X$  includes the age, age squared and sex of the passenger; and  $\alpha_j$  and  $\gamma_s$  are train station and schedule fixed effects. We use robust standard errors. Table A3 presents the results of these analyses. In column (1) we do not include any controls, in column (2) we add station and schedule fixed effects, and in column (3) we add the vector of passenger characteristics. Finally, in column (4) we include passengers below 16 and above 65 years of age. The results of all the specifications confirm that both sanctions and social norms interventions make people comply with low-cost civic duties at higher rates. The results are substantially unchanged if we use a probit model instead of the linear one.

**Table A3. Treatment Effects on Ticket Payment**

	(1) Paid ticket	(2) Paid ticket	(3) Paid ticket	(4) Paid ticket
T1 (Sanctions)	0.122*** (0.013)	0.111*** (0.013)	0.108*** (0.013)	0.108*** (0.013)
T2 (Norms)	0.054*** (0.013)	0.057*** (0.013)	0.057*** (0.013)	0.056*** (0.013)
Station and schedule FE	No	Yes	Yes	Yes
Passenger controls	No	No	Yes	Yes
N	7627	7627	7627	7795

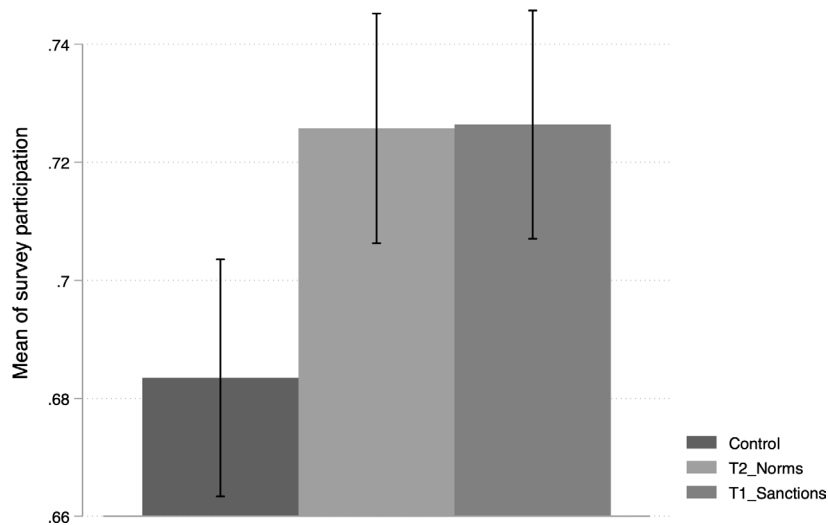
*Notes:* Robust standard errors in parenthesis. Significance level shown below \*p<0.10, \*\* p<0.05, \*\*\*p<0.01.

### A.9 Differential Attrition

In the course of our fieldwork, we approached 7,795 passengers with requests to answer our survey of those, 27.4 percent refused to take part in the survey. As a result, for part of the sample, we were unable to measure the main outcome of interest: willingness to sign the petition.<sup>9</sup> Below, we assess whether the non-response rate varied by treatment assignment. In Figure A4, we compare attrition rates between experimental conditions.

There is clear evidence of differential attrition with subjects exposed to any of our treatment messages being more likely to answer the survey. This is unsurprising, given that people in the control condition were less likely to pay their fare and, as a consequence, could have felt uneasy talking strangers (e.g., if they thought those could reproach their fare evasion). We address the differential attrition problem in two ways: i) by estimating a series of selection models, and ii) by imputing missing values on the outcome variable.

**Figure A4. Attrition by Treatment Condition**



*Notes:* The figure shows the means and the accompanying 95 confidence intervals of the indicated outcome by the treatment assignment status.

<sup>9</sup> Two different approaches can be taken here. The first approach is to focus on willingness to sign the petition as the only dependent variable; and consider non-participation in the survey as a differential attrition problem. This is the approach we follow below. A second approach is to note that participating in a survey conducted by members of a public university is a proxy for civic engagement and social accountability.

First, we estimate a Heckman selection model (Heckman, 1976). The model consists of two equations. The first equation predicts the selection into answering our survey, and thus the observability of the outcome. The second equation regresses the outcome on the covariates of interest. Note that the controls used in Table 1 (columns 1-3) and the treatment assignment, which reduces non-response, as shown above, are included among the covariates in both equations.<sup>10</sup> Importantly, the Heckman model recognizes that unobserved factors (e.g., fearfulness or prosociality) may affect both the outcome and the probability of selection in the sample, thus introducing bias to the estimates of interest. These unobserved factors are contained in the residuals of both equations. According to Heckman, this bias can be corrected in two steps: first, by computing the expected value of the error term from the first equation conditional on the covariates predicting selection in the sample, and second, by including this term in the main empirical model. We implement this correction in column 4 of Table 1. The table presents the result from the second equation of the Heckman model. The model reports reduced-form estimates.

In a similar spirit, we additionally estimate a probit regression that includes an endogenous treatment (fare payment) and accounts for the fact that the data are subject to endogenous sample selection. In this model, we can instrument the endogenous treatment with exposure to our experimental conditions. Again, the model allows us to address the potential problem that unobserved factors that influence the choice to fare payment may be correlated with the unobserved factors that affect the choice of answering our survey. The model includes the same control variables that we use in our main regressions (columns 1-3 of Table 1). The results are consistent with our main findings (column 5-6 of Table 1).

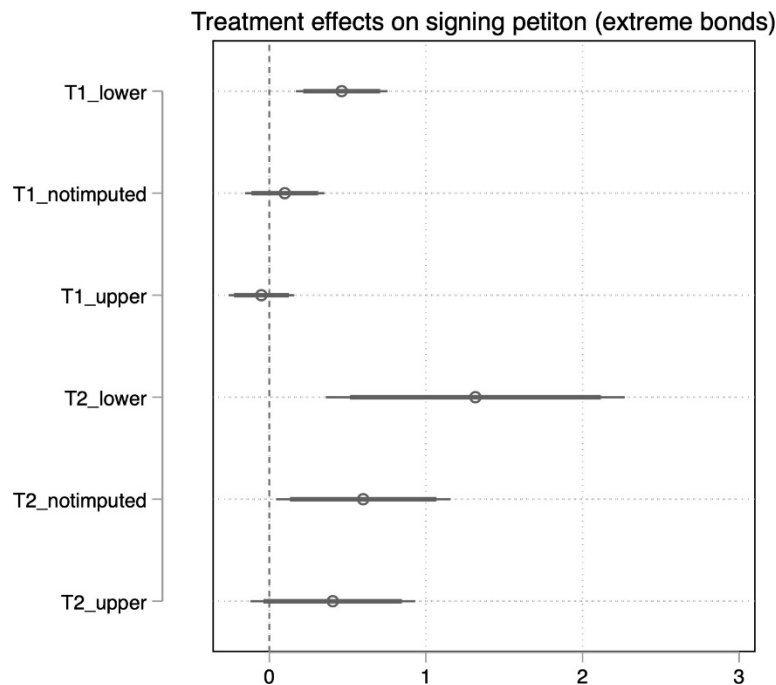
Second, we address the problem of the missing data in the outcome variable by imputing extreme values for respondents who refused to answer our survey. We independently observed these individuals' assignment to treatment as well as their compliance with fare payment. As explained above, our research assistants also approximated these subjects' basic demographic characteristics. To evaluate how sensitive our results are to the loss of non-respondents, we first assume that they were all willing to sign the petition. Such an imputation is likely to underestimate the effect of our treatment. In the second step, we assume that none of the non-respondents was

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<sup>10</sup> Our research assistants estimated gender and age also for subjects who refused to take the survey. Naturally, the estimates of age are more prone to error than the estimates of gender. However, the results are unchanged if we exclude the age variable from the selection models.

willing to sign the petition. This imputation, by contrast, plausibly underestimates the effect of treatment. Figure A5 shows the variation in estimated treatment effects for the observed outcome and the imputed ones (both for upper and lower-bound imputations).

**Figure A5. Treatment, Compliance, and Signing the Petition  
(extreme bounds approach)**



*Notes:* The figure shows the point-estimates and the accompanying 90/95 confidence intervals (thick and thin lines, respectively) of the regression of signing the petition on compliance instrumented with treatment conditions. Rows 1 and 4 present the estimates from the models in which we impute “0” values on the petition variable for respondents who refused to answer our questions. This imputation is likely to overestimate the effect of treatment, given that attrition was larger in the control condition. Rows 3 and 6, by contrast, present the estimates from the models in which we impute “1” values on the petition variable for respondents who refused to take the survey. These results are likely to underestimate the effect of our treatment. Finally, rows 2 and 5 present the results of the models using the original petition variable (without imputation). These models are also reported in columns 2 and 3 of Table 1. For the underlying regression table, see Table A4.

**Table A4. Treatment Effects on Signing the Petition (IV, extreme bounds approach)**

	(1)	(2)	(3)	(4)	(5)	(6)
	<b>IV = T1 (Sanctions)</b>			<b>IV = T2 (Norms)</b>		
	Petition lower-bound	Petition not imputed	Petition upper-bound	Petition lower-bound	Petition not imputed	Petition upper-bound
Paid_ticket	0.462*** (0.149)	0.099 (0.129)	-0.051 (0.106)	1.316*** (0.487)	0.600** (0.284)	0.405 (0.268)
Controls	Yes	Yes	Yes	Yes	Yes	Yes
N	4106	2894	4106	4083	2876	4083

Notes: Robust standard errors in parenthesis. Significance level shown below \*p<0.10, \*\* p<0.05, \*\*\*p<0.01.

#### **A.10 IV Assumptions**

To use our treatment messages as an instrumental variable for compliance, we must invoke five assumptions. First, we assume that our treatment is a relevant instrument of compliance, the pattern we documented in Table A3 (first-stage assumption). Second, we assume that people comply with treatment assignment, that is, they cannot choose to be treated if assigned to a control condition, and vice versa (monotonicity assumption). Third, we assume that there are no spillover effects of our treatment messages onto untreated individuals, e.g., those who travelled at different times of the day in treated stations (stable unit treatment value assumption, SUTVA, assumption). Fourth, we assume the treatment assignment and our outcomes do not have some common unobserved causes (exogeneity assumption). Assumptions (2) to (4) are easily justifiable in our case, given the random assignment to treatments during our field experiment. Fifth, and more problematic, we assume that the treatment affects accountability through its effect on paying the ticket rather than through other channel(s) (excludability assumption). While we cannot directly test this assumption, we propose two falsification tests that help us rule out this possibility (see the section on falsification tests in the main text).

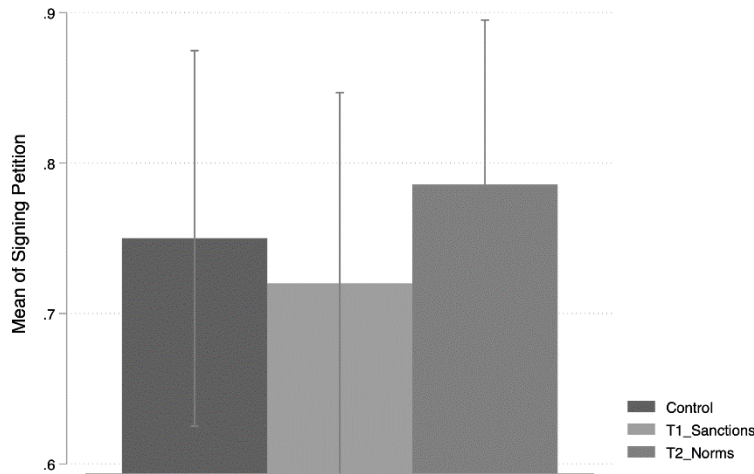
### A.11 Falsification Tests

**Table A5. Treatment, Compliance, and Signing Animal Rights Petition (falsification test)**

	(1) Reduced	(2) IV = T1 IV = T2form Sanctions Norms	(3)
T1_Sanctions	0.024 (0.033)		
T2_Norms	0.005 (0.033)		
Paid_ticket		0.200 (0.353)	0.011 (0.283)
Controls	Yes	Yes	Yes
N	1146	748	772

*Notes:* Robust standard errors in parenthesis. Significance level shown below: \* $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\* $p < 0.01$ .

**Figure A6. Falsification Test in the Online Experiment**



*Notes:* The figure shows the comparison of means of signing the petition across treatment conditions in the follow-up online experiment. Respondents were recruited among students of the University of Buenos Aires (N=154).



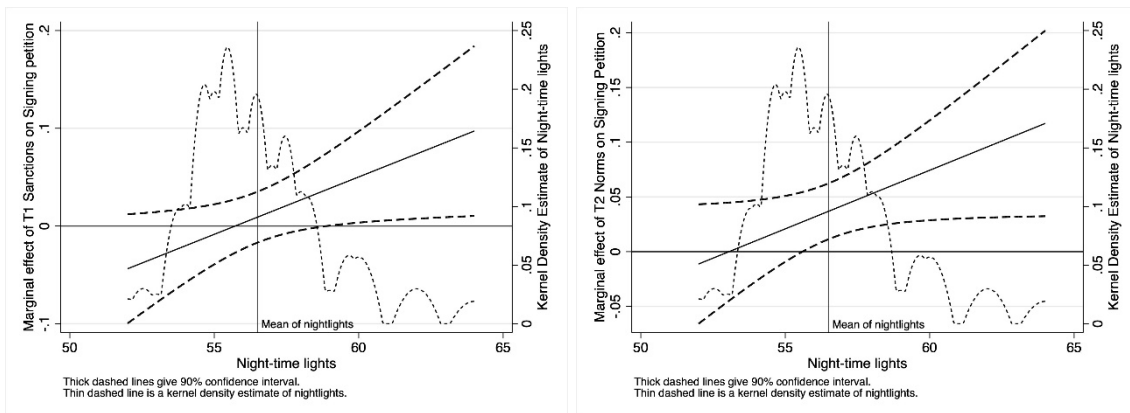
**Figure A7. Treatment Messages Conveyed as a Preview of Lottery Prizes**



*Notes:* The figure shows the T-shirts that participants of the follow-up survey could win in a lottery. They served as our informational treatments in the online experiment. The upper-left T-shirt shows the control message; the upper-right T-shirt shows the T1 (sanctions) message; the bottom T-shirt shows the T2 (norms) message.

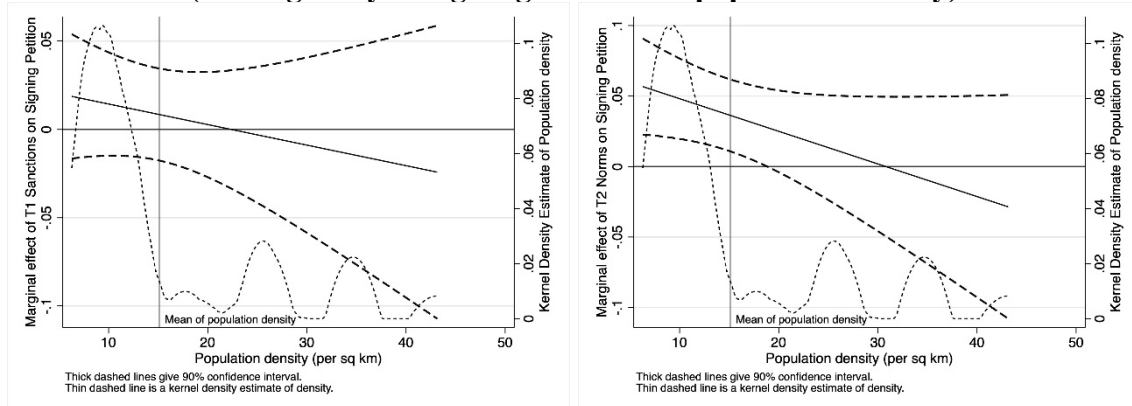
## A.12 Heterogeneity Analyses

**Figure A8. Treatment, Compliance, and Signing the Petition  
(heterogeneity along neighborhood's wealth)**



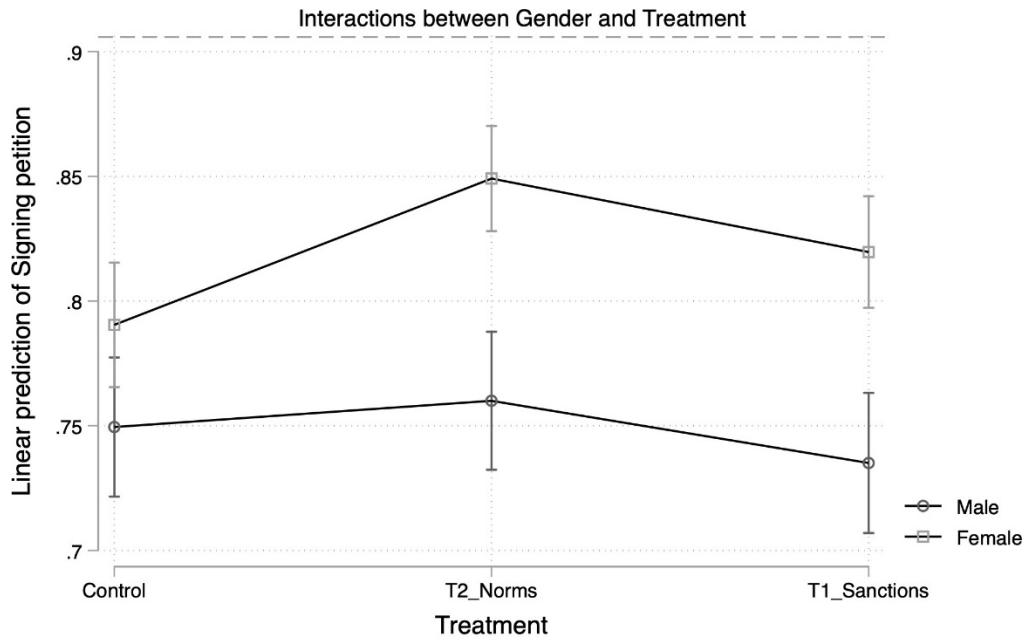
*Notes:* The figure shows the point-estimates and the accompanying 90/95 confidence intervals (thick and thin lines, respectively) of the regression of signing the petition on treatment conditions conditional on neighborhood's wealth (approximated with night-time lights). The left panel shows the effects of T1 Sanctions treatment (vis-à-vis control), while the right panel shows the effects of T2 Norms treatment.

**Figure A9. Treatment, Compliance, and Signing the Petition  
(heterogeneity along neighborhood's population density)**



*Notes:* The figure shows the point-estimates and the accompanying 90/95 confidence intervals (thick and thin lines, respectively) of the regression of signing the petition on treatment conditions conditional on neighborhood's population density. The left panel shows the effects of T1 Sanctions treatment (vis-à-vis control), while the right panel shows the effects of T2 Norms treatment.

**Figure A10. Treatment, Compliance, and Signing the Petition (heterogeneity along gender)**



**Note.** Vertical bars are 90% confidence intervals

*Notes:* The figure shows the point-estimates and the accompanying 90/95 confidence intervals (thick and thin lines, respectively) of the regression of signing the petition on treatment conditions conditional on respondents' gender.

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