Local Public Goods and Property Tax Compliance: Evidence from Residential Street Pavement*

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Abstract

Many developing countries face a challenge whereby low tax compliance limits public goods provision, while inadequate public goods further discourage compliance. This study examines whether improvements in local infrastructure can disrupt this negative feedback loop through a randomized experiment of first-time street pavement in Acayucan, Mexico. Among 56 eligible asphalting projects in poor neighborhoods, 28 were randomly assigned for paving. A theoretical model guides our analysis, highlighting belief updating about government efficiency and reciprocity from direct benefits. We use administrative property tax records to assess compliance responses and survey data to measure satisfaction with local government. Consistent with the direct benefits mechanism, property owners living on newly paved streets increased compliance by 3.2 percentage points (ITT) and 4.8 percentage points (LATE), the latter corresponding to a 5.5% increase in baseline compliance rates. Consistent with belief updating about government efficiency, a one standard deviation increase in exposure to pavement en route to downtown also raises compliance — by 1.5 percentage points (ITT) and 2.6 percentage points (LATE), the latter corresponding to a 3% increase in compliance rates. Consistent with our theoretical model, survey data indicates that individuals with worse initial beliefs about government efficiency exhibit larger belief updating when exposed to pavement projects. A back-of-the-envelope calculation suggests belief updating generated four times more revenue than direct reciprocity.

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1 Introduction

Governments depend on taxes to provide services and public goods. Yet, in many developing countries, there are concerns that low tax compliance and insufficient government funding create a negative feedback loop: citizens resist paying taxes when public goods are absent or of low quality, and governments, in turn, struggle to provide these goods without adequate revenue. This cycle may be responsible for reduced development outcomes worldwide.

This paper examines whether a shock to public good provision enhances tax compliance. Anecdotal and correlational evidence suggests that inadequate public services can reduce citizens' incentives to pay taxes in both developed and developing countries. For instance, in 2010, a Detroit News investigation highlighted the frustration of property owners like Fred Philips, who asked, "Why pay taxes when they aren't supplying services?" Similarly, Afrobarometer data (2019–2021) shows nearly half of surveyed Africans willing to pay more taxes for better services. However, such evidence is merely suggestive, as governments' decisions to provide public goods often correlate with other factors influencing tax compliance.

To address this causal identification challenge, we conducted a randomized experiment involving first-time road paving in underserved neighborhoods of a medium-sized Mexican city. Between 2007 and 2012, the city expanded its paved street network, randomly selecting half of 56 proposed asphalting projects for implementation (Gonzalez-Navarro and Quintana-Domeque 2016). Using administrative property tax records from 2005–2012, we assess whether benefiting from road paving improved tax compliance. Using administrative property tax records (2005–2012), we examine whether road paving improved compliance. Since the city's billing system prevents misreporting (Kleven et al. 2011), we can focus on timely compliance with property tax payments.

We model the decision to pay taxes by building on prior research and the specific context of our study. First, we exclude penalty and punishment considerations (Becker 1968) because property tax enforcement in our setting is virtually absent.[1] Second, we incorporate non-pecuniary (tax morale) factors, emphasizing the role of reciprocity (Besley 2020) when the individual directly benefits from public good

1. Tax compliance in economics is often analyzed within the framework of Becker's crime theory (Becker 1968), which predicts compliance based on a cost-benefit analysis of detection likelihood, penalties, and individual risk aversion (Allingham and Sandmo 1972). However, actual compliance frequently exceeds predictions based solely on these factors (Andreoni, Erard, and Feinstein 1998; Luttmer and Singhal 2014; Dwenger et al. 2016), a pattern observed in our study, where enforcement mechanisms are minimal.

provision—by getting pavement in the street their property lines.[2] This acts as decreasing the effective tax rate when the government delivers public goods.[3] Third, we examine how public goods provision signals the local government's expenditure efficiency. This effect, which we believe is novel in the literature, should arise regardless of a citizen's proximity to newly paved streets, as it depends solely on observing government actions. Empirically, our model allows us to relax the Stable Unit Treatment Value Assumption by predicting two effects: (1) an exposure effect, where compliance increases from observing public goods delivery anywhere in the city, and (2) perceived direct benefit effects, which depend on proximity to the intervention. This framework captures nuanced behavioral responses to public goods provision.

We then proceed to the empirical analysis, using administrative property tax records, measuring exposure to paved streets as the fraction of intersections that cross or run along newly paved streets on the shortest path from a property owner's house to the city center, key destinations, or a composite index of all such routes. Our findings reveal a significant impact of road surfacing on property tax compliance. Property owners adjacent to paved streets experienced a 3.2 percentage points (ITT) and 4.8 percentage points (LATE) increase in property tax compliance. Observing others' streets being paved also boosted compliance: a one-standard-deviation increase in exposure led to a 1.5 percentage point (ITT) and 2.6 percentage point (LATE) increase in compliance. Moreover, using survey data, we find that the effect on government satisfaction was stronger among those with lower baseline satisfaction. This finding aligns with our belief updating mechanism, under the assumption that satisfaction with the local government serves as a proxy for beliefs about government efficiency (e.g. Wang and Kim 2024), as belief updating about government efficiency is more pronounced when the signal is less expected.

To address the concern that exposure may confer direct benefits to individuals traveling through paved streets even when these streets are not adjacent to their properties, we assess the robustness of our findings using an alternative measure of exposure. This measure considers only the fraction of intersections that cross paved streets but are not along the shortest path—i.e., streets that are observed but not used in the individual's trajectory to their destination. We find that results are statistically indistinguishable between the two metrics. Furthermore, we show that

2. Individuals reciprocate government actions, such as the provision of public goods, based on a sense of fairness or altruism rather than external incentives or strategic considerations (Sobel 2005; Besley 2020; Besley, Jensen, and Persson 2023). For example, individuals may view tax payment as fulfilling an implicit social contract with the state, contributing to the collective resources necessary for its effective functioning (Musgrave 1992; Besley 2020), even when evasion appears materially advantageous.

3. This sense of obligation can depend on the perceived fairness of government actions and the quality of services provided. In environments plagued by corruption, individuals may feel justified in withdrawing their cooperation and refusing to comply with tax obligations (Feld and Frey 2007; Besley and Persson 2014; Stantcheva 2021).

while having a paved street in front of a property increases its value, exposure alone has no effect on property values or perceived travel time to the city center. These support our hypothesis that the exposure effect operates through the belief updating channel.

In additional robustness checks, we demonstrate that our results remain robust when using alternative destinations for our measure of exposure, as well as a harmonized index that combines all of them. Moreover, accounting for non-random exposure by recentering exposure measures, following Borusyak and Hull (2023), yields very similar estimates. Finally, an evaluation of selective attrition using the approach in Ghanem, Hirshleifer, and Ortiz-Beccera (2023) confirms that attrition does not compromise the validity of our findings.

We conclude our empirical investigation with a back-of-the-envelope calculation of the fiscal consequences of the street paving intervention. This assessment considers not only the additional revenue from property owners whose streets were directly paved but also from those indirectly exposed to the paving of others' streets. While the absolute levels of tax revenue are understandably small due to the modest size of the property tax, the relative effects are significant and provide valuable insights. Specifically, we document substantial fiscal spillovers: the ratio of increases in property tax revenue obtained from those *not* residing adjacent to newly paved streets but who are exposed to the pavement program, is four times greater than the increase in tax revenue from citizens whose properties directly abut the newly paved streets. This finding underscores the broader fiscal benefits of infrastructure improvements beyond the directly treated areas.

Our paper contributes to three main strands of the literature: the literature on state capacity and taxation, which emphasizes the ability to effectively collect taxes as essential for the proper functioning of modern states and as a key marker of state capacity (Besley 2020; Besley, Jensen, and Persson 2023); the literature on the role of public perception of government spending in incentivizing tax compliance, which highlights how government incompetence or corruption (Besley and Persson 2014) can deter citizens from complying with taxes; and the broader literature on non-pecuniary factors influencing tax compliance, often grouped under the umbrella term of tax morale (Luttmer and Singhal 2014; Pomeranz and Vila-Belda 2019; Slemrod 2019).[4]

Extant studies on non-pecuniary motives for tax compliance rely on interventions in which taxpayers receive letters with randomly varying messages designed

4. These factors include motivations such as honesty, fulfillment of civic duties, altruism, perceived fairness, valuation of government use of tax revenues, adherence to social norms, and peer effects. Despite their importance, these motivations are the hardest to measure and study empirically and, therefore, remain the least well-understood.

to prime specific dimensions of tax morale.[5] Results from these interventions tend to be mixed or null, with messages emphasizing traditional deterrence mechanisms proving more effective at inducing compliance than those appealing to intrinsic motivations (Kleven et al. 2011; Castro and Scartascini 2015; Dwenger et al. 2016; Meiselman 2018; Bérgolo et al. 2017; Antinyan and Asatryan 2024). In a recent review, Slemrod (2019) concludes: "In sum, a plethora of studies have failed to find evidence that appeals to tax morale, defined broadly, affect taxpayer behavior in the short run when delivered via a one-time mailing." As Luttmer and Singhal (2014) note, however, a null effect may result either from tax morale being irrelevant or from these messages failing to effectively alter it (Benabou and Tirole 2011).

Our study diverges from traditional letter-based interventions and informational approaches aimed at increasing the salience of public service provision (Montenbruck 2023) by focusing on the tangible impact of actual changes in public goods provision on tax compliance. Moreover, by analyzing a well-defined type of public good and leveraging granular, street-level project data, we overcome the limitations of prior research that examined larger areas and combined multiple interventions, yielding statistically insignificant results (Brockmeyer, Garfias, and Suárez Serrato 2024). Our study aims to offer new insights into how public goods delivery can break the cycle of low tax compliance and inadequate public services.

The paper proceeds as follows. Section 2 introduces a model of tax compliance to understand how the provision of a local public good affects property tax compliance. Section 3 describes the experimental design, the identification strategy, the data used in our study and defines the measure of exposure. Section 4 presents the econometric specifications and the baseline balance analysis. Section 5 displays the results. Section 6 offers an analysis of the fiscal spillover effects of street paving. Section 7 concludes.

2 A Model of Property Tax Compliance

We propose a static tax compliance model in which we highlight the key role played by perceptions of government expenditure efficiency in the decision to comply. These perceptions of government efficiency are modeled as being directly influenced by observable government actions, which in our specific context refers to expenditures on public good provision.

5. For studies emphasizing the moral duty to pay taxes, see Hallsworth et al. (2017), Meiselman (2018), and Bott et al. (2020). For studies informing taxpayers about public goods recently financed through taxes, see Castro and Scartascini (2015), Chirico et al. (2016), Bérgolo et al. (2017), Hallsworth et al. (2017), Meiselman (2018), Bott et al. (2020), De Neve et al. (2021), Giaccobasso et al. (2022), and Wilson and Rosenzweig (2023). For studies focusing on the compliance behavior of other citizens or fostering social recognition for compliance, see Castro and Scartascini (2015), Hallsworth et al. (2017), Meiselman (2018), Perez-Truglia and Troiano (2018), and Drago, Mengel, and Traxler (2020). A meta-analysis of nudging interventions on tax compliance, based on studies that implement randomized control trials, is provided by Antinyan and Asatryan (2024).

Setup. Consider a continuum of property owners of unit size, indexed by p. Each owner must decide whether to comply with property tax obligations ($C_p = 1$) or evade them ($C_p = 0$), where $\tau_p > 0$ denotes their individual tax liability. The revenue generated from property taxes is allocated towards the provision of local public goods. A central concept introduced in this framework is that taxpayers form an expectation regarding the fraction $\theta \in [0, 1]$ of total public expenditure G that is lost to inefficiencies, including corruption and mismanagement. This parameter, θ, directly affects the availability of funds for public goods and, as such, serves as a measure of government inefficiency. While total expenditure G is assumed to be common knowledge, the extent of inefficiency (θ) remains uncertain. This assumption is particularly relevant in institutional contexts where mayors not allowed immediate reelection, thereby imposing persistent uncertainty regarding the efficiency of government spending. To align closely with the empirical setting under consideration, we further assume that public good provision is limited to newly paved streets, with all non-wasted expenditure directed to this purpose.

Preferences. We assume citizen preferences can be represented by the following utility function u:

$$u(C_p) = y_p - \tau_p C_p + \lambda\Big(1 - \theta_p(E_p)\Big)GC_p + \alpha_p' B_p C_p, \tag{2.1}$$

where y_p denotes gross income, so that $y_p - \tau_p C_p$ reflects the fact that tax compliance reduces disposable income, and hence utility. Note that the tax bill τ_p is only paid when home owners choose to comply ($C_p = 1$).

We depart from the traditional Beckerian framework, which emphasizes fines as a primary deterrent against tax noncompliance, for several reasons. In most developing countries, nonpayment of property taxes does not result in significant legal repercussions, such as home repossession or other forms of enforcement aimed at recovering delinquent taxes. Local governments maintain records of unpaid taxes but rarely, if ever, initiate legal proceedings against noncompliant property owners. The only substantive legal mechanism incentivizing compliance arises during property transactions, where the government mandates the settlement of outstanding tax arrears and associated penalties before registering the property under the new owner's name. Given the absence of stringent legal enforcement, a key question emerges: why do we observe positive rates of compliance with property tax obligations?

To address this puzzle, we draw on the concept of "tax morale", a hypothesis widely explored in the contemporary literature on tax compliance (Luttmer and Singhal 2014). In contexts where enforcement is weak, tax morale can explain why individuals may still choose to pay property taxes due to intrinsic, non-pecuniary

motivations. For example, taxpayers may perceive compliance as an obligation under an implicit social contract with the state, contributing to the collective financial resources necessary for effective governance (Musgrave 1992; Besley 2020). Additionally, some individuals may be willing to pay taxes in exchange for public goods and services, even when the pecuniary benefits of noncompliance exceed those of payment.

Building on Sobel (2005), Besley (2020), and Besley, Jensen, and Persson (2023), we formalize this idea by modeling taxpayer preferences as incorporating an intrinsic utility component, denoted by $\lambda > 0$, which reflects the extent to which individuals value expenditures on public goods. A higher λ implies that an individual derives greater utility from tax payments that contribute to public good provision, serving as a measure of "civic mindedness". However, this civic-mindedness only applies to those who choose to comply ($C_p = 1$).

The key innovation in our model lies in incorporating taxpayers' expectations about government inefficiency, represented by $\theta_p(E_p)$. Specifically, the term $(1 - \theta_p(E_p))$ captures the notion that individuals value only the portion of their tax contributions that they believe is effectively allocated to public goods, adjusting for perceived waste or mismanagement. Consequently, an individual's sense of obligation to comply is contingent upon their perception of governmental efficiency. This framework aligns empirical findings that in environments characterized by widespread corruption, individuals may feel justified in withdrawing tax payments as a rational response to perceived government failures (Feld and Frey 2007; Besley and Persson 2014; Stantcheva 2021).

In summary, compliance with property tax obligations is more likely among individuals who exhibit stronger pro-public good preferences (higher λ), and hold more favorable perceptions of government expenditure efficiency $(1 - \theta_p(E_p))$.

To anticipate the experimental variation we exploit in our empirical strategy, we posit that individuals' expectations regarding θ are influenced by their exposure to public good, specifically the observation of newly paved streets ($E_p \geq 0$). In our framework, "exposure" refers to receiving a positive signal regarding the efficiency of government expenditures on public goods expenditures which shapes individuals' beliefs. A more precise definition of our exposure measure will be provided below.

The final component of equation (2.1), $\alpha_p' B_p C_p$, captures the perceived direct benefits of public good provision as a motivation for tax compliance. It is by now well established that provision of local public goods increases property values (e.g. Gibbons and Machin (2005)). Accordingly, the direct benefits derived from public good provision, represented by B_p, serve as an additional motivation for taxpayers to comply. In the specific example of street pavement, Gonzalez-Navarro and Quintana-Domeque (2016) provide empirical evidence that of substantial property value increases, on the order of 30% following infrastructure improvements. Our

model incorporates this empirically significant relationship, allowing us to account for direct benefits, such as the capitalization of public good improvements into property values, as a potential determinant of compliance behavior. Once again, this reciprocity motive manifests through increased utility for individuals who choose to comply ($C_p = 1$).

In this expression, B_p can be a vector of indicator variables, where each element indicates whether a street within a specific distance ring from the property was paved, and α_p is the corresponding vector of utility weights. This spatial structure can capture the intuition that the benefits of paved streets decline with distance from the property. While receiving direct benefits from public goods is not contingent on complying with property tax payments, the term captures any motivation individuals experience that compels them to comply/reciprocate when such benefits are realized, so we multiply by the term C_p. Importantly, while receiving direct benefits necessarily implies exposure to paved streets, the converse is not true: property owners may observe paved streets without deriving direct benefits from them.

Individual Compliance. Given our setup, the compliance decision is based on a threshold condition. It is optimal to comply if:

$$\theta_p(E_p) \leq 1 - \frac{\tau_p}{\lambda G} + \frac{\alpha_p' B_p}{\lambda G}. \tag{2.2}$$

Rewritten as $\tau_p - \alpha_p' B_p \leq \lambda(1 - \theta_p(E_p))G$, it is easy to see that the condition states that the likelihood of property tax compliance increases individuals' with individuals' perceptions of government efficiency $(1 - \theta_p(E_p))$ and the higher their civic-mindedness term (λ), both of which appear on the right hand side of the expression. Conversely, compliance decreases as the tax liability (τ_p) rises and increases with the perceived direct benefits of public good provision $(\alpha_p' B_p)$. This expression provides a foundation for the puzzle of compliance in the absence of enforcement mechanisms such as inspections or credible penalties (Andreoni, Erard, and Feinstein 1998; Luttmer and Singhal 2014; Dwenger et al. 2016). It underscores the role of non-pecuniary motivations in driving compliance while also highlighting these these intrinsic incentives are reinforced by the expectation that taxes are spent in a socially valuable manner.

Exposure. We now formalize our measure of exposure to newly paved streets. Suppose the city has a total of S unpaved streets. Normalizing the cost of paving a new street to one implies that the total number of streets paved is $(1 - \theta)G \ll S$, where the inequality "much less than" reflects the fact that, in our context, the probability that any unpaved street is paved is low.[6]

6. In Table A.1, we document that the percentage of unpaved streets in Acayucan that obtain pavement during the period of analysis averages 1.3% per year.

Consistent with our experimental design, we assume that all unpaved streets have an equal and independent probability of being selected for paving. For any unpaved street block segment s, we define anindicator variable d_s that takes a value of one if the street gets paved. The probability of this ocurring is given by:

$$\pi \equiv Pr(d_s = 1) = \frac{(1-\theta)G}{S}. \tag{2.3}$$

The probability decreases with θ, indicating fewer paved streets in the presence of greater government inefficiency. Conversely, it increases with G as more resources are available for asphalting projects.

Next, consider a property owner who regularly travels along S_p unpaved streets. We focus on daily transit routes as these represent the subset of the city's street network that individuals consistently observe. Let X_p denote the total length of the owner's commute, defined as the sum of all streets (both paved and unpaved) along their route. For each street s, let the indicator variable $d_{p,s}$ equal one if the street is both newly paved ($d_s = 1$) and part of property owner p's route. We then define the exposure of property owner p to newly paved streets as the fraction of their total trajectory that obtains pavement:

$$E_p = \frac{1}{X_p} \sum_s d_{p,s}. \tag{2.4}$$

Note that the number of newly paved streets observed by property owner p, given by the product $X_p E_p$, follows a Binomial distribution $X_p E_p \mid \theta \sim \text{Binomial}(S_p, \pi)$. This formalization captures how exposure to newly paved streets varies across individuals based on their location in the city and the stochastic nature of the paving process.

Beliefs. Property owners have prior beliefs about the unknown value of θ, which follow a Beta distribution $\theta_p \sim \text{Beta}(\kappa_{p,1}, \kappa_{p,2})$. The shape parameters $\kappa_{p,1}, \kappa_{p,2} > 0$ determine both the mean of the prior, denoted by θ_p^*, and its variance, which decreases with the concentration parameter $\kappa_p \equiv \kappa_{p,1} + \kappa_{p,2}$. This specification is flexible, as the shape parameters are individual-specific and can generate a wide range of prior belief distributions.[7]

Once public expenditure is realized, individuals gain access to new information that enables them to update their beliefs regarding government efficiency. Specifically, the observation of newly paved streets serves as an indicator of a more efficient government. We assume property owners update their beliefs using Bayes' rule and that the ratio G/S is small (i.e., $\pi \ll 1$). Exploiting the properties of conjugate distributions, we show in Appendix A.1 that the posterior expectation of (θ), con-

7. For example, they can generate distributions that are uniform, bell-shaped, skewed, or monotonically increasing or decreasing.

ditional on exposure and X_p, can be approximated by:

$$\theta_p(E_p) \approx \theta_p^* - \frac{\theta_p^* X_p}{\kappa_p} E_p \equiv \theta_p^* - \delta_p E_p, \tag{2.5}$$

where $\delta_p \equiv \frac{\theta_p^* X_p}{\kappa_p} \geq 0$ and the linear approximation error is of order $O((X_p E_p/\kappa_p)^2)$.

Equation (2.5) yields two key implications for how citizens update their beliefs about government efficiency. First, exposure to newly paved streets improves beliefs about government efficiency:

$$\frac{\partial \theta_p(E_p)}{\partial E_p} = -\delta_p \leq 0. \tag{2.6}$$

Since the probability of compliance increases with perceived efficiency $(1 - \theta_p(E_p))$, this updating mechanism provides one channel through which public good provision can foster tax compliance.

Second, when individuals are not exposed to newly paved streets ($E_p = 0$), their posterior beliefs remain unchanged ($\theta_p(0) = \theta_p^*$). This persistence of prior beliefs reflects a low baseline expectation of public good provision—the absence of new infrastructure is viewed as the *status quo* rather than as information about government efficiency.

Direct Benefits. We now specify how direct benefits from public good provision enter the utility function. Our key assumption to be able to separately identify the effects of exposure and direct benefits is that the direct benefits of newly paved streets vary with their proximity to the property. To capture this spatial dimension, we divide each property owner's commuting trajectory into distinct distance rings. For any street s in property owner p's regular commute, let $L(p, s)$ denote the distance between the property and the street, where $L(p, s) = 0$ for streets directly adjacent to the property. We define an indicator variable for each distance ring j:

$$b_{p,j} = \mathbf{1}[\exists s : d_{p,s} = 1 \text{ and } L(p, s) \in [a_{j-1}, a_j)], \tag{2.7}$$

where $[a_{j-1}, a_j)$ defines the boundaries of ring $j \in \{0, 1, \ldots, J\}$ and a_{-1} is set to zero. This indicator equals one if at least one newly paved street exists within a corresponding distance range.

The total direct benefits perceived by property owner p are then given by:

$$\alpha_p' B_p = \sum_j \alpha_{p,j} b_{p,j}, \tag{2.8}$$

where $\alpha_{p,j}$ captures the marginal benefit of having a paved street in ring j. These coefficients are expected to decrease with distance, reflecting the diminishing impact of

street improvements farther from the property. We will show empirical evidence supporting this negative relationship. Moreover, we will show that in our setting—where public goods consist of residential streets in peripheral neighborhoods—only streets paved directly in front of a property ($j = 0$) yield direct benefits large enough to impact compliance.

Updated Compliance Threshold. Substituting equations (2.5) and (2.8) into (2.2), the threshold condition for compliance becomes:

$$\theta_p^* \leq \eta_p + \delta_p E_p + \sum_j \gamma_{p,j} b_{p,j}. \tag{2.9}$$

Here, $\eta_p = 1 - \frac{\tau_p}{\lambda G}$ represents the baseline compliance threshold in the absence of exposure and direct benefits, and $\gamma_{p,j} = \frac{\alpha_{p,j}}{\lambda G}$. Compared to equation (2.2), this formulation explicitly incorporates the prior expectation of government inefficiency (θ_p^*), the realized level of exposure (E_p) and the perceived direct benefits from public good provision, captured through $\sum_j \gamma_{p,j} b_{p,j} = \sum_j \frac{\alpha_{p,j}}{\lambda G} b_{p,j}$.[8]

Testable Implications. Our framework yields three testable predictions that we empirically evaluate using our experimental setup:

1. Property tax compliance increases with greater exposure to street paving, as the observation of public goods provision enhances individuals' beliefs about government efficiency (equation (2.5)): $\delta_p \geq 0$.

2. Direct benefits from paved streets increase compliance, with effects that decay with distance between the property and the improved roads ($\alpha_{p,j}$ decreasing in j): $\alpha_{p,0} \geq \alpha_{p,1} \geq \alpha_{p,2} \geq ... \geq \alpha_{p,J} \geq 0$.

3. The effect of exposure is stronger among property owners who initially hold more pessimistic views about government efficiency, as δ_p increases with θ_p^*.

8. Note that the assumption of exposure effects being independent of distance to the plot and perceived direct benefits varying with distance is crucial for distinguishing the effects of exposure from those of direct benefits. Suppose that both effects were distance-independent, so that the benefit parameters $\alpha_{p,j}$ are constant and equal to α_p across all distance rings and each newly paved street belongs to a different ring from property p's perspective. Then $\sum_j \gamma_{p,j} b_{p,j} = \gamma_p \sum_s d_{p,s}$. Using the definition of exposure from equation (2.4), this simplifies to $\gamma_p X_p E_p$. The compliance condition then becomes:

$$\theta_p^* \leq \eta_p + (\delta_p + \gamma_p X_p) E_p = \eta_p + (\delta_p + \tilde{\gamma}_p) E_p = \eta_p + \tilde{\delta}_p E_p, \tag{2.10}$$

where $\delta_p \equiv \frac{\theta_p^* X_p}{\kappa_p}$ and $\tilde{\gamma}_p \equiv \gamma_p X_p$, so that $\tilde{\delta}_p$ would reflect a combination of effects of exposure and perceived direct benefits.

3 Experimental Design, Data and Measurement

Institutional and Political Context. Acayucan, located in southeastern Veracruz, Mexico, functions as the municipal seat of its namesake municipality, with a population nearing 85,000 in 2010. The city includes a central area with mostly paved streets and outer sections where pavement is gradually implemented. Often, homes are built and occupied many years before the streets are paved. This pattern is common throughout Mexico and Latin America.[9]

In Mexico, municipal governments handle most urban infrastructure responsibilities. Each administration, elected for three years, has significant discretion over budget allocations. This implies that every time a new administration begins, citizens must consider what their beliefs about government expenditure efficiency are. These local governance dynamics set the stage for the experimental intervention described below.

The Local Public Good Experiment. The policy we study is the first-time asphalting of residential non-arterial streets in peripheral neighborhoods of the city. These street projects vary in width from 8 to 15 meters and in length from 210 to 770 meters, with two lanes for traffic and one or two lanes for parking. Typically, each street block has around 16 adjacent properties. The paving materials used are either hot-mix asphalt or portland cement reinforced concrete. Once paved, the municipality is responsible for street maintenance, funded through general revenues.

Figure 1 illustrates an example of a paving project before and after completion. Urban street paving provides numerous benefits: it facilitates movement and access for vehicles, pedestrians, and cyclists, offers parking space, supports commercial deliveries, and enhances the area's visual appeal. Field observations confirmed that congestion was not an issue, consistent with the residential nature of these streets. Despite these benefits, the market does not independently provide street paving due to its nature as a public good, leading to free rider problems that hinder private provision.

9. Gonzalez-Navarro and Quintana-Domeque (2016) provides a more detailed description of the experimental design and the context in which the street pavement experiment took place.

Figure 1: Example of an Asphalting Project Before and After Pavement

(a) Before Pavement (b) After Pavement



Notes: The figure shows a typical treated street, before and after receiving the pavement. Source: Gonzalez-Navarro and Quintana-Domeque (2016).

Facing budget and time constraints, the Acayucan government could only pave a portion of the city's unpaved streets. So under an MOU with the PIs, the public works office identified 56 projects for potential paving. Each project consists of a contiguous sets of streets blocks that, when paved, connect to the existing pavement network. All candidate projects serve populated areas and are deemed eligible only if the streets had never been previously paved.

With the budget allowing for approximately only 28 out of the 56 equally attractive projects, the mayor and city council agreed to have the PIs randomly select the streets to be paved. Figure 2 shows the treatment and control group streets. The paving program was not accompanied by other government initiatives.

Figure 2: Location of the Asphalting Projects in Acayucan by Assignment



Notes:The figure illustrates the locations of streets designated for treatment (blue) and control (red) within Acayucan's street network.

Local governments allocate a portion of their budget to street paving, implying that all unpaved streets have some probability of eventually being paved. However, this probability is small: the percentage of newly paved streets in Acayucan averages 1.3% per year during the period of analysis (see Table A.1). To preserve the integrity of the experimental design, the list of experimental street projects and the subset selected for treatment was not disclosed to the public, ensuring that the expectations of the control group regarding future paving remained unchanged. The identities of selected projects were kept confidential, as budgeting decisions are done on an annual basis, whereas the study spanned multiple years. As a result, the selection process served as an internal guideline rather than a legally binding commitment. Residents were only informed when construction crews arrived, thereby minimizing any anticipation effects. Streets that were not selected for paving did not receive any form of compensation.

Table A.1 and Figure A.1 display the staggered implementation of paving projects from 2007 to 2012. Initially, 19 out of 28 designated streets (67 percent) were paved within the first three years. However, delays due to weather and technical issues prevented the completion of all projects within this period. No projects were completed in 2010, only one in 2011, and six in the final year. These last seven

projects had originally been assigned to remain unpaved, resulting in a situation of two-sided non-compliance.

Property Tax Data. We obtained administrative property tax data for Acayucan from 2005 to 2012. The primary analysis focuses on a balanced panel of 752 plots adjacent to the treatment and control street projects, constituting the experimental sample. Consequently, the total number of plot-year observations is 6,016 (752×8). Since we are interested in the effects on tax compliance holding the owner of the plot constant, we restrict our analysis sample to properties that remained under consistent ownership throughout the study period. As shown in subsection 5.3, there is no evidence of selective attrition attributable to treatment.

The average government-appraised property value in the experimental sample is 138,196 pesos (10,485 USD in 2012). This valuation is 35.7 percent lower than the citywide average, reflecting that the paving projects were primarily located in the poorer outskirts of the city. Notably, cadastral property values remained unchanged during the period 2006-2012, even for newly paved properties, though tax rates were adjusted for inflation. Annual property tax bills averaged 152.9 pesos (11.6 USD in 2012). Our main dependent variable is whether property taxes were paid in the year they were due. At baseline, around 85% of properties complied with on-time tax payments. The absence of post-paving updates to cadastral property valuations implies that any market valuation effects of the intervention constitute a transfer of wealth to benefiting residents. Moreover, this also implies that any observed changes in tax compliance cannot be attributed to changes in the tax bill, as tax liabilities remained unchanged despite increases in market value.

Measuring Exposure and Distance Rings. Our theoretical framework introduces two key measures: The individual's exposure to the newly paved street program, and direct benefits based on the plot's on proximity to the pavement. Here we detail their construction.

For each property p in our experimental sample (properties lining either treated or control streets), we measure exposure as follows: First, we select a destination node in the street network and determine the shortest path between this destination and the property using a distance-weighted algorithm (see Panel (a) of Figure 3 for an example). The total number of nodes along this trajectory corresponds to X_p in our model, which remains constant over time.[10] We then compute two exposure measures:

10. An underlying assumption is that the shortest path does not change due to the paving. This is likely the case since paved streets are located on the outskirts of the city and correspond to residential streets. Moreover, in Section 5.2, we present evidence that owners' perceptions of travel time to the city center remained unchanged, suggesting that their commuting routes did not change or that paving had minimal impact on travel times.

1. Intent-to-treat exposure (E_p^Z): The fraction of nodes along this path that are either on a street assigned for paving or where a street assigned for paving crosses/ends.

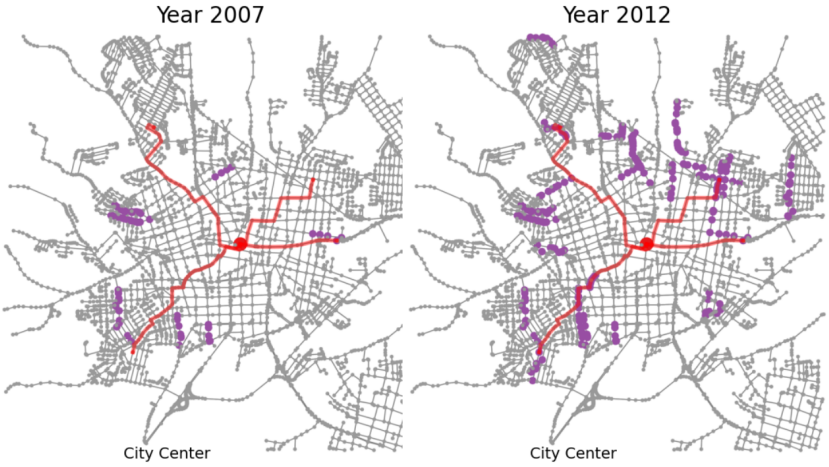
2. Realized exposure ($E_{p,t}$): Similar to the first measure, but based on streets that were actually paved rather than just assigned for paving.

Panel (b) of Figure 3 provides a visual representation of this approach using a single property as an example and realized exposure as the metric. The shortest path, shown in violet, consists of 44 nodes (i.e., $X_p = 44$). The property is located in front of a paved street (depicted in yellow), which counts as an exposed node. The path includes three additional nodes on paved streets, further increasing exposure. Additionally, the path crosses one node where a paved street ends; while this paved street is not part of the shortest path, it is visible and therefore contributes to exposure. In total, 5 out of 44 nodes induce exposure, resulting in a value of $E_{p,t}$ equal to 11.36 percent.

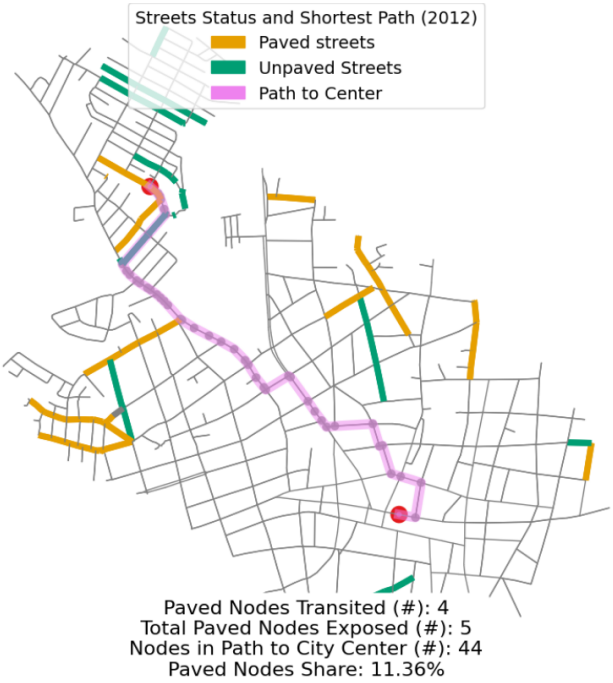
As a robustness check, we use an alternative exposure measure that excludes nodes on streets along the shortest path trajectory, ensuring that only nodes on paved streets that are visible but not directly traversed contribute to exposure. This adjustment addresses concerns that our exposure measure might be conflated with direct benefits from traveling on paved streets.

Figure 3: Construction of the Exposure Measure with the City Center as the Destination Node

(a) Examples of Shortest Path to the City Center



(b) Exposure Measure



Notes: Panel (a) illustrates the construction of the exposure measure using Acayucan's city center as the destination node. The shortest paths from four experimental plots to the city center are computed using a distance-weighted algorithm. Red lines represent the optimal routes, while purple circles denote nodes on streets paved through the intervention. Panel (b) visualizes the exposure measure for a single property. The shortest path from the property (northern red point) to the city center (southern red point) is shown in violet. In this example, the path passes through five (5) paved street nodes: one in front of the property, three along the shortest path, and one that belongs to paved streets that are not part of the shortest path but are visible. The realized exposure measure is 11.36 percent, calculated as the ratio of paved street nodes (5) to total nodes in the path (44) times 100.

Our primary exposure measure uses Acayucan's downtown main square as the destination node. This central plaza, home to both the city hall and main catholic church, functions as the focal point for commercial and administrative activities, making it a natural reference point. The assumption underlying our measure is that individuals who regularly travel the shortest route their residence and the city center will observe whether any streets along the route has been paved. This holds true even if the plot is not adjacent to a treated street, ensuring that all units in the experimental sample can have positive exposure. To test the robustness of our results, we also construct exposure measures using three additional key destinations: the municipal market, the main bank cluster (Banamex, Scotia and BBVA), and the principal health center. We also develop a composite exposure index that combines all four destination nodes.

Table A.2 and Figure A.2 present summary statistics for exposure measures to the city center. The intent-to-treat exposure averages 9.6%, while realized exposure increases from 2.57% to 12.4% as street paving progresses over time. As expected, properties adjacent to assigned-to-treatment streets (ITT=1) show higher average exposure, though properties on assigned-to-control streets (ITT=0) also exhibit positive exposure values. For ease of interpretation, we standardize all exposure measures by their standard deviation, so a one-unit increase corresponds to a one-standard-deviation change.

To measure proximity-based benefits, we construct a series of indicator variables for each plot in our experimental sample. These indicators track the treatment status of streets along the ordered sequence from the plot to the destination node. For the street immediately adjacent to property p, we define two indicators: $b^Z_{p,0}$, which equals one if the street was assigned to treatment (zero otherwise), and $b_{p,0}$, which equals one if the street was actually paved (zero otherwise). Moving outward along the route to the destination node, we similarly define $b^Z_{p,1}$ and $b_{p,1}$ for the next street block in sequence. This process continues sequentially until we reach the street containing the destination node.

These indicator variables serve as the empirical counterparts to equation (2.7). However, they are defined based on the sequence of streets blocks rather than directly on distance rings. This approach ensures that no street block is assigned to more than one distance ring and that, at most, only one street is paved or designated for paving within each ring. Under this framework, the average distance ring effectively corresponds to the average street length within the network.

Perceptions of Government Inefficiency and Property Valuations. As a proxy for beliefs about government inefficiency, we use a self-reported measure of satisfaction with the local government, collected via a dedicated household survey—the

Acayucan Standards of Living Survey (ASLS).[11] The baseline survey was conducted during February-March 2006, with a follow-up in February-March 2009. The target population consisted of all occupied residential structures on the streets selected for the experiment.[12] We calculate the exposure measure and proximity-based benefits for survey respondents using the same procedure described above.

The household questionnaire collected detailed demographic and economic information, including a Likert scale question on satisfaction with the local government. Respondents selected one of four options: (i) very unsatisfied, (ii) unsatisfied, (iii) satisfied, and (iv) very satisfied. We construct a binary indicator by combining the first two and the last two categories to distinguish between dissatisfaction and satisfaction. We also use other survey responses—such as travel time to the city center, self-reported property valuations, and per capita expenditures—to assess baseline balance and track changes in key outcomes. Additionally, to complement self-reported values, we incorporate professional property appraisals conducted immediately after each survey round.

4 Econometric Specification and Baseline Balance

Main Econometric Specification. Guided by our model, we estimate a linear probability model of tax compliance for plot p in year t with two-way fixed effects using OLS. In its general form, we obtain the intention-to-treat (ITT) effects of pavement assignment with the following specification:

$$C_{p,t} = \eta_p + \phi_t + \delta^Z E^Z_{p,t} + \gamma^Z B^Z_{p,t} + \text{controls}_{p,t} + \epsilon_{p,t},$$

where $C_{p,t} = 1$ if plot p's property taxes are current in year t, η_p represents property fixed effects, and ϕ_t represents year fixed effects. The variable $E^Z_{p,t}$ is the exposure measure, defined as the fraction of nodes along the shortest path to the city center that belong to streets *assigned* for paving or where such streets cross or end (E^Z_p), interacted with an indicator for $t \geq 2007$ to introduce time variation. $B^Z_{p,t} = 1$ if the street lining plot p is assigned to pavement, interacted with an indicator for $t \geq 2007$. The $\text{controls}_{p,t}$ include the cadastral property valuation and tax bill, both measured at baseline and interacted with year dummies. These controls, combined with plot fixed effects, account for any baseline cross-sectional differences in tax liabilities (τ_p), perceptions of government efficiency (θ^*_p), and the length of optimal

11. Gonzalez-Navarro and Quintana-Domeque (2016) provides a more detailed description of the survey.

12. The baseline survey was administered to 1,231 households living in 1,193 dwellings, with a response rate of 94%. In 2009, 1,083 households were interviewed. In over 95% of cases, household and individual questions were answered by a reference person who was targeted because he or she was either the household head or the spouse or partner of the head. Participants in the study (household respondents) were not informed about the ultimate objective of the survey.

trajectories (X_p) that may influence compliance according to our model.

When we take this model to the data, we begin with $\gamma^Z B^Z_{p,t} = \gamma^Z_0 b^Z_{0,p,t}$ and provide evidence that this is a good approximation, as additional benefits from distant assigned-to-be paved streets ($\gamma^Z B^Z_{p,t} = \gamma^Z_0 b^Z_{0,p,t} + \gamma^Z_1 b^Z_{1,p,t} + \gamma^Z_2 b^Z_{2,p,t}$) do not affect tax compliance. For this reason, our main specification is:

$$C_{p,t} = \eta_p + \phi_t + \delta^Z E^Z_{p,t} + \gamma^Z_0 b^Z_{0,p,t} + \text{controls}_{p,t} + \epsilon_{p,t}, \tag{4.1}$$

Next, we estimate an instrumented version of the model in which we utilize whether the street lining plot p has been assigned to pavement ($B^Z_{p,t}$) as an instrument for whether the street lining plot p is actually paved in year t ($B_{p,t}$). Similarly, we use assignment to treatment exposure ($E^Z_{p,t}$) as an instrument for realized street pavement exposure ($E_{p,t}$). This approach allows us to estimate local average treatment effects (LATE) through the following second-stage equation using 2SLS:

$$C_{p,t} = \eta_p + \phi_t + \delta \widehat{E}_{p,t} + \gamma \widehat{B}_{p,t} + \text{controls}_{p,t} + \nu_{p,t}.$$

Based on our model, we expect $\gamma \geq 0$ because $\gamma_p \equiv \frac{\alpha_p}{\lambda G} \geq 0$, and $\delta \geq 0$ because $\delta_p \equiv \frac{\theta^*_p X_p}{\kappa_p} \geq 0$.

Once again, in the empirical analysis, we begin with $\gamma \widehat{B}_{p,t} = \gamma_0 \widehat{b}_{0,p,t}$ and provide evidence that this is a good approximation, as additional benefits from distant paved streets ($\gamma \widehat{B}^Z_{p,t} = \gamma_0 \widehat{b}_{0,p,t} + \gamma_1 \widehat{b}_{1,p,t} + \gamma_2 \widehat{b}_{2,p,t}$) do not appear to affect tax compliance. The main 2SLS speecification becomes:

$$C_{p,t} = \eta_p + \phi_t + \delta \widehat{E}_{p,t} + \gamma_0 \widehat{b}_{0,p,t} + \text{controls}_{p,t} + \nu_{p,t}. \tag{4.2}$$

Remark. It is important to note that the instrument operates by increasing the probability that a street becomes part of the set of paved streets and, consequently, part of the streets defining an individual's exposure given an optimal trajectory. The monotonicity assumption is satisfied because assigning a street to treatment should not reduce the probability of it being paved, nor should it decrease exposure for individuals traveling along that trajectory.

Under this framework, the concept of a complier is tied to the street and, more specifically, to the set of individuals whose exposure depends on whether the street is part of a shortest path. Thus, the LATE interpretation applies to individuals whose exposure is defined by streets included in a shortest path. Moreover, the property fixed effects (η_p) are critical in this setting, as they condition the estimation on the shortest path relevant to each property owner p.

Baseline Balance. Table 1 presents evidence of baseline balance across plot-level ($n = 752$) and project-level ($n = 47$) characteristics in the top and middle panels, respectively. The bottom panel examines balance for respondents of the ASLS survey and property appraisals.

For plot-level variables, we assess balance across three key indicators: assessed property values, annual property tax bills, and the baseline compliance rate in 2006. For instance, the property tax compliance rate among property owners is 0.855 for those living adjacent to streets assigned for paving, compared to 0.846 for those adjacent to control streets, with a p-value of 0.830 for the difference. Since the ITT exposure measure applies to properties adjacent to both treatment and control streets, we assess balance by regressing standardized ITT exposure on each variable separately. Again, there is no evidence that exposure systematically predicts any of the plot-level characteristics.

For project-level variables, the middle panel examines balance in road length, average Katz centrality, and distance to the city center via the estimated shortest path. For instance, the average centrality of street projects is nearly identical between the treatment and control groups (p-value = 0.58) and is also unrelated to ITT exposure.

Finally, for ASLS plot-level data, we assess satisfaction with the local government, travel time to the city center, owner's self-reported valuation of the property, and per capita expenditure. The professional appraisals include property valuation and land valuation. Overall, none of the differences in average characteristics are statistically significant at conventional levels, and the magnitudes of these differences remain small. These baseline balance findings align with the randomization assessment conducted on other household-level data in Gonzalez-Navarro and Quintana-Domeque (2016).

Table 1: Baseline Balance by Street ITT Status and ITT Exposure

	Street ITT Status ($b^Z_{0,p}$)				ITT Exposure (E^Z_p)		
	ITT=1	ITT=0	Diff.	p-value	Coefficient	p-value	Obs.
Plot-Level							
Tax Compliance	0.855	0.846	0.008	0.830	0.010	0.582	752
	(0.353)	(0.361)	(0.038)		(0.018)		
Cadastral Property Value in USD	12,654.701	14,619.587	-1,964.886	0.368	-1,221.176	0.254	752
	(13,612.530)	(15,878.291)	(2,160.921)		(1,056.793)		
Tax Owed in USD	12.375	12.734	-0.359	0.499	-0.396	0.062	752
	(4.996)	(5.282)	(0.527)		(0.207)		
Project Level							
Road Length (Meters)	376.567	557.746	-181.179	0.086	7.201	0.917	47
	(149.510)	(507.344)	(103.427)		(69.057)		
Katz Centrality	0.017	0.016	0.001	0.581	0.0003	0.268	47
	(0.0009)	(0.0008)	(0.0003)		(0.0003)		
Distance to City Center (Meters)	1,260.63	1,310.41	-49.78	0.677	-148.71	0.211	47
	(381.53)	(430.58)	(118.86)		(117.44)		
ASLS Plot-Level Data and Property Appraisals							
Local Gov. Satisfaction, Binary	0.454	0.470	-0.016	0.772	0.015	0.543	722
	(0.499)	(0.500)	(0.054)		(0.024)		
Log of Property Appraised Value in USD	11.575	11.633	-0.058	0.557	0.076	0.246	334
	(0.650)	(0.796)	(0.099)		(0.065)		
Log of Property Owner Value in USD	11.758	11.844	-0.087	0.535	0.076	0.332	519
	(0.978)	(0.955)	(0.138)		(0.078)		
Log of Land Appraised Value in USD	10.272	10.139	0.133	0.199	0.067	0.291	334
	(0.608)	(0.499)	(0.103)		(0.063)		
Travel Time to the City Center	19.699	20.442	-0.743	0.548	-0.098	0.904	722
	(10.563)	(12.047)	(1.228)		(0.808)		
Monthly Per Capita Expenditure (USD)	103.6	98.1	5.4	0.062	11.21	0.310	707
	(79.19)	(81.84)	(11.21)		(10.92)		

Notes: Authors' calculations are based on Acayucan's administrative data, the Acayucan Standards of Living Survey (ASLS), and appraisal valuations. A total of 47 out of 56 projects are presented in the project-level analysis, as the absence of stayers in the remaining projects prevents the computation of ITT exposure for those cases. Monetary values are reported in 2012 USD. Mean Katz centrality quantifies the average influence of nodes in a network, accounting for both direct and indirect connections with a decay factor.

5 Results

5.1 Main Estimates

Table 2 presents the main empirical results of our paper, following equations (4.1) and (4.2). The first three columns display the intent-to-treat (ITT) effects from equation (4.1), estimated by OLS. Column (1) shows that property owners on streets assigned to be paved are 4.8 percentage points (pp) more likely to pay their property taxes. From a baseline mean of 85%, this represents a substantial increase of 5.6 percent. Column (2) shows the coefficient on pavement exposure on its own and indicates that a one standard deviation increase in the fraction of nodes along the shortest path trajectory between the plot and the city center that pass through streets assigned to be paved by the intervention raises tax compliance by 2.3 pp. Finally, Column (3) presents estimates from equation (4.1), which accounts for both effects. The ITT effects are estimated at 3.2 pp for the direct benefit and 1.5 pp for

the exposure to the pavement program, respectively.

The last three columns in the table present our 2SLS estimates, which use assignment to pavement and assigned to pavement exposure as instrumental variables for whether a plot's street block is paved and exposure to newly paved streets on the trajectory to downtown. Columns (4) and (5) analyze each treatment separately, while Column (6) jointly estimates the local average treatment effects (LATE) based on equation (4.2).

To measure instrument strength, we use the Generalized First-Stage Statistic, as proposed by Lewis and Mertens (2022), which allows for the assessment of instrument strength when there are two instruments and two endogenous variables under non-homoskedasticity. With only one endogenous variable, this statistic nests the robust F-statistic (for a single instrument) or the effective F-statistic by Olea and Pflueger (2013), regardless of the number of instruments.

The instruments satisfy the instrument strength criterion. In columns (4) and (5), the Generalized First-Stage Statistic for each instrument separately is 50.18 and 37.05, both exceeding the critical value of 23.1 for a 10% relative bias shown in the last row of the table. In column (6), the Generalized First-Stage Statistic for assessing the strength of both instruments for both endogenous variables is 29.44, which is above the critical value of 26.86 for a 10% relative bias.[13]

The main takeaway from the 2SLS analysis is that we obtain LATE estimates for the effects of paving in front of properties and along the shortest path to the city center on tax compliance. Property owners whose streets were paved as a result of being assigned to treatment are 4.8 pp more likely to pay their property taxes. Additionally, property owners whose fraction of paved intersections –either observed or transited– along the shortest path to the city center increased by one standard deviation due to the intervention are 2.6 pp more likely to comply with their property tax obligations.

These novel results provide compelling evidence that a meaningful shock to local public good provision has economically important effects on tax compliance. The point estimates are larger for properties lining the newly paved roads, and there is evidence that exposure to pavement provision along the path of a particular plot owner's path to the downtown also increases compliance. To our knowledge, this is the first time such evidence of a fiscal externality provided by the signaling value of public works has been documented in the literature.

13. The effective F-statistic and the critical value are computed using the `weakivtest` routine in `Stata` by Pflueger and Wang (2015). The Generalized First-Stage Statistic and the critical value are computed using the `weakivtest2` routine in `Stata` by Zhou (2024).

Table 2: Direct and Exposure Effects of Street Paving on Tax Compliance

	OLS			2SLS		
γ_0^Z: Plot p Assigned to Pavement $\times$ $I_{\geq 2007}$ ($b_{0\,p,t}^Z$)	0.048**		0.032*			
	(0.016)		(0.019)			
δ^Z: Assigned to Pavement Exposure $\times$ $I_{\geq 2007}$ ($E_{p,t}^Z$)		0.023***	0.015**			
		(0.005)	(0.007)			
γ_0: Plot p Paved ($\widehat{b}_{0\,p,t}$)				0.077**		0.048*
				(0.024)		(0.027)
δ: Pavement Exposure ($\widehat{E}_{p,t}$)					0.038***	0.026**
					(0.010)	(0.013)
Observations	6,016	6,016	6,016	6,016	6,016	6,016
Clusters	47	47	47	47	47	47
Mean Dep. Var (2006)	0.85	0.85	0.85	0.85	0.85	0.85
Generalized First-Stage Statistic				50.18	37.05	29.44
Critical Value 10%				23.1	23.1	26.86

Notes: The table reports estimates of the direct benefit and exposure effects of street paving on tax compliance for the pooled sample. Robust errors, clustered at the project level, are reported in parentheses. All regressions include plot fixed effects, year fixed effects and baseline controls (property value and the tax bill interacted with year dummies). The Pavement Exposure measure for each plot is defined as the fraction of intersections -observed or transited- along the shortest path trajectory between the closest node to the plot and the city center that passes through streets paved by the intervention (standardized by dividing for the average yearly exposure for ease the interpretation). The Assigned to Pavement Exposure measure is defined similarly, but uses the fraction of the shortest path trajectory that passes through streets assigned to be paved. The Generalized First-Stage Statistic is the one proposed by Lewis and Mertens (2022), which allows for the assessment of instrument strength when there are two instruments and two endogenous variables under non-homoskedasticity. In columns (4) and (5), one endogenous variable and one instrument, this statistic corresponds to the robust F-statistic or the effective F-statistic by Olea and Pflueger (2013).

In Table A.3, we estimate more flexible versions—including $b_{1\,p,t}^Z$ and $b_{2\,p,t}^Z$ for OLS regressions, as well as $\widehat{b}_{1,p,t}$ and $\widehat{b}_{2,p,t}$ for 2SLS regressions—compared to our parsimonious specifications (4.1) and (4.2). These estimates do not alter our findings from Table 2. This is not only consistent with our expectation that direct benefits decrease with distance, but also suggests that they fade away when pavement occurs beyond the adjacent street. Thus, our parsimonious specifications (4.1) and (4.2) appear reliable, and we adopt them as our main ones.

Finally, for completeness, Table A.4 in the appendix reports year-by-year and pooled ITT and LATE estimates of the effects of assigning adjacent streets to pavement or paving them on property tax compliance. These estimates are not guided by our model and do not account for exposure effects beyond adjacent street effects. The year-by-year ITT estimates also include p-values derived from randomization inference (Fisher 1935).

5.2 Learning vs. Direct Benefits

Effects on Satisfaction with the Local Government. In our model, the provision of public goods has two main effects: a direct benefit effect (e.g., reciprocity or wealth effects) and an updating of beliefs about government quality (learning effect). We employ two empirical measures: (i) whether the street in front of a household was paved (or assigned for pavement) and (ii) the fraction of paved (or assigned-for-pavement) intersections along the shortest path from the household to

the city center.

The first measure captures both direct benefits and learning, while we posit that the second measure primarily reflects learning. If the second measure indeed captures learning, our model predicts that individuals with worse prior beliefs should experience a stronger belief update when exposed to public good provision. Here, we test this prediction.

We proxy beliefs and perceptions of local government quality using reported satisfaction with the local government. Our model predicts that the effect of public good provision should be stronger among individuals who initially held lower beliefs about government quality. Specifically, our proposed information mechanism suggests that individuals who observe the delivery of the public good and previously held more negative prior beliefs will update their beliefs more significantly. It is worth noting that almost all respondents correctly attributed the pavement program to the local government.

We test this prediction in Table 3, using data from the Acayucan Standards of Living Survey, a two-wave panel (2006 and 2009). Consistent with our hypothesis, the results indicate that the effect of public goods provision on satisfaction with the local government in 2009 is stronger among those who reported lower satisfaction in 2006, prior to the intervention. The sum of coefficients represents the effect of exposure to paving for those who were satisfied at baseline. The sum of coefficients is close to zero (-0.012, OLS; -0.013, 2SLS), with associated p-values of 0.629 and 0.623.[14] Thus, we cannot reject the null hypothesis that the effect of exposure to paving for those who were satisfied at baseline is zero, consistent with our model.

14. The p-value for testing H_0 : Sum of coefficients $= 0$ against H_1 : Sum of coefficients $\neq 0$ measures the probability of observing a test statistic as extreme as (or more extreme than) the one computed from our sample, assuming the null hypothesis is true.

Table 3: Effects of Street Paving Along the Shortest Path to the City Center on Satisfaction with the Local Government

	Satisfied with the Local Government = 1 (2009)	
	OLS	2SLS
Assigned to Pavement Exposure $\times$ $I_{\geq 2007}$ ($E^Z_{p,t}$)	0.079**	
	(0.024)	
$E^Z_{p,t}$ $\times$ Satisfied in 2006	-0.091**	
	(0.031)	
Pavement Exposure ($\widehat{E}_{p,t}$)		0.087***
		(0.024)
$\widehat{E}_{p,t}$ $\times$ Satisfied in 2006		-0.100**
		(0.032)
Satisfied in 2006	0.221***	0.218***
	(0.046)	(0.045)
Observations	722	722
Clusters	56	56
Mean Dep. Var (2006)	0.45	0.45
Sum of Coefficients	-0.012	-0.013
p-value (Sum of Coefficients = 0)	0.629	0.623
Generalized First-Stage Statistic		218.01
Critical Values 10%		39.25

Notes: Authors' calculations use ALSS data from 722 georeferenced plots. The *Satisfied* variable takes the value of 1 if the household head was satisfied or very satisfied, and 0 if they were unsatisfied or very unsatisfied. 2SLS uses the *Fraction of Assigned to Paved* intersections and its interaction with satisfaction at baseline as instruments for their corresponding *Paved* versions. The p-value for testing H_0 : Sum of coefficients = 0 against H_1 : Sum of coefficients $\neq$ 0 measures the probability of observing a test statistic as extreme as (or more extreme than) the one computed from our sample, assuming the null hypothesis is true. The Generalized First-Stage Statistic is the one proposed by Lewis and Mertens (2022), which allows for the assessment of instrument strength when there are two instruments and two endogenous variables under non-homoskedasticity.

Effects on Property Values and Travel Time to the City Center. We now investigate two potential direct benefits of street paving: increases in property value (wealth effect) and reductions in travel time to the city center (time effect). Specifically, we examine the effects of our exposure measures—exposure to pavement (or assignment to it) in front of the house and exposure to paved (or assigned to paved) intersections along the shortest path from the plot to the city center—on property values and travel time. Table 4 presents this analysis.

Consistent with Gonzalez-Navarro and Quintana-Domeque (2016), we find that, using professional appraisal measures, paving the street where a property abuts increases land values by 55 log points (or 72%) and property values by 18 log points (or 19%). However, greater exposure to pavement en route to the city center, as measured by a higher fraction of intersections paved along the shortest path, does not increase land or property values. This suggests that our measure of exposure to pavement en route to the city center does not capture wealth effects.

We also examine the effects on travel time to the city center to assess whether paving has reduced transportation costs, either by providing pavement in front of the property or along the shortest path to the city center. Consistent with Gonzalez-Navarro and Quintana-Domeque (2016), we find no effect of paving the street where a property abuts on travel time to the city center using one's usual mode of transportation. The estimated effect is neither statistically significant nor quantitatively relevant—about half a minute on an average travel time of 20 minutes. Furthermore, we find no time effects based on exposure to pavement en route to the city center.

We conclude that our measure of exposure en route to the city center does not appear to capture direct benefits, whether through wealth increases (as reflected in property values) or time savings (as reflected in reduced travel time to the city center). This finding aligns with the interpretation that our exposure-to-intersection is not capturing direct benefits.

Table 4: Direct and Exposure Effects of Street Paving in Measures of Direct Benefits

	OLS				2SLS			
	Appraisal Valuation 2009		Owner Perception 2009		Appraisal Valuation 2009		Owner Perception 2009	
	Log Land Value	Log Property Value	Log Property Value	Travel Time to City Center (min)	Log Land Value	Log Property Value	Log Property Value	Travel Time to City Center (min)
Plot p Assigned to Pavement $\times$ $I_{\geq 2007}$	0.328***	0.107**	0.143	-0.405				
	(0.075)	(0.043)	(0.110)	(0.806)				
Assigned to Pavement Exposure $\times$ $I_{\geq 2007}$	-0.038	-0.021	0.048	-0.092				
	(0.031)	(0.015)	(0.058)	(0.735)				
Plot p Paved					0.545***	0.177***	0.225	-0.646
					(0.118)	(0.061)	(0.165)	(1.310)
Pavement Exposure					-0.057*	-0.029*	0.047	-0.083
					(0.034)	(0.017)	(0.062)	(0.813)
Observations	318	318	468	721	318	318	468	721
Clusters	53	53	51	53	53	53	51	53
Mean Dep. Var (2006)	10.22	11.62	11.75	20.37	10.22	11.62	11.75	20.37
Generalized First-Stage Statistic					23.72	23.35	31.46	30.19
Critical Value 10%					24.33	24.2	23.18	23.03

Notes: The table reports estimates of the direct and exposure effects of street paving on four measures of direct benefits. The logarithm transformation in home and land values in 2009 for a sample of 318 appraised houses in Acayucan, Mexico. Also, owner's perception of the house value (for 468 plots) and travel time to the city center in minutes (for 721 plots). For further details on the appraisal procedure and the variables compilation see Gonzalez-Navarro and Quintana-Domeque (2016).The regression includes the value of the dependent variable at baseline (2006) as a control. Clustered errors at the project level, are reported in parentheses. For details regarding the exposure measures definitions, refer to the main text. The continuous exposure variables are standardized for the ease of the interpretation. Estimation accounts for survey weights. The Generalized First-Stage Statistic is the one proposed by Lewis and Mertens (2022), which allows for the assessment of instrument strength when there are two instruments and two endogenous variables under non-homoskedasticity.

An Alternative Measure of Exposure to Intersections. The results on satisfaction with the local government suggest that exposure to paved intersections increases tax compliance through belief updating among those with worse priors. Furthermore, the findings on direct benefits indicate that paving the street in front of a house increases property values, whereas exposure to paved intersections neither increases property values nor provides time savings for households. This suggests that the effects of exposure to intersections indeed reflect an information/learning effect, driven by the updating of priors among those with worse baseline beliefs.

One could argue, however, that we are omitting other relevant margins of direct benefits. To address this concern, we replace the measure of exposure en route to the city center with an alternative measure that accounts only for the fraction of assigned or paved intersections that are *observed but not used*, thereby excluding benefits derived from transiting the road. While this may stretch the limits of our data, the results suggest that the information mechanism is not only plausible in our context but also empirically relevant.

Indeed, the analysis in Table 5 reveals that our ITT and LATE estimates for exposure to ITT-observed (but not used) intersections are 0.012 (SE = 0.007), very close to 0.015 (SE = 0.007) in Table 2, and 0.022 (SE = 0.013), similar to 0.027 (SE = 0.012) in Table 2. Thus, it appears that both measures of exposure to intersections—whether used and observed or only observed—capture the information/learning mechanism postulated in the model.[15]

15. In Table A.5 in the appendix, we decompose the exposure to intersections measure into exposure to transited intersections and exposure to observed-only intersections. The results once again indicate that the information/learning mechanism is empirically plausible.

Table 5: Effects of Street Paving in Front of Properties and Along the Shortest Path to the City Center on Tax Compliance: Observed (not used) Intersections

	OLS			2SLS		
γ_0^Z: Plot p Assigned to Pavement $\times$ $I_{\geq 2007}$ ($b_{0\,p,t}^Z$)	0.048***		0.047***			
	(0.016)		(0.017)			
Exposure to ITT Observed Intersections $\times$ $I_{\geq 2007}$		0.013	0.012			
		(0.008)	(0.007)			
γ_0: Plot p Paved ($\widehat{b}_{0\,p,t}$)				0.077***		0.071***
				(0.024)		(0.025)
Observed Paved Intersections Exposure					0.021*	0.022*
					(0.012)	(0.012)
Observations	6,016	6,016	6,016	6,016	6,016	6,016
Mean Dep. Var (2006)	0.85	0.85	0.85	0.85	0.85	0.85
Generalized First-Stage Statistic				50.19	19.38	16.49
Critical Values 10%				23.11	23.11	26.86

* Significant at 10%; ** Significant at 5%. *** Significant at 1%

5.3 Alternative Destinations, Non-random Exposure and Selective Attrition

Alternative destinations. Although the measure of exposure to paved (or assigned to be paved) intersections in the shortest path from the plot to the city center is intuitive, alternative landmark destinations, such as the main bank, hospital or health center, and market, could also be considered. Table A.6, panel A, in the appendix demonstrates that the results are robust to using these alternative destinations, as well as a harmonized index combining all of them.

Non-random exposure. Another potential concern with the exposure measure is the issue of non-random exposure (Borusyak and Hull 2023). However, in our context, this is unlikely to be a problem because the probability of any given street being paved is very small. Indeed, Table A.6, panel B, in the appendix, which employs recentered exposure measures following Borusyak and Hull (2023), produces estimates that very similar to those in Table 2.

Assessing selective attrition. Finally, we note that our primary sample is a balanced panel of properties with consistent ownership from 2005 to 2012. To avoid confounding treatment assignment, we exclude properties with ownership changes during the experiment. However, this may introduce bias if excluded properties differ systematically or vary by treatment. In Table A.8, columns 1–3, we find no evidence of differential attrition rates by treatment status over time, suggesting that attrition is not systematically related to treatment assignment. However, even with identical attrition rates, the characteristics of dropouts may systematically differ between treatment and control groups. To address this, we follow the approach

in Ghanem, Hirshleifer, and Ortiz-Beccera (2023) and report baseline compliance rates in columns 4–7 across assigned treatment stayers, control stayers, assigned treatment leavers, and control leavers. We cannot reject internal validity for stayers (i.e., the equality of baseline outcome distributions between assigned treatment and control stayers or between assigned treatment and control leavers) or for the study population (i.e., equality across all four subgroups). These exercises confirm that attrition does not compromise the validity of our findings. Full details, including an expanded sample and imputed outcomes for leavers, are provided in Appendix A.2.

6 Fiscal Implications and Spillovers of Street Paving

We conclude our analysis with a back-of-the-envelope calculation to evaluate the fiscal impact of the intervention. This analysis includes both experimental streets—those that either received pavement or remained unpaved as part of the experiment—and non-experimental unpaved streets that were not directly part of the experiment but were indirectly exposed to the paving projects. Importantly, both unpaved streets in the experimental group and unpaved streets in the non-experimental group could experience indirect exposure to the paving of nearby streets due to the intervention.

Thus, our calculation considers property tax revenue from three groups of properties: those directly affected on paved experimental streets, and those indirectly affected by nearby paving, either on unpaved experimental streets or on unpaved non-experimental streets. While the absolute levels of tax revenue remain modest due to the relatively small property tax base, the relative effects reveal significant fiscal spillovers. As shown in Table 6, focusing on experimental streets, the ratio of tax revenue generated by properties on unpaved streets to those on paved streets averages 0.71.

Moreover, when combining tax revenue from unpaved properties on experimental streets with those on non-experimental streets, and comparing it to revenue from paved properties, the average ratio rises to 4.41. This highlights the broader fiscal impact of the paving projects and underscores the importance of belief updating. Specifically, this calculation shows that tax revenue generated through belief updating—where citizens observe paving projects and adjust their compliance behavior—accounts for four times as much revenue as that generated through reciprocity effects from direct beneficiaries on newly paved streets. These findings reveal the critical role of indirect effects in driving fiscal outcomes.

Table 6: Increase in Revenue due to Paving, in 2012 USD

Year	Revenue			Number of plots			Relative Revenue	
	Paved (P)	Unpaved (U)	Non-experimental Unpaved (NE)	P	U	NE	U/P	(U + NE)/P
2007	118.11	138.19	192.41	107	645	6037	1.17	2.80
2008	166.46	188.99	838.97	140	612	6037	1.14	6.18
2009	301.92	156.53	1163.68	235	517	6037	0.52	4.37
2010	314.22	162.67	1210.79	235	517	6037	0.52	4.37
2011	335.60	168.14	1263.28	250	502	6037	0.50	4.27
2012	475.86	188.39	1941.63	347	405	6037	0.40	4.48

Notes: The counterfactual individual's difference in revenue due to paving was obtained throughout the multiplication of the marginal increase in probability of tax compliance with each individual's tax burden. The LATE estimates from table 2 and the computed exposure to paving were used for the computation. Finally, the table presents the aggregation (sum) of these counterfactual differences.

7 Discussion

This study highlights the role of public goods provision in improving tax compliance. We present a model of tax compliance that combines elements of tax morale with perceived government efficiency in using taxes to deliver public expenditure. In our context, public expenditure is represented by the pavement of previously unpaved streets in the periphery of Acayucan, Mexico, where 28 out of 56 eligible street projects were randomly assigned to be paved.

We find that the first-time paving of residential streets, a visible infrastructure improvement, leads to higher compliance rates among property taxpayers. Consistent with our model—where compliance responds to both perceived direct benefits and the broader observability of public goods provision—we document four key findings. First, property owners living on streets assigned to be paved (or finally paved) are more likely to pay property taxes, reflecting the positive impact of perceived direct benefits on compliance. Second, regardless of whether their own streets are assigned to be paved (or finally paved), property owners exposed to streets being paved (or finally paved) along their frequent commutes are also more likely to pay taxes, highlighting the positive impact of exposure on beliefs about government quality. Third, we find that pavement exposure increased satisfaction with the local government only among individuals who were initially, before the intervention, dissatisfied with the local government. This finding is consistent with exposure to pavement affecting the beliefs of individuals with lower priors. Taken altogether, these results are consistent with perceived direct benefits increasing with property value and belief updates being larger among individuals with more pessimistic prior views of the government, as predicted by our model.

We also document that the perceived direct benefits of paving, controlling for exposure, persisted over time, emphasizing the durability of public investments in fostering behavioral change. This durability underscores the potential of targeted public investments to yield sustained fiscal benefits. Moreover, a back-of-the-envelope calculation suggests significant fiscal spillovers from the paving intervention, encom-

passing both experimental streets and non-experimental streets indirectly exposed to paving. Property tax revenue from unpaved streets relative to paved streets averages 0.71, while combining unpaved and non-experimental streets increases this ratio to 4.41. This suggests that belief updating among citizens observing the intervention generates four times as much tax revenue as reciprocity effects from direct beneficiaries, underscoring the importance of indirect effects in enhancing fiscal outcomes.

This study contributes to three strands of literature. First, it enhances understanding of state capacity by demonstrating how effective public goods provision boosts tax collection, a key component of state capacity (Besley 2020; Besley, Jensen, and Persson 2023). Second, it underscores the relationship between public perceptions of government spending and tax compliance, showing that credible investments can mitigate the negative effects of perceived inefficiency or corruption (Besley and Persson 2014; Stantcheva 2021). Third, it extends research on non-pecuniary drivers of tax compliance, particularly tax morale, by providing evidence of the role of reciprocity and perceived benefits (Luttmer and Singhal 2014; Pomeranz and Vila-Belda 2019; Slemrod 2019).

By focusing on the tangible impact of a single public good—street paving—this study diverges from interventions that rely on informational campaigns or messaging to increase tax compliance. The use of granular, street-level data and the empirical analysis driven by a tax compliance model allowing for two types of effects, strengthens the causal interpretation of our results and addresses limitations of prior studies that often examine multiple interventions at broader scales (Brockmeyer, Garfias, and Suárez Serrato 2024).

From a policy perspective, these findings underscore the importance of investing in visible, high-quality public goods. Such investments not only demonstrate the effective use of taxpayer funds but also strengthen the implicit social contract between citizens and the state, fostering a virtuous cycle of improved tax compliance and enhanced public service delivery. This approach is particularly relevant for developing countries, where breaking the cycle of low compliance and inadequate public goods is critical for sustainable development.

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A Online Appendix

A.1 Derivation of Posterior Beliefs

This appendix provides a formal derivation of the posterior expectations presented in equation (2.5). We proceed by applying Bayes' rule and exploiting the properties of conjugate distributions. The proof proceeds in several steps:

Setup. By Bayes' rule, the posterior distribution is proportional to the product of the likelihood function and the prior distribution:

$$Pr(\theta|X_pE_p) \propto Pr(X_pE_p|\theta) \times Pr(\theta). \tag{A.1}$$

Under our assumptions that streets are paved with equal and independent probability (π), the number of paved streets observed by a property owner (X_pE_p) among the unpaved streets in their route (S_p) follows a Binomial distribution:

$$X_pE_p|\theta \sim \text{Binomial}(S_p, \pi), \tag{A.2}$$

where $\pi = \frac{(1-\theta)G}{S}$. The probability mass function is thus:

$$Pr(X_pE_p = k|\theta) = \binom{S_p}{k}\pi^k(1-\pi)^{S_p-k} \tag{A.3}$$

On the other hand, the prior follows a Beta distribution:

$$Pr(\theta) = \frac{\theta^{\kappa_{p,1}-1}(1-\theta)^{\kappa_{p,2}-1}}{B(\kappa_{p,1}, \kappa_{p,2})} \tag{A.4}$$

where $B(\cdot,\cdot)$ is the Beta function.

Poisson Approximation. Given G/S small, implying a low probability of a street being paved ($\pi \ll 1$), we can apply the Poisson approximation to the Binomial distribution:

$$Pr(X_pE_p = k|\theta) \approx \frac{(S_p\pi)^k e^{-S_p\pi}}{k!} \tag{A.5}$$

Substituting $\pi = \frac{(1-\theta)G}{S}$:

$$Pr(X_pE_p = k|\theta) \approx \frac{(S_p\frac{(1-\theta)G}{S})^k e^{-S_p\frac{(1-\theta)G}{S}}}{k!} \tag{A.6}$$

Posterior Distribution. The posterior distribution is proportional to:

$$Pr(\theta|X_pE_p = k) \propto \frac{(S_p\frac{(1-\theta)G}{S})^k e^{-S_p\frac{(1-\theta)G}{S}}}{k!} \times \frac{\theta^{\kappa_{p,1}-1}(1-\theta)^{\kappa_{p,2}-1}}{B(\kappa_{p,1}, \kappa_{p,2})} \tag{A.7}$$

For small G/S, we approximate $e^{-S_p \frac{(1-\theta)G}{S}} \approx 1$. After this approximation and rearranging terms:

$$Pr(\theta|X_pE_p = k) \propto \theta^{\kappa_{p,1}-1}(1-\theta)^{\kappa_{p,2}-1+k} \tag{A.8}$$

The form of this expression reveals that the posterior follows a Beta distribution with updated parameters:

$$\theta|X_pE_p \sim \text{Beta}(\kappa_{p,1}, \kappa_{p,2} + X_pE_p) \tag{A.9}$$

Posterior Expectation. Using the definition of the expected value of the Beta distribution, the posterior expectation is:

$$\mathbb{E}[\theta|X_pE_p] = \frac{\kappa_{p,1}}{\kappa_{p,1} + \kappa_{p,2} + X_pE_p}. \tag{A.10}$$

Given that $\theta_p^* = \frac{\kappa_{p,1}}{\kappa_{p,1}+\kappa_{p,2}}$ and $\kappa_p = \kappa_{p,1} + \kappa_{p,2}$, this expression can be written as:

$$\theta_p(E_p) = \frac{\theta_p^*}{1 + \frac{X_pE_p}{\kappa_p}}, \tag{A.11}$$

Finally, we apply a first-order approximation to the expectation. First, we rewrite the expectation in the form of a standard power series:

$$\theta_p(E_p) = \theta_p^* \left(1 + \frac{X_pE_p}{\kappa_p}\right)^{-1}. \tag{A.12}$$

Defining $Z_p \equiv \frac{X_pE_p}{\kappa_p}$, we have that when π is small,[16] $|\frac{X_pE_p}{\kappa_p}| < 1$, allowing us to use the expansion:

$$\theta_p(E_p) = \theta_p^*[1 - Z_p + Z_p^2 - Z_p^3 + ...] \tag{A.13}$$

For a first-order linear approximation, we keep only terms up to the first power and drop higher-order terms:

$$\theta_p(E_p) \approx \theta_p^*[1 - Z_p]. \tag{A.14}$$

Substituting and expanding the right-hand side:

$$\theta_p(E_p) \approx \theta_p^* - \frac{\theta_p^* X_p}{\kappa_p} E_p \tag{A.15}$$

The linear approximation error is of order $O((X_pE_p/\kappa_p)^2)$.

16. The equivalence between small E_p and small π follows from the fact that E_p is the observed fraction of paved streets, which has expected value π under our model assumptions.

A.2 Assessing Selective Attrition

Our primary sample consists of a balanced panel of properties with consistent ownership from 2005 to 2012. To avoid confounding treatment assignments with ownership changes, we exclude properties from the estimating sample if ownership changed during the experiment. Although we do not directly observe the causes of ownership transfers, they could stem from sales, intra-family transfers, or administrative record discrepancies. This sample selection may introduce biases if the excluded properties systematically differ from those retained or by treatment assignment. For example, if properties with high compliance rates are more likely to change ownership, the remaining sample may be biased toward lower compliance rates. Since treatment affects compliance rates, this bias could vary by treatment assignment. Such selective attrition could potentially compromise both the internal and external validity of our results.

In this section, we conduct a series of robustness exercises to characterize attrition patterns and examine the sensitivity of the results to alternative samples.

Attrition Patterns and Selection Bias. Our initial dataset in 2005 comprised 1,063 plots, of which 752 retained consistent ownership throughout the study period. Notably, 14.4% of the original plots were dropped from the sample before the experiment began in 2007, meaning the actual attrition rate over the experimental period is closer to 17.5%, aligning with typical rates observed in field experiments (Ghanem, Hirshleifer, and Ortiz-Beccera 2023).

We first test for differential attrition rates by treatment assignment and year. The first three columns of Appendix Table A.8 report cumulative attrition rates relative to 2005 and the results of differential attrition tests by treatment assignment. Our findings show no evidence of differential attrition rates by treatment status over time, suggesting that attrition is not systematically related to treatment assignment.

However, equal attrition rates alone do not guarantee internal validity. Even with identical rates, characteristics of dropouts could systematically differ between treatment and control groups. If these differences correlate with the outcome of interest, attributing changes solely to the treatment becomes challenging. Furthermore, external validity may be compromised if the remaining sample systematically differs from the original sample or target population.

To address these concerns, we conduct the two joint tests proposed by Ghanem, Hirshleifer, and Ortiz-Beccera (2023):

1. **Internal Validity for Stayers (IVal-R) test:** This text examines the equality of baseline outcome distributions for assigned treatment and control stayers, as well as assigned treatment and control leavers.

2. **Internal Validity for the Study Population (IVal-P) test:** This test examines the equality of baseline outcome distributions across all four treatment/response subgroups.

Both tests leverage all available baseline data, encompassing both stayers and leavers. Furthermore, Ghanem, Hirshleifer, and Ortiz-Beccera (2023) demonstrate that these testable restrictions are sharp, indicating that they represent the strongest implications that can be tested given the available data.

Columns (4)-(7) of Appendix Table A.8 report baseline (2005) compliance rates across the four groups over time. We observe slightly higher compliance rates among leavers, irrespective of treatment assignment. However, these differences are small and statistically insignificant. Columns (8) and (9) report results for the IVal-P and IVal-R tests, respectively, and we cannot reject the null hypothesis for either test across all years.

The combination of no differential attrition rates and no significant differences in baseline compliance rates across subgroups suggests that our results are not attributable to non-random attrition bias. These findings provide reassurance that attrition does not compromise the internal or external validity of our estimates.

Alternate Samples and Imputed Outcomes. To complement our analysis, we explore two alternative approaches: using an expanded sample and imputing outcomes for leavers.

Table A.9 replicates the results from Table **??** but includes all available observations in each year, without requiring the panel to be balanced. The results remain qualitatively unchanged, with similar point estimates across years. However, in a few cases, we lose precision, and the estimates are no longer statistically significant at conventional levels.

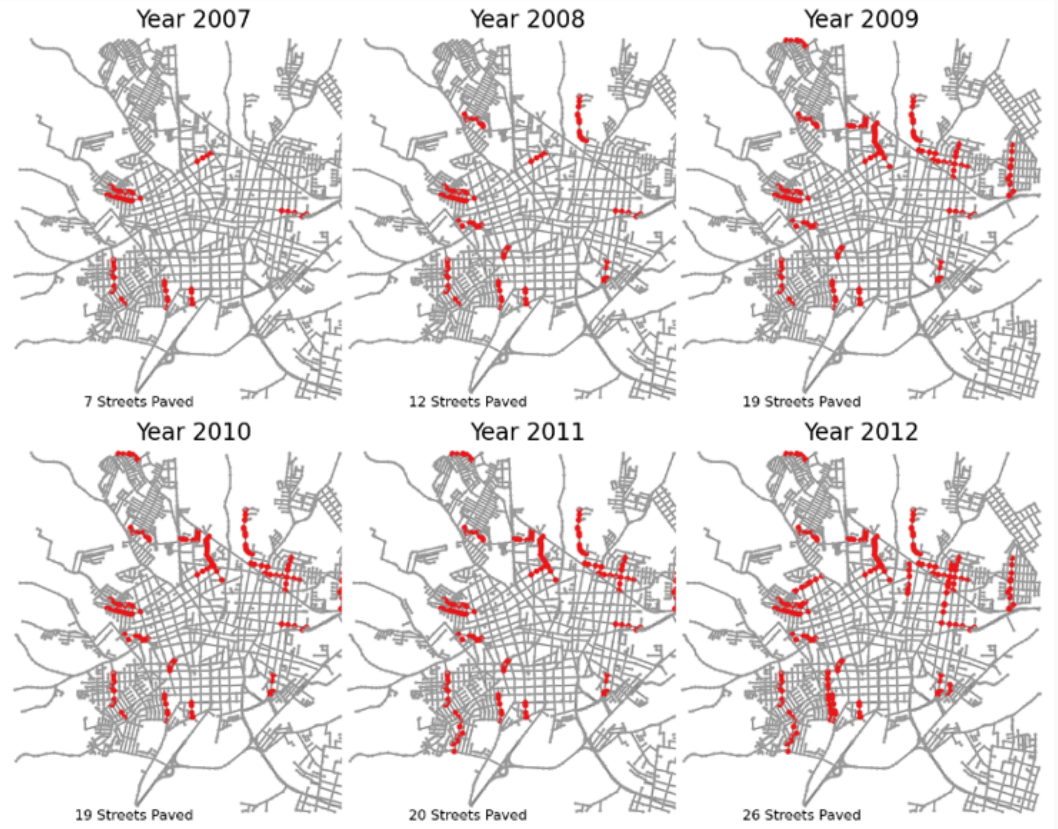
Figure A.3 presents LATE and ITT estimates using nine different techniques to impute missing tax compliance values for the leavers subgroup. These include straightforward methods (e.g., assuming complete compliance, non-compliance, or using the last observed compliance status) as well as machine learning approaches leveraging baseline covariates. We also bound potential biases using the minimum and maximum compliance rates observed during the eight-year period.[17]

Table A.10 reports the accuracy of these imputation methods, showing that both machine learning algorithms and simple heuristics reliably predict compliance status. Overall, these robustness exercises confirm that attrition does not undermine the reliability of our findings.

17. To validate these imputations, we conducted cross-validation exercises. The 752 plots with consistent ownership served as the training sample, while the predictions were tested on both the experimental sample and the leavers subgroup. The panel data structure allowed us to compare predictions with actual compliance outcomes before ownership changes occurred.

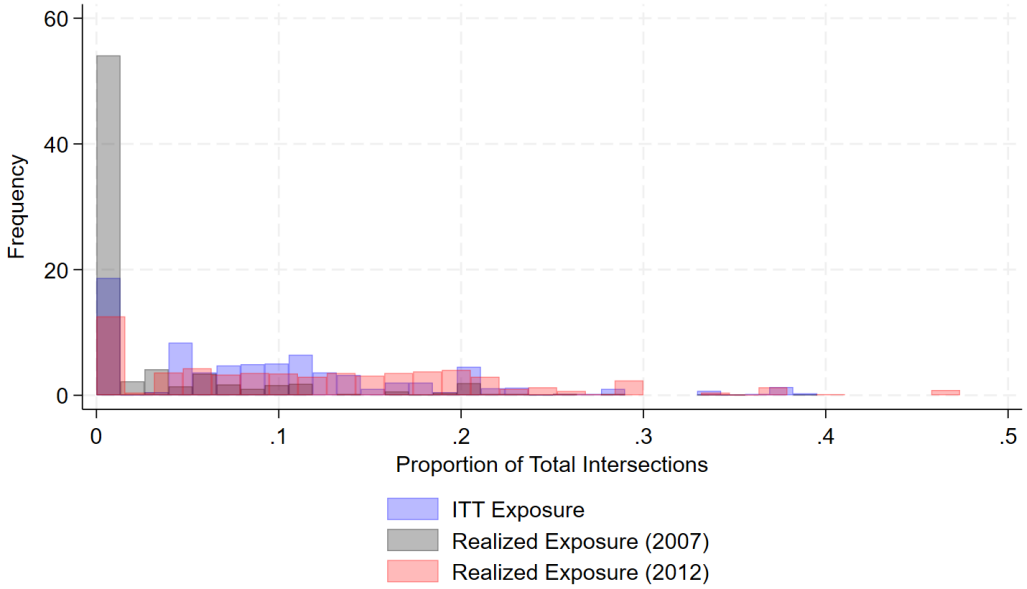
A.3 Tables and Figures

Figure A.1: Street Paving Rollout (2007–2012)



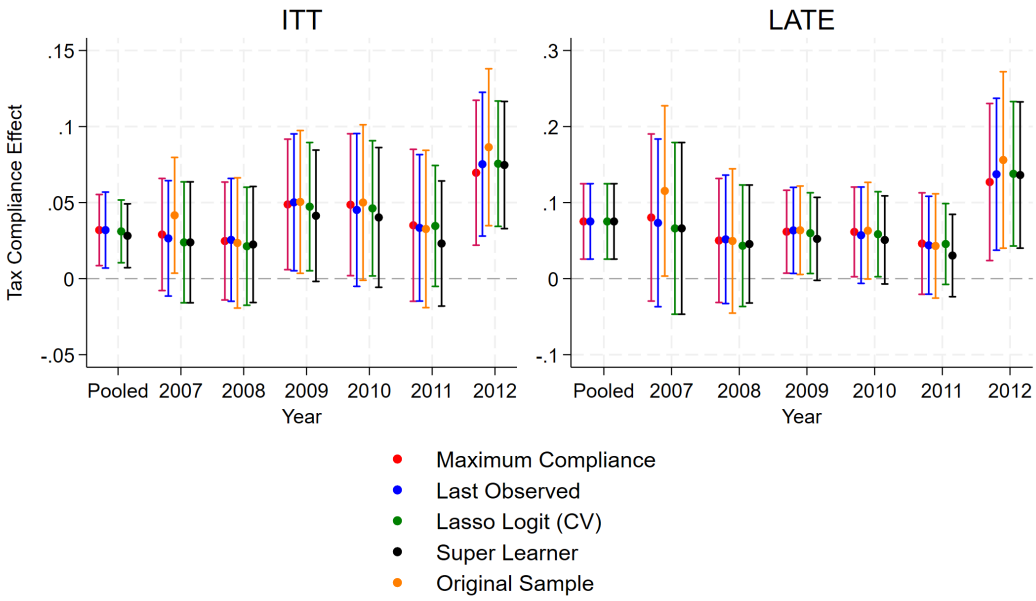
Notes: Authors' calculations based on Acayucan's administrative data, project rollout records, and Python's OSMnx library. Each red dot represents a node in the street network corresponding to a street paved under the experiment.

Figure A.2: Distribution of ITT and Realized Exposure in Route to City Center



Notes: Author's calculations based on Acayucan's administrative data, project rollout records, and Python's OSMnx library. See Section 3 for a detailed description of the construction of the exposure measures.

Figure A.3: ITT and LATE estimates based on different imputation methods



Notes: Authors' calculation using administrative data from Acayucan.

Table A.1: Probability of Street Paving in Acayucan, Mexico (2007–2012)

	Number of Experimental & Non-Experimental streets Unpaved	Number of Streets Paved by Year	Fraction of Streets Paved
2007	345	7	0.021
2008	338	5	0.014
2009	333	7	0.021
2010	326	0	0.003
2011	326	1	0
2012	325	6	0.018

Notes: Authors' calculations based on Acayucan's administrative data and project rollout. The average probability of a street being paved throughout the 2007–2012 period is 1.3%.

Table A.2: Exposure to Paved Streets Along the Route to the City Center

	by Street ITT Status				**Overall Average**	
	2007		**2012**		**2007**	**2012**
	ITT=1	ITT=0	ITT=1	ITT=0		
Realized Exposure ($E_{p,t}$)	5.1%	1.0%	14.4%	11.1%	2.57%	12.40%
ITT Exposure (E_p^Z)	15.0%	6.1%	15.0%	6.1%	9.6%	

Notes: The table presents exposure statistics by street ITT status for 2007 and 2012, along with the overall average exposure across all experimental observations (ITT=1 or ITT=0). Since ITT exposure is time-invariant, its 2007 value is repeated for 2012.

Table A.3: Accounting for direct benefits from distant paved streets

	OLS			2SLS		
δ^Z: Assigned to Pavement Exposure $\times$ I_{2007} ($E^Z_{p,t}$)	0.015**	0.016**	0.016**			
	(0.007)	(0.007)	(0.007)			
γ^Z_0: Plot p Assigned to Pavement ($b^Z_{0,p,t}$)	0.032*	0.031*	0.032			
	(0.019)	(0.019)	(0.019)			
γ^Z_1: Plot p Assigned to Pavement ($b^Z_{1,p,t}$)		-0.006	-0.006			
		(0.018)	(0.018)			
γ^Z_2: Plot p Assigned to Pavement ($b^Z_{2,p,t}$)			0.003			
			(0.023)			
δ: Pavement Exposure ($\widehat{E}_{p,t}$)				0.026**	0.027*	0.026*
				(0.013)	(0.014)	(0.015)
γ_0: Plot p Paved ($\widehat{b}_{0,p,t}$)				0.048*	0.048*	0.049
				(0.027)	(0.027)	(0.031)
γ_1: Plot p Paved ($\widehat{b}_{1,p,t}$)					-0.003	-0.004
					(0.029)	(0.033)
γ_2: Plot p Paved ($\widehat{b}_{2,p,t}$)						0.004
						(0.048)
Observations	6,016	6,016	6,016	6,016	6,016	6,016
Mean Dep. Var (2006)	0.85	0.85	0.85	0.85	0.85	0.85
Generalized First-Stage Statistic				29.44	21.84	7.63
Critical Values 10%				26.87	28.24	32.68

Note: Standard errors in parentheses. * p<0.10, ** p<0.05, *** p<0.01.

Table A.4: ITT and LATE Effects of Street Paving on Tax Compliance

	Pooled 2007-2012	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**
				ITT			
Assign to Paved	0.056**	0.050*	0.033	0.060**	0.059*	0.043	0.092**
	(0.022)	(0.029)	(0.024)	(0.026)	(0.030)	(0.030)	(0.031)
		[p=0.043]	[p=0.300]	[p=0.043]	[p=0.040]	[p=0.215]	[p=0.005]
				LATE			
Paved	0.090**	0.138	0.069	0.076**	0.075**	0.056	0.168**
	(0.033)	(0.084)	(0.054)	(0.032)	(0.036)	(0.040)	(0.062)
Observations	4,512	752	752	752	752	752	752
Generalized First-Stage Statistic	57.45	8.20	12.46	78.14	78.14	62.58	16.89
Critical Values 10%	23.1	23.1	23.1	23.1	23.1	23.1	23.1

Notes: The table reports the estimated ITT and LATE effects of street paving on tax compliance for the pooled sample and each year of the experiment. Robust errors, clustered at the project level, are reported in parentheses. All regressions include as baseline controls the tax bill and the property values. P-values in brackets are based on a randomization inference exercise with 400 random permutations of the treatment assignment. Generalized First-Stage F-Statistic presented with the respective 10% critical values by Olea and Pflueger (2013).

Table A.5: Decomposition into Transited and Observed Nodes

	ITT			LATE		
Exposure to ITT						
Transited Intersections × I_2007	0.021***		0.020***			
	(0.005)		(0.005)			
Exposure to ITT						
Observed Intersections × I_2007		0.013	0.012			
		(0.008)	(0.008)			
Exposure to Paved						
Transited Intersections				0.031***		0.034***
				(0.009)		(0.010)
Exposure to Paved						
Observed Intersections					0.020	0.027**
					(0.012)	(0.013)
Observations	6,016	6,016	6,016	6,016	6,016	6,016
Mean Dep. Var (2006)	0.85	0.85	0.85	0.85	0.85	0.85
Generalized First-Stage Statistic				83.53	19.38	11.28
Critical Values 10%				23.11	23.11	26.39

* Significant at 10%; ** Significant at 5%. *** Significant at 1%

Table A.6: Effects using alternative destinations and accounting for non-random exposure

Panel A: Exposure	**OLS**					**2SLS**				
In Route to:	City Center	Main Bank	Main Hospital	Main Market	Index	City Center	Main Bank	Main Hospital	Main Market	Index
Plot p Assigned	0.032**	0.033**	0.043***	0.033**	0.031*					
to Pavement ($b_{p,0}$)	(0.019)	(0.020)	(0.017)	(0.019)	(0.019)					
Exposure to Assigned	0.015**	0.014**	0.006	0.014**	0.017**					
to Pavement in Path (E_p)	(0.007)	(0.008)	(0.012)	(0.007)	(0.009)					
Plot p Paved						0.048**	0.058**	0.069***	0.053**	0.052**
($b_{p,0}$)						(0.027)	(0.029)	(0.025)	(0.028)	(0.027)
Exposure to Paved						0.026**	0.017*	0.008	0.020**	0.022**
in Path (E_p)						(0.013)	(0.011)	(0.011)	(0.010)	(0.011)
Observations	6,016	6,016	6,016	6,016	6,016	6,016				
Generalized First-Stage Statistic	.	.	.	.	.	29.442	36.984	15.378	38.235	33.774
Critical Values (10)	.	.	.	.	.	26.868	24.892	27.116	26.942	23.765
Panel B: Exposure re-Centered	**OLS**					**2SLS**				
In Route to:	City Center	Main Bank	Main Hospital	Main Market	Index	City Center	Main Bank	Main Hospital	Main Market	Index
Plot p Assigned	0.032**	0.033**	0.043***	0.033**	0.033**					
to Pavement ($b_{p,0}$)	(0.019)	(0.020)	(0.017)	(0.019)	(0.019)					
Exposure to Assigned	0.015**	0.014**	0.006	0.014**	0.014*					
to Pavement in Path (E_p)	(0.007)	(0.008)	(0.012)	(0.007)	(0.010)					
Plot p Paved						0.049**	0.058**	0.069***	0.054**	0.056**
($b_{p,0}$)						(0.027)	(0.029)	(0.026)	(0.028)	(0.027)
Exposure to Paved						0.025**	0.016*	0.007	0.019**	0.017**
in Path (E_p)						(0.012)	(0.011)	(0.011)	(0.010)	(0.010)
Observations	6,016	6,016	6,016	6,016	6,016	6,016				
Generalized First-Stage Statistic	.	.	.	.	.	31.312	36.987	15.544	38.197	27.562
Critical Values 10%	.	.	.	.	.	26.939	25.011	27.141	27.021	24.731

Notes: The table presents multiple robustness analysis for the main results in Table 2. **Panel A** presents the main results after changing the destinations that plots takes to other relevant -most common- places in the City Network. For instance, the main Bank (Banamex), the main Hospital (Acayucan's Health Center II), Main Market and an index that corresponds to the geometric mean of the previous locations. **Panel B** provides the recentered by the expected exposure to paving measures following Borusyak and Hull (2023). The expected exposure is computed as the fraction of unpaved total intersections -in the route to each destination- multiplied by the probability of a street being paved (on average 1.2%). For all estimates, standard errors, clustered at the project level, are reported in parentheses. All regressions include baseline property value and the tax bill interacted with year dummies as controls. For details regarding the exposure measures definitions, refer to the main text.

Table A.7: Characterization of Compliers

	Proportionality test	**Lower 95% confidence limit**	**Upper 95% confidence limit**
Plot-Level Variables (X_p) in 2006			
Tax Compliance	0.996	0.938	1.053
Median Property Value	0.790	0.664	0.916
Median Tax Owed	0.794	0.487	1.102
ALSS Plot-Level Data in 2006			
Local Gov. Satisfaction (Binary)	0.925	0.849	1.002
Median Property Appraised Value	1.064	1.024	1.104
Median Property Owner Value	1.048	0.992	1.104
Median Land Appraised Value	1.056	1.014	1.098
Median Center Transport Time (Minutes)	0.772	0.658	0.887

Notes: The table reports a proportionality test to characterize compliers based on binary covariates X_p. Specifically, it computes the sample analog of the ratio: $\frac{P(D_p(1)>D_p(0)|X_p=1)}{P(D_p(1)>D_p(0))}$, where $D_p(Z_p)$ is an indicator for treated units under assignment $Z_p \in \{0,1\}$ (Angrist and Pischke 2014). If the values in the first column are above one, compliers are more likely to have the corresponding characteristic X_p. Bootstrap simulations with 200 replications were used to derive the confidence limits.

Table A.8: Assessing Internal and External Validity in the Presence of Attrition

Years	Attrition Rates			Baseline (2005) Compliance Rates by ITT				Test of IVal-R	Test of IVal-P
	Share	Differential by ITT	P-value	ITT=1 Stayers	ITT=0 Stayers	ITT=1 Leavers	ITT=0 Leavers	P-value	P-value
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
2006	0.105	0.006	0.770	0.875	0.892	0.935	0.939	0.721	0.327
2006-2007	0.144	0.008	0.706	0.872	0.896	0.937	0.900	0.405	0.411
2006-2008	0.182	0.024	0.314	0.871	0.892	0.928	0.918	0.588	0.327
2006-2009	0.211	0.027	0.292	0.878	0.896	0.896	0.898	0.695	0.838
2006-2010	0.252	0.025	0.360	0.871	0.893	0.912	0.910	0.629	0.506
2006-2011	0.268	0.022	0.430	0.868	0.890	0.916	0.916	0.634	0.335
2006-2012	0.293	0.013	0.655	0.868	0.893	0.913	0.908	0.573	0.430

Notes: Authors' calculation using Acayucan administrative data. Table presents the internal validity for the respondent (stayers) and for the population tests as described in Ghanem, Hirshleifer, and Ortiz-Beccera (2023).

Table A.9: Paving Incidence on Tax compliance, Sample of Stayers by Year

	Pooled	**2007**	**2008**	**2009**	**2010**	**2011**	**2012**
				ITT			
Assign to Paved	0.044**	0.030	0.023	0.049**	0.050*	0.032	0.086**
	(0.016)	(0.019)	(0.021)	(0.024)	(0.026)	(0.026)	(0.026)
				LATE			
Paved	0.063	0.081	0.048	0.062**	0.064*	0.043	0.156**
	(0.047)	(0.056)	(0.045)	(0.030)	(0.033)	(0.035)	(0.058)
Observations	4,946	910	870	839	795	778	752
F-Stat First Stage	56.06	8.46	13.02	77.25	74.48	60.41	15.71

Notes: the stayers group for a respective year corresponds to the plots continuously observed from 2005 to that year. The ITT and LATE estimates are presented for both the assignment to paved and actually paved plots. The pooled estimates are computed for the stayers group for every year.

Table A.10: Prediction Accuracy of migration compliance for different models

Panel A: rolling window cross validation for stayers

	2005	2006	2007	2008	2009	2010	2011	2012
Minimum compliance	66.22	69.55	72.74	75.40	76.86	78.19	88.30	87.63
Maximum compliance	95.48	92.15	88.96	86.30	84.84	83.51	73.40	74.07
Last Observed	-	77.13	78.46	79.26	80.98	82.85	82.05	100.00
No one pays	11.70	15.03	18.22	20.88	22.34	23.67	33.78	33.11
Everyone pays	88.30	84.97	81.78	79.12	77.66	76.33	66.22	66.89
Random Forest (CV)	-	98.94	98.54	98.67	98.80	98.80	98.80	100.00
Lasso-logit (CV)	-	82.45	78.46	79.26	80.98	82.85	82.05	100.00
Boosting (CV)	-	89.36	86.97	84.97	86.57	87.23	83.11	100.00
Super learner (CV)	-	95.88	95.61	95.35	95.88	95.61	87.50	100.00
Stayers obs	752	752	752	752	752	752	752	752

Panel B: rolling window cross validation for leavers

	2005	2006	2007	2008	2009	2010	2011	2012
Minimum compliance	84.42	84.92	86.08	85.95	87.50	80.43	82.76	-
Maximum compliance	92.96	92.46	89.87	84.30	82.95	84.78	75.86	-
Last Observed	-	87.94	89.87	89.26	95.45	97.83	100.00	-
No one pays	10.55	11.06	13.92	19.83	19.32	17.39	27.59	-
Everyone pays	89.45	88.94	86.08	80.17	80.68	82.61	72.41	-
Random Forest (CV)	-	87.44	84.81	81.82	88.64	95.65	82.76	-
Lasso-logit (CV)	-	89.95	89.87	89.26	95.45	97.83	100.00	-
Boosting (CV)	-	88.94	87.34	83.47	93.18	97.83	93.10	-
Super learner (CV)	-	88.44	86.71	80.17	94.32	97.83	93.10	-
Leavers obs	199	199	158	121	88	46	29	-

Note: for 2012 in panel B, there are values because there are 2 pairs of payer-lot that are the same for all years but for some of the other years there is no record. Accuracy computes the percentage of cases correctly classified. Superlearner uses a weighted combination of random forest, boosting, lasso-logit and neural networks. Cross Validation suggests that the best fit corresponds to a 58% and 42% weighted version of Boosting and random forest