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THE FISCAL CONTRACT UP CLOSE: EXPERIMENTAL EVIDENCE FROM MEXICO CITY

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ABSTRACT

Can the provision of public goods strengthen the fiscal capacity of governments in developing countries and move them toward an equilibrium of widespread tax compliance? We present evidence of the impact of local public infrastructure on tax compliance, leveraging a large public investment experiment and individual property tax records from Mexico City. Despite the salience and large effects of these investments on access to infrastructure, property values, and local economic development, we find no changes in property tax compliance and can rule out even small increases. These null effects persist even when taxpayers are reminded about the tax-benefit link.

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1. Introduction

To explain the emergence of the modern fiscal state, social scientists emphasize a fiscal contract that secured new taxation powers for rulers in exchange for policies favored by elites (Schumpeter 1954; North 1981; Bates and Lien 1985; Tilly 1992; Cox 2016). Recent models of fiscal capacity expand this fiscal contract to the broader population by assuming that some taxpayers are "civic-minded" and participate in the contract if governments boost their tax morale by providing public goods (Levi 1989; 1997; Besley 2020). Fiscal contracts like these have the potential to hold governments accountable for providing public goods while simultaneously raising revenue to fund public goods—both key issues in developing countries. In practice, tax administrators also emphasize efforts to raise tax morale as a means to increasing tax compliance (Luttmer and Singhal 2014).

Can the provision of public goods strengthen the fiscal capacity of governments in developing countries and move them toward an equilibrium of widespread tax compliance? Should policymakers count on public good provision to raise tax revenue through a positive fiscal externality? Because empirically documenting the tax-benefit link and identifying its causal direction is challenging, the literature has not provided definitive answers to these questions. Large and salient public goods are typically not allocated randomly and the benefits that these public goods generate should, by definition, apply to the public at large rather than to specific target groups.

This paper studies the fiscal contract by leveraging administrative property tax data and the randomized provision of urban infrastructure in Mexico City. Property tax payments are tightly linked to the provision of local infrastructure, yet delinquency in our context is high, with 40% of households not paying their annual tax liability. To evaluate the fiscal contract, we study the *Hábitat* program, which between 2009 and 2011 randomly assigned poor neighborhoods to receive large transfers for investments in infrastructure like piped water, sewerage, electrification, road improvements, and community facilities.

The *Hábitat* program is an ideal opportunity to examine the fiscal contract. A prior evaluation found that these infrastructure projects led to substantial increases in land values and private investment (McIntosh et al. 2018) as well as higher local wages (Rogger et al. 2023), showing that the investments meaningfully affected local communities. The program's success reflects that the investments constituted a large intervention: spending per-property was 3.7 times the average property tax payment and 1.8 times the average monthly per capita income. This spending filled a gap in urban infrastructure for under-served communities and continued to operate for several years. For these reasons, the *Hábitat* program is a meaningful intervention to study whether the provision of public goods increases tax compliance. In the context of theories of the fiscal contract (e.g., Levi 1989; Besley 2020), our setting allows us to identify whether the marginal taxpayer is "civic-minded," in the sense that they would reciprocate by paying taxes if the government provides public goods.

We document three sets of results using property-level tax payment data and randomized treatment assignment. First, we show that the *Hábitat* program did not increase tax compliance on the extensive or intensive margins. Our estimates rule out even small increases in compliance: the minimum detectable effect (MDE) on the tax compliance share is 4.2 percentage points (pp), which corresponds to a 10.8% increase relative to the control group mean. These results hold for different measures of compliance, alternative inference methods, and multiple control groups, including properties that received the *Hábitat* program after the randomized evaluation and those that did not participate in program.

Second, we explore whether reciprocal (i.e., civic-minded) taxpayers are present among subsets of the population. Theories of the fiscal contract predict larger treatment effects for properties with smaller tax liabilities, lower tax rates, and in areas with higher initial compliance. We find no evidence of any heterogeneity along these margins. We also use causal forest methods to characterize heterogeneous treatment effects more systematically. The distribution of conditional average treatment effects is tightly centered around the mean estimate and an omnibus test fails to detect meaningful heterogeneity.

Finally, we rule out that our null effects are due to a lack of knowledge of the program or a lack of awareness of the link between tax payments and public goods. We document that *Hábitat* increased awareness of the program and trust in government officials. These results are consistent with the idea that residents attribute the benefits of the program to the government, which is necessary but not sufficient for an effect on tax compliance. We then combine variation from the *Hábitat* program with a letter experiment that informed taxpayers of their outstanding property tax due and reminded them that non-compliance incurs sanctions. One version of the letter also made the tax-benefit link salient by highlighting that property tax payments fund local public goods. On their own, the letters led to increases in the tax compliance share of between 5.5 and 7.5pp, which are larger effects than the MDE for the main *Hábitat* treatment. Yet, households that received both *Hábitat* and the letter that reinforces the tax-benefit link do not exhibit significantly higher compliance than households that only received the letter.

Taken together, our results are consistent with two possible interpretations. One possibility is that the public infrastructure program may not have been valuable enough to elicit a tax response from reciprocal taxpayers. This is unlikely because investments were sizable, created substantial benefits, and increased land values and trust in local leaders. We therefore conclude that reciprocal taxpayers are not present among the 40% of taxpayers that are delinquent. This finding implies that the assumption of civic-minded preferences in Besley (2020) is not applicable in our setting. Instead, our findings are in line with the idea that fiscal contracts are hard to sustain beyond a narrow elite because taxpayers can freeride on public goods and cannot credibly commit to paying taxes after receiving a benefit from the government (e.g., Gelbach 2008). Given limited enforcement capacity, taxpayer behavior is consistent with a cost-benefit consideration as in Allingham and Sandmo (1972). From a policy perspective, we find that while infrastructure investments are desirable in their own right (McIntosh et al. 2018), they do not generate a positive fiscal externality that reduces their net cost.

Our findings contribute to several literatures. While there is some evidence that supports the existence of fiscal contracts based on quasi-voluntary compliance, most of this evidence is observational or at high levels of aggregation (e.g., Ghura 1998; Timmons and Garfias 2015; Kresch et al. 2023), from surveys (e.g., Bodea and Lebas 2014; Ortega, Ronconi, and Sanguinetti 2016; Sjoberg et al. 2019), or from laboratory or information experiments (e.g., Alm, Jackson, and McKee 1992; Cummings et al. 2009; Montenbruck 2023; Beramendi, Cansunar, and Duch n.d.). A few recent studies use field experiments to study the effects of modest public investments in small cities (Gonzalez-Navarro and Quintana-Domeque 2015; Krause 2020; Carrillo, Castro, and Scartascini 2021; Khan et al. 2022), but the estimated effects in these studies are small in magnitude or statistically insignificant, and fall below our estimated minimum detectable effect. This paper contributes to the literature by providing experimental evidence of a large infrastructure intervention in the largest metropolitan area in North America, which yields precisely estimated null effects on tax compliance.

This paper also contributes to a growing literature on taxation and development (see, e.g., Besley and Persson 2013; Pomeranz and Vila-Belda 2019). An active stream of work studies property tax compliance and has shown the effectiveness of various types of tax enforcement interventions (Bergeron, Tourek, and Weigel 2023; Okunogbe 2021; Kapon, Del Carpio, and Chassang 2022; Brockmeyer et al. 2023) and of incentives for bureaucrats (Khan, Khwaja, and Olken 2016; 2019), while finding limited evidence for the effectiveness of tax morale interventions (Regan and Manwaring 2023; Dunning et al. 2017). Our findings cast further doubt that tax compliance can be meaningfully improved by boosting tax morale through the provision of public goods. Appendix A discusses connections with the existing literature in more detail.

2. Empirical Predictions from Theories of the Fiscal Contract

The overarching question we address is whether the provision of public goods can help governments in developing countries move towards a high tax compliance equilibrium. The theoretical model of Besley (2020) characterizes how a government's ability to collect substantial revenue depends on a fiscal contract in which the tax morale of citizens leads to quasi-voluntary tax compliance. Building on the ideas of Levi (1989; 1997), his model allows for some citizens to be "civic-minded": if the government provides public goods, then these citizens will act reciprocally and pay their taxes.[1]

According to the theory, in addition to weighing the material costs and benefits of paying taxes, civic-minded citizens are also reciprocal and their tax compliance is stimulated by the provision of public goods. We test this theoretical prediction in two ways. First, we examine the effects of the *Hábitat* program on tax compliance. Second, we combine the *Hábitat* program with an experiment in which taxpayers are reminded that their tax payments contribute to public goods. This second test focuses on citizens who randomly receive public goods and this reminder. As we detail in

[1] Besides this reciprocity with the government, Levi (1989; 1997) also highlights the importance of "ethical reciprocity," where observing other taxpayers' compliance encourages quasi-voluntary compliance. We incorporate this idea into our heterogeneity analysis.

Appendix B, if some taxpayers are civic-minded, the effect of receiving public goods should be even larger when taxpayers are reminded that their taxes funded them.

3. The *Hábitat* Program

The *Hábitat* program started in 2003 and targeted low-income neighborhoods by investing in local amenities. The program involved local communities in the selection of infrastructure projects that would increase residents' quality of life. These investments consisted primarily of heavy-infrastructure projects such as street paving, piped water and sewerage, but also included urban renovation projects like medians, sidewalks, community centers, sports facilities, and parks. Figure F.3 illustrates the types of projects that were implemented, as observed on Google Maps. In Mexico City, 50% of *Hábitat* project costs were paid by the federal government and 50% were paid by the city and municipal governments, which collect property taxes.

The *Hábitat* program was evaluated with a nationwide randomized controlled trial from 2009–2011 and later expanded. The evaluation studied eligible low-income urban neighborhoods, or "polygons," across 65 participating municipalities. Polygons were units specifically created for *Hábitat*, unrelated to other administrative boundaries. To be eligible, polygons had to include formally settled households in urban areas with at least 15,000 people, asset poverty rates of over 50%, deficient infrastructure and urban services, and no active conflict over land tenure. The evaluation followed a two-stage saturation design, as described in McIntosh et al. (2018). In the first stage, municipalities were assigned a polygon treatment probability drawn from a uniform distribution between 0.1 and 0.9. In the second stage, polygons were assigned into treatment or control within participating municipalities based on the first-stage assignment probability.

In Mexico City, randomization was conducted for a sample of 20 polygons across four municipalities totalling 7,947 properties (Figure 1, Panel A), with 397 properties per polygon on average.[2] Twelve of these polygons (containing 3,210 properties) were assigned to the treatment group. As we discuss below, we can precisely estimate effects despite the small number of clusters because we find a low

[2] The participating municipalities were Gustavo A. Madero, Iztapalapa, Tlalpan, and Tláhuac. Three additional municipalities (16 polygons) were initially randomized but dropped out for not meeting cost-sharing requirements. Because treatment assignment was randomized within each municipality, we follow McIntosh et al. (2018) in excluding these municipalities from the analysis. Implementation was high but not universal; we thus also present Complier Average Causal Effect estimates in Tables E.4 and E.5.

coefficient of inter-cluster correlation. Following our pre-analysis plan, we conduct robustness checks with additional control units, including those that were treated in later years (see Appendix C). Based on reports of *Hábitat* expenditures in McIntosh et al. (2018), we calculate that the program invested \$141 USD per property in the areas assigned to treatment. This amount is economically significant: it represents 3.7 times the average property tax payment of \$38 USD per property, and 1.8 times the average monthly per capita income in the sample of \$78 USD.

In the national evaluation of the program, McIntosh et al. (2018) found that the provision of infrastructure increased property values and crowded in private investment: for every dollar spent by the *Hábitat* program, they found that average private land values increased by \$2. As we show below using survey data from the evaluation, residents in treated areas were more likely to report knowing about the program, both nationally and in Mexico City (see Table 2 Panel A and Garfias, Lopez-Videla, and Sandholtz 2021). Following the successful randomized evaluation, the program continued to expand nationally. By 2017, *Hábitat* had been implemented in over 600 municipalities across all states, including in nearly 200 additional polygons in Mexico City.

Hábitat provides a unique opportunity to evaluate fiscal contract theory and experimentally estimate the impact of infrastructure provision on tax compliance. Not only was treatment assignment randomized across eligible municipalities, but this treatment represented a sizable in-equilibrium expansion of a prominent infrastructure program in Mexico between 2003 and 2018. Moreover, our treated communities are similar in characteristics to lower-income countries around the world.

4. Survey and Administrative Property Tax Data

Our main analysis is based on property-level administrative tax records from Mexico City. These data cover all properties in the city's cadaster, and include the bi-monthly tax bills and bi-monthly payment records for each property, spanning 2008–2012 (see Brockmeyer et al. 2023 for additional details). We consider two main measures of tax compliance: (1) an indicator for making any tax payment, and (2) the share of tax liability paid (the "compliance share").[3] We also consider additional outcomes: log of tax payments among taxpayers with positive payments in all years and log of a property's fiscal value.

[3]When calculating the compliance share, we take into account the early-bird discounts taxpayers receive when paying their annual liability in full before a deadline early in the year (Brockmeyer et al. 2023). Taxpayers paying before the early-bird deadline are considered fully compliant.

The administrative data also contain key property-level covariates: property tax liability starting in 2008; property tax rate; prior tax compliance; payment modality (in full or in installments); and other property features, including age, surface area, number of levels, and constructed area. From these property-level data, we construct neighborhood-level measures of initial tax compliance at the *Hábitat* polygon level.

We merge the administrative tax data with multiple data sources from the *Hábitat* program. First, we use administrative data on polygon treatment assignment. Second, we use a street-level visual survey of infrastructure that was conducted before and after the randomized implementation of the program, i.e., in March to July 2009 and February to March 2012 (McIntosh et al. 2018). This allows us to confirm that the program increased access to local amenities.

Third, concurrent with the visual survey, enumerators conducted a household survey. One household per street block was randomly selected to be surveyed and was randomly given a short or long questionnaire.[4] We use reported recognition of the program from the short survey and trust in local leaders and public officials from the long survey.

4.1 Descriptive Statistics and Balance

We merge data on *Hábitat* treatment and property tax records using the 2019 public cadaster, the earliest version available. We are able to merge 75% of the *Hábitat* treated properties and 68% of properties in the control polygons. Table E.1 shows there is no statistically significant difference in match rates between the treatment and control groups.[5]

The average property tax compliance rate in the control group is 49%, lower than the 65% average across all properties in Mexico City. This is consistent with *Hábitat* targeting poorer neighborhoods, where property owners face relatively higher average tax rates (Brockmeyer et al. 2023) and may struggle to pay.

Figure 1, Panel B, shows that the treatment and control units are balanced on pre-program characteristics. Table E.1 shows that assignment to *Hábitat* predicts significant differences in only 2 of 25 pre-treatment covariates (streets in treated neighborhoods are less likely to have stores or open

[4]The endline survey covered 430 of 436 blocks in participating city polygons. Six blocks in one treated polygon (1/3 of that polygon's blocks) were closed off due to safety concerns and not surveyed.

[5]The unmatched *Hábitat* properties likely exited the cadaster after the study period (i.e., between 2009 and 2019).

dumps). While we reject the null that the covariates do not jointly predict treatment assignment using traditional clustered standard errors, we cannot reject this joint null using the wild-cluster bootstrap (p=0.26) of Cameron, Gelbach, and L. Miller (2008), which we also use given the low number of clusters (Table E.2). Moreover, there are no substantive differences across treatment and control groups; a one standard-deviation change in each of the pre-treatment covariates is associated with an average change of 3.6pp (no larger than 12pp) in the likelihood of assignment to treatment.

5. Tax Compliance Effects of the *Hábitat* Program

Our baseline analyses estimate intent-to-treat (ITT) effects using the regression specification:

$$y_{i,p,t} = \theta Hábitat_{p,t} + \gamma \bar{y}_{i,p,PRE} + \sum_{m=1}^{M} \beta_c + \sum_{t=1}^{r} \delta_t + \zeta X_{PRE} + \varepsilon_{i,p,t}, \tag{1}$$

where i indexes properties; p, polygons; and c, municipalities. $Hábitat_{p,t}$ indicates whether a polygon was randomly selected to participate in the *Hábitat* program. As in McKenzie (2012), we rely on an ANCOVA specification that improves statistical power by conditioning on prior values of the outcome variable, $\bar{y}_{i,p,PRE}$. The specification also includes municipality-specific intercepts, β_c, to account for different municipal-level assignment probabilities, and period fixed effects, δ_t, that capture the mean for the control group in each time period. Finally, to increase precision, we include pre-*Hábitat* covariates, X_{PRE}. For each outcome, we follow the approach of Belloni, Chernozhukov, and Hansen (2014), who use a Lasso regression to select covariates that are predictive of the post-treatment outcome.

We conduct inference using three approaches. First, we allow for spatial correlation by clustering standard errors at the polygon level, the unit of treatment assignment. Second, due to the low number of clusters in the experiment, we also employ the wild-cluster bootstrap (Cameron, Gelbach, and L. Miller 2008) and report bootstrapped p-values. Finally, we also report p-values based on randomization inference, where the sharp null hypothesis is that the intervention had no effect on any of the treated units.

We start by confirming that *Hábitat* improved local infrastructure, which is a necessary pre-condition for an effect on tax compliance. Panel A of Table 1 reports the effects of the *Hábitat* program on street-level measures of infrastructure using the survey data. Columns (2)–(7) report effects on indicators for six types of infrastructure that were targeted by the program across all of Mexico. While we estimate positive effects for all measures, the two significant outcomes are piped water and sewerage. *Hábitat* increased access to piped water by 12pp, a 16% increase in coverage relative to the control group mean of 76%. This effect is statistically significant using the clustered standard errors reported in parentheses, has a p-value of 0.015 using the wild-clustered bootstrap (in square brackets), and has a p-value of 0.013 using randomization inference (in curly brackets). In the case of sewerage, the program increased access by 10pp, a 10% increase in likelihood. While the program targeted many types of infrastructures across the country, budget documents from Mexico City corroborate that the bulk of the *Hábitat* funds were directed to water access projects (SAF 2009; 2010; 2011).

To avoid making selected inferences on two of the six measures, we follow Anderson (2008) by creating an infrastructure index. This index normalizes each of the six measures and generates a weighted average that takes into account the covariance between them. We then normalize this index to have mean zero and unit standard deviation. Consistent with the fact that we estimate positive effects on all measures of infrastructure access, column (1) shows that the *Hábitat* program increased infrastructure by 0.22 standard deviation units. This effect is statistically significant using standard clustering procedures and the wild-cluster bootstrap. Overall, and in line with the national-level results in McIntosh et al. (2018), Panel A of Table 1 shows that the intervention led to a statistically significant increase in overall infrastructure and to economically important increases in piped water and sewerage, the main infrastructure investments in Mexico City.

Panel B of Table 1 reports the effects on property tax compliance outcomes. The first two columns show results for an indicator of making any property tax payments. Column (1) reports the estimate of θ absent controls. Column (2) reports the estimate from a regression with covariates that were selected via Lasso: average pre-*Hábitat* tax compliance at the polygon level, the share of paved streets in a property's block, and the number of streets in the block. Both columns show negative point estimates close to zero. Across all three inference approaches, we fail to reject the null hypothesis of a zero effect on this measure of tax compliance. Panel A of Figure 2 plots average tax compliance by year, which reveals identical patterns of compliance for properties in treatment and control polygons.

To account for partial compliance, columns (3)–(4) of Table 1 Panel B report estimates of Equation 1 on the compliance share, i.e., the ratio of tax payments to tax liabilities. In column (4), the Lasso selects the same variables as in column (2). We find similar null effects of *Hábitat* on tax compliance on the intensive margin. Importantly, these estimates also show that we can rule out small effects of *Hábitat* on tax compliance. For instance, the upper bound of the 95% confidence interval of the estimate in column (4) is 2pp, which is small relative to the 39% mean value of the dependent variable. Alternatively, we can use the estimated standard error of 1.5pp from column (4) to compute that the minimum value of θ that we could rule out while maintaining standard levels of statistical power is 4.2pp ($= 2.8 \times 1.5$pp).[6] This minimum detectable effect (MDE) corresponds to a 10.8% increase relative to the mean compliance share. Panel B of Figure 2 places the MDE in context by showing that properties in both treatment and control polygons experienced an average decline in the compliance share from nearly 50% in 2009 to around 25% in 2012. Relative to this 25pp decline, an MDE of 4.2pp shows that the provision of infrastructure is not economically or statistically powerful enough to meaningfully improve tax compliance.

The remaining columns of Table 1 report estimates of θ for other measures of compliance. Column (5) shows a null effect on the log of property tax in the sample of households that always make a tax payment.[7] Columns (6)–(7) also estimate null effects of *Hábitat* on the log of the fiscal property value, ruling out the possibility that tax revenue might increase as tax authorities update fiscal values in line with market values. These results are visualized in Panels C and D of Figure 2.

5.1 Heterogeneous Effects

The previous section shows that the provision of infrastructure does not have an impact on overall tax compliance. From a policy perspective, the mean effect is the relevant quantity to assess the existence of a potential fiscal externality. However, from the perspective of fiscal contract theory, it is also important to examine whether reciprocal taxpayers are present among subgroups of the population. We thus investigate heterogeneity along pre-specified, theoretically-relevant margins

[6] See McKenzie and Ozier (2019) for a discussion of ex-post MDEs in evaluating observed statistical power.

[7] Because of the null effects on tax compliance (columns (1) and (2)), selecting on payment after *Hábitat* implementation should not induce post-treatment bias.

as well as with the causal forest method of Wager and Athey (2015), which allows for a more systematic characterization of heterogeneous treatment effects.

We first explore heterogeneous effects along the following pre-specific margins of heterogeneity: tax liability in pesos, tax rate in basis points, and individual and pre-treatment neighborhood tax compliance. Based on prior work on the fiscal contract, we hypothesize larger causal effects for properties with a smaller tax liability, with a lower tax rate, and in areas with higher initial compliance, where taxpayers likely perceive their neighbors as also complying (see Levi 1989; 1997 and Appendix B for a theoretical discussion). To do so, we augment Equation 1 by including the pre-*Hábitat* value of a given dimension of heterogeneity as an additional control, as well as an interaction with the indicator for *Hábitat* treatment. As we show in Table E.7, we do not find statistically significant interactions along any margins of heterogeneity and the estimated coefficients on the interaction terms are economically small.[8]

Panel A of Figure 3 plots marginal effects evaluated at one interquartile range (i.e., the differences between the 25^{th} and the 75^{th} percentile) above the mean.[9] For reference, this figure also plots the baseline estimate from column (2) of Panel B of Table 1. This figure shows that even when we allow for the moderator variable to range widely, we obtain estimates that are quite close to the average effect. Panel B shows similar results for compliance share, our preferred measure. These figures compare confidence intervals with dashed lines indicating 10% and 20% increases relative to the mean of each dependent variable. Even allowing for heterogeneous effects, we can rule out increases in compliance share larger than 3.9pp (10% of its mean value).

While our analysis above focuses on the most theoretically-relevant margins of heterogeneity, we also pre-specified the estimation of a causal forest (Wager and Athey 2015). This method estimates conditional average treatment effects (CATE) for each unit of observation and relies on machine-learning methods to select relevant margins of heterogeneity. Panel C of Figure 3 plots the histogram of estimated CATEs for the indicator measure of tax compliance. This histogram is tightly centered around our mean estimate in Table 1. Panel D plots a similar histogram for the tax compliance

[8] As in Gibbons, Suárez Serrato, and Urbancic (2018), we normalize the measures of heterogeneity to have mean zero so the coefficients on the *Hábitat* indicator can be interpreted as the ITT evaluated at the mean of each variable.

[9] When considering pre-*Hábitat* tax compliance, we plot marginal effects at full compliance to avoid evaluating the marginal effects at more than 100% compliance.

share, which again shows that the distribution of estimated CATEs does not display meaningful heterogeneity. As these figures show, very few units have estimated CATEs that exceed 10% of the value of the dependent variables. As suggested by Athey and Wager (2019), we conduct an omnibus test of heterogeneity, which fails to detect a significant degree of heterogeneity.[10]

As motivated above, fiscal contract theory rests on the assumption that a meaningful fraction of taxpayers are reciprocal. The analyses in this section explored whether a fiscal contract could be re-established with tax-delinquent individuals. Using both pre-specified margins of heterogeneity and machine learning techniques, we did not detect a group of taxpayers with economically meaningful or statistically significant compliance responses to the provision of infrastructure. These results show that the average null effect holds more widely in our sample. In the context of Mexico City, these results indicate that marginal delinquent taxpayers are not motivated by civic-mindedness as understood by Besley (2020).

6. The *Hábitat* Program and the Tax-Benefit Link

This section further explores two mechanisms underlying the fiscal contract theory. First, reciprocal citizens will only increase their tax compliance if they perceive an improvement in the public goods provided by the government. This implies that those citizens are aware of the provision of public goods and their view of the government improves. We thus use survey data to estimate the effects of the *Hábitat* program on public awareness of the program and on trust in government officials. A second condition implicit in fiscal contract theory is that citizens realize that public goods are funded by their taxes. To explore this consideration, we combine variation from the *Hábitat* program with a tax compliance experiment that activated the tax-benefit link by reminding taxpayers that their taxes are used to fund public goods, including the types of goods that are provided by the *Hábitat* program.

6.1 Effects on Program Knowledge and Trust in Government Officials

We use household survey data from McIntosh et al. (2018) to estimate the effect of the *Hábitat* program on beneficiaries' perceptions. Because the administrative data includes all properties and the survey data includes only household per block, the number of observations in the survey data

[10] While the histogram and omnibus test fail to detect meaningful heterogeneity, these results could also arise in settings with limited statistical power.

is smaller. For this reason, we present results using both nationwide and Mexico City-level data.

Panel A of Table 2 reports results of regressions of beneficiary perception measures on the *Hábitat* treatment indicator, an indicator for the second round of the survey, and polygon-level fixed effects.[11] Column (1) shows that *Hábitat* increased knowledge of the program among beneficiary communities by 7.5pp at the national level, relative to a mean of 20%. This estimate is statistically significant using all three inference methods. When focusing just on data from Mexico City, column (2) shows a larger estimate of 9.4pp (relative to a mean of 14%). However, given the limited sample size, this effect is not precisely estimated.

Columns (3)–(6) estimate the effects of the program on trust in public officials and in local neighborhood leaders. Using national-level data, we estimate that *Hábitat* led to statistically significant increases in trust in public officials by 3.7pp and in neighborhood leaders by 3.4pp. When focusing on Mexico City, trust in public officials and neighborhood leaders increased by 5.6 and 8.7pp, respectively.

The results in Panel A of Table 2 are unsurprising given *Hábitat*'s considerable increase in public spending. As mentioned in Section 3, the additional funds spent in Mexico City per property corresponded to 3.7 times the average property tax payment. Similarly, as documented in Section 5, the program led to significant increases in local infrastructure. Moreover, the provision of infrastructure benefited the local economy: across Mexico, the program led to increases in local property values of $2 for every $1 spent by the government (McIntosh et al. 2018) and accelerated private-sector growth (Rogger et al. 2023). Together, these facts allay concerns that our null effects are driven by an ineffective intervention. Instead, our results suggest that marginal taxpayers are not reciprocal, or that efforts to re-establish a fiscal contract with these citizens require increases in spending larger than the already-substantial outlays of the *Hábitat* program.

6.2 Increasing the Salience of the Tax-Benefit Link

Because compliance with the property tax is low, the Ministry of Finance occasionally conducts compliance campaigns by contacting non-compliant taxpayers and encouraging them to pay their tax dues. We leverage variation from one such messaging campaign conducted in 2014 that focused

[11] In Table E.9, we present results from an ANCOVA estimation similar to Equation 1.

on the tax-benefit link. Between July 28 and August 11, the Ministry sent letters to 80,000 delinquent taxpayers, requesting that they pay their outstanding tax debt accumulated since 2009. A control group of 10,000 delinquent taxpayers received no letter.[12]

Half of the letters emphasized the tax-benefit link: that property tax revenue is used to fund public services, including the types of infrastructure projects that *Hábitat* provided. The other half of the letters emphasized the sanctions used to enforce tax compliance (e.g. fines and the risk of property seizure). The tax-benefit treatment is a softer version of the pure-sanctions treatment: while the tax-benefit letter does not emphasize sanctions as strongly, it is still an official letter from the government and also mentions that non-compliance incurs sanctions. Moreover, the letters suggest enforcement capacity: they were personalized, specify the periods of non-compliance, provide instructions for making payments, and request that taxpayers make payments within 15 days (see Figures F.1 and F.2).

We leverage this letter intervention to assess whether the increased salience of the tax-benefit link (or of the sanctions for non-compliance) increased the effect of *Hábitat*. First, in column (1) in Panel B of Table 2, we estimate the direct effects of the letter experiment within the *Hábitat* sample. We find that the tax-benefit and the pure-sanctions letters increased the share of taxpayers that had made any tax payment within 60 days of the intervention by 5.1pp and 7.3pp respectively. Both effects are statistically significant. Brockmeyer et al. (2023) report the results of this experiment across the entire city and find similar results.[13] Column (3) shows that we find comparable effects on the compliance share.

While the experiment increased compliance in the *Hábitat* sample, we find no evidence that *Hábitat* itself increased compliance, even when taxpayers received a letter linking their tax payments to the provision of public infrastructure. Columns (2) and (4) present estimates of the interactions of *Hábitat* with the letter treatments. The point estimate on the *Hábitat* indicator and the interactions with either treatment are all statistically insignificant. Even households that were made more

[12] As discussed in Brockmeyer et al. (2023), the enforcement campaign matches standard practice followed by the Ministry of Finance, including the taxpayers that were targeted, how the letters were sent, and the information contained in the letters. We can therefore interpret the results of the experiment as in-equilibrium effects.

[13] In the full population, the tax-benefit letter increased any tax compliance by 4.86pp and the pure-sanctions treatment increased it by 9.36pp (Brockmeyer et al. 2023, Figure 7, Panel B).

aware of the tax-benefit link through the compliance letters did not increase their tax compliance in response to the *Hábitat* program. These results support the conclusion that taxpayers are primarily driven by cost-benefit considerations à la Allingham and Sandmo (1972), instead of by reciprocal motives as in Besley (2020).

7. Conclusion

Taken together, our results indicate that the provision of public goods does not sway non-compliant taxpayers. In addition to finding a null mean effect, we fail to detect meaningful heterogeneity across taxpayers using multiple approaches. This is inconsistent with the idea that reciprocal residents exist in this population. Because *Habitát* investments were sizable, created substantial benefits, and led to improvements in program knowledge and trust in local leaders, it is unlikely that the program failed to elicit a response from delinquent, yet reciprocal taxpayers. We estimate a null effect of the *Habitát* program on tax compliance even when we activate the tax-benefit link through a complementary enforcement intervention. These results suggest that the marginal taxpayer in our setting does not behave in the way Besley (2020) proposes. Instead, the behavior of taxpayers is more consistent with the weighing of costs and benefits of non-compliance as in Allingham and Sandmo (1972).

These findings have theoretical and policy implications. Fiscal contract theories propose that taxation and accountability go hand in hand, both because coercive taxation fosters citizen political engagement and because governments elicit quasi-voluntary tax compliance from reciprocal citizens through favorable policies or the provision of public goods (e.g., Levi 1989; Moore 2004; Timmons 2005; Prichard 2015). There is limited but growing evidence for the first mechanism, that taxation leads to increased demands for accountability (e.g., Paler 2013; Gadenne 2017; Martínez 2024; Weigel 2020; Martin 2023). However, our findings cast doubt on the feasibility of fostering a fiscal contract based solely on quasi-voluntary tax compliance by regular citizens. From a policy perspective, our findings indicate that the provision of public goods does not have a significant fiscal externality. Even so, because the threat of coercion does raise tax compliance in our setting (Brockmeyer et al. 2023), as well as in others (e.g., Atinyan and Asatryan 2022; Bergeron, Tourek, and Weigel 2023; Okunogbe and Tourek 2024), policymakers continue to have effective policy tools at their disposal.

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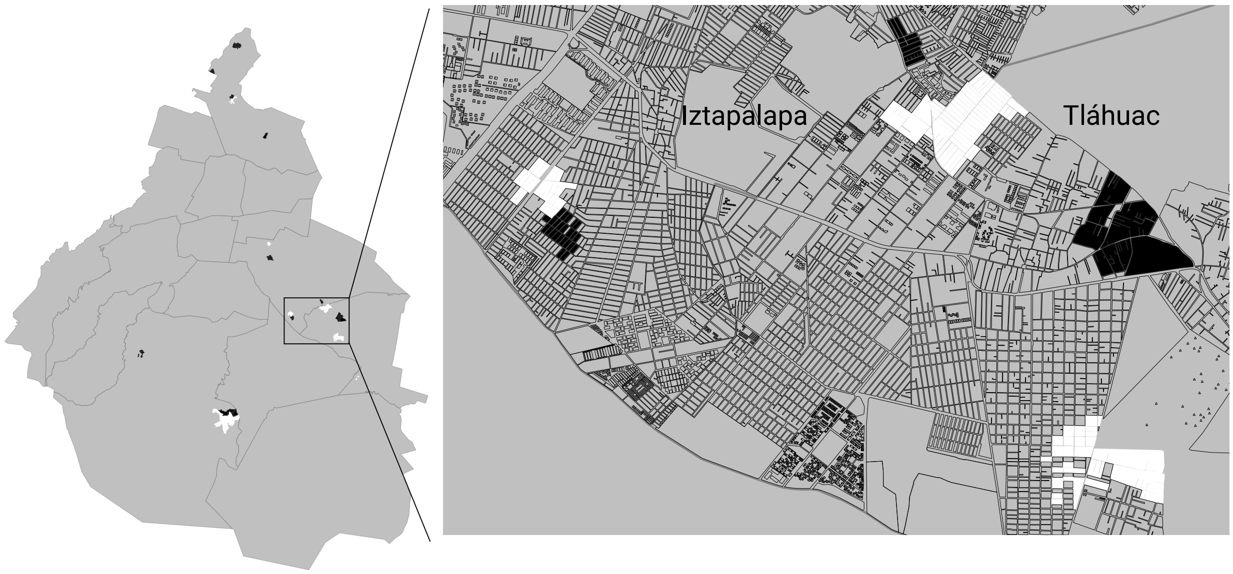
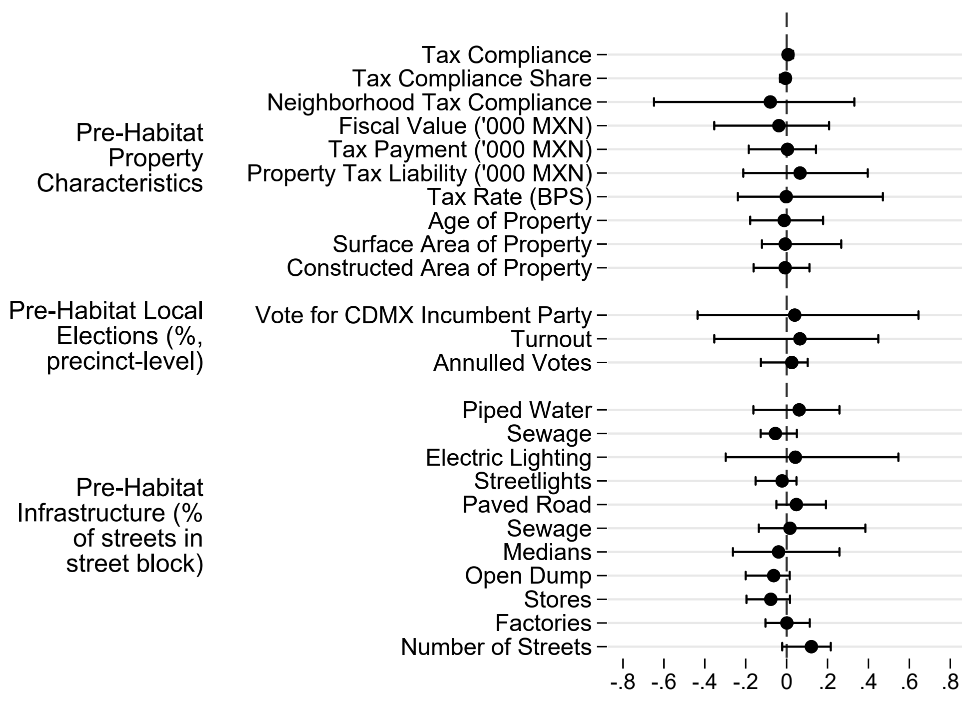
Figure 1: The *Hábitat* Program in Mexico City



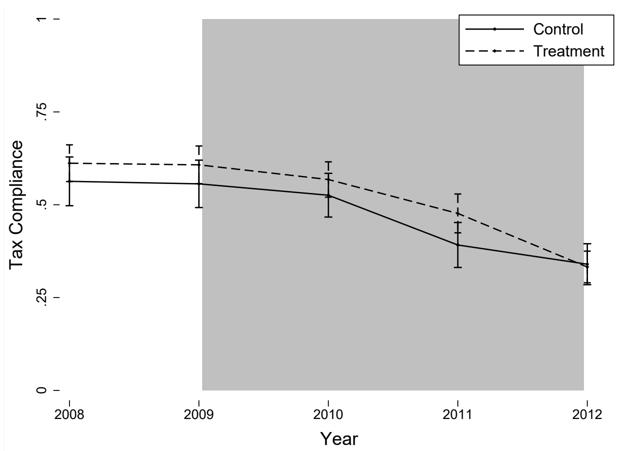
(a) Treated (black) and Control (white) Areas in Mexico City



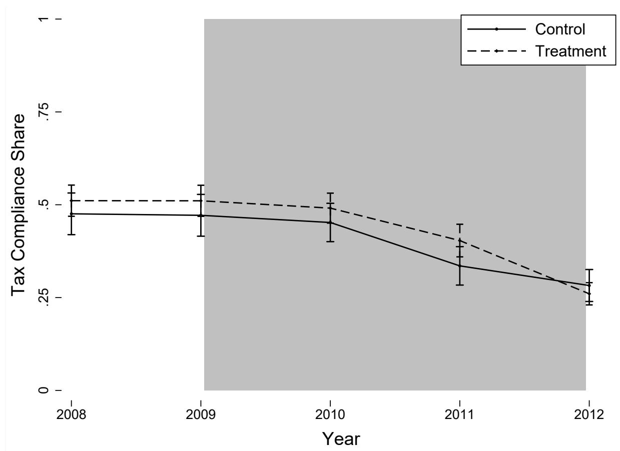
(b) Baseline Differences Between Treated and Control Groups

Panel A shows the intervention areas in Mexico City. Panel B reports results of OLS regressions of an *Hábitat* assignment indicator on standardized pre-treatment variables. We do not find statistically significant differences in any variables. For the tax compliance variables in particular, we find very precisely estimated null differences. The unit of analysis is the property. 95% wild-cluster bootstrap confidence intervals are clustered at the *Hábitat* polygon level. See Section 3 for additional discussion of Panel A and Section 4.1 for additional discussion of Panel B.

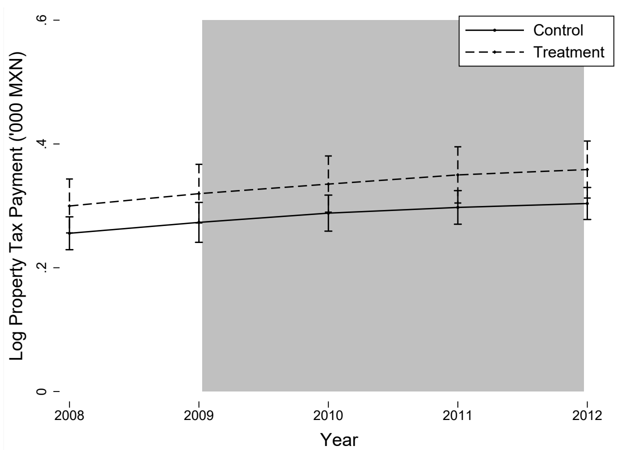
Figure 2: Tax Compliance Over Time



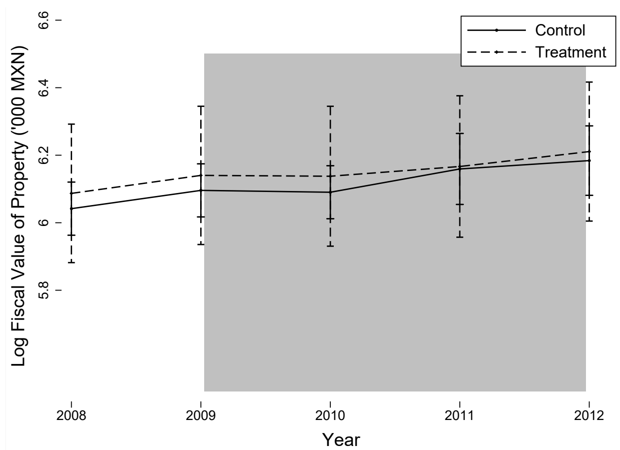
(a) Tax Compliance



(b) Tax Compliance Share



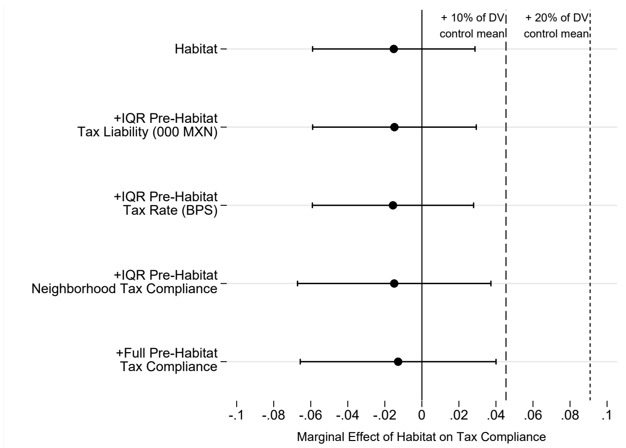
(c) Log Tax Payment



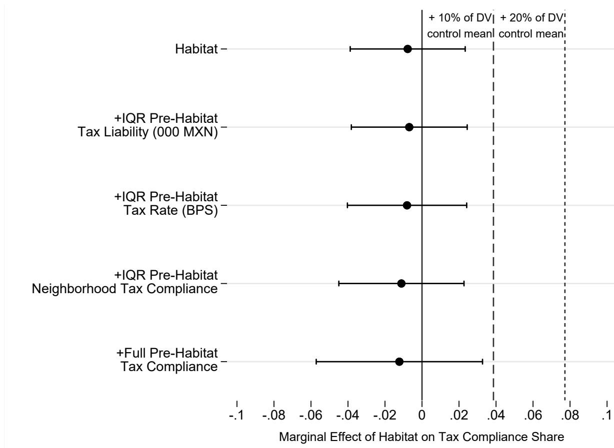
(d) Log Fiscal Value

These figures plot different measures of tax compliance over time. We aggregate property-year values at the *Hábitat* polygon-year level and weight them by the number of properties in each polygon. 95% confidence intervals based on the standard error of the mean. See Section 5 for additional discussion.

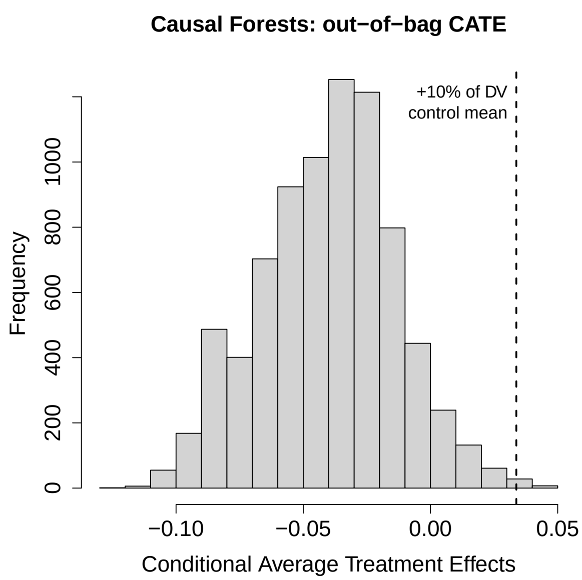
Figure 3: Heterogeneous Effects of *Hábitat*



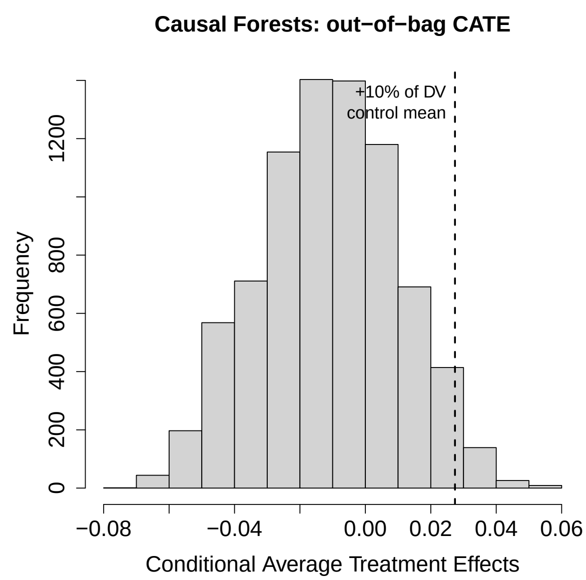
(a) Tax Compliance



(b) Tax Compliance Share



(c) Tax Compliance



(d) Tax Compliance Share

Panels A and B plot OLS estimates of the effect of *Hábitat* while allowing for heterogeneous effects along a number of dimensions. The average effect of *Hábitat* is included at the top of each panel and each row plots the differential effect of *Hábitat* for properties in the 25th and 75th percentiles of each moderator variable. See Tables E.7 and E.8 for point estimates. The vertical lines illustrate changes of 10% (dashed) and 20% (short dashed) in the outcome control mean. The unit of analysis is the property-year and the 95% confidence intervals are clustered at the *Hábitat* polygon level. Covariates are selected using a Lasso regression. Panels C and D plot the histogram of out-of-bag CATE estimates from a causal forest. We include all the available covariates (see Table E.1) and use 100,000 trees to estimate the forest. Following Athey and Wager (2019), we conduct an omnibus test of heterogeneity. We fail to reject the null of no heterogeneity for both compliance ($\hat{\beta} = -1.7$, with $p = 0.9$) and compliance share ($\hat{\beta} = -1.2$, with $p = 0.46$). See Section 5.1 for additional discussion.

Table 1: Intent-to-Treat Effects of *Hábitat* on Infrastructure and Tax Compliance

Panel A:	Infrastructure Index	Piped Water	Sewerage	Electrification	Streetlights	Medians	Sidewalks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hábitat	0.22	0.12	0.078	0.019	0.039	0.083	0.043
	(0.083)	(0.043)	(0.039)	(0.011)	(0.049)	(0.080)	(0.087)
	[0.017]	[0.015]	[0.067]	[0.36]	[0.52]	[0.42]	[0.75]
	{0.13}	{0.013}	{0.25}	{0.080}	{0.69}	{0.46}	{0.70}
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-*Hábitat* Outcome as Covariate	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of DV	0	0.76	0.80	0.95	0.75	0.45	0.48
Control SD of DV	1	0.43	0.40	0.22	0.44	0.50	0.50
R sq.	0.37	0.29	0.27	0.092	0.26	0.47	0.46
Num. *Hábitat* Polygons	20	20	20	20	20	20	20
Observations	2067	2067	2067	2067	2067	2067	2067
Panel B:	Any Property Tax Compliance		Share of Property Tax Compliance		Log Tax Payment ('000 MXN) (Full Compliers)	Log Fiscal Value ('000 MXN)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hábitat	-0.015	-0.015	-0.010	-0.0077	0.014	-0.0058	-0.0050
	(0.031)	(0.021)	(0.024)	(0.015)	(0.011)	(0.0054)	(0.0057)
	[0.67]	[0.57]	[0.72]	[0.67]	[0.39]	[0.36]	[0.44]
	{0.72}	{0.61}	{0.76}	{0.69}	{0.53}	{0.47}	{0.55}
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-*Hábitat* Outcome as Covariate	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	No	Yes
Control Mean of DV	0.45	0.45	0.39	0.39	0.29	5.87	5.87
Control SD of DV	0.50	0.50	0.45	0.45	0.28	0.72	0.72
R sq.	0.34	0.34	0.30	0.31	0.85	0.94	0.94
Num. *Hábitat* Polygons	20	20	20	20	19	20	20
Observations	31788	31740	31788	31740	6864	31788	31740

Notes: Panel A shows that, in Mexico City, the *Hábitat* program primarily increased access to water infrastructure. The panel reports OLS estimations of Equation 1 where the unit of analysis is the street. These regressions control for Lasso-selected controls, though Table E.3 shows that we obtain similar results without including these controls. Nationwide, by contrast, McIntosh et al. (2018) do not find significant changes in water infrastructure, but instead report improvements in streetlights, sidewalks, and road infrastructure. Panel B reports the OLS-estimated effects of *Hábitat* on various property tax compliance outcomes. We only report one result for log tax payments because the Lasso does not select any controls. Log fiscal values are assessed by the government and can be updated either by the government or by the taxpayer when upgrading the property. Figure 2 displays the event study coefficients for these outcome variables. To consider the imperfect implementation of *Hábitat* in treated polygons, we also report two-stage least squares estimates of the Complier Average Causal Effect on infrastructure and tax compliance in Appendix Tables E.4 and E.5, respectively. For both panels, standard errors clustered at the *Hábitat* polygon level are reported in parentheses, wild-cluster bootstrap p-values are reported in brackets, and randomization inference p-values are reported in curly brackets. In our main analysis, arrears tax payments are allocated to the year in which the liability arose. The results are almost unchanged when we allocate arrears payments to the year in which the payment was made (Table E.6, Figure E.2). See Section 5.1 for additional discussion.

Table 2: Mechanisms of the Null Effect of *Hábitat* on Tax Compliance

Panel A:	Effects of *Hábitat* on Beneficiaries' Perceptions					
	Knowledge of *Hábitat*		Trust in Public Officials		Trust in Neighborhood Leaders	
	(1)	(2)	(3)	(4)	(5)	(6)
Hábitat	0.075	0.094	0.037	0.056	0.034	0.087
	(0.027)	(0.087)	(0.020)	(0.047)	(0.014)	(0.049)
	[0.0050]	[0.36]	[0.074]	[0.26]	[0.024]	[0.13]
	{0.015}	{0.37}	{0.20}	{0.46}	{0.098}	{0.12}
Round 2	-0.098	-0.079	-0.059	-0.17	-0.053	-0.12
	(0.022)	(0.043)	(0.015)	(0.020)	(0.010)	(0.033)
	[0]	[0.25]	[0]	[0]	[0]	[0.010]
Polygon Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Pre-*Hábitat* Mean of Outcome	0.20	0.14	0.49	0.50	0.50	0.49
Pre-*Hábitat* SD of Outcome	0.40	0.35	0.20	0.21	0.21	0.21
R sq.	0.095	0.056	0.10	0.24	0.12	0.14
Sample	National	CDMX	National	CDMX	National	CDMX
Num. *Hábitat* Polygons	342	20	342	20	342	20
Observations	19417	813	11129	518	10899	517

Panel B:	Heterogeneous Effects of *Hábitat* on Tax Compliance by Mail Intervention			
	Tax Compliance		Tax Compliance Share	
	(1)	(2)	(3)	(4)
Tax-Benefit Treatment	0.051	0.064	0.055	0.069
	(0.020)	(0.031)	(0.020)	(0.030)
	[0.0060]	[0.015]	[0.0060]	[0.011]
Pure Sanctions Treatment	0.073	0.096	0.075	0.098
	(0.027)	(0.029)	(0.029)	(0.031)
	[0.049]	[0.053]	[0.042]	[0.039]
Hábitat		0.014		0.019
		(0.044)		(0.043)
		[0.89]		[0.82]
Tax-Benefit Treatment × *Hábitat*		-0.037		-0.040
		(0.041)		(0.040)
		[0.43]		[0.39]
Pure Sanctions Treatment × *Hábitat*		-0.071		-0.070
		(0.047)		(0.048)
		[0.19]		[0.22]
Municipio Fixed Effects	Yes	Yes	Yes	Yes
Covariates	No	No	No	No
Control Mean of DV	0.12	0.12	0.12	0.12
Control SD of DV	0.32	0.32	0.32	0.32
R sq.	0.025	0.029	0.026	0.029
Num *Hábitat* Polygons	19	19	19	19
Observations	951	951	951	951

Notes: Panel A reports OLS estimates of the effect of *Hábitat* on knowledge of the program as well as on trust in public officials and local leaders. The unit of analysis is the household. Households were surveyed pre- and post-intervention. Observations are weighted by a population weight so that our results are representative of all residents in the study neighborhoods. Knowledge of *Hábitat* is a dichotomous variable. Trust in public officials and neighborhood leaders is measured from three ordinal responses (no trust, some trust, a lot of trust), which we transform into three values: $\{0, 0.5, 1\}$. Panel B contains OLS estimates of the effects of *Hábitat* and the mail compliance intervention on tax compliance and tax compliance share. The unit of analysis is the property. The outcome in columns (1) and (2) is the share of properties that had made any payment towards their outstanding tax liability within 60 days after the intervention. The outcome in columns (3) and (4) is the average tax compliance share (share of the tax liability that was paid) 60 days after the intervention. See Section 3 for additional discussion. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses, wild-cluster bootstrap p-values are reported in brackets, and randomization inference p-values are reported in curly brackets.

Online Appendix

The Fiscal Contract Up Close: Experimental Evidence from Mexico City

The following text to be published online.

Contents

A. Relations to Existing Literature

Our findings contribute to the literature on tax morale and fiscal contracts, and in particular on the connection between public goods provision and tax compliance. Fiscal contract theories propose that, because enforcing taxation is costly, governments seek to foster voluntary compliance by taxpayers. This, in turn, creates an opportunity for taxpayers to hold their governments accountable: in exchange for voluntary compliance with their tax obligations, taxpayers can demand policy or public services (Bates and Lien 1985; Levi 1989; Timmons 2005; Besley 2020). These types of pacts have been described between rulers and elites, particularly during the emergence of the fiscal state in early modern Europe (Schumpeter 1954; North 1981; North and Weingast 1989; Tilly 1992; Cox 2016).

Beyond narrow elite pacts, however, fiscal contracts are in theory hard to sustain in part because taxpayers can freeride on public goods and cannot credibly commit to paying taxes after receiving a benefit from the government (e.g., Gelbach 2008). Despite this, there is suggestive empirical evidence that supports the idea of a connection between public goods provision and tax compliance. Most of this evidence is observational or at high levels of aggregation (e.g., Ghura 1998; Timmons and Garfias 2015; Kresch et al. 2023), or from survey (e.g., Bodea and Lebas 2014; Ortega, Ronconi, and Sanguinetti 2016; Sjoberg et al. 2019) and laboratory experiments (e.g., Alm, Jackson, and McKee 1992; Cummings et al. 2009; Beramendi, Cansunar, and Duch n.d.).[14] In this paper, we add field experimental evidence from one of the largest cities in the world to this body of evidence.

Our focus in this project is on citizen compliance with property taxes. These taxes are ideal to assess the presence of a fiscal contract because of their salience and their close connection to local public goods provision (e.g., Cabral and Hoxby 2015; Bordignon, Grembi, and Piazza 2017; Giaccobasso et al. 2022). Recent field experiments find positive but small impacts on property tax compliance. Gonzalez-Navarro and Quintana-Domeque (2015) report a 5 percentage point intent-to-treat effect on tax compliance as a result of street paving in Acayucan, a small Mexican city. Carrillo, Castro, and Scartascini (2021) report that rewarding a randomly chosen set of reliable taxpayers in Santa Fé, Argentina with the construction of a sidewalk increases the likelihood that taxpayers pay *on time* by 3 percentage points. In addition, using a difference-in-difference style design with some assumptions, they show that neighbors of lottery winners become 3-4 percentage points more likely to pay on time. The likelihood of non-payment is not affected. Krause (2020) estimates that trash collection services result in a non-significant increase in tax revenue of 4 percent in Carrefour, Haiti. In Punjab, Pakistan, Khan et al. (2022) show that earmarking a share of property tax revenues to finance local public services increases property tax compliance by about 10 percent but a treatment that combines elicitation of taxpayer preferences with earmarking for preferred goods generates a smaller and statistically insignificant effect on compliance.[15]

[14] To emerge, fiscal contracts require that citizens react to policy and public service provision by increasing their tax compliance, which is the focus of this paper.In addition, fiscal contracts require that citizens express demands to their governments when they face tax obligations and that governments respond to these demands. Government responsiveness to taxpayer demands has been documented cross- (e.g., Timmons 2005) and sub-nationally (e.g., Brollo et al. 2013; Gadenne 2017; Martínez 2024). In turn, there is growing evidence that links higher individual tax burdens to increased citizen engagement with the state. This includes recent field experimental evidence (Weigel 2020), survey experimental evidence (Paler 2013), and laboratory experimental evidence (Martin 2016; 2023).

[15] As an alternative to providing public goods, governments can offer more inclusive political institutions that better represent taxpayers. However, evidence of large effects of access to more inclusive political institutions among citizens is also limited. In a randomized digital participatory budgeting exercise in Freetown, Sierra Leone, Grieco et al. (2024) report no average effects on tax compliance from participation, despite an increase in perceptions of government legitimacy.

The magnitudes reported in these studies, while positive and in two of these cases significant, are small and fall below our estimated minimum detectable effect.[16] The costs of the interventions are much higher than the additional tax revenue they generate. Moreover, while these studies focus on a single service or investment, the intervention we consider in this paper consists of a large set of urban infrastructure investments in poor neighborhoods that are selected by a community-driven process, and with large, documented benefits. The possible set of projects include not only road paving and sidewalk constructions, but also piped water, sewerage, electrification, and public lighting, among others.[17]

More narrowly, this paper adds to existing evaluations of *Hábitat*, which have looked at the program's effects on households (Campuzano, Levy, and Zamudio 2007; McIntosh et al. 2018), on firms (Rogger et al. 2023), and on political behavior (Garfias, Lopez-Videla, and Sandholtz 2023). Using the saturation design of the randomization, McIntosh et al. (2018) report no strong spillovers of the program on untreated areas.

B. Theoretical Predictions from Models of the Fiscal Contract

This appendix reinterprets the model of Besley (2020) in a setting of property taxation. Besley (2020) considers a model of the fiscal contract based on reciprocity. Materialist citizens dislike taxation and have strong incentives to free ride. Building on ideas in Levi (1989), civic-minded citizens are reciprocal and are willing to pay taxes but only if the government provides public goods.

In our version of the Besley (2020) model, we assume taxpayers derive consumption utility from a return r on their wealth ω. Taxpayers dislike paying a tax t on their wealth ω and can "hide" a fraction n of their wealth at cost $\omega C(n) = \omega p \frac{n^2}{2}$, where p is the probability of detection.

The government provides public goods G, but politicians can also extract rents R. Taxpayers receive reciprocity utility, which is given by: $\omega(1-n)\lambda(G-R)$. The parameter λ captures the whether citizens are materialistic ($\lambda = 0$) or civic-minded ($\lambda > 0$).

Taxpayers set the optimal level of non-compliance n^* by maximizing their utility:

$$
\begin{aligned}
n^* \quad &= \quad \operatorname*{argmax}_n \; \underbrace{(G + \omega[r - t(1-n)])}_{\text{Post-tax Income}} - \underbrace{\omega p \frac{n^2}{2}}_{\text{Non-compliance Costs}} + \underbrace{\omega(1-n)\lambda(G-R)}_{\text{Reciprocity Utility}} \\
&\implies \quad n^* = \frac{t - \lambda(G-R)}{p}. \qquad \text{(A2)}
\end{aligned}
$$

More broadly, some formulations of the fiscal contract also highlight a social component, where the individual decision to comply not only depends on government actions, but also on other taxpayers' compliance. As Levi 1989 writes, "in individuals are not getting the gains they bargained for or if they feel they are being "suckers," they will try to withdraw from the contract" (p. 53). Consistent with this idea, Del Carpio (2022) finds that randomly disclosing the observed compliance rate leads to increases in property tax compliance in Peru, a context in which taxpayers underestimate their fellow citizens' compliance.

[16] An exception is Montenbruck (2023) who finds that informing taxpayers in Sierra Leone about recently provided public goods increases tax payments by 20 percent. Part of this effect is likely driven by an improved perception of government performance.

[17] Our findings are also consistent with null tax compliance findings in studies with treatments that only reduce transaction costs to benefit from government policies (e.g., De la O et al. 2022)

If citizens are civic-minded (i.e., $\lambda > 0$), providing public goods reduces non-compliance since $\frac{\partial n^*}{\partial G} = -\frac{\lambda}{p} < 0$. The fact that $\frac{\partial^2 n^*}{\partial G \partial \lambda} = -\frac{1}{p} < 0$ means that the provision of public goods would have a larger effect on non-compliance if the reciprocity utility carries more weight λ.

To explain quasi-voluntary compliance, Levi (1989; 1997) also considers the role of "ethical reciprocity," the norm that individuals comply with the government as long as they perceive others also complying, a precondition for civic-mindedness as understood here. This motivates our analysis of heterogeneity of the provision of public goods by initial neighborhood tax compliance. Because areas with higher initial compliance provide the conditions for ethical reciprocity, we expect a higher likelihood that $\lambda > 0$ among taxpayers in these neighborhoods.

In our analysis of the letter experiment, we interpret the reminder linking tax payments to the provision of public goods as an attempt to activate λ. In contrast, providing public goods does not impact tax compliance when the marginal citizen is not civic-minded (i.e., $\lambda > 0$).

Equation A2 is the analogue of Equation (3) in Besley (2020) and shows how our analysis related to his model. However, it is worth noting two features in our setting that are not present in the model. First, in contrast to the income tax setting where citizens can "hide" a fraction of their income, it is hard for citizens to hide real estate from authorities. Second, most taxpayers either comply or do not. To show that the hypotheses above do not depend on these features, we next consider a simplified version of the model of Brockmeyer et al. (2023), which features observable tax liabilities and compliance along the extensive margin.

Following Brockmeyer et al. (2023), let $u(c^{\text{Pay}})$ denote the consumption utility when paying property taxes and $u(c^{\text{Delinquent}})$ denote the consumption utility when citizens are delinquent. Because property tax payments lower consumption, we assume that $u(c^{\text{Delinquent}}) > u(c^{\text{Pay}})$. Tax delinquency is also accompanied by a tax morale cost $M_i(\lambda, G - R)$ that depends on a common component $m(\lambda, G - R)$ and an idiosyncratic term ε_i with CDF $F(\cdot)$ and density $f(\cdot)$. Citizens pay their tax when:

$$u(c^{\text{Delinquent}}) - M_i(\lambda, G - R) > u(c^{\text{Pay}}).$$

Borrowing from the formulation in Besley (2020), we assume that $m(\lambda, G - R) = \lambda(G - R)$. The fraction of delinquent taxpayers is given by:

$$\begin{aligned} N^{\text{Delinquent}} &= \mathbf{P}r\left(\varepsilon_i < [u(c^{\text{Delinquent}}) - u(c^{\text{Pay}})] - \lambda(G - R)\right) \\ &= F\left([u(c^{\text{Delinquent}}) - u(c^{\text{Pay}})] - \lambda(G - R)\right). \end{aligned}$$

From this equation, it follows that

$$\frac{\partial N^{\text{Delinquent}}}{\partial G} = -\lambda f\left([u(c^{\text{Delinquent}}) - u(c^{\text{Pay}})] - \lambda(G - R)\right) < 0.$$

As in the formulation from Besley (2020), we have that, in the presence of civic-minded citizens (i.e., $\lambda > 0$), providing public goods decreases the likelihood of non-compliance.

C. Additional Control Polygons

In additional analyses, we also use as additional control units the polygons from the scale-up phase of the program, and properties that were neither part of the initial nor of the scale-up phase of *Hábitat*.

From the scale-up phase, we use as additional controls those polygons that received *Hábitat* in 2014 but were not included in the program during the program's evaluation (2009–2011) or in 2013.[18] Tables E.12 and E.13 show a lack of baseline differences between treated and control units (as in Table E.1) when using polygons treated in 2014 as additional controls. Table E.14 shows similar intent-to-treat effects as in Table 1.[19]

As a third comparison group, we expand the set of control polygons to include all neighborhoods in Mexico City. We weight polygons using estimated entropy weights (Hainmueller 2012) to account for differences in baseline characteristics at the polygon/neighborhood-level. See Tables E.15 and E.16 baseline differences and Table E.17 for intent-to-treat effects.

D. Deviations from Pre-Analysis Plan (PAP)

- We are unable to test our fifth hypothesis (H5), that the average causal effect of the treatment on property tax compliance is higher immediately after implementation (2012) than later, during a new local administration (2013). This is because the available property tax data covers only up to 2012.

- We adjust our second hypothesis (H2): While the original H2 stated that the average causal effect of the treatment on property tax compliance is higher in properties with a lower *accumulated* tax liability, we use the pre-treatment (2008) tax liability. We make this adjustment because the available property tax data only includes tax liability starting in 2008, the first year of available data, and does not include the accumulated tax liability up to 2008.

- We do not use socio-demographic variables from the 2010 Population Census as inputs in the covariate-selection Lasso regression, and instead include baseline access-to-infrastructure covariates at the street-block level. This is because the census enumeration took place in June 2010, almost a year after implementation began, and are therefore measured post-treatment. Including these covariates does not change the results.

- Due to difficulties in implementation, we did not estimate Rank-Weighted Average Treatment Effects (RATE).

- We do not implement nor report the proposed regression discontinuity analysis to examine our third hypothesis (H3), on the heterogeneous effect of *Hábitat* on tax compliance at by tax rate. This is because of the low number of properties in the *Hábitat* sample around the discontinuous jump in tax rates.

- We implement and report a number of additional analyses that were not included in the pre-analysis plan:

[18]The data for the scale-up phase come from the Urban, Territorial, and Agrarian Development Secretariat (SEDATU), the agency that implemented the program starting in 2013.

[19]While adding 2013 polygons as controls would further increase the sample size, these polygons are not an ideal comparison group, since we measure our main outcome during this year. We are also unable to use additional information from 2015, because all beneficiary polygons during that year also received the program in 2014.

- In addition to reporting results for the main outcome in Table E.11 and for the secondary outcome in Table 1 (Panel A, columns 3 and 4) and Table E.5 (columns 3 and 4), we also report ITT and CACE effects on two additional outcomes: the total tax payment in properties with any payment (Table 1 Panel A column 5 and Table E.5 column 5) and the log fiscal value of the property (Table 1 Panel A columns 6 and 7, and Table E.5 columns 6 and 7).

- In addition to estimating the heterogeneous effect of *Hábitat* on tax compliance by pre-*Hábitat* tax liability (H2), tax rate (H3), and neighborhood tax compliance (H4), we also report heterogeneity by pre-*Hábitat* tax compliance (Figure 3 Panels A and B; Figure E.1; Table E.7 columns 4 and 8; Table E.8 column 7; and Table E.11 column 6).

- In addition to intent-to-treat (ITT) effects, we also estimate complier average causal effects (CACE) of *Hábitat*, using two-stage least squares (Tables E.4, E.5, and E.10). This is because, after the submission of the PAP, we learned about imperfect implementation of the program across polygons assigned to treatment.

- To confirm in our sample the nationwide finding in McIntosh et al. (2018) that *Hábitat* was successfully implemented and led to increased access to infrastructure, we estimate the effect of *Hábitat* on street-level access to infrastructure in Mexico City specifically (Table 1 Panel B and Table E.4). As McIntosh et al. (2018), we focus on access to six types of infrastructure investments, and also construct an aggregate index using the approach in Anderson (2008).

- To explore the mechanisms behind our estimated null, we include two additional sets of analyses, that are suggested in the PAP (Section 6.3, p. 14) but not explicitly described. First, we examine a compliance intervention that sent reminders via mail to delinquent taxpayers. We estimate the effect of *Hábitat* among those taxpayers that also randomly received a reminder to pay their taxes that highlighted the connection between the property tax and local public infrastructure provision (Table 2 Panel B and Table E.10). Second, we estimate the effect of *Hábitat* on beneficiary recall of the program and their trust in public officials (Table 2 Panel A and Table E.9).

E. Appendix Tables/Figures

E.1 Experimental Sample

Table E.1: Baseline Differences Between Treated and Control Properties

	Property Features										
Panel A:	Missing from Tax Property Data	Pre-*Hábitat* Tax Compliance	Pre-*Hábitat* Tax Compliance Share	Pre-*Hábitat* Neighborhood Tax Compliance	Pre-*Hábitat* Fiscal Value of Property ('000 MXN)	Pre-*Hábitat* Tax Payment ('000 MXN)	Pre-*Hábitat* Tax Liability ('000 MXN)	Pre-*Hábitat* Tax Rate (BPS)	Age of Property	Surface Area of Property	Constructed Area of Property
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Hábitat	0.059	-0.045	-0.048	-0.13	0.39	0.23	0.48	0.0086	-0.044	0.080	0.67
	(0.063)	(0.14)	(0.13)	(0.40)	(0.37)	(0.22)	(0.36)	(0.38)	(0.049)	(0.090)	(0.52)
	[0.40]	[0.77]	[0.75]	[0.78]	[0.54]	[0.40]	[0.35]	[0.98]	[0.44]	[0.65]	[0.35]
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of DV	0.32	0.56	0.48	0.56	428.7	0.21	0.79	16.0	24.4	312.8	110.1
Control SD of DV	0.47	0.50	0.45	0.17	600.9	0.76	4.86	34.5	30.3	1474.9	111.2
R sq.	0.17	0.065	0.056	0.54	0.0022	0.0012	0.0026	0.014	0.0022	0.011	0.0014
Num. *Hábitat* Polygons	20	20	20	20	20	20	20	20	20	20	20
Observations	11289	7947	7947	7947	7947	7947	7947	7947	7947	7947	7947

	Local 2009 Elections (Precinct-Level)			
Panel B:	% CDMX Incumbent	% Turnout	% Annulled Votes	% Delegación Incumbent
	(1)	(2)	(3)	(4)
Hábitat	-0.037	0.096	-0.019	-0.037
	(0.23)	(0.38)	(0.25)	(0.23)
	[0.89]	[0.81]	[0.95]	[0.89]
Municipio Fixed Effects	Yes	Yes	Yes	Yes
Control Mean of DV	0.42	0.32	0.088	0.42
Control SD of DV	0.088	0.048	0.017	0.088
R sq.	0.81	0.50	0.27	0.81
Num. *Hábitat* Polygons	20	20	20	20
Observations	7947	7947	7947	7947

	Infrastructure (Street-Block Level)										
Panel C:	% Piped Water	% Sewerage	% Electrification	% Streetlights	% Paved Roads	% Sidewalks	% Medians	% Open Dump	% Stores	% Factories	Number of Streets
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)
Hábitat	0.085	-0.26	-0.17	-0.16	0.025	-0.22	-0.28	-0.42	-0.51	0.035	1.90
	(0.094)	(0.28)	(0.28)	(0.15)	(0.13)	(0.21)	(0.21)	(0.16)	(0.20)	(0.31)	(1.17)
	[0.41]	[0.48]	[0.61]	[0.33]	[0.87]	[0.38]	[0.24]	[0.021]	[0.043]	[0.92]	[0.16]
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of DV	0.96	0.98	0.98	0.96	0.93	0.68	0.56	0.62	0.60	0.012	4.96
Control SD of DV	0.17	0.095	0.11	0.15	0.23	0.44	0.45	0.43	0.32	0.055	2.42
R sq.	0.10	0.088	0.042	0.053	0.15	0.60	0.40	0.11	0.22	0.010	0.18
Num. *Hábitat* Polygons	20	20	20	20	20	20	20	20	20	20	20
Observations	7935	7935	7935	7935	7935	7935	7935	7935	7935	7935	7935

Notes: This table reports OLS estimates of *Hábitat* as a predictor for the observable characteristics of the sample. The unit of analysis is the property. All pre-treatment variables except the missingness indicator (Panel A, column 1) are standardized and the untransformed mean and standard deviation are reported. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses and wild-cluster bootstrap p-values are reported in brackets. We merge the property tax records with the *Hábitat* treatment assignment and survey data at the property level using the 2019 public cadaster. The unmatched *Hábitat* properties likely exited the cadaster after the study period (i.e., between 2012 and 2019). The resulting data is a balanced panel, so there are no additional attrition concerns. See Section 4.1 for additional discussion.

Table E.2: Baseline Differences Between Treated and Control Properties

	Assignment to *Hábitat* (1)		Assignment to *Hábitat* (1)
Pre-*Hábitat* Tax Compliance	0.0068	% Annulled Vote	0.025
	(0.0093)		(0.037)
	[0.77]		[0.64]
Pre-*Hábitat* Tax Compliance	-0.0053	% Streets with Piped Water in Block	0.061
	(0.010)		(0.028)
	[0.87]		[0.21]
Pre-*Hábitat* Neighborhood Tax Compliance	-0.080	% Streets with Sewage in Block	-0.055
	(0.17)		(0.033)
	[0.75]		[0.28]
Pre-*Hábitat* Fiscal Value ('000 MXN)	-0.038	% Streets with Electric Lighting in Block	0.043
	(0.11)		(0.036)
	[0.77]		[0.86]
Pre-*Hábitat* Tax Payment ('000 MXN)	0.0042	% Streets with Streetlights in Block	-0.022
	(0.0062)		(0.025)
	[0.57]		[0.42]
Pre-*Hábitat* Property Tax Liability ('000 MXN)	0.066	% Streets with Paved Road in Block	0.048
	(0.12)		(0.038)
	[0.64]		[0.34]
Pre-*Hábitat* Tax Rate (BPS)	-0.000041	% Streets with Sewage in Block	0.016
	(0.00068)		(0.072)
	[0.96]		[0.84]
Age of Property	-0.012	% Streets with Medians in Block	-0.039
	(0.011)		(0.041)
	[0.19]		[0.45]
Surface Area of Property	-0.0069	% Streets with Open Dump in Block	-0.063
	(0.0098)		(0.042)
	[0.71]		[0.21]
Constructed Area of Property	-0.0073	% Streets with Stores in Block	-0.077
	(0.0100)		(0.042)
	[0.45]		[0.17]
% Vote for CDMX Incumbent Party	0.040	% Streets with Factories in Block	0.0012
	(0.15)		(0.025)
	[0.86]		[0.98]
% Turnout	0.065	Number of Streets in Block	0.12
	(0.12)		(0.030)
	[0.75]		[0.071]
	F-statistic	471.98	
	F-test P-value	0.00	
	Wild Cluster Bootstrap P-value	0.26	
	Municipio Fixed Effects	Yes	
	Share of Treated Properties	0.40	
	R sq.	0.45	
	Num. *Hábitat* Polygons	20	
	Observations	7935	

Notes: This table reports estimations via OLS. The unit of analysis is the property and pre-treatment variables are standardized. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses and wild-cluster bootstrap p-values are reported in brackets. See Section 4.1 for additional discussion.

Table E.3: Intent-to-Treat Effects of *Hábitat* on Infrastructure; No Covariates

	Infrastructure Index	Piped Water	Sewerage	Electrification	Streetlights	Medians	Sidewalks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hábitat	0.21	0.12	0.085	0.027	0.028	0.085	0.035
	(0.085)	(0.041)	(0.039)	(0.0099)	(0.053)	(0.083)	(0.088)
	[0.024]	[0.034]	[0.075]	[0.060]	[0.65]	[0.40]	[0.81]
	{0.18}	{0.017}	{0.17}	{0.043}	{0.81}	{0.51}	{0.76}
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-*Hábitat* Outcome as Covariate	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of DV	0	0.76	0.80	0.95	0.75	0.45	0.48
Control SD of DV	1	0.43	0.40	0.22	0.44	0.50	0.50
R sq.	0.36	0.28	0.26	0.080	0.22	0.41	0.44
Num. *Hábitat* Polygons	20	20	20	20	20	20	20
Observations	2067	2067	2067	2067	2067	2067	2067

Notes: This table reports estimations via OLS. The unit of analysis is the street. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses, wild-cluster bootstrap p-values are reported in brackets, and randomization inference p-values are reported in curly brackets. See Section 5 for additional discussion and Panel A of Table 1 for a version of these results with covariates.

Table E.4: Complier Average Causal Effect of *Hábitat* on Infrastructure

Panel A: Lasso-Selected Covariates	Infrastructure Index	Piped Water	Sewerage	Electrification	Streetlights	Medians	Sidewalks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hábitat Implementation	0.43	0.23	0.15	0.041	0.076	0.16	0.083
	(0.22)	(0.11)	(0.084)	(0.015)	(0.100)	(0.18)	(0.17)
	[0.042]	[0.031]	[0.073]	[0.075]	[0.55]	[0.51]	[0.78]
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-*Hábitat* Outcome as Covariate	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of DV	0	0.76	0.80	0.95	0.75	0.45	0.48
Control SD of DV	1	0.43	0.40	0.22	0.44	0.50	0.50
Wald F Statistic of Excluded Instrument	10.6	11.2	11.1	8.24	11.1	12.0	11.9
Anderson-Rubin 95% Confidence Interval	[0.04,...]	[0.04,...]	[-0.02,...]	[..., 0.08]	[-0.21,...]	[-0.17,...]	[-0.24,...]
Num. *Hábitat* Polygons	20	20	20	20	20	20	20
Observations	2067	2067	2067	2067	2067	2067	2067

Panel B: No Covariates	Infrastructure Index	Piped Water	Sewerage	Electrification	Streetlights	Medians	Sidewalks
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hábitat Implementation	0.41	0.22	0.16	0.053	0.053	0.16	0.066
	(0.21)	(0.099)	(0.086)	(0.013)	(0.10)	(0.18)	(0.17)
	[0.051]	[0.033]	[0.068]	[0.0070]	[0.67]	[0.47]	[0.82]
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-*Hábitat* Outcome as Covariate	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of DV	0	0.76	0.80	0.95	0.75	0.45	0.48
Control SD of DV	1	0.43	0.40	0.22	0.44	0.50	0.50
Wald F Statistic of Excluded Instrument	10.5	11.1	11.0	10.9	11.2	11.8	11.8
Anderson-Rubin 95% Confidence Interval	[0.01,...]	[0.04,...]	[-0.00,...]		[-0.29,...]	[-0.20,...]	[-0.27,...]
Num. *Hábitat* Polygons	20	20	20	20	20	20	20
Observations	2067	2067	2067	2067	2067	2067	2067

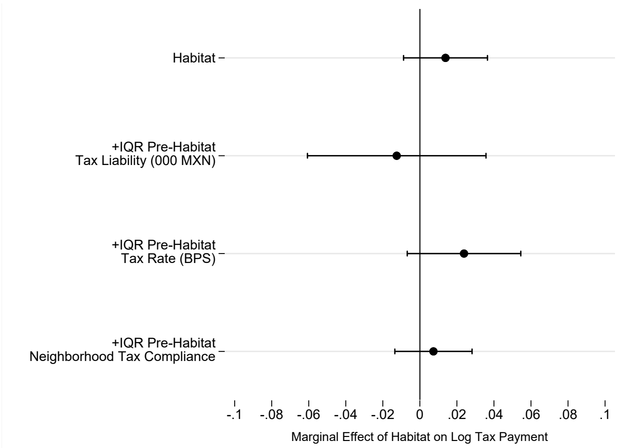
Notes: This table reports estimations via 2SLS, where *Hábitat* implementation is instrumented with randomized assignment. The unit of analysis is the street. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses and wild-cluster bootstrap p-values are reported in brackets. See Section 3 for additional discussion, Panel B of Table 1 for the intent-to-treat version of these results with covariates, and Table E.3 for a version without covariates.

Table E.5: Complier Average Causal Effect of *Hábitat* on Tax Compliance

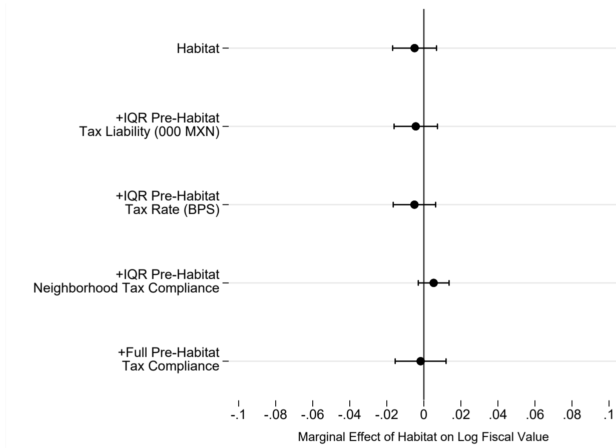
	Any Property Tax Compliance		Share of Property Tax Compliance		Log Tax Payment ('000 MXN) (Full Compliers)	Log Fiscal Value ('000 MXN)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hábitat Implementation	-0.024	-0.028	-0.016	-0.014	0.023	-0.0093	-0.0095
	(0.050)	(0.038)	(0.039)	(0.026)	(0.020)	(0.0089)	(0.011)
	[0.69]	[0.58]	[0.73]	[0.65]	[0.43]	[0.37]	[0.44]
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-*Hábitat* Outcome as Covariate	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	No	Yes
Control Mean of DV	0.45	0.45	0.39	0.39	0.29	5.87	5.87
Control SD of DV	0.50	0.50	0.45	0.45	0.28	0.72	0.72
Wald F Statistic of Excluded Instrument	16.2	15.8	16.1	15.8	11.7	17.2	11.9
Anderson-Rubin 95% Confidence Interval	[...,.090129]	[-0.18, 0.10]	[...,.078572]	[-0.10,...]	[-0.02,...]	[..., 0.01]	[...,.017986]
Num. *Hábitat* Polygons	20	20	20	20	19	20	20
Observations	31788	31740	31788	31740	6864	31788	31740

Notes: This table reports estimations via 2SLS, where *Hábitat* implementation is instrumented with randomized assignment. The unit of analysis is the property-year. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses and wild-cluster bootstrap p-values are reported in brackets. Covariates are selected using a Lasso regression. See Section 3 for additional discussion and Panel B of Table 1 for the intent-to-treat version of these results.

Figure E.1: Heterogeneous Effects of *Hábitat*: Log Payment & Log Fiscal Value



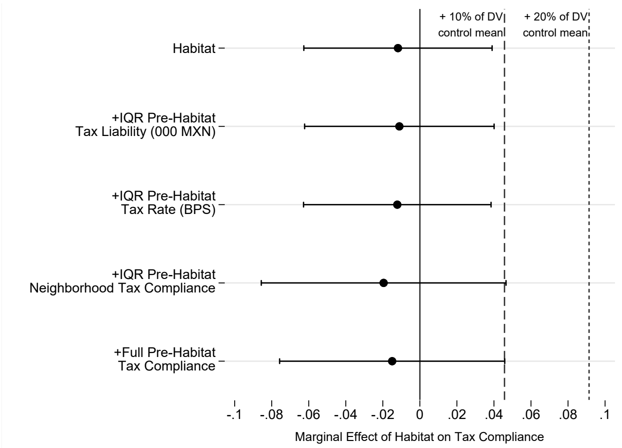
(a) Log Tax Payment



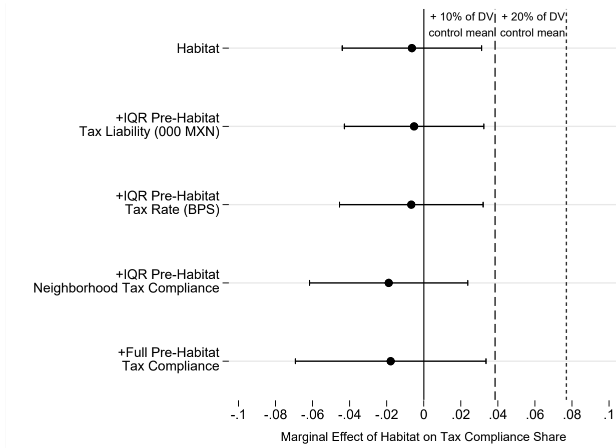
(b) Log Fiscal Value

Notes: The figures plots the effect of *Hábitat* from OLS estimations when each moderator changes by its interquartile range. The average effect of *Hábitat* is included at the top of each panel. The unit of analysis is the property-year. 95% confidence intervals are clustered at the *Hábitat* polygon level and each regression includes covariates selected using a Lasso regression. See Section 5.1 for additional discussion and Panels A and B of Figure 3 for the other two outcome variables.

Figure E.2: Heterogeneous Effects of *Hábitat* (Reallocating Arrears Payments)



(a) Tax Compliance



(b) Tax Compliance Share

Notes: This figure is identical to Figure 3 Panels A and B, except that tax payments and associated compliance outcomes are constructed differently. In our main analysis, arrears payments are allocated to the year in which the liability arose. For this figure, we allocate arrears payments to the year in which the payment was made.

Table E.6: Intent-to-Treat Effect of *Hábitat* on Tax Compliance (Reallocating Arrears Payments)

	Any Property Tax Compliance		Share of Property Tax Compliance	
	(1)	(2)	(3)	(4)
Habitat	-0.016	-0.012	-0.012	-0.0064
	(0.035)	(0.024)	(0.028)	(0.018)
	[0.70]	[0.71]	[0.72]	[0.77]
	{0.71}	{0.69}	{0.74}	{0.76}
Municipio Fixed Effects	Yes	Yes	Yes	Yes
Pre-Habitat Outcome as Covariate	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes
Control Mean of DV	0.45	0.45	0.38	0.38
Control SD of DV	0.50	0.50	0.45	0.45
R sq.	0.32	0.32	0.28	0.29
Number of Habitat Polygons	20	20	20	20
Observations	31788	31740	31788	31740

Notes: This table is identical to Table 1, Panel B, columns 1-4, except that tax payments and associated compliance outcomes are constructed differently. In our main analysis, arrears payments are allocated to the year in which the liability arose. Here, we allocate arrears payments to the year in which the payment was made. See Section 5.1 for additional discussion.

Table E.7: Intent to Treat: Heterogeneous Effects of *Hábitat* on Tax Compliance and Tax Compliance Share

	Any Property Tax Compliance				Share of Property Tax Compliance			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hábitat	-0.015 (0.021) [0.57]	-0.013 (0.021) [0.66]	-0.015 (0.020) [0.54]	-0.015 (0.021) [0.55]	-0.0070 (0.015) [0.71]	-0.0063 (0.016) [0.76]	-0.0056 (0.014) [0.75]	-0.0074 (0.014) [0.67]
Hábitat × Pre- Tax Liability ('000 MXN)	0.0013 (0.00083) [0.13] {0.25}				0.0016 (0.0010) [0.11] {0.26}			
Hábitat × Pre- Tax Rate (BPS)		-0.00052 (0.00026) [0.57] {0.61}				-0.00031 (0.00027) [0.74] {0.72}		
Hábitat × Pre-Neighborhood Tax Compliance			0.0032 (0.10) [0.99] {0.97}				-0.047 (0.066) [0.60] {0.15}	
Hábitat × Pre- Tax Compliance				0.0063 (0.022) [0.80] {0.86}				
Hábitat × Pre- Tax Compliance Share								-0.0095 (0.023) [0.71] {0.76}
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre- Outcome as Covariate	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of DV	0.45	0.45	0.45	0.45	0.39	0.39	0.39	0.39
Control SD of DV	0.50	0.50	0.50	0.50	0.45	0.45	0.45	0.45
Control Mean of Moderator	0.79	15.9	0.56	0.56	0.79	15.9	0.56	0.48
Control SD of Moderator	4.87	34.5	0.17	0.50	4.87	34.5	0.17	0.45
Control Interquantile Range of Moderator	0.042	5.73	0.36	1	0.042	5.73	0.36	0.93
R sq.	0.34	0.34	0.34	0.34	0.31	0.31	0.31	0.31
Num. *Hábitat* Polygons	20	20	20	20	20	20	20	20
Observations	31740	31740	31740	31740	31740	31740	31740	31740

Notes: This table reports estimations via OLS. The unit of analysis is the property-year. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses, wild-cluster bootstrap p-values are reported in brackets, and randomization inference p-values are reported in curly brackets. Covariates are selected using a Lasso regression. See Section 5.1 for additional discussion and Panels A and B of Figure 3 for a visualization of these results.

Table E.8: Intent to Treat: Heterogeneous Effects of *Hábitat* on Payment and Fiscal Value

	Log Tax Payment ('000 MXN) (Full Compliers)			Log Fiscal Value ('000 MXN)			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Hábitat	-0.011 (0.022) [0.69]	0.021 (0.013) [0.25]	0.026 (0.011) [0.10]	-0.0044 (0.0056) [0.49]	-0.0044 (0.0054) [0.47]	-0.011 (0.0040) [0.10]	-0.0058 (0.0051) [0.33]
Hábitat × Pre-*Hábitat* Tax Liability ('000 MXN)	-0.029 (0.019) [0.26] {0.16}			0.00020 (0.00031) [0.73] {0.90}			
Hábitat × Pre-*Hábitat* Tax Rate (BPS)		0.00079 (0.00060) [0.14] {0.63}			-0.00011 (0.00017) [0.47] {0.68}		
Hábitat × Pre-*Hábitat* Neighborhood Tax Compliance			-0.15 (0.067) [0.072] {0.0015}			0.14 (0.024) [0.0050] {0.00050}	
Hábitat × Pre-*Hábitat* Tax Compliance							0.0085 (0.0052) [0.16] {0.31}
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-*Hábitat* Outcome as Covariate	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of DV	0.29	0.29	0.29	5.87	5.87	5.87	5.87
Control SD of DV	0.28	0.28	0.28	0.72	0.72	0.72	0.72
Control Mean of Moderator	0.45	16.2	0.65	0.79	15.9	0.56	0.56
Control SD of Moderator	0.97	42.2	0.12	4.87	34.5	0.17	0.50
Control Interquantile Range of Moderator	0.042	3.41	0.11	0.042	5.73	0.36	1
R sq.	0.85	0.85	0.85	0.94	0.94	0.94	0.94
Num. *Hábitat* Polygons	19	19	19	20	20	20	20
Observations	6864	6864	6864	31740	31740	31740	31788

Notes: This table reports estimations via OLS. The unit of analysis is the property-year. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses, wild-cluster bootstrap p-values are reported in brackets, and randomization inference p-values are reported in curly brackets. Covariates are selected using a Lasso regression. See Section 5.1 for additional discussion, Table E.7 for a version of these results for the outcomes tax compliance and tax compliance share, and Figure 3 for a visualization of the heterogeneity results for tax compliance and tax compliance share.

Table E.9: Effects of *Hábitat* on Beneficiaries' Perceptions (ANCOVA)

	Knowledge of *Hábitat*		Trust in Public Officials		Trust in Neighborhood Leaders	
	(1)	(2)	(3)	(4)	(5)	(6)
Hábitat	0.038	0.042	0.018	-0.014	0.024	0.036
	(0.015)	(0.063)	(0.013)	(0.026)	(0.0094)	(0.018)
	[0.016]	[0.56]	[0.21]	[0.64]	[0.0090]	[0.039]
	{0.021}	{0.61}	{0.18}	{0.50}	{0.012}	{0.13}
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of Outcome	0.13	0.12	0.45	0.38	0.46	0.40
Control SD of Outcome	0.14	0.14	0.081	0.050	0.089	0.050
R sq.	0.40	0.38	0.45	0.31	0.48	0.54
Sample	National	CDMX	National	CDMX	National	CDMX
Num. *Hábitat* Polygons	342	20	342	20	342	20
Observations	342	20	342	20	342	20

Notes: This table reports estimations via OLS. The unit of analysis is the polygon, aggregated from household responses from the pre- and post-intervention *Hábitat* evaluation survey. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses, wild-cluster bootstrap p-values are reported in brackets, and randomization inference p-values are reported in curly brackets. Knowledge of *Hábitat* is a dichotomous variable; trust in public officials and neighborhood leaders is measured from three ordinal responses (no trust, some trust, a lot of trust), which we transform into three values: $\{0, 0.5, 1\}$. The analysis is weighted by a population weight to be representative of all residents in the study neighborhoods. This ANCOVA estimation is similar to Equation 1. See Panel A of Table 2 for a version of these results using OLS.

Table E.10: Heterogeneous Effects of *Hábitat* on Tax Compliance by Mail Intervention (CACE)

	Cumulative Compliance Indicator	Compliance Share
	(1)	(2)
Tax-Benefit Treatment	6.35	6.84
	(2.95)	(2.87)
	[0.012]	[0.011]
Pure Sanctions Treatment	9.63	9.78
	(2.80)	(2.99)
	[0.048]	[0.037]
Hábitat Implementation	3.49	4.27
	(6.92)	(6.86)
	[0.83]	[0.75]
Tax-Benefit Treatment × *Hábitat*	-7.01	-7.40
	(7.40)	(7.32)
	[0.39]	[0.35]
Pure Sanctions Treatment × *Hábitat*	-14.3	-14.0
	(9.15)	(9.33)
	[0.14]	[0.16]
Municipio Fixed Effects	Yes	Yes
Covariates	No	No
Control Mean of DV	11.7	11.7
Control SD of DV	32.1	32.1
Wald F Statistic of Excluded Instrument	3.61	3.61
R sq.	0.017	0.019
Num. *Hábitat* Polygons	19	19
Observations	951	951

Notes: This table reports estimations via 2SLS, where *Hábitat* implementation is instrumented with randomized assignment. The unit of analysis is the property. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses and wild-cluster bootstrap p-values are reported in brackets. See Section 5.1 for additional discussion and Panel B of Table 2 for an intent-to-treat version of these results.

Table E.11: Intent-to-Treat Effects of *Hábitat* on Tax Payment

	Tax Payment ('000 MXN) (Full Sample)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Hábitat	0.27	0.33	0.17	0.32	0.34	0.34		
	(0.19)	(0.23)	(0.061)	(0.22)	(0.24)	(0.24)		
	[0.25]	[0.21]	[0.0010]	[0.27]	[0.15]	[0.22]		
	{0.17}	{0.063}						
Hábitat × Pre-*Hábitat* Tax Liability ('000 MXN)			0.093					
			(0.046)					
			[0.34]					
			{0.85}					
Hábitat × Pre-*Hábitat* Tax Rate (BPS)				0.0026				
				(0.0038)				
				[0.53]				
				{0.76}				
Hábitat × Pre-*Hábitat* Neighborhood Tax Compliance					-0.22			
					(0.47)			
					[0.66]			
					{0.55}			
Hábitat × Pre-*Hábitat* Tax Compliance						-0.27		
						(0.22)		
						[0.24]		
						{0.0060}		
Hábitat Implementation							0.44	0.63
							(0.28)	(0.39)
							[0.12]	[0.084]
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Pre-*Hábitat* Outcome as Covariate	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	Yes	Yes	Yes	Yes	No	Yes
Control Mean of DV	0.19	0.19	0.19	0.19	0.19	0.19	0.19	0.19
Control SD of DV	0.74	0.74	0.74	0.74	0.74	0.74	0.74	0.74
Control Mean of Moderator			0.79	15.9	0.56	0.56		
Control SD of Moderator			4.87	34.5	0.17	0.50		
Control Interquantile Range of Moderator			0.042	5.73	0.36	1		
Wald F Statistic of Excluded Instrument							15.8	10.9
Anderson-Rubin 95% Confidence Interval							[-0.59, 1.20]	
Num. *Hábitat* Polygons	20	20	20	20	20	20	20	20
Observations	31788	31740	31740	31740	31740	31740	31788	31740

Notes: This table reports estimations via OLS (columns 1–6) and 2SLS (columns 7 and 8), where *Hábitat* implementation is instrumented with randomized assignment. The unit of analysis is the property-year. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses, wild-cluster bootstrap p-values are reported in brackets, and randomization inference p-values are reported in curly brackets. Covariates are selected using a Lasso regression. See Table E.7 for a version of these results for the tax compliance and tax compliance share and Table E.8 for log tax payment and log fiscal value.

E.2 Alternative Sample: *Hábitat* 2014 as Extra Control Units

Table E.12: Baseline Differences Between Treated and Control Properties (*Hábitat* 2014 as Extra Control Units)

	Property Features									
	Pre-*Hábitat* Tax Compliance	Pre-*Hábitat* Tax Compliance Share	Pre-*Hábitat* Neighborhood Tax Compliance	Pre-*Hábitat* Fiscal Value of Property ('000 MXN)	Pre-*Hábitat* Tax Payment ('000 MXN)	Pre-*Hábitat* Tax Liability ('000 MXN)	Pre-*Hábitat* Tax Rate (BPS)	Age of Property	Surface Area of Property	Constructed Area of Property
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Hábitat 2014 as Extra Control Units	0.040	0.025	0.23	0.11	0.037	0.090	0.037	-0.014	0.068	0.19
	(0.15)	(0.14)	(0.85)	(0.089)	(0.028)	(0.072)	(0.26)	(0.082)	(0.089)	(0.12)
	[0.84]	[0.89]	[0.83]	[0.33]	[0.33]	[0.31]	[0.98]	[0.82]	[0.63]	[0.19]
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of DV	0.63	0.52	0.63	558.5	0.65	2.17	23.7	27.8	175.6	125.4
Control SD of DV	0.48	0.45	0.084	1903.7	5.22	18.2	55.3	21.1	1112.4	371.8
R sq.	0.022	0.020	0.63	0.016	0.013	0.014	0.051	0.076	0.012	0.0036
Num. *Hábitat* Polygons	38	38	38	38	38	38	38	38	38	38
Observations	57214	57214	57214	57214	57214	57214	57214	57214	57214	57214

	Local 2009 Elections (Precinct-Level)			
	% CDMX Incumbent	% Turnout	% Annulled Votes	% Delegación Incumbent
	(1)	(2)	(3)	(4)
Hábitat 2014 as Extra Control Units	-0.0076	-0.055	-0.17	-0.0076
	(0.16)	(0.29)	(0.17)	(0.16)
	[0.95]	[0.83]	[0.40]	[0.95]
Municipio Fixed Effects	Yes	Yes	Yes	Yes
Control Mean of DV	0.36	0.35	0.086	0.36
Control SD of DV	0.11	0.069	0.021	0.11
R sq.	0.71	0.50	0.18	0.71
Num. *Hábitat* Polygons	38	38	38	38
Observations	57214	57214	57214	57214

Notes: This table reports estimations via OLS following the same specifications as Table E.1, but using an alternative sample. The unit of analysis is the property. Pre-treatment variables are standardized in the estimation, but the untransformed mean and standard deviation are reported. The sample includes additional control polygons that received *Hábitat* in 2014, three years after the end of the experimental evaluation. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses and wild-cluster bootstrap p-values are reported in brackets. See Panel B of Figure 1 for a version of these results with the standard sample with covariates and Table E.1 for a version of these results for the standard sample without them.

Table E.13: Baseline Differences Between Treated and Control Properties (*Hábitat* 2014 as Extra Control Units)

	Assignment to *Hábitat* (1)
Pre-*Hábitat* Tax Compliance	-0.00023 (0.0021) [0.87]
Pre-*Hábitat* Tax Compliance	0.00040 (0.0020) [0.88]
Pre-*Hábitat* Neighborhood Tax Compliance	0.018 (0.071) [0.84]
Pre-*Hábitat* Fiscal Value ('000 MXN)	0.036 (0.051) [0.53]
Pre-*Hábitat* Tax Payment ('000 MXN)	-0.0014 (0.00097) [0.17]
Pre-*Hábitat* Property Tax Liability ('000 MXN)	-0.040 (0.051) [0.49]
Pre-*Hábitat* Tax Rate (BPS)	0.000045 (0.00013) [0.80]
Age of Property	-0.00075 (0.0020) [0.70]
Surface Area of Property	0.0024 (0.0035) [0.52]
Constructed Area of Property	0.0072 (0.0025) [0.034]
% Vote for CDMX Incumbent Party	0.00056 (0.011) [0.96]
% Turnout	-0.0028 (0.020) [0.90]
% Annulled Vote	-0.0057 (0.0087) [0.56]
% Vote for Municipal Incumbent Party	-0.0022 (0.0088) [0.85]
F-statistic	1.01
F-test P-value	0.47
Wild Cluster Bootstrap P-value	0.32
Municipio Fixed Effects	Yes
Share of Treated Properties	0.056
R sq.	0.38
Num. *Hábitat* Polygons	38
Observations	57214

Notes: This table reports estimations via OLS following the same specifications as Table E.2, but using an alternative sample. The unit of analysis is the property; pre-treatment variables are standardized. The sample includes additional control polygons that received *Hábitat* in 2014, three years after the end of the experimental evaluation. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses and wild-cluster bootstrap p-values are reported in brackets.

Table E.14: Intent-to-Treat Effects of *Hábitat* on Tax Compliance (*Hábitat* 2014 as Extra Control Units)

	Any Property Tax Compliance		Share of Property Tax Compliance		Log Tax Payment ('000 MXN) (Full Compliers)	Log Fiscal Value ('000 MXN)
	(1)	(2)	(3)	(4)	(5)	(6)
Hábitat 2014 as Extra Control Units	0.0089	0.0035	0.0053	-0.00057	0.011	0.00021
	(0.026)	(0.019)	(0.018)	(0.012)	(0.010)	(0.0067)
	[0.78]	[0.87]	[0.81]	[0.96]	[0.41]	[0.97]
	{0.78}	{0.86}	{0.81}	{0.96}	{0.35}	{0.98}
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Pre-*Hábitat* Outcome as Covariate	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	No
Control Mean of DV	0.53	0.53	0.44	0.44	0.41	5.83
Control SD of DV	0.50	0.50	0.45	0.45	0.60	0.91
R sq.	0.32	0.32	0.28	0.28	0.87	0.94
Num. *Hábitat* Polygons	38	38	38	38	36	38
Observations	228856	228856	228856	228856	64624	228856

Notes: This table reports estimations via OLS following the same specifications as Table 1, but using an alternative sample. The unit of analysis is the property-year. The sample includes additional control polygons that received *Hábitat* in 2014, three years after the end of the experimental evaluation. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses, wild-cluster bootstrap p-values are reported in brackets, and randomization inference p-values are reported in curly brackets. Covariates are selected using a Lasso regression.

E.3 Alternative Sample: All Neighborhoods

Table E.15: Baseline Differences Between Treated and Control Properties (All Neighborhoods)

	Property Features									
	Pre-*Hábitat* Tax Compliance	Pre-*Hábitat* Tax Compliance Share	Pre-*Hábitat* Neighborhood Tax Compliance	Pre-*Hábitat* Fiscal Value of Property ('000 MXN)	Pre-*Hábitat* Tax Payment ('000 MXN)	Pre-*Hábitat* Tax Liability ('000 MXN)	Pre-*Hábitat* Tax Rate (BPS)	Age of Property	Surface Area of Property	Constructed Area of Property
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Hábitat (All Neighborhoods)	-0.092	-0.085	-0.40	0.056	0.0076	0.052	0.055	-0.014	0.075	0.0020
	(0.11)	(0.095)	(0.46)	(0.053)	(0.018)	(0.043)	(0.11)	(0.061)	(0.043)	(0.0016)
	[0.50]	[0.48]	[0.49]	[0.51]	[0.71]	[0.49]	[0.79]	[0.83]	[0.18]	[0.41]
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Control Mean of DV	0.73	0.59	0.73	871.4	1.41	4.01	25.8	27.8	188.1	195.8
Control SD of DV	0.44	0.43	0.10	3137.1	8.91	31.5	65.1	22.3	1896.1	29020.1
R sq.	0.018	0.012	0.23	0.011	0.012	0.011	0.068	0.024	0.0083	0.000052
Num. *Hábitat* Polygons	1380	1380	1380	1380	1380	1380	1380	1380	1380	1380
Observations	1858610	1858610	1858610	1858610	1858610	1858610	1858498	1858610	1858610	1858610

	Local 2009 Elections (Precinct-Level)			
	% CDMX Incumbent	% Turnout	% Annulled Votes	% Delegación Incumbent
	(1)	(2)	(3)	(4)
Hábitat (All Neighborhoods)	0.44	-0.23	-0.35	0.39
	(0.12)	(0.20)	(0.11)	(0.11)
	[0.011]	[0.40]	[0.022]	[0.011]
Municipio Fixed Effects	Yes	Yes	Yes	Yes
Control Mean of DV	0.30	0.42	0.098	0.33
Control SD of DV	0.11	0.063	0.026	0.12
R sq.	0.53	0.21	0.17	0.53
Num. *Hábitat* Polygons	1380	1380	1380	1380
Observations	1858207	1858207	1858207	1858207

Notes: This table reports estimations via OLS following the same specifications as Table E.1, but using an alternative sample of all neighborhoods in Mexico City. Control neighborhoods are weighted using entropy weights (Hainmueller 2012) so that they match the means of pre-*Hábitat* outcomes and covariates. The unit of analysis is the property. Pre-treatment variables are standardized in the estimation, but the untransformed mean and standard deviation are reported. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses and wild-cluster bootstrap p-values are reported in brackets.

Table E.16: Baseline Differences Between Treated and Control Properties (All Neighborhoods)

	Assignment to *Hábitat* (1)
Pre-*Hábitat* Tax Compliance	0.000090 (0.0039) [0.99]
Pre-*Hábitat* Tax Compliance	0.00090 (0.0039) [0.87]
Pre-*Hábitat* Neighborhood Tax Compliance	-0.018 (0.041) [0.71]
Pre-*Hábitat* Fiscal Value ('000 MXN)	0.21 (0.12) [0.13]
Pre-*Hábitat* Tax Payment ('000 MXN)	-0.0045 (0.0026) [0.15]
Pre-*Hábitat* Property Tax Liability ('000 MXN)	-0.21 (0.12) [0.14]
Pre-*Hábitat* Tax Rate (BPS)	0.00012 (0.00011) [0.39]
Age of Property	0.00097 (0.0069) [0.93]
Surface Area of Property	0.048 (0.028) [0.24]
Constructed Area of Property	0.000042 (0.000059) [0.40]
% Vote for CDMX Incumbent Party	0.038 (0.018) [0.057]
% Turnout	-0.0092 (0.020) [0.72]
% Annulled Vote	-0.031 (0.026) [0.37]
% Vote for Municipal Incumbent Party	0.051 (0.016) [0.0080]
F-statistic	2.26
F-test P-value	0.00
Wild Cluster Bootstrap P-value	0.04
Municipio Fixed Effects	Yes
Share of Treated Properties	0.0017
R sq.	0.37
Num. *Hábitat* Polygons	1380
Observations	1858095

Notes: This table reports estimations via OLS following the same specifications as Table E.2, but using an alternative sample of all neighborhoods in Mexico City. Control neighborhoods are weighted using entropy weights (Hainmueller 2012) so that they match the means of pre-*Hábitat* outcomes and covariates. The unit of analysis is the property; pre-treatment variables are standardized. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses and wild-cluster bootstrap p-values are reported in brackets.

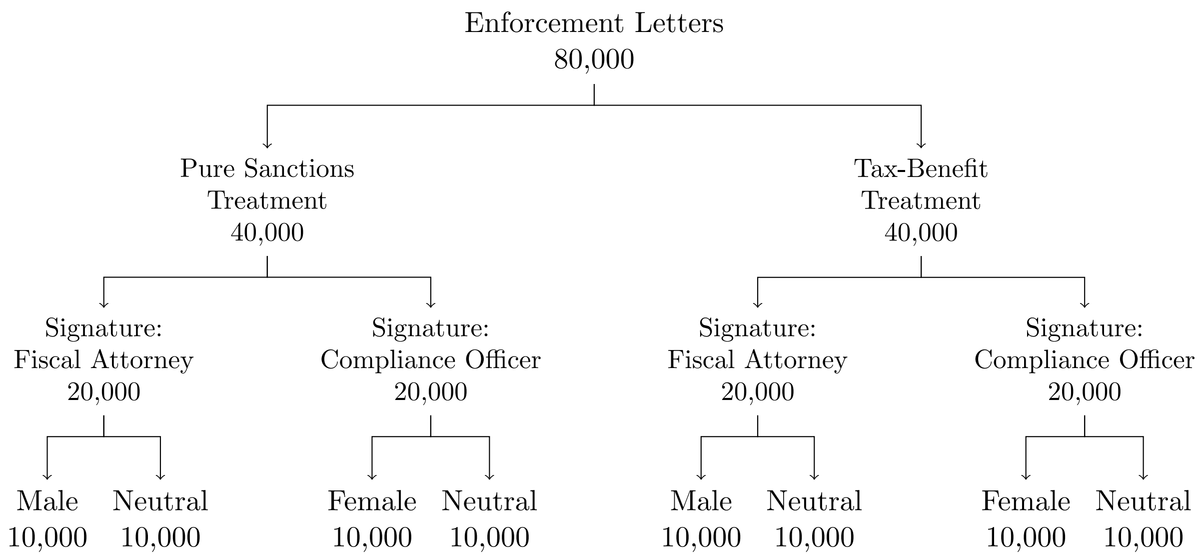
Table E.17: Intent-to-Treat Effect of *Hábitat* on Tax Compliance (All Neighborhoods)

	Any Property Tax Compliance		Share of Property Tax Compliance		Log Tax Payment ('000 MXN) (Full Compliers)	Log Fiscal Value ('000 MXN)
	(1)	(2)	(3)	(4)	(5)	(6)
Hábitat (All Neighborhoods)	-0.040	-0.020	-0.033	-0.017	0.0086	0.0053
	(0.020)	(0.011)	(0.016)	(0.0088)	(0.0064)	(0.0038)
	[0.089]	[0.14]	[0.081]	[0.11]	[0.32]	[0.24]
Municipio Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Pre-*Hábitat* Outcome as Covariate	Yes	Yes	Yes	Yes	Yes	Yes
Covariates	No	Yes	No	Yes	No	No
Control Mean of DV	0.66	0.67	0.53	0.53	0.55	6.14
Control SD of DV	0.47	0.47	0.44	0.44	0.75	1.04
R sq.	0.31	0.32	0.27	0.28	0.88	0.97
Num. Neighborhoods	1380	1380	1380	1380	1342	1380
Observations	7434440	7432828	7434440	7432828	3137008	7434440

Notes: This table reports estimations via OLS following the same specifications as Table 1, but using an alternative sample of all neighborhoods in Mexico City. Control neighborhoods are weighted using entropy weights (Hainmueller 2012) so that they match the means of pre-*Hábitat* outcomes and covariates. The unit of analysis is the property-year. The sample includes all neighborhoods in Mexico City. Standard errors clustered at the *Hábitat* polygon level are reported in parentheses, wild-cluster bootstrap p-values are reported in brackets, and randomization inference p-values are reported in curly brackets. Covariates are selected using a Lasso regression.

F. Compliance Experiment Appendix

Figure F.1: Experiment Design



Notes: This diagram represents the different treatment arms of the communication intervention discussed in Section 6.2, in which the Ministry of Finance sent letters to encourage payment of outstanding property tax debt. Letter recipients were selected from a pool of taxpayers who had become delinquent between bimester 4 of 2009 and bimester 3 of 2014. The letters were sent between July 28 and August 11, 2014. A control group of 10,000 delinquent taxpayers received no letters.

Figure F.2: Letters to Tax Non-Compliers

[Taxpayer name]
[Taxpayer address]

Tax-Benefit Treatment

With our tax payment, we all contribute to improving our city
Invitation Letter for Payment of the Property Tax

As you know, a large part of the social programs and investments in infrastructure and security that the Government of Mexico City implements are financed by property tax revenues. Your contribution is therefore very important, and we would be pleased if you could update your property tax account as soon as possible and cover the outstanding tax debt for the above mentioned building for the tax period(s) ***** within 15 working days upon receipt of this letter. We ask you to update your account to avoid incurring surcharges.

With the revenues obtained from property taxes in your city, we finance the following public goods, among others:

- Food pensions for the elderly;
- School uniforms and school supplies for children;
- The operation of health centers and hospitals of the Government of Mexico City;
- Street lights and sidewalks in your neighborhood.

Pure Sanctions Treatment

Avoid major inconvenience and regularize your tax status
Invitation Letter for Payment of Property Tax

According to the registers of the Federal District's Treasury, you have outstanding **property tax** debt for the tax period(s) *****. We would therefore be grateful if you could update your tax status within 15 working days of receipt of this letter.

Delay in property tax payment can be sanctioned with fines and interest costs, and with interventions that the fiscal authority conducts to ensure effective tax collection as per the Tax Code, which can lead to the seizure of property.

Avoid major inconvenience and regularize your tax status.

[Boxed: Information about payment and further details on the back]

[Signature]: [Name, Title]
(For gender-neutral signatures, only the initials of the first name are provided.)

Notes: This figure shows the enforcement letters sent to taxpayers between July 28 and August 11, 2014, which are discussed in Section 6.2.

Table F.1: Cadastral Value Distribution of Experiment Sample

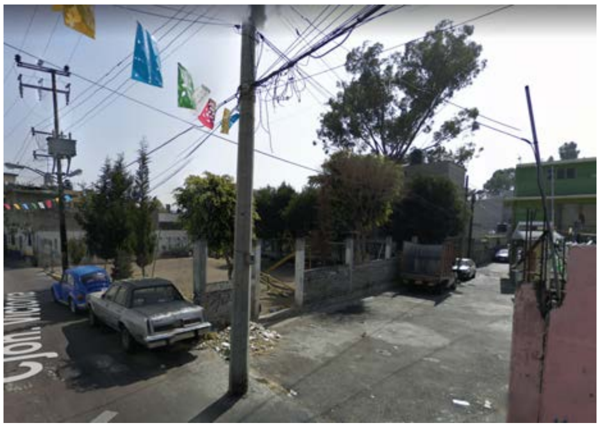
	All Taxpayers	Experiment Sample
Mean	589,530.8	533,087.3
SE	(636.2)	(2310.3)
Min	993.7	17,178.4
Max	11,711,063.3	11,670,532.6
20th Percentile	229,784.6	256,034.0
50th Percentile	391,487.2	419,170.5
80th Percentile	730,281.1	678,949.6

Notes: This table compares the cadastral value between the delinquent taxpayers targeted in the tax compliance intervention and the full population of taxpayers, showing very similar distributions.

Figure F.3: Illustrative Examples of *Hábitat* Investments

(a) Pre-*Hábitat*, 2008 (b) Post-*Hábitat*, 2014

1. Playground in San Lucas, Iztapalapa



2. Street Lights and Upgraded Electricity Infrastructure in San Lucas, Iztapalapa

3. Sidewalk & Bus Stop in Solidaridad, Tlalpan

Notes: This figure shows photos before and after the implementation of *Hábitat* projects, as discussed in Section 3.