Local Infrastructure and the Development of the Private Sector: Evidence from a Randomized Trial*

Leonardo Iacovone (World Bank)
Craig McIntosh (UCSD)
Daniel Rogger (World Bank)
Luis F. Sánchez-Bayardo (World Bank)

March 7, 2023

Abstract

We study how local public infrastructure investment effects neighborhood economies. By tracking the impacts of US$68million of randomized investments in Mexican municipalities, we document how government investment leads to sustained increases in the size, employment, and profitability of treated private-sector companies. Within the first few years of investment, wages rise to compensate for higher costs of living, inefficient firms die, and more efficient firms grow faster. Over the subsequent decade firms continue to grow at an increased rate in terms of their capital stock, employment, and profitability, suggesting durable improvements in local demand and the structure of the private sector. Our results provide novel evidence of the linkages between government investment, small business growth, and the dynamics of local economies.

Keywords: Infrastructure, Firm Productivity, Randomized Trials
JEL Codes: R11, O18, D22, C93

*We gratefully acknowledge financial support from the World Bank's Development Impact Evaluation i2i and Knowledge for Change Program trust funds, Bureaucracy Lab and Governance Global Practice. We are grateful to Niclas Moneke and Marta Santamaria and seminar participants at the 11th European Meeting of the Urban Economics Association at the London School of Economics and Political Science, the University of Oxford, and the World Bank for helpful comments. The findings, interpretations, and conclusions expressed in this paper are entirely those of the authors. They do not necessarily represent the views of the World Bank and its affiliated organizations, or those of the Executive Directors of the World Bank or the governments they represent.

1 Introduction

Urban infrastructure investment has been argued to be a key driver of the development of the modern state [Lucas, 1988; Jones and Romer, 2010]. Public infrastructure investments are a key component of this spending, amounting to $2.5 trillion per year globally [Woetzel et al., 2017]. By shaping the urban environment, government policy may directly influence the nature of private sector development. This paper investigates this possibility, tracking the response of tens of thousands of firms across Mexico to a nationwide randomized control trial. The results indicate that government can indeed shift the nature of the economy through changes to the urban environment in which firms operate. Firm growth in cities is a function of the nature of urbanization because features of the urban environment change incentives for entry, exit, and investment [Glaeser, Luca and Moszkowski, 2020]. The physical environment in which metropolitan firms are located influences their productivity through its impacts on transport costs, input access, worker matching, and property prices [Glaeser and Gottlieb, 2009]. The right investments by the state can strengthen the incentives for firms to form, invest, and hire, thereby generating self-sustaining economic growth as well as providing a source of tax revenues to recoup the cost of the investments. In this sense, public infrastructure is potentially a key policy lever to influence the dynamics of firm productivity [Reinikka and Svensson, 1999; Bryan, Glaeser and Tsivanidis, 2020].

Traditionally, the value of local infrastructure investments have been assessed through their impacts on the amenity value of urban locations, typically measured through property prices [Roback, 1982]. Such an approach implies no productive effect of these investments, and thus zero-sum changes to property values across localities (as in Almagro and Domınguez-Iino [2019]. A more recent literature has focused on understanding the productive effects of infrastructure investment, whether through shifts in the production function [Haughwout, 2002] or through agglomeration effects [Eberts and McMillen, 1999]. Agglomeration is key because it introduces the possibility that infrastructure investment generates self-reinforcing improvements in productivity that would not be offset elsewhere in the economy. Whether the empirical study of agglomeration takes a reduced form [Greenstone, Hornbeck and Moretti, 2010] or a structural approach [Tsivanidis, 2019], identifying the causal impact of infrastructure is challenging. The few experimental studies on the impact of infrastructure that exist tend to be at too small a scale to permit the consideration of effects on private sector agglomeration (examples include [Gonzalez-Navarro and Quintana-Domeque, 2016] who study block-level street paving within one Mexican city, or Galiani et al. [2017], Harari, Wong et al. [2018], and Cattaneo et al. [2009] who examine the randomized construction of individual homes within slums). The literature has lacked experimental evidence at sufficient scale to allow us to compare the 'amenities' and 'agglomeration' narratives of the impact of infrastructure investment on firm behavior.

We contribute to this literature with an analysis of a randomized experiment of a large-scale ($68 million), federally implemented program that makes a broad set of coordinated investments, substantially upgrading the residential amenities of marginalized neighborhood in Mexico's cities ("Programa Hábitat").[1] We combine detailed maps of the footprint of the Programa Hábitat spending between 2009 and 2011 with censuses of Mexican firms conducted in 2008, 2013, and 2018. This pairing of experimental spending variation with firm-level censuses allows us to speak with unusual clarity to the linkage between government infrastructure investment and the birth, death, and growth of firms in the private sector. Multiple post-treatment census waves allow us to document both short- and medium-run effects of the spending. The sharply defined borders of Hábitat catchment areas permit a straightforward analysis of geographic spillovers, and hence a test for agglomeration effects experienced by firms proximate to new infrastructure but not directly receiving benefits. With randomized variation in a set of changes that look like neighborhood gentrification, we can provide an unusually clear lens on the ways in which residential transformation of urban spaces alters the composition and growth of firms.

We find a clear shift in the structure of economic activity in treated areas. In the short-term, changes from the program are consistent with a wage shock coming from increased property values.[2] One year after the end of investment through the program wages have jumped by 20%, with impacts confined entirely to the services (nontradeable) sector, but in this sector we also see an increase in the total number of workers, and a modest increase in revenues and capital investment. Over the medium term, looking six years after the end of investment, wages remain elevated, the number of workers has converged to the control group average, but the increases in capital stocks and revenue have accelerated. By 2018 service sector firms in treatment neighborhoods have revenues 9% higher and capital stocks 17% higher than the control neighborhoods.

[1] The program invested in paving local streets, building sidewalks, connecting residences to power and sewerage, and improving community centers, but not improve transport infrastructure connecting intervention neighborhoods with the rest of the city.

[2] A precursor paper studying the residential impacts of the same experiment found that rents rose by 18% and overall property prices by 10%, along with a surge in private investment in homes and a dramatic drop in violent crime McIntosh et al. [2018].

Firms have re-optimized the capital-intensity of their production, reflecting a more 'mature' operating model.

On the extensive margin, the treatment leads to an immediate shake-out with roughly 2% of the stock of firms being pushed out by the intervention on top of the natural level of turnover, with these firms being replaced by newer, smaller, faster-growing firms. All of the impacts on firm birth and death occur in the short term, with the long-run impacts coming on the intensive margin. Firms with high initial value added per worker are less like to 'die' in treatment neighborhoods, and increase their capital investments the most under the program. Hence, despite the almost exclusively residential nature of the investments made through the program, substantial private-sector benefits resulted.

Digging into the mechanisms for this impact, we uncover several key factors. First, firms are more likely to formalize. Using the classification suggested by Busso, Fazio and Levy [2012], we find that while overall rates of formalization are very low, they rise significantly in treated neighborhoods (by a third, .029 to .036, using the 'weak' definition, and doubling, .003 to .006 using the 'strict' definition of formality). Financial services are deepened, with the use of credit and access to a bank account rising, and firm internet access improves as well. All of these effects are concentrated in the service sector where the overall profitability impacts are strongest. Increases in investment do not appear to be being driven by property prices via collateralization, in that they are similarly strong for firms that do and do not own the land on which they operate. This pattern of revenue growth, formalization, and a shift towards the service sector all appear to be consistent with the treatment having created meaningful structural change in the local customer base. The changes could best be described as 'gentrification'.[3]

Using the sharply defined geographical boundaries for where the intervention took place, we explore spatial spillovers by comparing buffer zones around treatment and control polygons. Despite having excellent statistical power for this analysis, we uncover no evidence of (positive or negative) spillovers even on businesses as close as 100 meters to the intervention polygons. This pattern suggests that this form of infrastructure investment generated economic changes that were highly localized, and did not create agglomeration impacts for neighboring firms who were only indirectly effected.

Similarly, we do not find evidence of heterogeneity in either the direct impacts or the spillover effects by local market access (distance weighted market size), suggesting that spatial trade patterns are not important mediators of direct or indirect effects in

[3] Using population census data from 2020 we find that there is limited change in neighborhood population size, structure or characteristics, providing further evidence that the changes we see are in fact a change in the structural parameters of neighborhood economies.

this context. The implication is that general equilibrium effects (positive or negative) are not an important part of the welfare story, and narrowly calculated benefits in study neighborhoods capture total benefits well.

Consequently, we conduct an accounting exercise using changes in value added taxes, social security contributions, and revenue taxes to calculate that the tax take via the private sector rises by almost $5 million per year in study neighborhoods, meaning that the program would pay for itself in 14 years simply through firm taxation. This suggests that cost-benefit evaluations of infrastructure program using property prices alone to value benefits may miss an important vehicle for cost recovery via the private sector.

These results contribute to our understanding of the mechanisms through which urban investment generates productivity.[4] On the one hand, improvements in urban amenity values operate like a cost shock to the firm, pushing out a set of unproductive firms. At the same time, increases in property values improve consumer spending power and lead to a dynamic improvement in the growth prospects of more productive firms. In many ways, these impacts mimic the effects of exposure to international trade which acts both to cull less productive firms and a vehicle for expansion, generating impacts on both the extensive and intensive margins of firm productivity [Mayer, Melitz and Ottaviano, 2021; Melitz and Redding, 2014]. What is different here is that these benefits are seen entirely among service firms that provide the non-tradeable products that can benefit from localized shifts in demand. The implication is that governments have a tool to drive sustainable increases in private-sector efficiency through investments in infrastructure.

The paper thus contributes to the burgeoning empirical literature on the mechanisms of growth and gentrification in the urban economics literature. Work on gentrification has emphasized increases in the skill of workers [Su, 2022], decreases in retail prices [Borraz et al., 2021], employment shifting from manufacturing to services [Hartley, Lester et al., 2013], and an increase in firm churn induced by exit of low-price firms the and growth of larger, higher-priced firms [Glaeser, Luca and Moszkowski, 2020]. A voluminous literature has tackled the impact of transport infrastructure on cities [Duranton and Turner, 2012], on market integration [Casaburi, Glennerster and Suri, 2013; Donaldson, 2018; Brooks and Donovan, 2020], and how such changes may be capitalized into land prices [Donaldson and Hornbeck, 2016; Tsivanidis, 2019]. By exploiting randomized variation in a program that generates infrastructural gentrification, we are able to nail down linkages between the constructed environment and the endogenous location

[4] Given the important role played by local actors and residents in deciding the specific investments to be made in Hábitat, our study also speaks to the large literature on Community-Driven Development (CDD) programs [Paxson and Schady, 2002; Labonne and Chase, 2009; Mansuri and Rao, 2004].

and growth decisions of private firms. As such, this paper provides one of the most granular and well-identified assessments available of the ability of public infrastructure investment to stimulate private sector development.

The rest of the paper is structured as follows: Section 2 provides the context for the study and describes the experimental design; Section 3 describes the data used, Section 4 provides the simple experimental results, and Section 5 explores the mechanisms through which the impacts are realized. Section 6 examines spillovers and conducts aggregate cost-benefit analysis at the neighborhood level, and Section 7 concludes.

2 Context and Experimental Design

2.1 The Hábitat Program

The Hábitat program was created under Mexico's Ministry of Social Development (SEDESOL) in 2003 to provide federal support for improvements in the infrastructure of marginalized neighborhoods in cities across the country. The core purpose of the program is to make a suite of coordinated investments in residential amenities for previously under-served neighborhoods, thereby increasing livability and social cohesion [Campuzano et al., 2007]. The targeting and funding rules for the program are formulaic and centralized; the program has tightly defined eligibility rules and requires matching investments from state and municipal governments. In terms of project selection, on the other hand, Hábitat pursues decentralized community-driven mechanism to allocate funding across potential investments. Because of the presence of simultaneous investment across multiple dimensions of urban infrastructure, the program provides a unique opportunity to observe the impacts of dramatic improvements in residential amenities.

Typical Hábitat investment includes a mixture of physical infrastructure (street paving, sidewalk and median construction, electrification and sewerage connections, etc.) with spending on community centers, sports fields, and trainings. Figures 1 and 2 show 'before and after' photographs from Google in two intervention neighborhoods in Guadalajara, and illustrate the nature of typical changes in the neighborhood: street paving improved, and sidewalks, crosswalks, and bollards installed. Importantly, the very large majority of the spending under the program is for *residential* amenities. While a previous study analyzing the same experiment as this paper has shown that the program results in dramatic improvements in the walkability and crime levels in treatment communities [McIntosh et al., 2018], Hábitat funds are typically spent on inputs that are not directly productive for the private sector. Table A1 provides a breakdown of the money spent through the program, showing that roughly half of the spending went to street paving, almost a quarter to a set of social and community development activities (such as after-school youth activities in community centers and domestic violence prevention training). Even the money spent on roads and paving is primarily used to improve residential neighborhoods and is not, for example, building trunk roads that connect these peripheral neighborhoods better with central parts of the city. Hence this study examines how the residential livability of a neighborhood, which we might more typically think of under the rubric of 'gentrification', drives outcomes for the private sector.

Several features of the program that drive location selection are important in terms of understanding the context of this study. First, the program has clearly specified rules for the ways in which local layers of government must co-contribute to investments in order to unlock the federal spending that comes through Hábitat. These cost-sharing rules require local governments to providing 50% of project costs: municipalities provide 40%, the states 8%, and the beneficiaries 2%. So the study universe consists of municipalities that were willing and able to meet these matching requirements.

Second, the program has clearly specified poverty targeting criteria, and explicitly sidesteps conflict over tenancy rights in the many informally settled slums of Mexico by requiring that a neighborhood has no active conflict over ownership in order to be eligible. In order to be eligible to benefit from Hábitat, a neighborhood must consist of settled households in a marginalized urban areas with concentrations of asset poverty greater than 50%, located in cities of 15,000 inhabitants or more, with a deficit of infrastructure and urban services, and with at least 80% of the lots having no active conflict over property rights. This means that our study areas are typically poor outlying neighborhoods of major cities with high poverty and poor infrastructure, but relatively high levels of home ownership. Eligibility was established in a very concrete spatial manner, whereby Hábitat defined 'polygons' that were clearly demarcated contiguous blocks that met the requirements for the program and in which the local layers of government were willing to invest. A Hábitat polygon is smaller than a locality and is a designation not used by other layers of government. Figure 3 illustrates the size of the treatment and control polygons relative to the overall city of Mérida.

The actual investments made in a polygon are determined by the interplay of a set of technical experts from the program who make recommendations based on observed infrastructure deficits, and a locally driven project selection component. The carefully orchestrated role played by local residents in proposing and vetting the use of funds makes this program similar in spirit to the large set of Community Driven Development (CDD) programs implemented across the developing world [Mansuri and Rao, 2004]. Explicit in the decisionmaking process was that municipal government would assume all maintenance costs of Hábitat infrastructure once the construction phase was completed.

2.2 The Design of the Hábitat Experiment

We follow an experimental phase of the implementation of Hábitat in 2009-2012, in which a set of 370 'polygons' (or neighborhoods) in 68 municipalities across urban Mexico were randomly assigned to treatment (full details of the experimental design are provided in Ordóñez-Barba et al. [2013] and McIntosh et al. [2018]). These sites contain 14,276 distinct blocks located in 38 cities, representing most of the large urban areas of Mexico. Study polygons contained 3% of the population and 1% of the surface area of study municipalities.

The randomization was conducted in 2009, the project selection process began immediately thereafter in treatment neighborhoods, and investments in the experimental locations ran from 2010-2012. 176 polygons were assigned to the treatment, and 194 to the control. The experiment featured a two-level randomization (first the saturation of treatment was randomly assigned at the municipality level between .1 and .9, and then treatment was randomly assigned at the polygon level to match the municipality saturation as closely as possible). $68 million in federal, state, and municipal funding was invested in treatment polygons during the period of the study. The control group was never treated with the program, meaning that the research design provides an opportunity to study both short- and long-term impacts of these investments.

3 Data

3.1 Hábitat database

The Hábitat database contains detailed geospatial information of the blocks, called *manzanas*, included in the study. The Hábitat study relies on INEGI's identification system of blocks, which in most part is standardized across the Agency's different projects. This makes it relatively simple to intersect data from the program with broader data sources complied by the Mexican government, such as the population and firm censuses. Each block is identified to the polygon it belongs within the project and its corresponding treatment/control status.

The Hábitat data contains substantial richness; it is possible to observe the exact type, amount, and location of each infrastructure upgrade a polygon received and on which year it occurred (2009, 2010 or 2011). Because the actual investments made in a given location were endogenous (both to the decisions of the Hábitat engineering team and to the community-driven selection process) we largely abstract away from this and analyze the treatment with a simple binary indicator.

3.2 Economic census database

The second data source is the Economic Censuses implemented by INEGI (Mexico's Statistical Agency) every five years. For this project, we use information of the firm censuses conducted in 2008, 2013 and 2018. The objective of these censuses is to capture the information of firms which have a fixed location (i.e. not stands, stalls or other temporary buildings), irrespective of their formality status, on their yearly data on labor, income and expenses, capital stock and other relevant data regarding their performance. Businesses covered by the census are classified into manufacturing, services and construction sectors. The census has a very high response rate, above 98% of all firms surveyed. The timing of these censuses is remarkably fortuitous for a study of Hábitat, given that the first interval allows us to conduct a before-after analysis of the short-term impacts of the program on the private sector, and the 2018 wave allows us to examine impacts 7 years after the cessation of investment. INEGI uses unique identifiers for each business surveyed. Thus, if a firm appears in two or more censuses, it is possible to link the data collected and create a panel. That is, it is possible to follow firms through the censuses and hence to measure firm creation and destruction. Table 2 provides summary statistics for the more than million firms operating in the cities in which Habitat was implemented, across the three survey years used in the study .

The INEGI survey also contains detailed information regarding firms' geographical location. Thus, firms can be placed on the block on which they are located within a city. This is crucial, as this geospatial information makes possible to cross this database with the Hábitat database and identify those firms contained within Hábitat polygons. We are able to locate 84,119 firms within Hábitat polygons. Given that there are slightly more control polygons, the majority of businesses are located in such polygons (roughly 60% of firms). In terms of sectors, the vast majority of businesses belong to commerce and services (over 90% of total). This is consistent with the sectoral composition of firms across the country. Within this group, most are grocery stores (around 25% of total), and stationer's shops and beauty salons (approx. 4% each). Manufacturing firms tend to be concentrated in activities related to the production of food and beverages and varied activities related to construction and housing. Around 3% of firms produce corn tortillas and 1% are bakeries. Ironworks, furnishing and milling activities businesses comprise close to 1% of the total each.

The core variables used as outcomes for analysis are: firm revenue, capital stock, paid workers and wage bill. All financial variables are adjusted for inflation so as to represent constant 2008 US dollar values. In terms of size, most of firms located in Hábitat polygons are microbusinesses. The median of paid workers is 1, which means that the typical firm only "employs" the owner of the firm. However, there are some firms that employ up to 50 workers. In line with the nature of microbusinesses, most firms have rather small yearly revenues (a typical firm makes US$ 15,600) and limited assets (less than US$ 2,500 for the typical firm).

The universal nature of INEGI's firm census allows us to contextualize the study universe in a very simple way, by comparing the Hábitat control polygons to the broader universe of the cities in which these firms are located. Figure 4 provides a visual representation of this comparison, showing the densities for our four major study outcomes: log revenue, number of paid workers, log wage bill, and log capital stock (we do not represent paid workers in logs because the majority of firms in the census have no or one paid employees). Despite the purposive poverty-targeting of the Hábitat rules, the firms in control neighborhoods prove to be surprisingly representative of their cities as a whole. They are slightly smaller in terms of revenues, and they are substantially more likely to have no paid workers. In terms of wage bill and capital stocks they track the broader distributions quite closely. Overall this suggests that our study neighborhoods contain firms that are similar to broader urban Mexico as a whole, albeit using slightly less labor and generating slightly lower revenue.

Along with small average size and high levels of informality, another key feature of this business environment is high turnover. Because INEGI ascribes a unique panel identifier to each firm we can track them across rounds even if they move locations. Figure 5 exploits this property of the data to illustrate the process of firm birth and death in control polygons during the three waves of data available from INEGI. In this figure the green lines indicate firms that we see created between 2008 and 2018, the red lines firms that we see die, and the blue line indicates firms that exist in all three rounds. The width of each line represents its share of the overall sample of firms ever observed.

Rates of churn are very high; 29% of firms in both subsequent rounds are newly born in that five-year period, and 18-20% of firms previously observed die in each subsequent round. Only 17% of all firms observed survive through all three rounds of the data. In general the stock of firms grows over time as the birth rates are roughly 10% higher than death rates in both subsequent rounds. Thus, the general picture is one of rapid creation and destruction of firms, and so we suspect that changes in the fortune of firms may have impacts on both the extensive margin of existence as well as the intensive margin of success among extant firms.

3.3 Summary Statistics and Balance

Table A2 focuses on the firms located with study polygons to examine the balance of the experiment. It uses the pre-treatment data (2008) to present comparative summary statistics for the treatment and control polygons, and tests for balance using a specification similar to the one subsequently used to test for impacts (regression on a treatment dummy, fixed effects for municipalities, and standard errors clustered at the polygon level, the unit of assignment). When we pool all firms together we examine 28 outcomes and find no evidence of significance. Once we disaggregate by manufacturing and service sector separately we some significant difference, but overall the table presents 84 comparisons and finds 6 covariate imbalanced at the 10% level and 5 at the 5% level, in line with what we would expect by random chance. Overall these results suggest a well-balanced experiment. In our impact analysis we include the baseline polygon-level average level of the outcome variable as an ANCOVA control, which should remove any residual imbalances that do exist.

4 Results

4.1 Firm-Level Impacts

Our analysis uses a post-treatment cross-sectional ANCOVA specification:

$$Y_{ijm1} = \beta_0 + \delta\tau_{jm1} + \rho\bar{Y}_{jm0} + \gamma_m + \epsilon_{ijm1} \tag{1}$$

where Y_{ijm1} is the post-treatment outcome for firm i in polygon j and municipality m, $\bar{Y}_{jm0}$ is the ANCOVA control (baseline mean outcome in that polygon), γ_m is a set of municipality fixed effects, and ϵ_{ijm1} is a random error which we cluster at the polygon level to account for the design effect. In this specification, the estimand $\hat{\delta}$ on the post-treatment polygon-level dummy τ_{jm1} gives the intention-to-treat effect (ITT) of Hábitat on firms in treatment polygons. We use both 2013 and 2018 as outcome data, but always use 2008 as the year for the ANCOVA control. The variable $\bar{Y}_{jm0}$ is calculated at the polygon-level to solve the problem that would otherwise arise in using a firm-level baseline outcome (whose existence is endogenous if the treatment leads to extensive margin impacts).

Table 3 provides our main analysis of the impact of the program, using all extant firms in each round of the data and so providing an omnibus test that combines the intensive and extensive margin impacts of the program. The first two rows pool all types of firms together, and present impacts in 2013 (three years after the end of treatment) and 2018 (eight years later) in separate columns. Columns 3-4 analyze only manufacturing firms, and Columns 5-6 only trade and services firms.

Looking first at the short-term results that pool sectors, we see that a program has been shown elsewhere to have led to an 18% increase in residential rents and a 10% increase in property prices has an impact that is consistent with a response to a cost shock: a substantial increase in the wage bill. However, far from cutting back on this now more-expensive labor, we also see an increase in the number paid workers in treatment areas. The wage bill increases by 20% ($.203 thousand over a base of $1.07 thousand), and the number of paid workers increases by 39% (.52 workers over a base of 1.35) indicating that both the number of workers and the wage per worker have increased in treatment areas. Both capital stock and revenue rise in the short term to an extent that is quantitatively meaningful (5%) but not significant (although both t-statistics above 1). Hence within a year or two of the cessation of the Hábitat investment, costs and employment have risen substantially and revenues have not kept pace.

Over the longer term however, the 2018 data paints a substantially rosier picture. Now 6-7 years after investments ended, revenue has risen by 9% ($2.1 thousand over a base of $23.4 thousand), capital stock by 17% ($1.08 thousand over a base of $6.2 thousand), and while the impacts on the number of employees have largely faded the impacts on the wage bill remain largely intact. Taken as a whole, this time path of impacts is suggestive of Hábitat investments acting in the short term as a cost shock to firms without compensation on the revenue side, but over the longer term as the dynamics of greater residential wealth lead to superior demand, firms grow more quickly over the long term while remaining able to cover the higher wage bills necessitated by higher local residential costs. The positive longer-term impacts on revenues indicate that Hábitat induces meaningful medium-term changes to the demand faced by local firms.

The subsequent columns of Table 3 disaggregate these impacts by firm sector. In line with the literature on gentrification [Glaeser and Gottlieb, 2009; Lester and Hartley, 2014; Glaeser, Luca and Moszkowski, 2020] we find the impact of these residential amenity improvements to be entirely confined to service-sector firms. Because manufacturing firms typically sell into tradable markets where local demand changes do not translate into changes in prevailing price, if anything the treatment effect of Hábitat may be predominantly negative, in the form of a shock to the prevailing local wage without changing output prices. Service sector firms, on the other hand, are poised to benefit from localized changes in the demand for (and possibly price of) local non-tradeable goods. In this sector we see revenues jump even in the short term by 5%, and over the longer term service firm revenues in treatment areas are higher by 10%, with capital stocks soaring by 23%. Hence this highly localized program has a very substantial benefit for, and only for, firms operating in the non-tradeable sector.

As discussed previously, in a business environment with such a high degree of churn, impacts on the stock of surviving firms could arise either through intensive margin changes for surviving firms, or through the selective margin by driving firm birth and death. We investigate these two dimensions in turn, beginning by considering firm creation and destruction as outcomes in a standard experimental context. To do this, Table 4 examines firm birth and death as outcomes of the treatment, so as to understand the extent to which the overall treatment impacts of the program on the composition of firms may be arising from entry and exit. The top panel of this figure defines the universe as all firms that existed in 2008, and examines an outcome variable which is a dummy for that firm having exited the market by the time of the post-treatment survey (2013 or 2018). The probit regression results show that the program leads to short-term excess firm death of 1.5 percentage points, or an increase of 3.5% over the control group death rate of 43 percent.[5] This differential actually decreases slightly when we look at 2018, providing preliminary evidence that most of the firm exit generated by the program is experienced immediately. As before, these effects are confined entirely to service sector firms and the program had no effect in the manufacturing sector.

The lower panel of this table looks at firm entry, now taking the universe as the endline sample of firms and defining a dummy variable for whether that firm is newly

[5] Note that the percentages in this table are the fraction of baseline firms, while the percentages presented in Figure 5 are the percentage of all firms ever observed.

born since the baseline. Here the story is a mirror image (although less significant); increases in the rate of firm birth of around 1.5 percentage points, entirely concentrated in the service sector, and mostly experienced in the short term. Taken as a whole then, we can summarize the extensive margin results quite simply by saying that as of 2018 the treatment had resulted in about 1.5 percent of the total distribution of firms being different than the ones that would have existed in the absence of the program, with no effect on the total number of firms.

We can dig deeper into the dynamics of entry and exit by looking at the firm birth and death that occurs between 2013 and 2018; this helps us to understand whether the program continues to exert a dynamic selection effect on the composition of firms. In Table A3 we therefore use the post-treatment 2013 survey as our baseline and examine entry and exit between that year and 2018. Using this (admittedly endogenous) post-treatment yardstick for subsequent growth we see no extensive margin impacts, meaning that the compositional effects of the program were relatively immediate. Hence the program leads to a short-term shake-out on the extensive margin but does not exert subsequent composition effects.

Given these meaningful but not qualitatively massive extensive margin effects, we suspect that the program has led to growth of firms on the intensive margin. While this story is difficult to tell with perfect experimental clarity, a simple way of posing the question is to restrict the sample to the (endogenous) group of firms that survive from baseline, and looking at impacts on these continuing market participants. Table 5 conducts this exercise and finds impacts that are roughly twice as large as the overall impacts found in Table 3. Here we see really large effects; for example service firms in the treatment area that survive from 2008 to 2018 see revenues that are 15% higher and capital stocks that are fully one third higher than comparable firms in the control. Hence the overall treatment effect is a composite of a large increase in the size of surviving firms with a relatively small increase in the turnover of firms on the extensive margin. Because newly entering firms are on average smaller than incumbents, this increase in churn actually dampens the total ITT effect of the treatment on firms size relative to the impact on ongoing firms. We now turn to a more detailed analysis of the ways in which the treatment altered the composition of market participants.

4.2 Heterogeneity in Impacts

We can perform a straightforward test of heterogeneity for firms that were observed at baseline; hence we begin our analysis by looking at the intensive margin heterogeneity of treatment effects for firms present both at baseline and endline. This analysis is presented in Table 6. While the short-term results are more equivocal, it is clear from this table that by 2018 firms that were in the top half of the original distribution of firm quality have grown more in the treatment neighborhoods. Revenue, capital stock, paid workers, and wage bill all display significant interactions, and indeed insignificant uninteracted treatment terms meaning that all of the impact of the program arises in the top half of the original productivity distribution. So the new opportunity provided in these neighborhoods is exclusively seized by productive firms.

Similarly, for firms present at baseline it is straightforward to ask whether firms that were initially less productive are those most likely to exit as a result of the program. Recalling that the treatment effect on death of firms appears in 2013 and not 2018, we again find evidence of the strongest firms surviving best. In Table A4, we see the uninteracted Habitat treatment dummy suggesting an elevation of about 2 percentage points in the probability of firm death, and the interaction effect on being in the top quartile of baseline productivity is -2 percentage points, meaning that these most efficient 25% of firms see no elevation in exit. Therefore, all of the short-term firm death caused by the program is occurring in the unproductive firms.

The analysis of firm entry is less straightforward in that by definition we do not observe pre-treatment heterogeneity. What we can do is to examine whether there are differences between the attributes of newly created firms between the treatment and control; these differences would be a composite of true extensive margin selection effects on entry as well as the intensive impacts of the treatment on firm growth between creation and the time of the survey. This analysis, in Table A5, also lines up with the idea that the treatment is having meaningful impacts on the distribution of firm productivity, with entering service-sector firms being superior on most core outcomes in 2018. So, while we cannot cleanly say that these firms *entered* being more productive, it does appear to be the case that firm growth was fastest and firm death lowest among firms that were originally productive in the treatment, and new treated firms grew faster. Thus heterogeneity in the response to treatment by more productive firms plays an important role in explaining the total effects we observe.

5 Mechanisms for Firm-Level Impacts

The question of what is driving these results is important not just to better understand the nature of change in Hábitat neighborhoods, but also to appreciate whether this was a broader structural change in the local economy. We turn now to each of three core elements of structural transformation - access to credit, firm formality, and the response of residents and consumers.

5.1 Access to Credit and Financial Services

First, we find significant evidence of a financial channel behind the transformation and expansion of firms in treated polygons. Point estimates, while small in absolute terms, are sizeable when we compare them to the control mean. As described in Table 7, these businesses have a 20 percent higher probability of having secured a loan in 2013, an effect driven entirely by firms in the service sector. This loan is not obtained through informal lenders but through the formal financial system (i.e. banks or savings cooperatives). We then see a higher probability of surviving firms having a bank account in 2018, which we interpret as the loan directly helping businesses to obtain formal access to the financial system.

The nature of the businesses in these areas is such that their key margin along which they can expand their economic complexity is through machinery and fixed assets rather than the adoption of more sophisticated types of technologies such as IT equipment. As shown previously, we find the businesses in the treated polygons expand their capital stock and purchases of machinery and equipment, supported by their expanded access to loans. The right-hand columns of Table 7 illustrate that use of computers is not changed, and service firms see an increase in access to the internet which while strongly significant is only a half of a percentage point.

Importantly, this expanded access to credit does not appear to be mechanically driven by an expansion in the value of businesses' collateral as the value of the owned property increases. This is shown in Table A6, which analyzes the uptake and sources of credit, as well as the uses to which it is put, splitting the sample according to whether businesses own the property on which they operate or not. While businesses with land collateral have somewhat better baseline access to credit (13% versus 9% for those without), the treatment effects of Habitat are virtually identical: a short-term expansion of 2.5 percentage points in 2013 and no longer-term effect. Unsurprisingly firms with land collateral are more likely to be served by formal banks and less likely to rely on savings banks, and non-landed firms put more of their money into land acquisition and inputs. But the reduced-form change in credit access is not being driven by land, removing as a potential explanation for mechanisms the fact that the private sector expands under residential investment strictly through the collateral value channel. Since collateral value does not seem to drive changes on the supply side, it appears that demand-side shifts arising from improved sales and profitability are the most reasonable explanation for the credit expansion.

5.2 Firm Formalization

The second mechanism at play which we think explains the changes occurring among businesses in the treated polygon is formalization. Our measure of formalization relies on the definition of Busso, Fazio and Levy [2012] and focuses on the level of social security contributions paid by the business and not on the formal aspect of having a tax ID which is typically a common attribute among businesses in Mexico. The advantage of using the level of social security contribution as a measure of formality is that this better captures the fact that the business is not only legal from a tax perspective but it is substantially contributing to generate higher quality formal jobs as its employees are covered by social security benefits (i.e. pension, health insurance, etc.). As suggested by Busso and Levy we estimate that businesses should be on average paying the equivalent of 18 percent of total wages in social security contributions to be fully complying with their social security regulations ("strict formality definition"). However, this is an upper bound of their contribution and firms paying social security contributions that are below 18 percent of the wage total could still be fully compliant with social security regulations. We therefore assess a second measure of formality as those firms that pay any social security contributions ("liberal formality definition"). Accordingly, the latter is our preferred measure of being formal.

As shown in Table 8 we find an increase in the probability of being formal that is driven primarily by businesses in the services sector (although this is a rare case where we see positive impacts of Habitat on manufacturing firms as well). While point estimates are small in absolute terms we should observe that the prevalence of formalization among these types of businesses is very low (0.2 percent using our stricter definition of formality and 2.7 percent using our more liberal definition) and our result imply an increase in the likelihood of being formal compared to the control mean equal to 15 percent in 2013 and 24 percent in 2018. While the results are mainly driven by the transformation of incumbent businesses that change their status from informal to formal, there are also some effects in the service sector on the extensive margin, with newly entered firms being more likely to have formalized as well.

5.3 Neighborhood Population

Together, our results imply broad changes in the characteristics of firms serving residents in treatment neighborhoods. How do residents respond in turn? This matters for the interpretation of our findings, with our firm level results implying changes in the nature of local consumption. With greater revenues for dominantly non-tradeable items, it is likely that much of the implied consumption is local. To what extent are our results driven by new and different populations moving into treatment neighborhoods, or by existing populations changing their consumption patterns?

McIntosh et al. [2018] document an increase in private investment in housing, with householders incorporating the higher amenity value of their surroundings into home upgrading. In particular, they observe significant upgrades to flooring and plumbing, with a 12 percent increase in the likelihood of a home containing a flush toilet. They also document the fact that though home ownership rates do not change significantly in treatment areas, property values rise substantially and rental costs rise by almost 20 percent. This finding is consistent with the increase in wage bills that we observe for firms in Table 3.

We assess the issue more broadly by analyzing the Mexican Census of Population and Housing 2020, also provided by INEGI and integrated with the same set of blocks and polygons as our core analysis. Table 9 presents our results. We regress, at the polygon level, measures from the 2020 Population Census on a treatment dummy and values of the variable from the 2010 Population Census. As such, the analysis is in the form of an ANCOVA specification, allowing us to present the most precise assessments our data allow. We split the analysis into variables related to the levels (or corresponding percentage) of the variable (Panel A) and inverse hyperbolic sine transformations of those same variables (Panel B) to check for robustness from outliers.

Overall, we do not find significant effects of the Hábitat program on the structure or characteristics of the population. Columns 1-3 assess the size of the population within our study polygons in terms of total, female and male populations. In each case the coefficient is small and insignificant at the usual levels. Similarly, we find no evidence that there is a higher proportion of adults or children in treatment neighborhoods (Column 4). Columns 5-7 indicate that the population of treatment neighborhoods are similar to control along a number of important margins. They are no more educated, no more likely to be employed, nor married.

It does not, therefore, seem that the changes in the private sector we observe are driven by significant changes in the demographic characteristics of populations in Hábitat neighborhoods. This is consistent with the limited change in home ownership rates observed in McIntosh et al. [2018]. Rather, the results are consistent with the upgrading of neighborhoods changing the consumption patterns of local residents. A remaining question is whether these changes were driven by wealth effects from the injection of investment capital from Programa Hábitat or other indirect effects of the program. McIntosh et al. [2018] provides detailed measurements of house price changes based on the assessments of professional property assessors from the Instituto de Administración Avaluos de Bienes Nacionales (INDAABIN), the Mexican government's institute of real estate valuation. They find that "the treatment group had almost triple the real rate of appreciation as the control", implying significant increases in the wealth of many of the treatment polygon's residents.

Bringing together the insights from the economic and population censuses, and the results from McIntosh et al. [2018], we see that Programa Hábitat had impacts on the nature of the private sector that do not seem to have been driven by changes in the underlying population being served but rather their core spending power. The program seems to have shifted the structure of the local economy - by which we mean the consumption choices of neighborhood residents and production choices by neighborhood firms - to a different equilibrium. That equilibrium had many characteristics of a more mature service economy - bigger, more capital-intensive firms with a greater likelihood of indicators of formality for example. This interpretation implies that government infrastructure investments can induce structural economic change at a very localized level.

6 Accounting for Broader Benefits

6.1 Spillover Effects

Spatial spillover effects are critical for interpreting the underlying model of economic geography revealed by the program. One possibility is that these investments have generated a meaningful change in the agglomeration externalities of these urban neighborhoods, in which case we should see increases in investment and TFP in surrounding areas as in Greenstone, Hornbeck and Moretti [2010], and the narrow consideration of the Hábitat polygons would represent an under-estimate of total benefits. Alternatively, it may be that we are seeing localized benefits more in line with an improvement in neighborhood amenities ala Rosen-Roback, in which economic activity is spurred by an increase in demand from greater local housing wealth, but no underlying change in factor productivity has occurred. In this sense the spatial footprint of impacts allows us to test this 'productivities' interpretation against the 'amenities' story.

Our study provides an unusual and high-powered way to think about this question, in that we know precisely the physical boundaries of where infrastructural improvements were actually made. Because we can define the spatial footprint of the investments so precisely, we can look for spatial spillover with a high level of granularity. To do this, we begin from the outlines of the study polygons, and define 'buffers' around these of different distances in the same way for both treatment and control neighborhoods. We then use the addresses in the INEGI data to identify the firms within each buffer as we did to place them in the study polygons, and examine the differential outcomes for firms within a given buffer from treatment polygons relative to the same buffer for control polygons. This approach is simply experimental in an attractive way, and has very similar statistical power to the overall study (especially as we look at larger buffers that contain more firms).[6]

The results of this analysis of are presented in Table 11. The top panel of this table shows spillovers in 2013, and the bottom panel 2018. The outcomes appear in rows, and the buffers in columns. The distance buffers are non-inclusive, meaning that the 100-250m buffer does not include the 100m buffer. Given that spillovers may both shift the composition of firms as well as altering outcomes on the intensive margin for pre-existing firms, for each buffer we show both the total effect (all endline firms) and the intensive margin effect (only firms that existed at both baseline and the relevant endline). Overall, this table shows remarkably little evidence of spatial spillovers, especially given the strong impacts of the treatment within study polygons. At the most proximate distance, 0-100m, there is never evidence of spillovers either on all firms or on the intensive margin. We do see some evidence of positive impacts in the 100-250m buffer, especially on the intensive margin. However, these same outcomes often show negative point estimates only slightly farther away, and are also not found in the nearest blocks. Hence we certainly have no spatial monotonicity in the impacts as we move away from study polygons, and it appears that the simplest summary of these effects is that there are no spillovers.

In terms of trying to connect the effects from Hábitat more deeply into a geographic

[6] As in the main analysis we cluster at the polygon level and include municipality fixed effects. In some cases the same blocks enter the buffers for multiple polygons, in which case the treatment dummy for that buffer is replaced with the continuous share of that buffer that is within the given distance from treatment polygons.

model of how the surrounding cities function, we have taken a couple of approaches. The first of these is to as whether the effects of receiving the program are amplified by the degree of market access for the recipient neighborhood. If market linkages are key to driving the productive impact of the program, we would expect that investment has a larger effect in neighborhoods with deeper market access. To pose this question, we expand the spatial lens of the program to calculate inverse distance-weighted local market size (using both overall population and non-poor population) for each locality based on distance to centroids of adjacent localities in the same municipality. This analysis, presented in Table A7, finds no evidence of heterogeneity in the impacts of Habitat across measures of market access. Neither the population- nor wealth-adjusted measures of market access show any relationship with local effects, meaning that the impact on firm behavior was similar whether the neighborhood invested in was economically remote or well-situated relative to outside demand.

We can move further into continuous space by considering the universe of blocks (manzanas) that are outside of study polygons but within 1 km of them, and looking for heterogeneity in the spillover effects of treatment in exposure to market access. This analysis provides a different lens on the lack of spillovers illustrated above in Table 11. We account for the endogeneity in targeting of Hábitat by controlling for the fraction of each block that was eligible for the program (in treatment or control), and then including a dummy for treatment in that block, and the interaction with this variable and the measures of market access. In Table A8, we find no evidence that heterogeneity in market access is modulating the spillover effects of the program, as would be the case if Hábitat *did* drive outcomes in adjacent areas with strong market access and *did not* otherwise. Here again, there is no sign of greater effects in well-connected adjacent markets. The conclusion from the lack of spillovers, lack of sensitivity to market access, and the lack of heterogeneity in spillovers all points in the same direction. The program does not appear to have driven broader market mechanisms, either through agglomeration or differentially enhancing business growth in central market locations. Rather, it appears to have generated a relatively homogeneous and remarkably localized set of impacts that are narrowly concentrated in the immediate location of the investments. The conclusion is that a welfare analysis focusing on experimentally identified changes in firm behavior within study polygons is sufficient, and a tallying of the broader economic impacts of the program will not be driven by general equilibrium changes outside of the study areas due to agglomeration or trade patterns.

6.2 Fiscal Impacts

Based on the above, we now present an analysis whose purpose is to estimate the aggregate financial implications of the intervention on the tax take realized by the government through taxing firms in treatment polygons. The starting point for this is to estimate the total impacts of the intervention at the polygon level, rather than at the firm level as we have done so far. Table 12 calculates the totals across all firms at the polygon level, and then taking these polygon totals as outcomes runs a regression that includes the baseline total and municipality fixed effects and controls to examine the impact on overall averages for the polygon. Because these regressions average across polygons of very different size, we weight the analysis by the number of firms in each polygon at baseline. We focus here on the core outcomes that are directly and credibly taxable by the Mexican government: these are revenue, value added, and payments to social security (all impacts in this table are in thousands of US dollars). While the number of units is different and the estimates are somewhat less precise, the findings here generally mirror the firm-level impacts, showing strong improvements in capital investment and payments to social security, and increases in revenue and value added in 2018 that while insignificant in this specification are large in absolute magnitude.

Table 13 then takes the 2018 polygon-level impacts for revenue, value added, and payments to social security, and uses them to simulate the change in revenue recovery by the government solely arising from shifts in business behavior. We first multiply each polygon-level impact times 176, the number of treatment polygons in the study, to recover the total change across the whole study in that measure. We then multiply each of these totals times the marginal tax rate applied to each measure. For Value Added, we use the 16% VAT tax rate which is universally applied in Mexico, including to very small firms such as those we study here. For contributions to *IMSS*, the federal social security system, these payments are asked directly in the survey and so we use a rate of 100%. For total revenue we use the 2% tax rate that is applied to firm revenue. These calculations result in increased government receipts of 1.2, 2.6, and .9 million dollars per year respectively across these three forms of taxation. Finally we can put this 4.7 million annual increase in receipts up against the 67 million dollar cost of the program to calculate that these receipts will cover the cost of the program in a little over 14 years. This estimate is conservative in that it ignores impacts on property taxes (either for firms or households), but is strongly suggestive that private sector taxation represents a meaningful channel through which the costs of infrastructure programs can be recouped by governments even when the private sector was not the target of those investments, and even when the impacted businesses are small.

7 Conclusion

We bring together a large-scale experiment in the construction of infrastructure with a three-round census on firm activity in urban Mexico. Studying a program that spent $68 million over three years and across 65 municipalities, we find a powerful and durable response of private-sector firms to improvements in neighborhood amenities. Wages, employment, and capital investment all rise, and firms appear to shift onto a higher path of revenue growth that is continuing to expand relative to the control six years after the end of the program. Firm formalization and use of financial services improves. Hence, despite the fact that the program made few investments that directly effect productivity, firm profitability improves substantially. These improvements are localized entirely to the service sector.

Analysis of mechanisms suggests that these changes were driven by a gentrification in the consumer base, leading to a durable shift in demand. Our evidence supports the idea that the urban amenity value of intervention neighborhoods improved in manner that was highly localized to the places where the investments were made. No evidence of spatial spillovers or market-mediated heterogeneity is found. Consequently, while this type of investment does not kick-start an endogenous process of agglomeration-driven productivity, the improvements in consumer demand from changes in local residential wealth are substantial. Our finding that the program would pay for itself in 14 years simply through taxes on firms suggests that links to the private sector are an under-appreciated pathway for the recouping of the costs of investment in residential infrastructure.

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Tables

Table 1: Descriptive statistics: firms in cities where Habitat was implemented

	Obs.	Mean	Std. Dev.	Min.	Max.	p25	Median	p75
2008								
Value added	1,093,412	30.0	86.8	-361.6	2,569.4	1.2	4.8	15.9
Revenue	1,093,412	79.2	256.9	0.0	3,079.5	4.5	13.6	39.9
Capital stock	1,093,412	27.8	106.1	0.0	883.3	0.5	2.1	9.7
Investment	1,093,412	0.8	4.6	0.0	39.3	0.0	0.0	0.0
Value added per paid worker	1,093,412	11.2	44.1	-361.6	2,122.8	1.0	3.3	8.2
Paid workers	1,093,412	3.0	6.4	1.0	51.0	1.0	1.0	2.0
Wage bill	1,093,412	8.6	31.8	0.0	272.5	0.0	0.0	3.5
Wage	380,910	3.7	2.8	0.0	196.2	2.0	3.3	4.6
2013								
Value added	1,227,956	28.9	84.7	-814.3	2,318.2	1.4	4.9	15.7
Revenue	1,227,956	71.5	236.5	0.0	2,641.0	3.8	11.3	34.2
Capital stock	1,227,956	25.2	97.8	0.0	800.9	0.4	1.9	8.0
Investment	1,227,956	0.8	4.2	0.0	35.0	0.0	0.0	0.0
Value added per paid worker	1,227,956	11.9	45.6	-177.9	1,960.9	1.3	3.7	8.6
Paid workers	1,227,956	2.8	5.9	1.0	46.0	1.0	1.0	2.0
Wage bill	1,227,956	7.2	26.4	0.0	226.3	0.0	0.0	2.7
Wage	368,899	3.6	2.4	0.0	170.2	2.1	3.2	4.5
2018								
Value added	1,506,250	45.4	143.1	-1,790.0	3,531.9	2.5	7.3	22.2
Revenue	1,506,250	103.7	348.6	0.0	4,090.6	6.6	17.1	45.7
Capital stock	1,506,250	27.1	106.8	0.0	1,210.2	0.3	1.9	7.6
Investment	1,506,250	0.8	4.3	0.0	47.0	0.0	0.0	0.0
Value added per paid worker	1,506,250	17.2	81.6	-1,790.0	3,261.0	2.1	4.9	10.3
Paid workers	1,506,250	3.2	6.7	1.0	52.0	1.0	1.0	3.0
Wage bill	1,506,250	9.3	31.9	0.0	338.1	0.0	0.0	4.6
Wage	580,802	3.7	2.1	0.0	75.3	2.3	3.3	4.6

Variables in thousands of 2013 US dollars, except for paid workers.

Notes: Table provides summary statistics at the firm level for all firms included in INEGI's census within any of the cities in which the Hábitat program was implemented. The 2008 wave forms the baseline for this study, and the the 2013 and 2018 waves are post-treatment.

Table 2: Descriptive statistics: by location

	2008				2013				2018			
	Obs.	Mean	Median	Std. Dev.	Obs.	Mean	Median	Std. Dev.	Obs.	Mean	Median	Std. Dev.
Firms in Habitat polygons												
Value added	36,289	7.4	2.5	13.4	44,395	6.8	2.7	11.5	51,078	9.9	4.2	24.9
Revenue	36,289	20.4	8.7	33.2	44,395	16.7	7.5	27.3	51,078	24.2	11.4	60.9
Capital stock	36,289	7.7	1.6	20.5	44,395	6.8	1.6	18.3	51,078	6.9	1.5	24.5
Investment	36,289	0.1	0.0	0.7	44,395	0.2	0.0	0.6	51,078	0.3	0.0	1.6
Value added per paid worker	36,289	5.0	2.1	9.2	44,395	4.9	2.5	8.0	51,078	6.1	3.4	14.2
Paid workers	36,289	1.5	1.0	1.4	44,395	1.4	1.0	1.4	51,078	1.6	1.0	1.8
Wage bill	36,289	1.4	0.0	5.0	44,395	1.2	0.0	4.6	51,078	1.9	0.0	7.4
Wage	6,687	2.9	2.7	1.7	6,540	2.9	2.8	1.6	11,305	3.0	2.9	1.5
Firms in rest of the city												
Value added	1,057,123	30.8	4.9	88.1	1,183,561	29.7	5.0	86.1	1,455,172	46.6	7.4	145.4
Revenue	1,057,123	81.2	13.6	260.9	1,183,561	73.6	11.3	240.6	1,455,172	106.5	17.5	354.1
Capital stock	1,057,123	28.4	2.2	107.8	1,183,561	25.9	2.0	99.5	1,455,172	27.8	1.9	108.5
Investment	1,057,123	0.8	0.0	4.6	1,183,561	0.8	0.0	4.3	1,455,172	0.8	0.0	4.4
Value added per paid worker	1,057,123	11.4	3.4	44.8	1,183,561	12.1	3.7	46.4	1,455,172	17.6	4.9	82.9
Paid workers	1,057,123	3.0	1.0	6.5	1,183,561	2.9	1.0	6.0	1,455,172	3.3	1.0	6.8
Wage bill	1,057,123	8.8	0.0	32.3	1,183,561	7.4	0.0	26.9	1,455,172	9.6	0.0	32.4
Wage	374,223	3.7	3.3	2.8	362,359	3.6	3.2	2.4	569,497	3.7	3.3	2.1

Note: For variables other than workers, values are in thousands of 2013 Mexican Pesos.

Notes: Table provides summary statistics at the firm level for all firms included in INEGI's census within any of the cities in which the Hábitat program was implemented, by location within city. The 2008 wave forms the baseline for this study, and the the 2013 and 2018 waves are post-treatment.

Table 3: Main regression results, all firms

Sample: Existing firms in endline, including those missing in baseline

	All sectors		Manufacturing		Trade and Services	
Dependent variable	2013	2018	2013	2018	2013	2018
Revenue	0.801	2.100**	0.356	0.440	0.883*	2.327**
	(0.487)	(0.925)	(0.903)	(1.371)	(0.508)	(1.026)
	16.447	*23.455*	*20.358*	*25.855*	*15.901*	*23.104*
Capital stock	0.521	1.080***	-0.544	-0.312	0.715**	1.347***
	-0.372	(0.404)	(0.838)	(0.748)	(0.360)	(0.400)
	6.368	*6.225*	*10.912*	*8.778*	*5.722*	*5.832*
Paid workers	0.052***	0.022	0.111	-0.029	0.049**	0.031
	(0.020)	(0.028)	(0.068)	(0.083)	(0.020)	(0.027)
	1.354	*1.544*	*1.870*	*2.184*	*1.282*	*1.455*
Wage bill	0.203***	0.204**	0.373	-0.111	0.188***	0.256**
	(0.061)	(0.103)	(0.248)	(0.291)	(0.061)	(0.106)
	1.069	*1.782*	*2.958*	*4.124*	*0.805*	*1.458*
Observations	[44,200]	[50,869]	[5,365]	[6,072]	[38,791]	[44,750]

The table shows the value of coefficient β of the described in the following specification: $y_T = \alpha + \beta * Habitat + \phi * \bar{y}_{2008}^{Polygon} + FE_{Municipality} + FE_{Polygonsize} + \epsilon$. Each coefficient denotes a different regression. Standard errors clustered by Habitat polygon shown in parenthesis. Mean values of control groups in italics. Number of observations in square brackets.

Table 4: Effect of Habitat on probability of exit and entry of firms

	All sectors		Manufacturing		Trade and Services	
	2013	2018	2013	2018	2013	2018
Probability of exit						
Habitat	0.015*	0.013	0.006	0.008	0.017*	0.013
	(0.008)	(0.010)	(0.019)	(0.021)	(0.009)	(0.010)
	0.433	*0.596*	*0.461*	*0.624*	*0.429*	*0.592*
Observations	[36,109]	[36,109]	[4,777]	[4,777]	[31,286]	[31,286]
Probability of entry						
Habitat	0.014	0.007	0.005	-0.000	0.015	0.008
	(0.014)	(0.010)	(0.021)	(0.016)	(0.014)	(0.010)
	0.543	*0.718*	*0.534*	*0.713*	*0.544*	*0.718*
Observations	[44,200]	[50,869]	[5,365]	[6,072]	[38,791]	[44,750]

Table presents coefficients from a linear probability model. The probability of exit (top panel) is estimated among all firms present at baseline explaining whether they have exited by the indicated round. The probability of entry (bottom panel) is estimated among all firms present in the post-treatment waves explaining whether the firm is a new entrant in that round. Standard errors clustered by Habitat polygon shown in parenthesis. Mean values of control groups in italics. Number of observations in square brackets.

Table 5: Main regression results, surviving firms

Sample: Existing firms in both baseline and endline

	All sectors		Manufacturing		Trade and Services	
Dependent variable	2013	2018	2013	2018	2013	2018
Revenue	0.705	4.054**	0.691	1.334	0.659	4.468*
	(0.615)	(2.053)	(1.214)	(2.681)	(0.667)	(2.284)
	19.444	*29.645*	*24.445*	*33.818*	*18.724*	*29.020*
Capital stock	0.531	2.128**	-1.785	-0.990	0.895**	2.762***
	(0.417)	(0.858)	(1.131)	(1.862)	(0.423)	(0.872)
	7.514	*8.661*	*13.874*	*14.210*	*6.579*	*7.762*
Paid workers	0.046**	0.054	0.143	0.115	0.040*	0.053
	(0.023)	(0.055)	(0.094)	(0.174)	(0.023)	(0.051)
	1.372	*1.606*	*1.951*	*2.431*	*1.287*	*1.487*
Wage bill	0.227***	0.395	0.580*	0.410	0.197**	0.435*
	(0.083)	(0.240)	(0.348)	(0.693)	(0.079)	(0.234)
	1.211	*2.160*	*3.454*	*5.386*	*0.884*	*1.703*
Observations	[20,163]	[14,279]	[2,565]	[1,784]	[17,582]	[12,487]

The table shows the value of coefficient β of the described in the following specification: $y_T = \alpha + \beta * Habitat + \phi * \bar{y}_{2008}^{Polygon} + FE_{Municipality} + FE_{Polygonsize} + \epsilon$. Each coefficient denotes a different regression. Standard errors clustered by Habitat polygon shown in parenthesis. Mean values of control groups in italics. Number of observations in square brackets.

Table 6: Heterogeneity in main results

	Initial Value Added per Worker						Initial Revenue					
Dependent variable	2013			2018			2013			2018		
	25th	50th	75th	25th	50th	75th	25th	50th	75th	25th	50th	75th
Revenue												
Habitat*percentile	-0.155	0.480	0.501	-2.316	6.378**	5.921	-1.072	2.115**	2.830**	-3.006	4.182	7.129
	(0.915)	(0.864)	(1.085)	(2.705)	(3.153)	(4.180)	(0.848)	(0.912)	(1.290)	(2.645)	(2.894)	(4.852)
Habitat	0.837	0.631	0.684	4.665*	0.842	2.561	1.044	-0.316	-0.020	4.731**	1.822	2.089
	(0.695)	(0.732)	(0.641)	(2.384)	(1.825)	(2.433)	(0.722)	(0.691)	(0.654)	(2.346)	(1.585)	(1.683)
Capital stock												
Habitat*percentile	-0.869	0.955*	0.544	-1.570	3.935***	3.554**	0.355	1.270**	1.886**	-1.622	2.762**	4.283***
	(0.587)	(0.579)	(0.745)	(1.413)	(1.351)	(1.558)	(0.688)	(0.617)	(0.850)	(1.458)	(1.122)	(1.554)
Habitat	0.755	0.073	0.410	2.490**	0.023	1.166	0.485	-0.118	0.029	2.462***	0.613	0.908
	(0.459)	(0.478)	(0.415)	(0.962)	(1.035)	(0.899)	(0.460)	(0.461)	(0.441)	(0.932)	(0.860)	(0.796)
Paid workers												
Habitat*percentile	-0.063	0.052	-0.007	-0.168*	0.194**	0.102	-0.059	0.141***	0.134**	-0.114	0.213***	0.139
	(0.052)	(0.053)	(0.058)	(0.091)	(0.082)	(0.102)	(0.054)	(0.050)	(0.066)	(0.094)	(0.081)	(0.106)
Habitat	0.063**	0.023	0.049*	0.094	-0.045	0.028	0.062**	-0.025	0.014	0.079	-0.058	0.024
	(0.027)	(0.033)	(0.028)	(0.060)	(0.065)	(0.061)	(0.027)	(0.031)	(0.025)	(0.060)	(0.056)	(0.056)
Wage bill												
Habitat*percentile	-0.272	0.224	-0.081	-0.584	0.870**	0.365	-0.261	0.486***	0.505**	-0.352	0.885**	0.695
	(0.175)	(0.175)	(0.192)	(0.455)	(0.401)	(0.459)	(0.187)	(0.170)	(0.225)	(0.472)	(0.363)	(0.497)
Habitat	0.296***	0.122	0.254***	0.529**	-0.061	0.298	0.292***	-0.022	0.098	0.471*	-0.086	0.218
	(0.096)	(0.115)	(0.098)	(0.267)	(0.284)	(0.278)	(0.097)	(0.106)	(0.088)	(0.265)	(0.240)	(0.242)
Observations	[17,990]	[17,990]	[17,990]	[14,279]	[14,279]	[14,279]	[17,990]	[17,990]	[17,990]	[14,279]	[14,279]	[14,279]

Table presents interaction effects between Habitat and initial value added per worker, or initial revenue. Columns interact treatment with dummies for being in the 25th, 50th, and 75th percentile of these covariates, with each pair of interaction and Habitat coefficient representing a different regression.

Table 7: Regressions on mechanisms

Dependent variables	Firm had access to a loan (=0 no, =1 yes)			Firm had a bank account (=0 no, =1 yes)			Firm used computers (=0 no, =1 yes)			Firm had access to internet (=0 no, =1 yes)		
	All	Manufacturing	Trade and Services	All	Manufacturing	Trade and Services	All	Manufacturing	Trade and Services	All	Manufacturing	Trade and Services
All existing firms in 2013 (baseline 2008)												
Habitat	0.022***	-0.012	0.027***	0.002	-0.012	0.005	0.001	-0.003	0.003	0.003**	-0.003	0.005***
	(0.008)	(0.011)	(0.009)	(0.010)	(0.012)	(0.010)	(0.005)	(0.010)	(0.005)	(0.001)	(0.004)	(0.001)
	0.112	*0.107*	*0.112*	*0.061*	*0.091*	*0.057*	*0.09*	*0.084*	*0.09*	*0.008*	*0.011*	*0.007*
Observations	[44,200]	[5,365]	[38,791]	[44,200]	[5,365]	[38,791]	[44,200]	[5,365]	[38,791]	[44,200]	[5,365]	[38,791]
All existing firms in 2018 (baseline 2008)												
Habitat	0.001	-0.003	0.001	0.011**	0.005	0.012**	-0.006	-0.024***	-0.003	-0.004	-0.022***	-0.001
	(0.006)	(0.009)	(0.006)	(0.005)	(0.011)	(0.005)	(0.006)	(0.009)	(0.006)	(0.006)	(0.008)	(0.006)
	0.103	*0.089*	*0.105*	*0.055*	*0.086*	*0.051*	*0.088*	*0.085*	*0.087*	*0.074*	*0.076*	*0.074*
Observations	[50,869]	[6,072]	[44,750]	[50,869]	[6,072]	[44,750]	[50,869]	[6,072]	[44,750]	[50,869]	[6,072]	[44,750]
Surviving firms from 2008 to 2013												
Habitat	0.022**	-0.010	0.027***	0.003	-0.017	0.006	-0.002	-0.010	0.001	0.003*	-0.008	0.005***
	(0.010)	(0.015)	(0.010)	(0.008)	(0.016)	(0.008)	(0.005)	(0.013)	(0.006)	(0.002)	(0.005)	(0.002)
	0.111	*0.104*	*0.112*	*0.062*	*0.102*	*0.055*	*0.085*	*0.092*	*0.084*	*0.009*	*0.019*	*0.007*
Observations	[20,163]	[2,565]	[17,582]	[20,163]	[2,565]	[17,582]	[20,163]	[2,565]	[17,582]	[20,163]	[2,565]	[17,582]
Surviving firms from 2008 to 2018												
Habitat	0.001	-0.015	0.004	0.016**	0.011	0.019***	0.002	-0.021	0.006	0.003	-0.017	0.008
	(0.008)	(0.015)	(0.008)	(0.007)	(0.021)	(0.007)	(0.009)	(0.014)	(0.009)	(0.008)	(0.014)	(0.009)
	0.091	*0.090*	*0.091*	*0.060*	*0.116*	*0.052*	*0.083*	*0.096*	*0.081*	*0.069*	*0.085*	*0.066*
Observations	[14,279]	[1,784]	[12,487]	[14,279]	[1,784]	[12,487]	[14,279]	[1,784]	[12,487]	[14,279]	[1,784]	[12,487]
Entrants from 2008 to 2013												
Habitat	0.022**	-0.013	0.028***	0.002	-0.008	0.005	0.006	0.004	0.008	0.004***	0.001	0.005***
	(0.009)	(0.015)	(0.009)	(0.011)	(0.014)	(0.012)	(0.006)	(0.013)	(0.007)	(0.001)	(0.003)	(0.002)
	0.112	*0.110*	*0.112*	*0.061*	*0.081*	*0.058*	*0.094*	*0.078*	*0.096*	*0.007*	*0.004*	*0.007*
Observations	[24,037]	[2,800]	[21,209]	[24,037]	[2,800]	[21,209]	[24,037]	[2,800]	[21,209]	[24,037]	[2,800]	[21,209]
Entrants from 2008 to 2018												
Habitat	0.000	0.004	-0.001	0.006	0.002	0.006	-0.005	-0.023**	-0.002	-0.006	-0.024**	-0.004
	(0.007)	(0.011)	(0.007)	(0.006)	(0.011)	(0.006)	(0.006)	(0.011)	(0.006)	(0.005)	(0.010)	(0.006)
	0.108	*0.089*	*0.111*	*0.053*	*0.073*	*0.050*	*0.089*	*0.081*	*0.090*	*0.077*	*0.072*	*0.077*
Observations	[36,590]	[4,288]	[32,263]	[36,590]	[4,288]	[32,263]	[36,590]	[4,288]	[32,263]	[36,590]	[4,288]	[32,263]

The table shows the value of coefficient β of the described in the following specification:
$y_T = \alpha + \beta * Habitat + \phi * \bar{y}_{2008}^{Polygon} + FE_{Municipality} + FE_{Polygonsize} + \epsilon$. Each coefficient denotes a different regression. Standard errors clustered by Habitat polygon shown in parenthesis. Mean values of control groups in italics. Number of observations in square brackets.

Table 8: Effects on Formality

	All				Manufacturing				Trade and Services			
	2013		2018		2013		2018		2013		2018	
	Relaxed	Strict	Relaxed	Strict	Relaxed	Strict	Relaxed	Strict	Relaxed	Strict	Relaxed	Strict
Survivors												
Habitat	0.004*	0.003***	0.007*	0.003*	0.010	0.009*	-0.028**	0.001	0.005*	0.002**	0.012***	0.003**
	(0.003)	(0.001)	(0.004)	(0.001)	(0.012)	(0.005)	(0.013)	(0.006)	(0.003)	(0.001)	(0.004)	(0.001)
	0.027	*0.002*	*0.029*	*0.003*	*0.065*	*0.002*	*0.079*	*0.008*	*0.021*	*0.002*	*0.021*	*0.002*
Observations	[20,163]	[20,163]	[14,279]	[14,279]	[2,565]	[2,565]	[1,784]	[1,784]	[17,582]	[17,582]	[12,487]	[12,487]
Entrants												
Habitat	0.005	0.000	-0.000	0.001*	-0.004	0.001	-0.011	-0.002	0.006*	0.000	0.001	0.002**
	(0.003)	(0.001)	(0.003)	(0.001)	(0.008)	(0.001)	(0.007)	(0.002)	(0.003)	(0.001)	(0.003)	(0.001)
	0.021	*0.003*	*0.022*	*0.003*	*0.039*	*0.003*	*0.042*	*0.004*	*0.019*	*0.003*	*0.019*	*0.003*
Observations	[24,037]	[24,037]	[36,590]	[36,590]	[2,800]	[2,800]	[4,288]	[4,288]	[21,209]	[21,209]	[32,263]	[32,263]

Table presents coefficients from linear probability models explaining whether a firm is ‘formal’ according to a Relaxed or Strict definition. The top panel examines only endline firms that were present in the baseline (survivors), and the bottom panel only endline firms that were not present (entrants). Standard errors clustered by Habitat polygon shown in parenthesis. Mean values of control groups in italics. Number of observations in square brackets.

Table 9: Impacts of Habitat on Residential Outcomes

	Structure of Population				Characteristics of Population		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total population	Total population: female	Total population: male	Population 18+ yrs old (% of tot pop)	Avg. years of schooling	Population 12+ yrs old employed (% of workforce)	Population 12+ yrs married (% of pop 12+)
Panel A: Levels							
Habitat	-196.396	-102.319	-104.807	-0.004	-0.064	0.000	-0.002
	(175.372)	(91.515)	(87.927)	(0.003)	(0.039)	(0.001)	(0.003)
Control mean	18268	9354	8904	0.695	9.245	0.980	0.530
Panel B: Inverse Hyperbolic Sine							
Habitat	-0.030	-0.027	-0.031	-0.046	0.000	-0.050	-0.054
	(0.030)	(0.029)	(0.030)	(0.033)	(0.007)	(0.033)	(0.034)
Control mean	8.721	8.044	8.008	8.352	2.903	8.024	7.865
Observations	367	367	367	367	367	367	367

Note: The table presents ANCOVA regressions using the population census. Panel A presents variables either in the units they were originally in the censuses (Columns 1, 2, 3 and 5) or were transformed relative to total population or a subgroup of that variable where relevant. Panel B shows those same variables transformed using the inverse hyperbolic sine transformation. Throughout, all regressions are weighted by the polygon total population in 2010 and conditioned on the value of the variable in 2010.

Table 10: Impacts of Habitat on Residential Outcomes

	Structure of Population				Characteristics of Population		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Total population	Total population: female	Total population: male	Population 18+ yrs old (% of tot pop)	Avg. years of schooling	Population 12+ yrs old employed (% of workforce)	Population 12+ yrs married (% of pop 12+)
Panel A: Levels							
Habitat	-196.396	-102.319	-104.807	-0.004	-0.064	0.000	-0.002
	(175.372)	(91.515)	(87.927)	(0.003)	(0.039)	(0.001)	(0.003)
Control mean	18268	9354	8904	0.695	9.245	0.980	0.530
Panel B: Inverse Hyperbolic Sine							
Habitat	-0.030	-0.027	-0.031	-0.046	0.000	-0.050	-0.054
	(0.030)	(0.029)	(0.030)	(0.033)	(0.007)	(0.033)	(0.034)
Control mean	8.721	8.044	8.008	8.352	2.903	8.024	7.865
Observations	367	367	367	367	367	367	367

Note: The table presents ANCOVA regressions using the population census. Panel A presents variables either in the units they were originally in the censuses (Columns 1, 2, 3 and 5) or were transformed relative to total population or a subgroup of that variable where relevant. Panel B shows those same variables transformed using the inverse hyperbolic sine transformation. Throughout, all regressions are weighted by the polygon total population in 2010 and conditioned on the value of the variable i 2010.

Table 11: Spillover Effects

Dependent variable	0m-100m All firms	0m-100m Intensive margin	100m-250m All firms	100m-250m Intensive margin	250m-500m All firms	250m-500m Intensive margin	500m-1km All firms	500m-1km Intensive margin
				2013				
Revenue	-1.414	-6.182	5.051	13.341*	-8.126	-8.423	-1.290	0.133
	(5.043)	(5.717)	(5.016)	(6.975)	(6.181)	(7.811)	(2.453)	(3.623)
	49.500	*56.150*	*52.560*	*58.480*	*70.780*	*76.080*	*66.810*	*74.450*
Capital stock	-0.097	-0.347	1.354	3.467	0.347	2.019	-1.299	-1.240
	(1.709)	(2.413)	(1.626)	(2.644)	(1.409)	(2.188)	(0.978)	(1.481)
	16.180	*19.280*	*16.250*	*19.250*	*19.320*	*22.850*	*19.040*	*22.810*
Paid workers	0.002	0.005	0.011	0.018*	-0.010	-0.010	-0.000	-0.001
	(0.007)	(0.009)	(0.009)	(0.011)	(0.008)	(0.009)	(0.005)	(0.006)
	0.149	*0.156*	*0.155*	*0.162*	*0.175*	*0.183*	*0.173*	*0.182*
Wage bill	1.302	2.371	4.842	12.664*	-5.882	-5.607	-1.351	-1.044
	(4.887)	(6.752)	(5.891)	(7.580)	(5.066)	(6.466)	(3.554)	(4.421)
	42.820	*49.180*	*45.330*	*51.760*	*58.230*	*65.780*	*57.550*	*64.730*
Observations	[44,772]	[20,781]	[71,955]	[34,404]	[117,639]	[55,593]	[236,901]	[117,517]
				2018				
Revenue	-7.219	-1.168	3.353	23.077*	-15.512	-13.609	-1.947	0.847
	(6.543)	(9.593)	(6.244)	(12.501)	(9.975)	(12.255)	(3.793)	(6.214)
	65.470	*78.530*	*75.260*	*83.930*	*97.700*	*106.400*	*96.290*	*107.000*
Capital stock	-1.093	2.769	1.195	5.124	-2.260	-2.467	-1.335	-1.456
	(1.730)	(3.188)	(1.825)	(3.550)	(2.289)	(3.602)	(1.065)	(1.790)
	16.750	*24.000*	*18.130*	*23.430*	*22.310*	*28.330*	*21.560*	*27.390*
Paid workers	0.004	0.019	0.018**	0.033**	-0.008	-0.013	-0.001	-0.003
	(0.008)	(0.014)	(0.009)	(0.015)	(0.007)	(0.010)	(0.006)	(0.008)
	0.165	*0.181*	*0.175*	*0.187*	*0.192*	*0.206*	*0.197*	*0.209*
Wage bill	4.558	14.776	12.502*	24.541*	-3.966	-9.448	-1.878	-3.187
	(6.040)	(11.396)	(6.947)	(12.667)	(5.722)	(8.319)	(4.329)	(6.354)
	54.880	*68.550*	*61.850*	*72.640*	*74.320*	*87.330*	*76.720*	*87.620*
Observations	[51,320]	[14,621]	[82,250]	[24,846]	[132,906]	[41,252]	[262,565]	[87,186]

Table uses the same firm-level ANCOVA specification as the main results, but now estimated only using untreated firms just outside of study polygons. Distance from polygon perimeter increase across columns, and the outer bands are non-inclusive of nearer bands. Column 'All firms' includes every endline firm in that band, 'Intensive margin' includes only firms present at baseline. Control group means are in italics, number of observations in hard brackets. Every coefficient is from a different regression.

Table 12: Aggregated Effects on Polygon Totals

	Revenue			Value Added			Payments to social security			Capital stock		
Firms at endline: 2013	All	Manufacturing	Trade and Services	All	Manufacturing	Trade and Services	All	Manufacturing	Trade and Services	All	Manufacturing	Trade and Services
Habitat	-57.877	18.630	-84.749	-105.510	9.074	-120.074	7.999***	0.357	7.287***	114.809	-50.751	123.014
	(228.542)	(40.758)	(212.978)	(100.116)	(20.256)	(91.702)	(2.818)	(0.362)	(2.530)	(132.512)	(32.355)	(109.327)
	8,647.735	*1,121.108*	*7,566.687*	*3,624.336*	*530.934*	*3,104.415*	*14.639*	*4.687*	*9.941*	*2,963.468*	*552.845*	*2,427.277*
Observations	[370]	[354]	[370]	[370]	[354]	[370]	[370]	[354]	[370]	[370]	[354]	[370]
Firms at endline: 2018	All	Manufacturing	Trade and Services	All	Manufacturing	Trade and Services	All	Manufacturing	Trade and Services	All	Manufacturing	Trade and Services
Habitat	255.551	78.715	174.254	41.711	57.989	-27.166	14.989***	0.634	14.238***	387.744***	-2.279	353.340***
	(363.336)	(80.825)	(352.312)	(154.382)	(40.070)	(141.414)	(4.683)	(0.720)	(4.923)	(142.277)	(29.168)	(124.542)
	13,858.476	*1,656.831*	*12,280.029*	*5,812.708*	*841.349*	*4,989.227*	*21.216*	*5.895*	*15.260*	*3,139.583*	*514.283*	*2,628.438*
Observations	[370]	[354]	[370]	[370]	[354]	[370]	[370]	[354]	[370]	[370]	[354]	[370]

Outcomes in this table are the polygon-level sum of firm-level variables, and so provide impacts on total value of outcomes at the polygon level. Every coefficient is from a different regression. Mean values of control groups in italics. Number of observations in square brackets.

Table 13: Fiscal Impacts of Program

	Impact per polygon	Total impact across all treatment polygons	Nominal annual taxation rate	Total Annual Tax Treatment Effect
Habitat Treatment Effects:				
Value Added	$41,711	$7,341,136	0.16	$1,174,582
Payments to IMSS	$14,989	$2,638,064	1	$2,638,064
Total Revenue	$255,551	$44,976,976	0.02	$899,540
			Total Annual Tax Recovery:	**$4,712,185**
			Cost of program:	$67,000,000
			Number of years required to recoup cost from business taxes alone:	**14.22**

Table presents the results of a simple accounting exercise that takes the polygon-level impacts from the prior table, scales them to represent the total impact in all treatment polygons, and then uses marginal rates on the three core taxable business outcomes to calculate fiscal recovery by the government. This number is then compared against the total cost of all treatment to provide a number of years required to pay off those costs.

Figures

Figure 1: Before-and-After Pictures of Hábitat Treatment Neighborhood 1 in Guadalajara

Figure 2: Before-and-After Pictures of Hábitat Treatment Neighborhood 2 in Guadalajara

Figure 3: Hábitat Polygons in the City of Mérida, blue=treatment, purple=control, remainder are non-study neighborhoods.

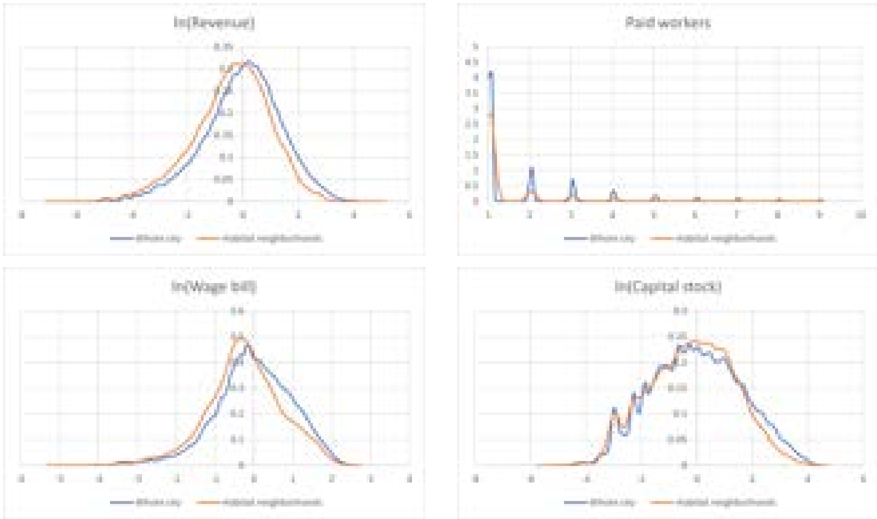

Figure 4: Descriptive densities of outcomes for firms in Hábitat study areas compared to City-wide averages.

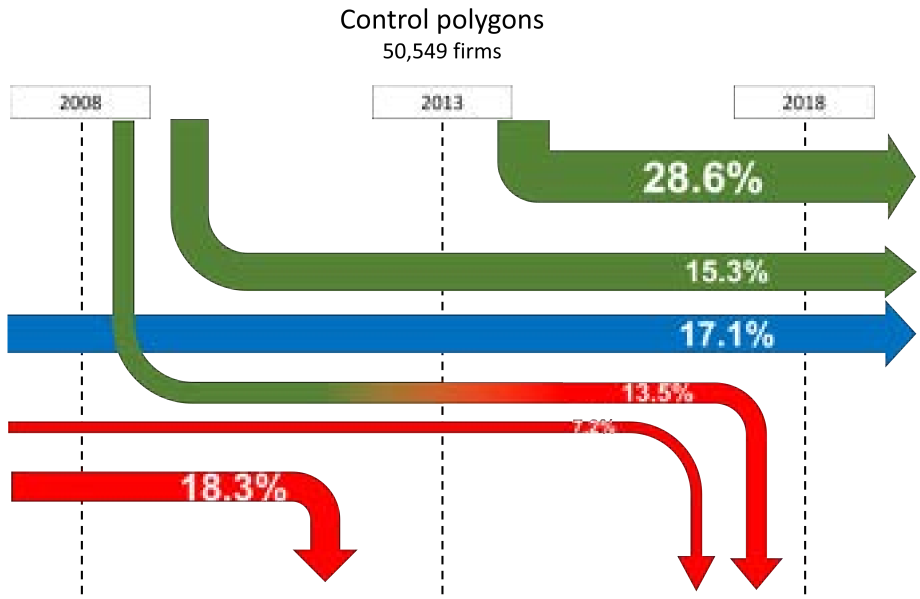

Figure 5: Dynamics of Firm Birth and Death in the Control. Firms across all three census rounds are divided into six strata based on the rounds in which they existed. Green firms are observed to be born, red firms are observed to die, and blue firms persist through all three rounds.

Appendix A Appendix Tables

Table A1: Habitat Spending

Total Investments in Treatment Polygons, 2009-2011 (US Dollars)

Name of Program (Subprogram)	2009-2011				
	Total Investment	**Federal**	**State**	**Municipal**	**Households Benefitted**
Social and Community Development	**13,922,853**	**7,026,328**	**373,053**	**6,523,472**	**256,443**
Improvement of Urban Environment:	**53,729,286**	**26,359,409**	**4,978,304**	**20,857,825**	**169,607**
Paving	**32,850,121**	**15,905,294**	**3,586,772**	**12,246,184**	43,054
Sewers	**4,877,761**	**2,465,322**	**301,398**	**2,047,825**	7,672
Drinking water	**2,644,150**	**1,320,621**	**93,867**	**1,160,632**	5,071
Community Development Centers	**2,842,852**	**1,397,310**	**198,192**	**1,160,519**	17,536
Sidewalks and medians	**2,459,935**	**1,300,516**	**260,256**	**830,340**	4,447
Public lighting	**1,752,960**	**881,141**	**30,248**	**824,506**	5,327
Trash collection	**1,801,580**	**915,702**	**63,868**	**767,860**	72,370
Total spending	**67,743,983**	**33,431,659**	**5,364,410**	**27,414,167**	**428,590**

Source: SEDESOL

Summary of the Hábitat program spending by category and level of government. Source: McIntosh et al. [2018]

Table A2: Balance

Balance tables - Firm level

	Habitat – Treatment vs Control																	
	All sectors						Manufacturing						Trade and Services					
	Treatment		Control		t-test diff (C-T)		Treatment		Control		t-test diff (C-T)		Treatment		Control		t-test diff (C-T)	
	Mean	Obs	Mean	Obs	w/o FE	Mun. FE	Mean	Obs	Mean	Obs	w/o FE	Mun. FE	Mean	Obs	Mean	Obs	w/o FE	Mun. FE
Age of firm (years)	5.7	24,189	5.2	36,141	-0.466*	-0.466	6.7	3,129	6.4	4,448	-0.289	-0.289*	5.5	20,931	5.1	31,564	-0.406	-0.406
	[0.239]	[176]	[0.142]	[194]			[0.405]	[175]	[0.265]	[193]			[0.247]	[176]	[0.146]	[194]		
Value added	95.2	14,663	93.1	21,626	-2.110	-2.110	127.2	2,025	142.2	2,752	15.016	15.016*	88.9	12,517	83.4	18,769	-5.431	-5.431
	[4.098]	[176]	[3.615]	[194]			[10.725]	[174]	[9.065]	[180]			[4.135]	[176]	[2.774]	[194]		
Revenue	265.3	14,663	257.7	21,626	-7.581	-7.581	297.9	2,025	317.1	2,752	19.283	19.283*	259.3	12,517	246.1	18,769	-13.186	-13.186
	[9.192]	[176]	[8.563]	[194]			[21.329]	[174]	[17.975]	[180]			[9.203]	[176]	[7.928]	[194]		
Overall revenue	323.6	14,663	311.9	21,626	-11.665	-11.665	417.9	2,025	459.5	2,752	41.564	41.564	305.0	12,517	282.7	18,769	-22.220	-22.220
	[13.988]	[176]	[15.416]	[194]			[37.037]	[174]	[44.406]	[180]			[13.975]	[176]	[11.794]	[194]		
Capital stock	108.4	14,663	90.8	21,626	-17.526**	-17.526	168.9	2,025	172.2	2,752	3.345	3.345	96.0	12,517	76.7	18,769	-19.304***	-19.304
	[5.639]	[176]	[6.214]	[194]			[13.753]	[174]	[15.757]	[180]			[5.447]	[176]	[5.045]	[194]		
Capital stock: machinery	15.8	14,663	15.7	21,626	-0.174	-0.174	51.2	2,025	53.7	2,752	2.459	2.459	9.2	12,517	9.1	18,769	-0.066	-0.066
	[0.870]	[176]	[1.057]	[194]			[3.861]	[174]	[3.545]	[180]			[0.669]	[176]	[0.554]	[194]		
Capital stock: computer	0.9	14,663	0.8	21,626	-0.044	-0.044	0.9	2,025	1.1	2,752	0.248	0.248	0.9	12,517	0.8	18,769	-0.099	-0.099**
	[0.067]	[176]	[0.063]	[194]			[0.145]	[174]	[0.228]	[180]			[0.067]	[176]	[0.047]	[194]		
Investment	1.8	14,663	1.9	21,626	0.032	0.032	2.6	2,025	3.0	2,752	0.361	0.361	1.7	12,517	1.6	18,769	-0.095	-0.095
	[0.279]	[176]	[0.253]	[194]			[0.369]	[174]	[0.380]	[180]			[0.297]	[176]	[0.245]	[194]		
Materials	157.6	14,663	154.0	21,626	-3.586	-3.586	141.7	2,025	148.6	2,752	6.917	6.917*	161.1	12,517	154.7	18,769	-6.395	-6.395
	[5.381]	[176]	[4.944]	[194]			[10.357]	[174]	[8.366]	[180]			[5.322]	[176]	[5.062]	[194]		
Energy expenditure	12.5	14,663	10.6	21,626	-1.884**	-1.884	28.9	2,025	26.3	2,752	-2.650	-2.650	9.3	12,517	8.0	18,769	-1.360*	-1.360
	[0.579]	[176]	[0.665]	[194]			[1.658]	[174]	[1.664]	[180]			[0.492]	[176]	[0.533]	[194]		
ue added per paid worker	63.9	14,663	64.8	21,626	0.926	0.926	56.5	2,025	60.5	2,752	4.023	4.023	64.3	12,517	63.4	18,769	-0.897	-0.897
	[2.486]	[176]	[2.315]	[194]			[3.827]	[174]	[2.857]	[180]			[2.437]	[176]	[1.740]	[194]		
Value added per worker	26.0	14,663	26.4	21,626	0.424	0.424	27.4	2,025	29.0	2,752	1.601	1.601	25.7	12,517	25.9	18,769	0.236	0.236
	[0.978]	[176]	[0.739]	[194]			[2.045]	[174]	[1.313]	[180]			[0.967]	[176]	[0.746]	[194]		
Revenue per paid worker	188.9	14,663	188.4	21,626	-0.562	-0.562	140.5	2,025	146.2	2,752	5.660	5.660	196.6	12,517	192.5	18,769	-4.128	-4.128
	[5.413]	[176]	[5.078]	[194]			[7.557]	[174]	[6.280]	[180]			[5.315]	[176]	[4.936]	[194]		
Revenue per worker	75.3	14,663	76.1	21,626	0.784	0.784	67.0	2,025	68.9	2,752	1.972	1.972	76.9	12,517	77.2	18,769	0.322	0.322
	[2.290]	[176]	[1.941]	[194]			[4.233]	[174]	[2.811]	[180]			[2.231]	[176]	[2.053]	[194]		
Capital per paid worker	70.6	14,663	57.9	21,626	-12.789**	-12.789	76.1	2,025	71.2	2,752	-4.866	-4.866	68.7	12,517	54.7	18,769	-13.955***	-13.955
	[3.560]	[176]	[3.462]	[194]			[4.720]	[174]	[4.212]	[180]			[3.697]	[176]	[3.378]	[194]		
Capital per worker	28.2	14,663	23.9	21,626	-4.300**	-4.300	34.3	2,025	33.2	2,752	-1.116	-1.116	27.0	12,517	22.4	18,769	-4.644**	-4.644
	[1.458]	[176]	[1.424]	[194]			[2.181]	[174]	[1.971]	[180]			[1.526]	[176]	[1.407]	[194]		
Workers: dependent	3.3	14,663	3.2	21,626	-0.094	-0.094	4.1	2,025	4.1	2,752	-0.008	-0.008	3.2	12,517	3.0	18,769	-0.118*	-0.118**
	[0.055]	[176]	[0.050]	[194]			[0.129]	[174]	[0.129]	[180]			[0.056]	[176]	[0.029]	[194]		
Workers	3.4	14,663	3.3	21,626	-0.115	-0.115	4.2	2,025	4.2	2,752	0.003	0.003	3.2	12,517	3.1	18,769	-0.145**	-0.145**
	[0.063]	[176]	[0.056]	[194]			[0.136]	[174]	[0.137]	[180]			[0.066]	[176]	[0.031]	[194]		
Workers: paid	1.5	14,663	1.5	21,626	-0.037	-0.037	2.2	2,025	2.2	2,752	0.033	0.033	1.4	12,517	1.3	18,769	-0.036	-0.036*
	[0.028]	[176]	[0.029]	[194]			[0.112]	[174]	[0.093]	[180]			[0.028]	[176]	[0.020]	[194]		
Workers: blue	0.4	14,663	0.4	21,626	-0.024	-0.024	1.1	2,025	1.1	2,752	0.027	0.027	0.3	12,517	0.3	18,769	-0.021	-0.021
	[0.024]	[176]	[0.026]	[194]			[0.101]	[174]	[0.085]	[180]			[0.022]	[176]	[0.018]	[194]		
Workers: white	0.0	14,663	0.0	21,626	-0.002	-0.002	0.1	2,025	0.1	2,752	0.030	0.030**	0.0	12,517	0.0	18,769	-0.006	-0.006
	[0.007]	[176]	[0.005]	[194]			[0.010]	[174]	[0.016]	[180]			[0.008]	[176]	[0.003]	[194]		
Wage bill	18.5	14,663	18.3	21,626	-0.176	-0.176	46.4	2,025	51.2	2,752	4.784	4.784	13.6	12,517	13.1	18,769	-0.472	-0.472
	[1.152]	[176]	[1.354]	[194]			[4.787]	[174]	[4.411]	[180]			[1.040]	[176]	[0.941]	[194]		
Wage bill + benefits	20.2	14,663	20.1	21,626	-0.040	-0.040	50.2	2,025	56.3	2,752	6.167	6.167	14.9	12,517	14.4	18,769	-0.510	-0.510
	[1.315]	[176]	[1.565]	[194]			[5.385]	[174]	[5.060]	[180]			[1.192]	[176]	[1.081]	[194]		
Wage bill: blue	16.9	14,663	16.8	21,626	-0.147	-0.147	43.8	2,025	47.3	2,752	3.516	3.516	12.3	12,517	12.0	18,769	-0.286	-0.286
	[1.057]	[176]	[1.215]	[194]			[4.438]	[174]	[3.847]	[180]			[0.919]	[176]	[0.859]	[194]		
Wage bill: white	1.5	14,663	1.5	21,626	-0.029	-0.029	2.6	2,025	3.9	2,752	1.268	1.268**	1.3	12,517	1.1	18,769	-0.186	-0.186
	[0.170]	[176]	[0.182]	[194]			[0.434]	[174]	[0.685]	[180]			[0.186]	[176]	[0.125]	[194]		
Wage	36.3	2,829	37.7	3,858	1.432	1.432	39.1	805	40.8	1,058	1.721	1.721*	35.1	1,991	36.5	2,769	1.339	1.339
	[0.741]	[167]	[0.541]	[184]			[1.065]	[140]	[0.786]	[160]			[0.889]	[162]	[0.645]	[174]		
Wage: blue	38.1	2,614	38.6	3,646	0.551	0.551	39.7	784	41.4	1,028	1.650	1.650	37.4	1,804	37.5	2,589	0.097	0.097
	[0.712]	[167]	[0.581]	[183]			[1.123]	[138]	[0.815]	[159]			[0.776]	[161]	[0.694]	[172]		
Wage: white	37.4	606	37.8	633	0.396	0.396	42.0	127	43.1	173	1.104	1.104	35.3	459	35.5	442	0.191	0.191
	[3.394]	[107]	[1.118]	[122]			[2.332]	[57]	[1.688]	[64]			[3.889]	[87]	[1.177]	[102]		

Standard Errors in square brackets. Number of clusters below observations in square brackets.

Table presents summary statistic by treatment arm, and tests of balance between the treatment and control. The first column of balance tests is the simple clustered comparison of means, and the second column uses the municipal FE that are implied by the research design.

Table A3: Entry and exit of firms after 2013

	All sectors		Manufacturing		Trade and Services	
	exit	entry	exit	entry	exit	entry
Habitat	-0.004	-0.005	-0.008	-0.019	-0.003	-0.003
	(0.013)	(0.010)	(0.021)	(0.018)	(0.013)	(0.010)
	0.391	*0.472*	*0.396*	*0.468*	*0.390*	*0.472*
Observations	[44,200]	[50,869]	[5,365]	[6,072]	[38,791]	[44,750]

Table presents LPM estimates on whether firms exit (odd columns) or enter (even columns) between 2013 and 2018. Because the 2013 outcome is endogenous to treatment this analysis is used simply to describe the time path of relative treatment effects. Mean values of control groups in italics. Number of observations in square brackets.

Table A4: Heterogeneity of probability of exit of firms

	Initial value added per worker						Initial revenue					
	2013			2018			2013			2018		
	25th	50th	75th	25th	50th	75th	25th	50th	75th	25th	50th	75th
Habitat*percentile	0.026*	-0.010	-0.023*	0.010	0.004	-0.006	0.007	-0.017	-0.013	-0.003	-0.001	0.008
	(0.014)	(0.011)	(0.014)	(0.012)	(0.011)	(0.012)	(0.015)	(0.012)	(0.014)	(0.012)	(0.011)	(0.013)
Habitat	0.006	0.017*	0.019**	0.008	0.008	0.012	0.012	0.021**	0.018*	0.012	0.011	0.010
	(0.009)	(0.010)	(0.009)	(0.011)	(0.011)	(0.011)	(0.010)	(0.011)	(0.009)	(0.011)	(0.011)	(0.011)
Observations	[36,109]	[36,109]	[36,109]	[36,109]	[36,109]	[36,109]	[36,109]	[36,109]	[36,109]	[36,109]	[36,109]	[36,109]

Table presents LPM estimates on whether firms present in the baseline round had exited by each endline round. Heterogeneity is tested through the interaction of treatment and a dummy for being in the Nth percentile of baseline value added and revenue, percentile varied across columns. Standard errors clustered at the polygon level are in parentheses.

Table A5: Characteristics of entrants in Habitat polygons

	All sectors		Manufacturing		Trade and Services	
	2013	2018	2013	2018	2013	2018
Revenue	0.985*	1.641	-0.164	-0.135	1.165*	1.834
	(0.581)	(1.013)	(1.138)	(1.238)	(0.593)	(1.139)
	13.925	*21.023*	*16.785*	*22.651*	*13.536*	*20.786*
Capital stock	0.529	0.806**	0.874	0.312	0.544	0.885**
	(0.416)	(0.369)	(0.917)	(0.547)	(0.407)	(0.393)
	5.403	*5.268*	*8.322*	*6.592*	*5.004*	*5.075*
Paid workers	0.087*	0.049	0.135*	-0.026	0.080***	0.057*
	(0.030)	(0.031)	(0.082)	(0.082)	(0.029)	(0.032)
	1.339	1.520	1.798	2.085	1.277	1.442
Wage bill	0.227**	0.210*	0.328	-0.131	0.209**	0.250**
	(0.097)	(0.117)	(0.293)	(0.310)	(0.092)	(0.118)
	0.949	*1.634*	*2.525*	*3.617*	*0.738*	*1.363*
Observations	[24,037]	[36,590]	[2,800]	[4,288]	[21,209]	[32,263]

Table is estimated only among firms that newly entered in each of the endline rounds, using a treatment dummy to examine differences between attributes of entrants. Every coefficient is from a different regression. Mean values of control groups in italics. Number of observations in square brackets.

Table A6: Credit Impacts for Businesses that Do and Do Not Own Land

	Did you get a credit, loan or financing for the firm's operation?		What is the source of the credit, loan or financing?				Uses of credit, loan or financing received					
			Banks		Savings bank		Equipment or expansion		Acquisition of building or vehicle		Input acquisition (merchandise, materials, raw materials, etc.)	
	Not owner	Owner	Not owner	Owner	Not owner	Owner	Not owner	Owner	Not owner	Owner	Not owner	Owner
All firms in 2013												
Habitat	0.025***	0.024**	0.009*	0.013**	0.013***	0.003	0.014**	0.022***	0.002**	-0.001**	0.012**	0.005
	(0.009)	(0.011)	(0.005)	(0.006)	(0.004)	(0.006)	(0.006)	(0.008)	(0.001)	(0.001)	(0.006)	(0.007)
	0.0993	*0.131*	*0.0332*	*0.0449*	*0.0205*	*0.0316*	*0.0326*	*0.0433*	*0.000928*	*0.00261*	*0.0318*	*0.0517*
Observations	[22,577]	[16,336]	[22,577]	[16,336]	[22,577]	[16,336]	[22,577]	[16,336]	[22,577]	[16,336]	[22,577]	[16,336
All firms in 2018												
Habitat	-0.002	0.004	0.002	0.003	-0.001	-0.005	-0.004	-0.011**	-0.000	0.000	-0.004	0.000
	(0.007)	(0.009)	(0.004)	(0.005)	(0.003)	(0.005)	(0.003)	(0.005)	(0.001)	(0.001)	(0.004)	(0.007)
	0.0981	*0.119*	*0.0349*	*0.0476*	*0.0206*	*0.0325*	*0.0258*	*0.0348*	*0.00153*	*0.00207*	*0.0520*	*0.0649*
Observations	[28,464]	[16,439]	[28,464]	[16,439]	[28,464]	[16,439]	[28,464]	[16,439]	[28,464]	[16,439]	[28,464]	[16,439

Table analyzes experimental impacts on financial impacts at the firm level, splitting the sample according to whether firms did or did not own the land on which the business operated at baseline. Every coefficient is from a different regression. Mean values of control groups in italics. Number of observations in square brackets.

Table A7: Heterogeneity by Market Access

Sample: All firms in 2018 (baseline 2008)	Revenue		Capital stock		Wage bill		Value added		Capital per worker	
Habitat	2.076**	2.070**	1.067***	1.116***	0.198**	0.210**	0.872**	0.843**	0.425*	0.438*
	(0.905)	(0.955)	(0.403)	(0.405)	(0.101)	(0.104)	(0.405)	(0.418)	(0.240)	(0.255)
Habitat*wealth_adj_street_z		0.044		0.554		0.245**		-0.123		-0.091
		(0.766)		(0.361)		(0.106)		(0.348)		(0.252)
Habitat*pobtot_adj_street_z	0.602		0.197		0.186*		0.089		-0.223	
	(0.777)		(0.341)		(0.104)		(0.330)		(0.240)	
Control mean	*23.46*	*23.46*	*6.225*	*6.225*	*1.782*	*1.782*	*9.699*	*9.699*	*3.904*	*3.904*
Observations	50,869	50,869	50,869	50,869	50,869	50,869	50,869	50,869	50,869	50,869

Table examines heterogeneity in overall program impacts by two different measures of market access. The first is the inverse distance-weighted population at the centroid of each neighborhood. The second uses population above the poverty line instead of total population.

Table A8: Spillovers by Market Access

	Revenue Trade and Services			Capital stock Trade and Services			Paid workers Trade and Services			Wage bill Trade and Services			Value added per worker Trade and Services		
	Orig	Pop dens	Wealth	Orig	Pop dens	Wealth	Orig	Pop dens	Wealth	Orig	Pop dens	Wealth	Orig	Pop dens	Wealth
Habitat	-1.947	-1.428	-2.325	-1.335	-0.512	-0.911	-0.001	0.003	0.001	-1.878	1.329	-0.397	0.232	0.522	0.285
	(3.793)	(4.119)	(3.777)	(1.065)	(1.148)	(1.026)	(0.006)	(0.006)	(0.005)	(4.329)	(4.094)	(3.954)	(0.820)	(0.875)	(0.825)
% area of manzana exp. to Habitat	-36.446***	-36.718***	-36.236***	-9.506***	-9.909***	-9.717***	-0.040***	-0.042***	-0.041***	-29.181***	-30.772***	-29.931***	-6.632***	-6.778***	-6.659***
	(6.114)	(6.177)	(6.124)	(1.335)	(1.418)	(1.386)	(0.006)	(0.007)	(0.006)	(4.723)	(4.750)	(4.726)	(1.091)	(1.121)	(1.104)
Habitat*pop density		-0.963			-1.380			-0.006			-5.576			-0.492	
		(1.851)			(0.962)			(0.005)			(3.507)			(0.424)	
Pop density mkt access		-0.076			0.863			0.003			2.001			0.257	
		(4.717)			(1.600)			(0.008)			(5.857)			(0.862)	
Habitat*wealth			1.467			-1.678			-0.006			-6.195			-0.147
			(2.516)			(1.142)			(0.006)			(4.363)			(0.520)
Wealth mkt access			-1.876			1.719			0.007			3.926			0.609
			(6.074)			(1.887)			(0.010)			(6.801)			(1.054)
Observations	262,565	262,565	262,565	262,565	262,565	262,565	262,565	262,565	262,565	262,565	262,565	262,565	262,565	262,565	262,565
16.2	16.9	16.10	16.11	16.18	16.19	16.20	16.27	16.28	16.29	16.36	16.37	16.38	16.45	16.46	16.47

Table examines heterogeneity in the spillover effect of Habitat on untreated firms in adjacent neighborhoods up to 1 km away. Regression includes the (endogenous) fraction of the neighborhood eligible for Habitat, whether adjacent to a treatment polygon, and interactions of treatment with our two measures of market access.

Appendix B Technical Appendix

This section describes with detail the steps taken in the gathering, cleaning and matching of the Hábitat and Economic Census data. The process required several adjustments to make data compatible across the three censuses as well as with the geospatial data used in both sources. Most of the procedures done are straightforward (finding compatiable variables across sources, deflating money variables, linking Hábitat polygons with the firms located within them, etc.) but we consider that they require a further description. This section is organized as following: .....

Hábitat database

As broadly described in the data section, the Hábitat database contains detailed geospatial information of the blocks, called *manzanas*, included in the study. The Hábitat study relies on INEGI's identification system of blocks, which in most part is standardized across the Agency's different projects. This makes fairly simple to cross data of different projects. At the same time, each block is identified to the polygon it belongs within the project and its corresponding treatment/control status.

The Habitat data contains substantial richness; it is possible to observe the exact type, amount, and location of each infrastructure upgrade a polygon received and on which year it occurred (2009, 2010 or 2011). Because the actual investments made in a given location were endogenous (both to the decisions of the Hábitat engineering team and to the community-driven selection process) we largely abstract away from this and analyze the treatment with a simple binary indicator.

Economic census database

The economic census microdata provided by INEGI comprises the events held in 2008, 2013 and 2018. The census has a very high response rate, above 98% of all firms surveyed. The timing of these censuses is remarkably fortuitous for a study of Hábitat, given that the first interval allows us to conduct a before-after analysis of the short-term impacts of the program on the private sector, and the 2018 wave allows us to examine impacts 7 years after the cessation of investment.

The census covers all businesses in Mexico that have a fixed location (including informal businesses) and belong to the manufacturing, services or construction sectors. The censuses have a very high response rate (more than 98%), given that firms are required by law to respond, and INEGI has the mandate to make individual firm data confidential.

INEGI uses unique identifiers for each business surveyed. Thus, if a firm appears in two or more censuses, it is possible to link the data collected and create a panel. That is, it is possible to follow firms through the censuses and also identify firm created and destruction.

Another characteristic of the census database is that firms also contain detailed information regarding their geographical location. Thus, firms can be tracked to the block they are located within a city. This is crucial, as this geospatial information makes possible to cross this database with the Hábitat database and identify those firms contained within Hábitat polygons. The variables used for analysis are: firm revenue, capital stock, paid workers and wage bill. Additional variables are used to try to understand mechanisms under the Hábitat program relates to firms' performance.

We are able to locate 84,119 firms within Hábitat polygons. Given that there are slightly more control polygons, the majority of businesses are located in such polygons (roughly 60% of firms). In terms of sectors, the vast majority of businesses belong to commerce and services (over 90% of total). This is consistent with the sectoral composition of firms across the country. Within this group, most are grocery stores (around 25% of total), and stationer's shops and beauty salons (approx. 4% each). Manufacturing firms tend to be concentrated in activities related to the production of food and beverages and varied activities related to construction and housing. Around 3% of firms produce corn tortillas and 1% are bakeries. Ironworks, furnishing and milling activities businesses comprise close to 1% of the total each.

In terms of size, most of firms located in Hábitat polygons are microbusinesses. The median of paid workers is 1, which means that the typical firm only "employs" the owner of the firm. However, there are some firms that employ up to 50 workers. In line with the nature of microbusinesses, most firms have rather small yearly revenues (a typical firm makes US$ 15,600) and limited assets (less than US$ 2,500 for the typical firm).