Paving Streets for the Poor: Experimental Analysis of Infrastructure Effects*

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Abstract

This study is the first providing experimental evidence on the role of infrastructure in reducing poverty for the urban poor. We do so by means of a first-time street asphalting randomized experiment. Within two years of the intervention, households whose streets were finally paved, and were present both before and after its implementation, increased their consumption of durable goods and acquired more motor vehicles. These impacts were driven in part by street pavement boosting housing wealth, which fueled a rise in collateralized credit use, but also by an increase in the marginal utility of vehicles. A cost-benefit analysis indicates that the economic returns to street pavement in this developing urban context are at least as large as the construction costs.

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1 Introduction

Two concomitant facts about the developing world merit attention. The first is that urbanization is proceeding rapidly in many developing countries (Henderson, 2002). The second and more important fact is that the urban poor throughout the developing world do not benefit from basic urban equipment such as piped water, electricity, sewerage lines, and asphalted roads (UN-Habitat, 2003). While several randomized evaluations on the impacts of private goods such as smokeless cook stoves on the lives of the poor have been (and are being) conducted (Hanna, Duflo and Greenstone, 2012), not much is known about the effects of public goods such as infrastructure (Banerjee and Duflo, 2011). This is not surprising given the challenges such an evaluation entails.[1] Our aim in this paper is to fill this gap.

In pursuing that endeavor, we must acknowledge the two main difficulties in evaluating the effects of public infrastructure on the lives of the poor. Infrastructure allocation normally occurs in places that provide the highest returns, either political or economic (e.g., Duflo and Pande, 2007; Joanis, 2011). Hence, any promising attempt to measure its impacts requires the use of a credible source of exogenous variation in its provision. Not only that, but we need data before and after the provision of infrastructure to distinguish genuine improvements in living conditions of individuals benefitted by infrastructure from neighborhood recomposition effects, in which families leave their homes or new neighbors arrive in response to the local public good (Tiebout, 1956).

We overcome these two obstacles by combining a randomized infrastructure experiment (provision of first-time asphalting of streets in inhabited residential neighborhoods) with the collection of data from a dedicated survey before and after the intervention.[2] This allows us to provide an answer to the question: "Does the provision of infrastructure improve standards of living for the poor in the developing world?"

[1] While there is an extensive literature assessing the effects of infrastructure on economic outcomes, both at the macro (Haughwout, 2002; Donaldson, 2010; Duranton and Turner, 2010) and micro level (Van de Walle, 2002; Davis 2011, Dinkelman, 2011; Duflo and Pande, 2007), none of the previous studies provides experimental evidence.

[2] Asphalting of streets is also known as road surface or pavement.

The study takes place in Acayucan (Mexico), where the city expands its pavement grid over time via "street asphalting projects", each defined as a contiguous set of unpaved street segments connecting to the existing pavement grid. Figures 1 and 2 illustrate the intervention under analysis. From the public works office's set of 56 candidate street asphalting projects, we randomly selected half of them to be treated with pavement using simple randomization.[3] We run a baseline survey in 2006, and a follow-up survey in 2009, with pavement placed in the intervening period.

We find that pavement had positive effects on households' acquisition of durable goods, motor vehicle ownership, home improvements, and collateral credit use. The number of durable goods owned by the household increased by 12%, motor vehicle ownership augmented by more than 40%, and home improvements doubled. Collateral-based credit use from financial institutions more than doubled, and loan size per adult went from virtually zero to 1,643 pesos (200 U.S. dollars at 2009 PPP exchange rate), equivalent to 2 months per capita expenditure. All these changes point to a substantial reduction in material poverty.

Our evidence does not show either effects on transportation costs or labor market outcomes, rather it seems that increases in durable consumption and vehicle ownership were generated in part by street pavement boosting housing wealth, which fueled a rise in collateral credit use. Indeed, not only do homeowners represent 95% of the households in our sample, but all the available indicators of property values reveal sizeable gains. Using professional appraisals measures, we find that residential properties abutting paved streets increased in value by 16%, with a corresponding gain in land value of 54%. The estimated effect of street pavement on housing values was a 25%-increment when using homeowners' estimates. Two other pieces of market-value information point in the same direction: Rents raised by 31% in paved streets; and for the houses that were purchased between 2006 and 2009 the price paid was 85% higher on paved streets, although the coefficient on this last measure is not statistically significant on account of the limited number of transactions.

[3] We assigned half to intent-to-treatment and half to control using simple randomization by means of a random number generator function in MS Excel.

While it is certainly true that the impacts of pavement on household outcomes are consistent with a wealth effect, the rise in motor vehicle acquisition can also be explained by complementarities with street pavement. When streets are paved, vehicle access and parking is facilitated, meaning that the marginal utility of a vehicle increases. Using a parsimonious model in which a representative household derives utility from (non-durable) consumption, housing services, and vehicle services, we obtain three straightforward predictions about how the consumption ratios of the three goods depend on the complementarity between pavement and vehicles. In particular, the ratio between housing services and non-durable consumption should not be affected by pavement, while the vehicle to housing and vehicle to non-durable consumption ratios should increase as long as the marginal utility of vehicles rises with pavement. These testable implications are taken to the data and confirmed. In other words, the changes in consumption are consistent with wealth effects, but the disproportionate effect on vehicle acquisition points to pavement strongly raising the marginal utility of motor vehicles.

Finally, using the most conservative estimate of land value improvements, we perform a cost-benefit analysis by summing up the increases in land values of plots along paved streets. The comparison of benefits to construction costs reveals that street paving had a benefit to cost ratio of 1.09.

The structure of paper is as follows. Section 2 describes the experimental design. Section 3 explains the empirical strategy. Section 4 contains the analysis of the experimental effects, while Section 5 explores why households changed their consumption in response to pavement provision. Section 6 reports a cost-benefit analysis, and Section 7 concludes.

2 Experimental Design

2.1 Institutional Context

Acayucan is one of Mexico's 56 metropolitan areas encompassing three municipalities with a combined population of 105,000 (INEGI, 2007). The city has a central core where most streets have been paved, and outer sections where street pavement is gradually rolled out. Residences are built and inhabited long before streets are paved, as shown in Figure 1. This situation is common throughout Mexico and other Latin American countries (Fernandes, 2011), suggesting that the results from our analysis are potentially relevant for many other countries.

Municipal governments in Mexico are responsible for most of the elements of their urban infrastructure. Each three-year administration has ample leeway as to budgetary allocations. The municipal budget consists mainly of transfers from general funds obtained from the federal value-added tax, the federal income tax, and oil revenues. Less than 10% of the municipal budget derives from local taxes (consisting of the property tax and business-permit fees). Property-tax receipts, especially in small cities, play a less significant role in Mexico than they do in the U.S. Cadastral property valuations are very low and rarely updated.

2.2 The Experiment

The intervention consists of first-time asphalting of residential non-arterial streets, varying in width from 8 to 15 meters, and allowing for two lanes of vehicular traffic and one or two lanes for parking. The pavement material used is either hot-mix asphalt concrete or portland cement reinforced concrete. Like most infrastructure, the lion's share of costs are borne initially: the transportation literature estimates annual cost of maintenance to be only 1.5% of construction costs (BITRE, 1978), or 0.3%-0.7% using the cost estimates in Chen, Lin and Luo (2003). After a street is paved, maintenance is a municipal responsibility and is funded from general revenues.

Street pavement in an urban context provides multiple services: it facilitates vehicle, pedestrian and cyclist movement and access, provides accessible space for vehicle parking, allows commercial vehicles to deliver goods, and has a significant impact on the visual appearance of the area. Moreover, fieldwork confirmed that congestion was not a concern

– as expected given the residential nature of the streets. A valid question is then why the market does not provide street pavement to begin with. One reason is that residential street pavement is a pure public good (non-rivalrous and non-excludable), and hence, free rider incentives prevent private provision.

The government of Acayucan faced budget and temporal constraints that would not allow it to pave all streets that were deemed suitable candidates. In fact, the public works office had a set of 56 independent street pavement project candidates located throughout the city: contiguous unpaved street segments that connected with the existing city pavement grid and with relatively high population densities. These pavement projects ranged from 300 to 1,200 meters in length.

Given that the administration could afford to pay for only 28 of the 56 projects, the mayor and the city council reasoned that it would be in everybody's interest (not only for a third party) to evaluate the paving program, but also for the same third party to select, at random, the 28 streets to be paved. We assigned 28 streets to intent-to-treatment and 28 to control using simple randomization by means of a random number generator function in MS Excel. Figure 3 shows the location of those streets assigned to the intent-to-treat group ($Z = 1$) and those assigned to the control group ($Z = 0$).

It is important to bear in mind that every municipal administration in Acayucan allocates a portion of its budget to street paving, but the municipality did not announce to the population the list of experimental street projects. In other words, the selection was not legally binding in any way that could be announced to the population, but rather served as an internal guideline in the annual budgeting process. While this eliminates potential biases from anticipation effects in the housing values at baseline, people living in an intent-to-treat street could have learned they were part of the intent-to-treat group with the arrival of measurement teams, construction crews, and machinery. We will investigate below whether anticipation effects are present among those units assigned to treatment but finally unpaved. Finally, streets not selected for pavement did not receive any form of compensation. Indeed, the pavement program was not accompanied by any other government intervention.

By February 2009, right before our follow-up survey, 17 of the streets in the treatment group had been completely paved, and the other 11 were under way (the municipal government attributed the delays to foul weather and various technical difficulties).[4] However, and most importantly, the administration did fulfill the requirement of not paving those streets assigned to the control group.

2.3 Data Sources

The data for this study come from pre- and post-intervention rounds of a dedicated household survey (the Acayucan Standards of Living Survey, ASLS) and professional appraisals of residential-property values.[5] Importantly for our purposes, the part of the ASLS questionnaire focused on consumption and income is very detailed, following the Mexico National Survey of Household Income and Expenditure (ENIGH, Encuesta Nacional de Ingresos y Gastos de los Hogares). The baseline survey was fielded in February-March 2006, and the follow-up survey was fielded in February-March 2009. Professional appraisals were performed immediately after the survey work in each round.[6]

The target population of the survey consisted of all occupied residential structures on the streets that were selected for the experiment.[7] The baseline survey was administered to 1,231 households living in 1,193 dwellings, with a response-rate of 94%.[8] In 2009, 1,083

[4]All the 11 projects were visited by the measurement teams. In addition, in two of them, construction crews started their tasks.

[5] A full description of the ASLS can be found in Gonzalez-Navarro and Quintana-Domeque (2010).

[6] A very short business census was applied to all business units with their main entrance on the street project in 2006 and 2009. Mobile business units were excluded from analysis (for example, a seller on a motorcycle, or a water distributor going around on a truck). The supervisors of each survey team were in charge of locating all business units on their street project and administering the questionnaire. The questions included the type of business, years of operation, employees, total sales, expenditures, changes in profits, and whether the business unit is located in a house, a special purpose commercial locale, or on the street. Results based on these data (available upon request) do not reflect impacts on businesses or their characteristics.

[7] We created a sampling frame from all inhabited residential dwellings in January 2006. As Deaton (1997) recognizes, the use of outdated or otherwise inaccurate sampling frames is an important source of error in survey estimates. The sampling procedure was clustered sampling: From the list of dwellings in each cluster we chose at random a specified fraction to be interviewed.

[8] Some dwellings contained more than one household (defined as a group of one or more persons living in

households were interviewed. In 900 cases we found the same household that we had interviewed in 2006, and in 156 cases we found that a new household was in residence. In order to assess neighborhood *recomposition* occurring on account of newcomers moving into new constructions, all families living in residences built between baseline and follow-up were also interviewed (N=27). Table A1 details survey response rates.[9]

The household questionnaire collects detailed information for each individual in the household and characteristics at the household level. In over 95% of the cases household and individual questions were answered by a reference person who was thus targeted because he or she was either the household head or the spouse/partner of the head.

We did not inform participants in the study (household respondents and the professional appraiser) about the ultimate objective of the survey/appraisals.[10] We also trained field workers not to mention the phrase "street pavement" to respondents. Thus, any behavioral bias among the treatment group (Hawthorne effects) and among the control group (John Henry effects) was minimized.

2.3.1 Consumption and Credit Measures

Consumption of non-durable goods is measured by monthly household per capita expenditure. We have two measures of per capita expenditure: one indirect measure, based on expenditures on eight major items (food, phone, gas, electricity, education, rent or mortgage, clothes, and entertainment), and one direct measure, based on total reported expenditures. Durable goods consumption is measured using two indices: one of vehicle ownership (the sum of automobile, truck, and motorcycle binary indicators) and one of household durables

the same house and sharing food expenditures). The procedure in the case of such multiple households was to interview all of them. It is worth noting that neither quota sampling nor substitution of non-responding households or individuals (whether refusals or non-contacts) was permitted at any stage.

[9] We determined that there was a risk that not all of the streets selected for treatment would in fact be treated by the time of the follow-up survey. Indeed, as we have seen in the previous subsection, there were 11 such cases. In order to maximize the power of our tests, sampling was done with a higher intensity in the intent-to-treat group (List, Sadoff, and Wagner, 2011). We sampled at a rate of 70% in the intent-to-treat (ITT) group and at a rate of 50% in the control group.

[10] The data collected for this study underwent the approval process of the Institutional Review Panel at Princeton University (Research Protocol 3104).

(the sum of refrigerator, washing machine, microwave oven, air conditioning, video player, and computer binary indicators).

The ASLS asks for credit use at the individual level for all adults. Credit use and loan size are available for collateral-based credit (composed of mortgages, home-equity lines, and collateralized bank loans); non-collateralized credit (composed of appliance and furniture store credit, bank-card credit, vehicle loans, and *casas de crédito popular* loans); credit from informal lenders, credit from family and friends, and credit from government entities. In addition, the ASLS asks whether anyone in the household has a bank account (checking, direct deposit, or savings).

2.3.2 Measuring Property Values

The main challenge in assessing changes in property value occurring in small geographical areas over a short time span is the paucity of transactions. Moreover, in the case of a developing country, transactions registered in the state property registry are unreliable indicators of transaction prices, since the term often used is gift, donation, or inheritance, in order to reduce registration fees. Even for properties registered as having been sold, in many cases there is a substantial delay between the date of the transaction and the date of registry. It is often the case that an individual buys and moves into a house and only later pays the registration fee. In the U.S., in contrast, property registries are the main data source for home-price indices (Case and Shiller, 1987). Again, however, these registries are useful for assessing changes in property values only over large geographical areas, such as entire cities.

To compensate for the small number of transactions to be expected in our setting, we obtained two independent measures of property value: professional appraisals and homeowner valuations. The fact that professional appraisals are used by banks to determine property values, and hence the size of mortgages, indicates that they are a reliable source of market valuation. In our case, we used the services of a professional appraiser contracted by local banks who is also a real-estate agent in the city.[11] Each appraisal consisted of a visit by the expert to the property and a careful evaluation of the approximate sale price of the property. Appraisers define market value as "The most probable price, as of a specified date, in cash, for which the specified property rights should sell after reasonable exposure in a competitive market under all conditions requisite to fair sale, with the buyer and seller each acting prudently, knowledgeably, and for self-interest, and assuming that neither is under undue duress." We obtained professional appraisals of residential property (and land) value for half of the successfully interviewed households (578 properties), on account of budgetary constraints.[12]

The second main source of property valuation we obtained were homeowners' self-reports. There is an established literature in economics using self-reported home values (see Davis (2011) for a recent example). The reliability of self-reported home values has long been assessed in the housing economics literature (in developed countries see Kish and Lansing, 1954; Kain and Quigley, 1972; Goodman and Ittner, 1992; Kiel and Zabel, 1999; Bucks and Pence, 2006; Banzhaf and Farooque, 2012; in developing countries see Jimenez, 1982). This literature concludes that the evolution of self-reported housing values generally mimics that of actual prices. In the ASLS, we asked: "Approximately how much money do you think this house could sell for nowadays?" For the 2006 ASLS sample, Gonzalez-Navarro and Quintana-Domeque (2009) show that owners overestimate the value of their homes relative to the appraiser but that the bias is explained by long tenure: short-tenured homeowners provide value assessments that are on average the same as professional appraisals.[13] This suggests the professional appraiser's valuation is a better indicator of market value.

Finally, to circumvent the drawbacks of our previous measures due to homeowner self-reported biases (e.g., priming effects) and potentially mechanical assessor models for valuing

[11] We used the services of the same agent in 2006 and 2009 in order to minimize heterogeneity of assessment practices.

[12] The appraiser did not enter the properties because piloting revealed that non-participation would be extremely high, which would have compromised our entire evaluation.

[13] In the 2009 sample the mean difference between log appraised value and log homeowner valuation is -0.39 for the whole sample, and only -0.04 for short-tenured homeowners (≤ 5 years living in the dwelling).

properties, we complement homeowners' self-reports and assessor valuations with data on transaction prices paid by recent buyers and housing rental rates. Data on recent transactions are obtained by asking recent buyers – those arriving between baseline and follow-up – how much they paid for the property. Housing rental rates are obtained by asking renters how much they pay per month in rent. This last measure has the advantage of overcoming the worry regarding the forward-looking behavior of house price data, i.e., that the control group was effectively treated (at least with some non-zero probability, and some discounting for the fact that the treatment is delayed) too.

2.3.3 Other Measures

The ASLS also contains information on labor supply (households respondents are asked, for instance, to specify the number of hours each working adult work per day/per week), transportation costs (the time it takes to go to the city center using the habitual means of transport and the price of a taxi from the home to the city center), health (self-reported symptoms of poor health among household members over the previous year), and children's schooling (school enrollment and absenteeism).

3 Empirical Strategy

3.1 Baseline Balance, Reduced-Form and 2SLS estimates

In line with the established impact-evaluation literature (e.g., Kling, Liebman, and Katz, 2007), we present reduced-form (intent-to-treat, ITT) and two-stage least squares (2SLS) estimates. The first thing we need to check, however, is whether randomization worked as intended, i.e., balancing pre-treatment characteristics across the intent-to-treat ($ITT = 1$) and the control ($ITT = 0$) groups. This is precisely the purpose of Table 1, which reports balance tests for the main outcome variables.

Note that we present balance tests for variables with different units of observation:

dwellings, households, and individuals (sometimes partitioned into adults and children). For this reason, the number of observations can vary substantially from variable to variable. We indicate with the symbol ⓘ whether the variable refers to an individual or not, and are clear whenever we refer to adults or children. The stayer sample has 900 households, 1,351 children, 2,362 adults, and 898 dwellings.

The table shows that randomization was successful in balancing pre-treatment characteristics across the intent-to-treat and the control groups.[14] We assess a total of 53 variables and find evidence of balanced characteristics across the groups (see Table A2). Only two variables are significantly different across the groups: labor income and non-collateral based credit amount, both at the 10% significance level.

We then proceed to estimate:

$$Y_{2009} = \alpha_0 + \alpha_1 Z + \alpha_2 Y_{2006} + \epsilon_1 \tag{1}$$

where Y_{2009} is the outcome of interest in 2009, Z is the intent-to-treat indicator, and Y_{2006} is the outcome at baseline, included to improve precision. Hence, unless otherwise indicated, all of our estimates use the two rounds and not only the follow-up cross-section. The ITT parameter is α_1 in equation (1). We cluster standard errors at the street-pavement-project level (56 clusters) and use survey weights.[15]

We also present 2SLS estimates using pavement group assignment as an instrumental variable for the street being paved, so Z is the excluded instrument for an indicator D of

[14] An alternative test of equality of means is a two sample t-test with unequal variances between groups using Welch's (1947) approximation. This alternative provides a solution to the Fisher-Behrens problem of testing the significance of the difference between the means of two normal populations with different variances. The standard errors using this alternative test were very similar to the regression based standard errors, so we follow usual practice. See Deaton (2009) for further discussion.

[15] Survey weights (or expansion factors) represent the inverse of the probability that a dwelling or household is included in the sample. In constructing them, the survey firm took into account the proportion of households selected for participation in each cluster and cluster-specific non-response. The use of weights is immaterial for all of the results because unit non-response was extremely low and uncorrelated to treatment.

being paved in the equation:

$$Y_{2009} = \beta_0 + \beta_1 D + \beta_2 Y_{2006} + \epsilon_2 \tag{2}$$

The parameter β_1 in equation (2) is the ITT parameter divided by the regression-adjusted compliance rate (the fraction of units that were finally paved among those originally selected to be paved), and can be interpreted as the TOT parameter under the following three conditions:

C1: One-sided non-compliance (Bloom, 1984; Angrist, Imbens, and Rubin, 1996): only units assigned to receive street pavement ($Z = 1$) can potentially end up being unpaved ($D = 0$); all units assigned to the control ($Z = 0$) do comply with not being finally paved ($D = 0$).

C2: Absence of anticipation effects: no average effect of pavement-group assignment ($Z = 1$) on those units in streets assigned to be paved that were not finally paved ($Z = 1$, $D = 0$).

C3: Absence of indirect treatment effects: no average effect of paving on units in the control group ($Z = 0$).

3.2 Interpreting 2SLS estimates as TOT effects

In the experiment at hand, condition **C1** is clearly satisfied: while some streets assigned to the treatment group were not paved (i.e., $0 < Pr(D_i = 1|Z_i = 1) < 1$), all the streets assigned to the control group remained unpaved (i.e., $Pr(D_i = 0|Z_i = 0) = 1$).

To assess condition **C2**, whether anticipation effects are negligible when estimating the effects of street pavement, the most natural outcome to look at is home value, which is likely to adjust in anticipation of street pavement provision. For example, McMillen and McDonald (2004) detect a house price adjustment, in anticipation of the opening of a new transit line in Chicago, up to 6 years before the actual opening of the line, which coincided with the *announcement* of the route for the proposed transit line. Here, a potential concern (although there was no announcement) is that non-compliers, i.e., people living along the ITT streets that were not finally paved, learned that their street had been selected –by observing the arrival of measurement teams– and modified their estimates of property value in 2009. In that case, non-compliers would be affected by owning a house on a street selected for paving in the near future, and "being in a selected project" could not be used as an instrument for "being paved". The appraiser could have updated his estimations of such properties as well. Note, however, that rents have the advantage of overcoming the worry regarding the forward-looking behavior of house price data.

We examine the presence of anticipation effects on housing values by focusing on the subsample of households living in streets assigned to pavement but finally unpaved ($Z = 1$, $D = 0$) and those living in streets assigned to the control group ($Z = 0$). We run a regression of home value in 2009 on an indicator variable that takes the value 1 if ($Z = 1$, $D = 0$), and 0 if ($Z = 0$), controlling for the home value in 2006. Table 2 shows that there is no change in home values for those homes in the intent-to-treat group that were not finally paved, suggesting that the expectations of non-compliers regarding home values did not change. This evidence supports condition **C2**.

Finally, we need to assess condition **C3**, whether paving has an effect on units in the control group. These indirect effects may operate mainly through property market sorting and connectivity. In the first case, paving a street can make "undesirable" neighbors concentrate more heavily in untreated areas, so there could be negative indirect effects on the property prices of the control group. Regarding connectivity, it is important to acknowledge that distance to the nearest paved street decreased for the control group, and this may affect the price for houses in the control group.

To investigate the potential effects of paving on property market sorting, i.e., whether changes in the desirability and price of treated properties affect the market price of control properties through neighborhood recomposition, we investigate whether the intervention under analysis affected either the rate or composition of movers in/out of the paved neighborhood. Column 1 in the top panel of Table 3 shows that out-migration is uncorrelated with intent-to-treat. Similarly, column 2 indicates that out-migration is unrelated to pavement status.[16] The bottom panel in Table 3 shows no statistically significant differences in the means of per capita expenditure, durable goods, and vehicle ownership between out-migrants from control streets and those from paved streets.[17] Regarding immigration flows, in 18% of the 2006 dwelling sample we found a new family in 2009.[18] The top panel in Table 4 shows that the likelihood of new households arriving to the experimental streets between 2006 and 2009 is not affected by either intent-to-treatment or pavement status of the street. In addition, the lower panel in the table shows that there is no statistically significant difference in average (socioeconomic) characteristics of immigrants to assignment-to-treatment (or paved) streets and those arriving to control (or unpaved) streets. Hence, the evidence reported in Tables 3 and 4 fails to support that paving affected the rate or composition of movers in/out of the paved neighborhood.[19]

We now assess the importance of connectivity effects, focusing our analysis on control streets. Figure 4 shows a positive relationship between changes in home value and changes in the distance to the nearest paved streets for those in the control group. Barring a discrete change in the value of houses in the control areas due to pavement in the treatment areas, we estimate the following model for the group of homes in the group assigned to control ($Z = 0$):

$$Y_{2009} = \gamma_0 + \gamma_1 \Delta d + \gamma_2 Y_{2006} + \epsilon_3$$

[16] Although not reported here, being a renter is the most important correlate of out-migration in this setting.

[17] Differences along other dimensions were also checked, with similar results.

[18] The sampling frame in 2006 was occupied dwellings. In 2009, some of these dwellings may have been temporarily unoccupied, hence the higher out-migration than immigration rate.

[19] In Table A3 we report *item* non-response rates in 2009 for each variable and their differences by treatment status for units that responded in 2006. None of the item response rates is significantly different between assignment-to-treatment and control groups.

where Y_{2009} is appraised home value in 2009, and Δd is the change in distance to the nearest paved street between 2006 and 2009 (measured in street blocks). The connectivity effect is captured by γ_1. Our estimate for γ_1 is -0.034 (se=0.026), so that a decrease of one street block in the distance between a given house and the pavement grid is correlated with a 3% higher housing value. Given that the average reduction in distance to the nearest paved street among the control group was 0.68 street blocks, the estimated downward bias is around 2.3 percentage points, which is not statistically significant, and relatively small compared to the ITT and 2SLS estimates reported below.

While our test for connectivity effects gives us some information on indirect effects that vary with distance to nearest paved street, it does not shed light on indirect effects that may operate more globally. Indeed, our empirical design cannot account for effects due to the increase in the number of places one can drive to after paving, nor can it account for paving increasing the size of the market available to support downtown economic activity, which could benefit everyone in the city. Perhaps, in our context of streets in a circular city (and not of roads connecting different cities), and given that the fraction of treated properties due to the intervention in the city is small (around 5%), these global effects are less likely to be important. In any case, some caution is warranted in interpreting our 2SLS estimates as potential lower bounds for the TOT effects.

4 Experimental Effects of Street Pavement

4.1 Effects on Consumption and Credit

We begin by presenting, in the three columns of Table 5, our main experimental estimates for the effects of street-pavement on consumption of durable and non-durable goods. ITT and 2SLS estimated effects are presented in the first two columns and the mean of the corresponding variable for the control group in 2009 in the third. Street-pavement provision has a strong positive effect on the number of durable goods owned by the household: out of six durable goods, control households have an average of 2.4 goods, while the mean for households on paved streets is 2.7 goods (12% higher). There is also a significant effect on the rate of ownership of a motorized vehicle (motorcycle, car, or truck). Whereas the household-vehicle index is 0.25 (out of three) in the control group, in the treated it is 0.35, corresponding to a 43% increase.

In Acayucan, as in many other cities in developing countries, households improve and expand their houses over time. We find that street pavement leads to a doubling in the average number of home improvements a household engaged in over the previous six months: from 0.4 to 0.8 reforms. The types of home improvements we inquired about related to flooring, plumbing, electrical installations, toilets, room remodeling, and air conditioning. The effect is confirmed by the 50% increase in the likelihood that the family had bought materials for home improvements in the previous six months (from 15% of households in the control group to 24% among the treated group).

Finally, we find that the provision of street pavement has no statistically significant effect on monthly per capita expenditure, i.e., non-durable consumption, measured by the sum of itemized expenditures or a direct measure of total household expenditures.

Credit effects are reported in Table 6. We find that pavement increases the percentage of individuals who use collateral-based credit from close to 2% among the control group to nearly 5% among the treated. The increased use of collateral-based credit is also reflected in the average loan size, on average 135 pesos among the control group and 1,643 pesos (equivalent to 2 months per capita expenditure) among the treated: a more than tenfold increase. While this is an important finding, we do not have the required information to determine whether the increase in collateral-based credit use is due to an increase in either the demand or the supply of credit (Field and Torero, 2004). For all other types of credit, such as non-collateral based, credit from family and friends, credit from government entities, and credit from informal sources, we do not observe any changes either in the number of individuals using credit or in the extent of the credit.[20]

4.2 Effects on Transportation and Labor

We measure the effects of street pavement on transportation costs in Table 7 in terms of money and time: the cost of a taxi to the city center and the time it takes to go to the city center by one's usual means of transportation. We find that in both respects the savings for those who benefit from pavement over the control group are neither large (0.6 pesos and less than 1 minute) nor statistically significant. In addition, field visits did not reveal new bus routes in these neighborhoods after pavement was provided.

Similarly, we find no effect on labor outcomes either in terms of labor supply or earnings (in Table A4 we report no effect on the extensive margin either). However, we do find a reduction in the percentage of families for which a household member plans to migrate in search of work, which fell from 47% to 37% as a result of treatment.

Finally, and regarding other outcomes, Table A4 shows no significant effects on either school attendance or self-reported health for either adults or children (see Cattaneo et al. (2009) for health impacts of providing cement floors in Mexico).

4.3 Effects on Property Values

We now turn to estimate the effects of street-pavement on home and land values. Results are reported in Table 8. Using the professional-appraisal measures, we find that pavement increases home values by 16% and land values by 54%. According to homeowners' valuations, we estimate that street pavement raises property values by 25%. The fact that two independent measures of property value move in the same direction suggests that paving accounts for substantial rises in home values. Note that the difference in the magnitudes of

[20] We also find a seven-percentage-point increase in bank accounts over a control-group rate of 14%, which is close to being statistically significant at the 10% level.

the estimated impacts with the two measures is not statistically significant.[21]

Table 8 also shows that rents on treated streets are 31% higher than rents on control streets, and that the amount paid for recently purchased houses on intent-to-treat and paved streets are 44% and 85% higher than in the control group, although it must be conceded that this last estimate is imprecise on account of the small sample size and the fact that we cannot control for the outcome at baseline. Nevertheless, it is reassuring to corroborate the qualitative findings obtained from appraisals and homeowner valuations with these supplemental indicators. The similarity of the effects on owners' self-reported prices (a stock variable) and rental rates (a flow variable) is remarkable, and corroborates our previous results on the absence of anticipation effects.

We interpret these estimates as pointing to local infrastructure affecting homeowners through land value increases, which is consistent with previous findings documented by Brueckner (1982) and Haughwout (2002), among others. Home value increases can explain the expansion of credit and durable good consumption. A wealth effect seems the most likely explanation for the observed rises in durable good consumption, especially given the lack of evidence for the alternative hypothesis of increments in labor income based on reduced transportation costs. Understanding the effects of pavement on household outcomes is the purpose of the next section.

5 Understanding Pavement Effects on Household Outcomes

While the previous analysis is crucial to assess the causal effects of street pavement on the lives of the poor, it does not tell us how pavement affects household behavior. In this section we seek to understand what drives the observed changes in their household behavior, namely

[21] Focusing on short-tenured homeowners ($\leq$ 5 years living in the dwelling), we obtain reduced-form and 2SLS estimates of 0.24 (se=0.27) and 0.36 (se=0.38), which are imprecisely estimated on account of the reduced number of observations (N=49).

significant increases in home improvements, vehicle acquisition, and other durable goods. Because the primary residence typically constitutes the single most important depository of wealth for homeowners, changes in its asset value can be expected to have important consumption effects (Campbell and Cocco, 2007) as long as households can borrow against housing wealth or dissave (Muellbauer, 2007). Of course, if households are credit constrained and have no savings, there will be no response in terms of durable good acquisition.

5.1 Credit Constraints

Distributional impacts are always important for evaluating public policy. In our context, while there was no built-in distributional bias, there could be an implicit one because of variation in credit constraints that affect who can best take advantage of the intervention. If collateral-poor households were unable to transform their increased housing wealth into consumption because of credit constraints, one would expect little or no treatment effects among this group in terms of consumption and collateralized credit outcomes. To determine the empirical relevance of this argument, we estimate consumption and credit effects by housing wealth terciles at baseline in Table 9. The table presents 2SLS estimates of the following equation:

$$Y_{2009} = \pi_1 DH_1 + \pi_2 DH_2 + \pi_3 DH_3 + \pi_4 H_1 + \pi_5 H_2 + \pi_6 H_3 + \pi_7 Y_{2006} + \epsilon_4 \tag{3}$$

where Y_{2009} is a household outcome in 2009, $H_j = 1$ if the household is in the j^{th} tercile of the appraised housing value distribution in 2006, and we use pavement-group assignment Z and its interaction with housing value tercile indicators in 2006 (ZH_1, ZH_2, ZH_3) as instrumental variables for DH_1, DH_2, and DH_3.[22]

The results in the table show a U-shaped pattern of impacts for vehicle acquisition, durable goods, and collateral based credit use with respect to housing wealth at baseline.

[22] The approach here is quasi-experimental in the sense that we are stratifying our sample ex-post randomization.

The effects are imprecisely estimated, but the qualitative results are consistent with pavement allowing both "poor" and "rich" households to borrow against housing wealth. We interpret this as evidence that credit constraints do not restrict the poorest households in the sample from benefitting from street pavement.[23]

5.2 Disentangling Complementarity from Wealth Effects

The observed increases in vehicle acquisition can also be explained by complementarities with street pavement. When streets are paved, vehicle access and parking is facilitated, meaning that the marginal utility of a vehicle rises. To test for complementarities between street pavement and vehicles, we use a parsimonious model in which a representative household derives utility from (non-durable) consumption C, housing services $\widehat{H}$, and vehicle services V. More specifically, the representative homeowner maximizes a Cobb-Douglas utility function:

$$U(C, \widehat{H}, V) = C^{\alpha} \widehat{H}^{\beta} V^{1-\alpha-\beta} \tag{4}$$

where α, $\beta > 0$ and $\alpha + \beta < 1$. The case of renters is ignored because homeowners constitute 95% of the sample.

The production of housing is a function of residential private investment i and public investment D, where we think of D as indicating whether the street is paved or not. The focus of the model is on the impact of an *exogenous* manipulation of D for a set of households located in houses on unpaved streets. We write $H = H(i; D)$, where D is outside the household's control and hence included after the semicolon. Similarly, vehicle services are a function of vehicle ownership v and public investment D, so that $V = V(v; D)$.

It is conceptually important to distinguish housing services *consumption* − which may vary from period to period − from the *asset* value of the house the family owns. Pavement

[23] Although purely speculative the U-shaped pattern of impacts may reflect that the middle-wealth households replaced their old vehicles and durable goods with new ones between 2006 and 2009, while the poorest started buying and the richest bought additional vehicles and durable goods. Unfortunately, we do not have the required information to test this explanation.

has a large immediate impact on the asset value of the house, and hence on the family's wealth position. Although the increased housing services may be all consumed in the short run, this is not necessarily the case in the long run. The problem of intertemporal housing choice with frictions is a complex one (see Ortalo-Magné and Rady, 2006). Still, we can abstract from these considerations while keeping the essential tradeoffs at stake by allowing the homeowner to choose the fraction of housing that is consumed: $\widehat{H} = (1-\mu)rH$, where μ is the fraction spent on non-housing goods.

The household budget constraint is given by:

$$m + rp_h H = C + p_i i + p_v v + (1-\mu)rp_h H \tag{5}$$

Households have two sources of income: a lump sum m and rents from housing wealth $rp_h H$. These resources are spent on consumption C (with price normalized to one), investment in home improvements i (with price p_i), vehicles v (with price p_v), and housing services $\widehat{H} = (1-\mu)rH$. We do not consider depreciation because it plays no role in the analysis. Given that housing is both an asset and a consumption good, (5) simplifies to:

$$m + \mu rp_h H = C + p_i i + p_v v \tag{6}$$

The FOCs imply the following ratios at the optimizing bundle:

$$\frac{C^*}{\widehat{H}^*} = \frac{\alpha}{\beta} p_h \tag{7}$$

$$\frac{V^*}{\widehat{H}^*} = \left[\frac{1-\alpha-\beta}{\beta}\right] \left[\frac{p_h}{p_v}\right] \frac{\partial V(v^*; D)}{\partial v} \tag{8}$$

$$\frac{V^*}{C^*} = \left[\frac{1-\alpha-\beta}{\alpha}\right] \left[\frac{1}{p_v}\right] \frac{\partial V(v^*; D)}{\partial v} \tag{9}$$

By assuming a Cobb-Douglas utility function, three straightforward predictions about how the consumption ratios of the three goods depend on the complementarity between pavement and vehicles emerge. In particular, the ratio between housing services and non-durable consumption (equation 7) should not be affected by pavement, since relative prices are unaffected. In contrast, both the vehicle to housing (equation 8) and vehicle to non-durable consumption ratios (equation 9) should increase as long as vehicle marginal utility rises with public investment (street pavement), providing a test for complementarities that is independent of wealth effects.

To test these predictions, we estimate the following reduced-form model of multiple equations for each household h in each street s:

$$\frac{C_{h,s}}{\widehat{H}_{h,s}} = a_1 + b_1 D_s + \varepsilon_{h,s,1} \tag{10}$$

$$\frac{V_{h,s}}{\widehat{H}_{h,s}} = a_2 + b_2 D_s + \varepsilon_{h,s,2} \tag{11}$$

$$\frac{V_{h,s}}{C_{h,s}} = a_3 + b_3 D_s + \varepsilon_{h,s,3} \tag{12}$$

where a_i corresponds to the average ratio in the control group, and b_i corresponds to the average difference in the ratio between treatment and control groups. Hence, under random assignment of D:

- $a_1 = E\left[\frac{\alpha}{\beta} p_h\right] = \frac{\alpha}{\beta} p_h$, $b_1 = 0$
- $b_2 = \left[\frac{1-\alpha-\beta}{\beta}\right]\left[\frac{p_h}{p_v}\right] E\left[\frac{\partial V(v^*;1)}{\partial v} - \frac{\partial V(v^*;0)}{\partial v}\right]$
- $b_3 = \left[\frac{1-\alpha-\beta}{\alpha}\right]\left[\frac{1}{p_v}\right] E\left[\frac{\partial V(v^*;1)}{\partial v} - \frac{\partial V(v^*;0)}{\partial v}\right]$
- $a_2 = \left[\frac{1-\alpha-\beta}{\beta}\right]\left[\frac{p_h}{p_v}\right] E\left[\frac{\partial V(v^*;0)}{\partial v}\right]$
- $a_3 = \left[\frac{1-\alpha-\beta}{\alpha}\right]\left[\frac{1}{p_v}\right] E\left[\frac{\partial V(v^*;0)}{\partial v}\right]$

We can then test the following set of qualitative predictions:

P1: $b_1 = 0$ The mean ratio of consumption to housing services is the *same* for households on paved and unpaved streets.

P2: $b_2 > 0$ (Complementarity test) The mean ratio of vehicles to housing services is *higher* for households on paved streets.[24]

P3: $b_3 > 0$ (Complementarity test) The mean ratio of vehicles to consumption is *higher* for households on paved streets.

We estimate equations (10)-(12) by means of seemingly unrelated regressions (SUR), and test our qualitative predictions **P1**, **P2** and **P3** using standard t-tests. We use appraised house value as our proxy for housing services and the vehicle index as our measure of vehicle services. The top panel in Table 10 displays the results. The first column shows that the estimate for b_1 is virtually zero (0.010) and not statistically significant. Hence, the average ratio between consumption and housing services is the same for households on paved and unpaved streets, consistent with our first qualitative prediction. Column (2) reports an estimate of 0.011 for b_2, which is positive and statistically different than zero at the 10% level. This confirms the existence of complementarities between vehicles and street pavement. Given that the mean ratio of vehicle services and housing in unpaved streets is around 0.019, this is quite a large difference in ratios. Finally, the last column shows that, on average, the ratio of vehicle to consumption is higher among households on paved than unpaved streets. The estimate for b_3 is 0.018 with a p-value<0.1. This confirms again the existence of complementarities. As a robustness check, in Table A5, we present 2SLS estimates where assignment to treatment is the instrument for pavement and obtain very similar results.

In addition, if the model is correctly specified, the following conditions must be satisfied:

S1: $a_1 = \frac{b_2}{b_3}$

S2: $a_1 = \frac{a_2}{a_3}$

S3: $\frac{b_2}{b_3} = \frac{a_2}{a_3}$

S4: $a_1 = \frac{b_2}{b_3} = \frac{a_2}{a_3}$

[24] Lower if vehicle and street pavement are substitutes, equal if they are neither complements nor substitutes.

The bottom panel in Table 10 displays the results of the corresponding tests for predictions **S1**, **S2**, **S3** and **S4** using Wald-tests. Remarkably, we cannot reject any of the constraints individually (p-values always greater than 0.51), nor jointly (p-value=0.7303). Hence, **S1**, **S2**, **S3** and **S4** are satisfied in the data.

However, since the test-statistic for Wald tests is not invariant to nonlinear transformations (Gregory and Veall, 1985), we also proceed with an alternative approach, using a Likelihood ratio test, which is invariant. In this case, we estimate these same equations (10)-(12) by SUR subject to **S4**, and perform a Likelihood ratio test of the restricted model against the unrestricted one. As indicated by the Likelihood ratio test in the bottom panel of the table, we cannot reject that the constrained model is nested in the unconstrained one (p-value=0.6209).

This section illustrates how a simple economic model, which boils downs to testing the joint null of *homothetic* preferences and *no cross-partial* in the vehicle services function $V = V(v; D)$, can be combined with a randomized intervention to generate crystal clear testable implications on how local public infrastructure affects household consumption decisions. The results decidedly point to street pavement having important effects on household consumption patterns by raising the marginal utility of vehicles. However, we must recognize that we cannot necessarily isolate the benefit to the household of having a paved road, keeping all else equal. Indeed, the increase in the collateral value of the house could also be partly coming from homeowners' housing investments or pavement serving as a coordination device leading neighbors to clean their porches and paint their houses, among other things, all of which is part of the economic return to investment in pavement.

6 Cost-Benefit Analysis

With zero marginal price for street use, the benefits of a paved street are defined as the increase in consumers' surplus that users derive from the street improvement. Users of a street can be divided into two sets of individuals: those living on properties abutting the street that is paved and users not living there. We obtain an estimate of consumer surplus for the group of individuals living on properties adjacent to paved streets, being unable to obtain estimates for those living in other streets. Hence, to the extent that we are not capturing indirect benefits, this is an under-estimate.

Our consumers' surplus estimate is obtained by summing up the increases in land value over plots on treated streets. A similar approach is used in Jacoby (2000).[25] Construction costs are measured as the sum of municipal expenditures on each street that was treated with pavement. Specifically, the municipality reported that the total cost of paving the streets in this study amounted to $11,304,642$ pesos.

Table 11 reports the results of this cost-benefit analysis. There are 814 plots on treated streets. The average plot on these streets is valued at 27,844 pesos. Multiplying this value by the estimated impact of street pavement on land value (54%) gives an average benefit per plot of 15,081 pesos, for a total benefit of 12,275,585 pesos. The last column shows that the increases in land values represent 109% of construction costs. Our cost-benefit analysis indicates that the economic returns to street pavement in this context are at least as large as the construction costs, even if we consider the typical deadweight losses generated by taxation in developing countries (Auriol and Warlters, 2012).

7 Conclusion

Does the provision of infrastructure improve standards of living for the poor in the developing world? This study provides an answer to this question by combining the first randomized infrastructure experiment of street pavement provision with the collection of data from a dedicated survey before and after the intervention takes place.

Our double-strategy allows us to tackle the standard *endogeneity* concern in infrastructure allocation as well as to assess any potential *recomposition* effects from households moving

[25] Alternative strategies to estimate consumers' surplus can be found in Kaufman and Quigley (1987).

into (or out of) streets with and without street pavement between baseline and follow-up. We show that the randomization was successful in balancing pre-treatment average characteristics between assignment-to-treatment and control streets, and we do not find evidence of neighborhood recomposition.

We find quite encouraging results for the urban poor that receive infrastructure. Within two years of the intervention, households whose streets were finally paved, and were present both before and after its implementation, increase their consumption of durable goods and acquire more motor vehicles. These effects are driven in part by street pavement boosting housing wealth, which fuels a rise in collateralized credit use. The effects appear to be present for both the relatively poor and the relatively rich households in our sample. Not only that, a parsimonious model allows us to disentangle complementarity effects from wealth effects. Our model predicts that the ratio between housing services and non-durable consumption should not be affected by pavement, while the vehicle to housing and vehicle to non-durable consumption ratios should increase as long as the marginal utility of vehicles rises with pavement. These testable implications are taken to the data and confirmed.

Finally, we estimate a lower bound for the total benefit to cost ratio of street pavement at around 1.09, which justifies serious consideration of cost-sharing mechanisms in the provision of localized benefit public goods. In the absence of credit constraints, property taxes triggered by public goods that increase land value could be used as a funding device for localized impact urban infrastructure, such as Colombia's urban improvements tax or U.S. road paving special assessment districts (see Diamond, 1983).

Our main conclusion is that provision of street pavement reduces poverty, at least as measured by the increases in the acquisition of durable goods, motorized vehicles, and property values. This is an important result, but some limitations must be acknowledged. Perhaps the two most substantial ones are the short-term nature of the evaluation, and that it was conducted in just one city. Replications of our double-strategy in different places, as well as long-run follow-ups on our Acayucan households, may be fruitful avenues for future research.

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Figures and Tables

Figure 1: Before Pavement

Figure 2: After Pavement

Figure 3: Acayucan Street Projects

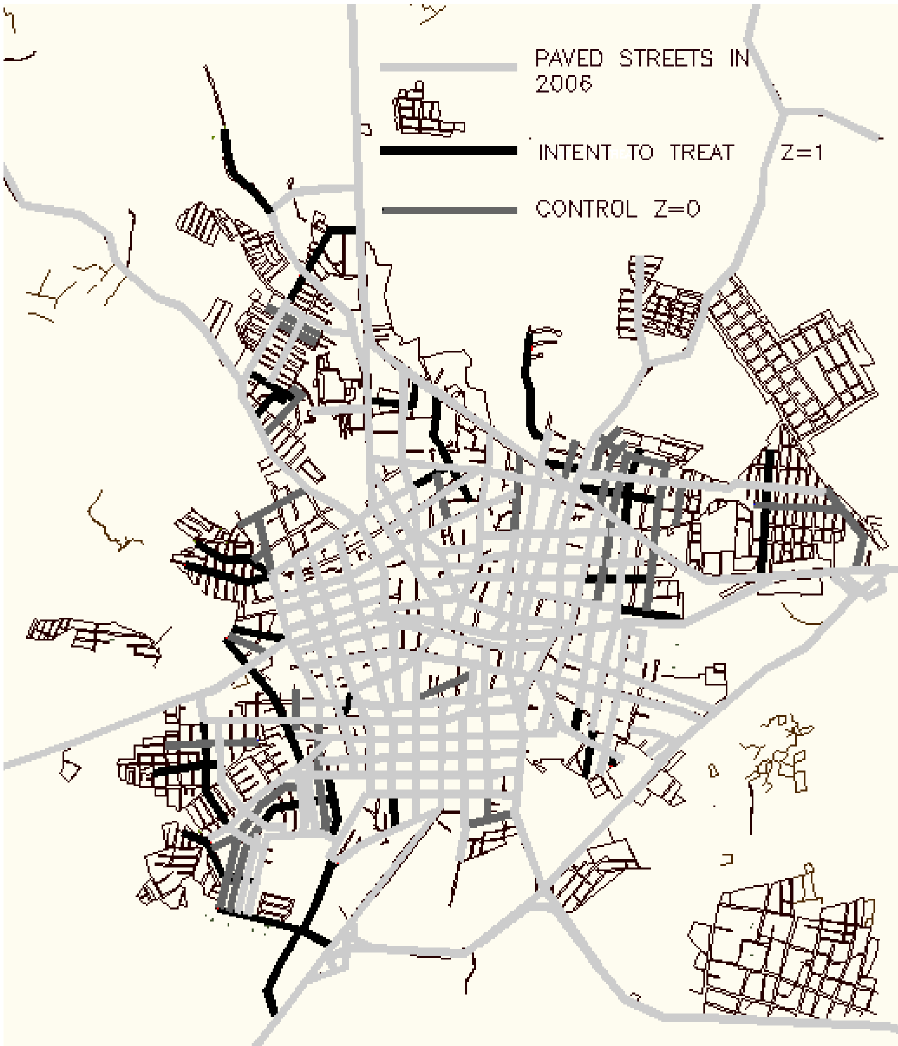
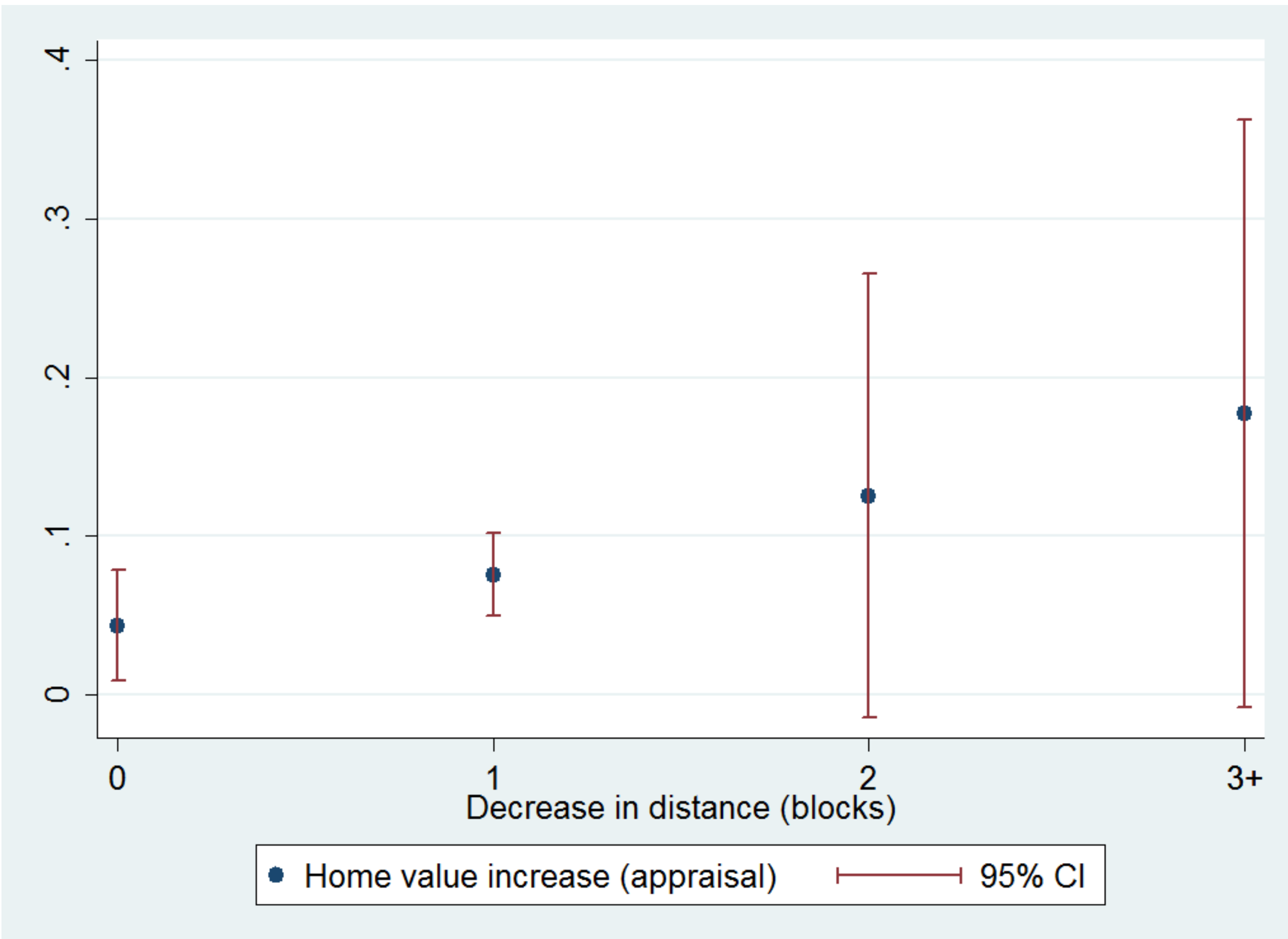


Figure 4: Change in House Value in Control Group



The figure uses the estimates from a regression of the change in home value on a constant and three indicator variables of change in distance (1 block, 2, blocks, 3+ blocks) in the control group.

Table 1: Pre-Intervention Balance in Means

Variable	ITT=1	ITT=0	*Diff.*	Variable	ITT=1	ITT=0	*Diff.*
Consumption				**Credit (continued)**			
Monthly log per	6.77	6.69	0.08	Bank account (=1)	0.154	0.166	−0.012
capita expenditure	(0.073)	(0.050)	(0.087)		(0.030)	(0.018)	(0.035)
	[461]	[403]	[864]		[481]	[410]	[891]
Monthly log sum of	6.60	6.49	0.11	Credit card (=1)	0.097	0.087	0.010
itemized expenditures	(0.079)	(0.045)	(0.090)		(0.026)	(0.012)	(0.028)
per capita	[474]	[409]	[883]		[480]	[410]	[890]
Durable goods (0-6)	2.12	2.04	0.08	**Labor and Transportation**			
	(0.163)	(0.075)	(0.178)	Weekly hours	48.45	47.59	0.86
	[487]	[413]	[900]	workedⓘ	(1.43)	(1.19)	(1.84)
Vehicles (car/truck/motorcycle)	0.203	0.226	−0.023		[498]	[429]	[927]
(0-3)	(0.050)	(0.033)	(0.059)	Monthly log labor	7.97	7.80	0.17*
	[487]	[413]	[900]	income ⓘ	(0.082)	(0.051)	(0.095)
Home improvements	0.541	0.474	0.067		[408]	[382]	[790]
(0-11)	(0.048)	(0.054)	(0.071)	Plans to migrate	0.410	0.417	−0.007
	[487]	[413]	[900]	in search of work (=1)	(0.030)	(0.022)	(0.037)
Bought materials	0.254	0.219	0.035		[431]	[370]	[801]
for home improvement (=1)	(0.022)	(0.020)	(0.029)	Cost of taxi to	20.66	20.21	0.45
	[485]	[409]	[894]	city center	(0.909)	(0.820)	(1.21)
Credit					[482]	[407]	[889]
Collateral-based	0.029	0.027	0.002	Time to city center	19.90	20.86	−0.96
credit (=1)ⓘ	(0.006)	(0.007)	(0.009)	(minutes)	(0.947)	(0.890)	(1.29)
	[1,047]	[937]	[1,984]		[487]	[412]	[899]
Collateral-based	658	429	229	**Housing**			
credit amountⓘ	(272)	(152)	(308)	Log owner estimate	11.75	11.81	−0.06
	[1,047]	[937]	[1,984]	of house value	(0.12)	(0.10)	(0.15)
Non-collateral	0.050	0.034	0.016		[269]	[262]	[531]
based credit (=1)ⓘ	(0.008)	(0.006)	(0.010)	Log professional	11.64	11.60	0.04
	[1,047]	[937]	[1,984]	appraisal property	(0.08)	(0.05)	(0.10)
Non-collateral based	496	237	259*		[295]	[253]	[548]
credit amountⓘ	(134)	(75)	(151)	Log professional	10.27	10.14	0.13
	[1,047]	[937]	[1,984]	appraisal land	(0.07)	(0.05)	(0.09)
Credit from family	0.006	0.004	0.002		[295]	[253]	[548]
and friends (=1)ⓘ	(0.003)	(0.003)	(0.004)	Log rent	6.48	6.50	−0.02
	[1,047]	[937]	[1,984]		(0.13)	(0.11)	(0.17)
Informal private	0.003	0.007	−0.004		[34]	[22]	[56]
credit (=1)ⓘ	(0.001)	(0.003)	(0.003)	Nearest paved street	1.49	1.35	0.14
	[1,047]	[937]	[1,984]	(street blocks)	(0.16)	(0.15)	(0.22)
					[487]	[411]	[898]

ⓘ denotes individual-level outcomes. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Individual variables regarding credit and labor outcomes for individuals aged 18+. Variable definitions: Nearest paved street (distance in blocks from the dwelling to the nearest paved street); collateral-based credit (mortgages, home-equity lines, and collateralized bank loans); non-collateralized credit (appliance- and furniture-store credit, bank-card credit, vehicle loans, and *casas de crédito popular*); informal credit (credit from informal lenders); credit card (bank account): indicator that someone in the household has a credit card (bank account); monthly log per capita expenditure (based on total self-reported expenditure in the household); monthly log sum of itemized expenditures per capita (based on the sum of household expenditures on food, phone, gas, electricity, education, rent/mortgage, clothes, and entertainment); durable goods (sum of indicators for refrigerator, washing machine, microwave oven, air conditioning, video player, and computer); vehicles (sum of indicators for car, truck, and motorcycle); home improvements (sum of indicators for improvements in flooring, walls, roofing, sewerage connection, plumbing, toilets, electrical installations, room construction, remodeling, security measures, and improvements to house front); materials purchased for home improvements (in the previous 6 months); weekly hours worked (in the previous week); plans to migrate in search of work (if someone in the household is expected to migrate in search of work); cost of taxi to city center (self-reported cost of a taxi from home to city center); time to city center (self-reported time to commute from home to city center). Significance levels reported only for *Diff*: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2: Anticipation Effects on Housing Value

Log professional appraisal of property value	
Assigned to treatment but unpaved	0.007
	(0.036)
	[344]
Log owner estimate of property value	
Assigned to treatment but unpaved	0.067
	(0.167)
	[338]

"Assigned to treatment but unpaved" is a dummy for observations from street projects assigned to pavement but unpaved by the time of the second survey. Sample consists of street projects assigned to control and assigned to treatment but unpaved. Regressions include a constant and the corresponding dependent variable at baseline. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Out-migration decision and out-migrant characteristics

Out-migration rate				Household Out-migrated (=1)					
				ITT	2SLS	Mean Control (2006)			
				0.008	0.013	0.230			
				(0.027)	(0.044)	(0.022)			
				[1,171]	[1,171]	[533]			

Out-migrant Characteristics	Log(PCE)			Durable Goods			Vehicle Ownership		
	ITT	2SLS	Mean Control (2006)	ITT	2SLS	Mean Control (2006)	ITT	2SLS	Mean Control (2006)
	0.059	0.102	6.71	−0.063	−0.109	1.94	0.081	0.139	0.173
	(0.117)	(0.197)	(0.078)	(0.231)	(0.399)	(0.155)	(0.074)	(0.118)	(0.038)
	[266]	[266]	[119]	[271]	[271]	[120]	[271]	[271]	[120]

ITT column uses assignment to pavement as independent variable. 2SLS column instruments pavement with assignment to pavement. Regressions include a constant. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Significance levels reported only for ITT and 2SLS: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Immigration decision and immigrant characteristics

Immigration rate				Household Immigrated (=1)					
				ITT	2SLS	Mean Control (2009)			
				−0.00	−0.012	0.175			
				(0.024)	(0.040)	(0.017)			
				[1,083]	[1,083]	[497]			

Immigrant Characteristics	Log(PCE)			Durable Goods			Vehicle Ownership		
	ITT	2SLS	Mean Control (2009)	ITT	2SLS	Mean Control (2009)	ITT	2SLS	Mean Control (2009)
	−0.033	−0.060	6.90	0.297	0.532	2.04	0.071	0.127	0.284
	(0.095)	(0.170)	(0.055)	(0.222)	(0.383)	(0.154)	(0.105)	(0.178)	(0.060)
	[181]	[181]	[83]	[183]	[183]	[84]	[183]	[183]	[84]

ITT column uses assignment to pavement as independent variable. 2SLS column instruments pavement with assignment to pavement. Regressions include a constant. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Significance levels reported only for ITT and 2SLS: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Impacts of Street Pavement on Consumption

	ITT	2SLS	Mean Control (2009)
Durable goods (0-6)	0.166*	0.274*	2.36
	(0.091)	(0.147)	(0.077)
	[900]	[900]	[413]
Vehicles (car/truck/motorcycle) (0-3)	0.063*	0.104*	0.245
	(0.037)	(0.059)	(0.027)
	[900]	[900]	[413]
Home improvements (0-11)	0.258**	0.424**	0.400
	(0.112)	(0.202)	(0.064)
	[900]	[900]	[413]
Materials purchased	0.052*	0.086*	0.146
for home improvement (=1)	(0.027)	(0.046)	(0.021)
	[894]	[894]	[409]
Monthly log per	0.047	0.077	6.73
capita expenditure	(0.047)	(0.075)	(0.040)
	[864]	[864]	[403]
Monthly log sum of	0.035	0.057	6.62
itemized expenditures	(0.049)	(0.079)	(0.041)
per capita	[883]	[883]	[409]

ITT column uses assignment to pavement as independent variable. 2SLS column instruments pavement with assignment to pavement. Regressions include a constant and the corresponding dependent variable at baseline. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Significance levels reported only for ITT and 2SLS: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Impacts of Street Pavement on Credit Use

	ITT	2SLS	Mean Control (2009)
Collateral-based credit (=1)ⓘ	0.017*	0.028*	0.018
	(0.009)	(0.014)	(0.004)
	[1,984]	[1,984]	[937]
Collateral-based credit amountⓘ	914*	1,508*	135
	(516)	(787)	(45)
	[1,984]	[1,984]	[937]
Non-collateral based credit (=1)ⓘ	−0.001	−0.001	0.069
	(0.012)	(0.020)	(0.009)
	[1,984]	[1,984]	[937]
Non-collateral based credit amountⓘ	256	422	823
	(360)	(589)	(208)
	[1,984]	[1,984]	[937]
Credit from family and friends (=1)ⓘ	0.001	0.002	0.004
	(0.003)	(0.005)	(0.002)
	[1,984]	[1,984]	[937]
Informal private credit (=1)ⓘ	0.001	0.001	0.002
	(0.002)	(0.003)	(0.002)
	[1,984]	[1,984]	[937]
Credit card (=1)	0.033	0.055	0.155
	(0.032)	(0.052)	(0.021)
	[890]	[890]	[410]
Bank account (=1)	0.043	0.071	0.138
	(0.027)	(0.045)	(0.020)
	[891]	[891]	[410]

ⓘ denotes individual-level outcomes. ITT column uses assignment to pavement as independent variable. 2SLS column instruments pavement with assignment to pavement. Regressions include a constant and the corresponding dependent variable at baseline. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Significance levels reported only for ITT and 2SLS: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Impacts of Street Pavement on Transportation and Labor

	ITT	2SLS	Mean Control (2009)
Cost of taxi to	−0.360	−0.587	18.14
city center (pesos)	(0.487)	(0.767)	(0.697)
	[889]	[889]	[407]
Time to city center (minutes)	−0.598	−0.989	19.04
	(0.920)	(1.52)	(0.789)
	[899]	[899]	[412]
Distance to nearest paved street	−0.46***	−0.75***	0.67
(street blocks)	(0.10)	(0.13)	(0.08)
	[898]	[898]	[411]
Weekly work hours ⓘ	2.31	3.77	47.29
	(1.42)	(3.46)	(1.14)
	[927]	[927]	[429]
Monthly log labor	0.034	0.057	7.83
income ⓘ	(0.055)	(0.087)	(0.047)
	[790]	[790]	[382]
Plans to migrate	−0.063*	−0.104*	0.474
in search of work (=1)	(0.033)	(0.055)	(0.027)
	[801]	[801]	[370]

ⓘ denotes individual-level outcomes. ITT column uses assignment to pavement as independent variable. 2SLS column instruments pavement with assignment to pavement. Regressions include a constant and the corresponding dependent variable at baseline. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Significance levels reported only for ITT and 2SLS: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: Impacts of Street Pavement on Property Values

	ITT	2SLS	Mean Control (2009)
Log professional appraisal of property value	0.09***	0.16***	11.52
	(0.03)	(0.04)	(0.06)
	[548]	[548]	[253]
Log professional appraisal of land value	0.32***	0.54***	10.07
	(0.06)	(0.10)	(0.06)
	[548]	[548]	[253]
Log owner estimate of property value	0.16*	0.25*	12.01
	(0.09)	(0.15)	(0.08)
	[531]	[531]	[262]
Log rent	0.17*	0.31**	6.55
	(0.09)	(0.13)	(0.10)
	[56]	[56]	[22]
Log transaction price recent purchases ¶	0.44	0.85	10.82
	(0.65)	(1.22)	(0.38)
	[29]	[29]	[8]

¶Transaction price is amount paid by new homeowners (arriving between baseline and follow up), hence the dependent variable at baseline is not included. ITT column uses assignment to pavement as independent variable. 2SLS column instruments pavement with assignment to pavement. Regressions include a constant and the corresponding dependent variable at baseline. For log rent, we use as baseline control the rent paid by the family previously living in the same house in 2006. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Significance levels reported only for ITT and 2SLS: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 9: Heterogenous Pavement Effects on Household Outcomes by Appraised Housing Value Terciles at Baseline using 2SLS

	Vehicle index	Durable goods index	Collateral based credit use
Pavement × H_1	0.226*	0.049	0.062
	(0.137)	(0.400)	(0.059)
Pavement × H_2	0.074	0.023	−0.017
	(0.132)	(0.313)	(0.064)
Pavement × H_3	0.212*	0.094	0.072
	(0.128)	(0.406)	(0.058)
H_1	0.029	0.971***	0.016
	(0.036)	(0.145)	(0.014)
H_2	0.118**	1.33***	0.044
	(0.053)	(0.182)	(0.028)
H_3	0.177***	1.33***	0.069***
	(0.061)	(0.225)	(0.026)
Baseline outcome	0.500***	0.618***	−0.025
	(0.088)	(0.050)	(0.032)
N	418	418	413

Instruments for (*pavement* × H_j) are (*assignment to pavement* × H_j). H_j is professionally appraised housing value at baseline. Regressions include a constant and the corresponding lagged dependent variable. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10: Testing for Complementarities

	$\frac{log(C)}{log(H)}$	$\frac{V}{log(H)}$	$\frac{V}{log(C)}$
$\widehat{b_i}$	0.010	0.011*	0.018*
	(0.007)	(0.006)	(0.010)
$\widehat{a_i}$	0.582***	0.019***	0.032***
	(0.005)	(0.003)	(0.005)
N		[386]	
		Ratios	
$\widehat{b_2}/\widehat{b_3}$		0.590***	
		(0.042)	
$\widehat{a_2}/\widehat{a_3}$		0.588***	
		(0.010)	
		Wald Tests	
$H_0 : a_1 = \frac{b_2}{b_3}$		$\chi^2(1)$=0.03 p =0.8579	
$H_0 : a_1 = \frac{a_2}{a_3}$		$\chi^2(1)$=0.42 p =0.5185	
$H_0 : \frac{b_2}{b_3} = \frac{a_2}{a_3}$		$\chi^2(1)$=0.00 p =0.9752	
$H_0 : a_1 = \frac{b_2}{b_3} = \frac{a_2}{a_3}$		$\chi^2(2)$=0.63 p =0.7303	
		Likelihood Ratio Test	
H_0 : Constrained model nested in unconstrained		$\chi^2(2)$=0.95 p =0.6209	

Seemingly unrelated regression estimates. Bootstrapped standard errors (200 replications) clustered at the pavement-project level in parentheses. The Likelihood Ratio test is obtained estimating constrained and unconstrained models using means at the cluster level. Significance levels: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 11: Cost-Benefit Analysis

	Plots	Average value	Impact of Pavement	Gains per plot	Total gains	Gain/Cost ratio
	814	27,844***	0.54***	15,081***	12,275,585***	1.09***
Standard error		(1,508)	(0.10)	(3,006)	(2,446,579)	(0.22)

Plots column reports the number of plots abutting streets that were paved (residential, non-residential, and vacant). The average value of a plot is estimated by means of professional appraisals. Impact coefficient is taken from Table 8. Total costs are municipal-authority estimates of costs of the pavement program undertaken as part of this study. Figures in 2009 Mexican pesos. 2009 PPP exchange rate 8.5 pesos to the U.S. dollar. Nominal February 2009 exchange rate 14.6 Mexican pesos to the U.S. dollar. Standard errors clustered at the pavement-project level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Appendix (Not for Publication)

A.1 Dataset structure

Our dataset contains three types of households: those interviewed in 2006 and 2009 (stayers); those interviewed in 2006 but not subsequently because they moved out (out-migrants); and immigrant households for which we only have information from the 2009 round. Table A1 shows that by the time of the follow-up survey in 2009, 271 baseline households (in our original sample) had moved out, while 183 immigrant households (not in our original sample) moved into the experimental streets.

Table A1: Non-Response and Recontact

	2006 Dwellings		2009 Households
Eligible selected	1,275	Target Households at Follow-up	1,231
Completed	1,193	Completed at follow up	900
Response rate	94%	Household moved	271
		Non-response	56
		Other	4
		Recontact rate	73%
		New households	183
		of which:	
		Subdivision	22
		Substitution	120
		New household	14
		New construction	27
		Completed in 2009	1,083

Eligible-dwelling category excluded plots without a dwelling, unoccupied dwellings, and temporary-use dwellings. The 2006 non-response is in terms of dwellings selected from the frame, and the number of dwellings with completed household survey. The 2009 recontact is in terms of households. The fact that there were 1,231 households in 1,193 dwellings in 2006 means that in some cases there was more than one household per dwelling. "Completed at follow-up" means that at least one member of the household was interviewed in 2006. "New households" means that no member of the household was interviewed in 2006. "Subdivision" means that in 2006 a household member created a new household but occupying the same plot: for example, the son having got married, continued to live in his parent's house but did not share food expenses with them. "Substitutions" means households encountered for the first time in 2009 who occupy the house inhabited by a family interviewed in 2006: for example, the house is rented. "New household" means the interviewed family is still in residence but there is now an additional household: for example, a room in the house is now rented out. "New construction" means households interviewed in 2009 whose residence was constructed since 2006.

A.2 Other Outcomes

Table A2: Pre-Intervention Balance in Means (Other Outcomes)

Variable	ITT= 1	ITT= 0	*Diff.*	Variable	ITT= 1	ITT= 0	*Diff.*
Demographic Indicators				**Labor**			
Household members	4.08	4.08	0.00	Works (=1) ⓘ	0.65	0.63	0.02
	(0.09)	(0.08)	(0.12)		(0.016)	(0.019)	(0.025)
	[487]	[413]	[900]		[1,018]	[911]	[1,929]
Female (=1)ⓘ	0.52	0.54	−0.02	Unemployedⓘ	0.050	0.072	−0.022
	(0.008)	(0.012)	(0.014)		(0.011)	(0.016)	(0.019)
	[1,997]	[1,716]	[3,713]		[595]	[532]	[1,127]
Adult schoolingⓘ	7.79	7.45	0.34	Government welfare	0.069	0.082	−0.013
	(0.44)	(0.32)	(0.54)	program participant	(0.017)	(0.016)	(0.023)
	[916]	[815]	[1,731]		[486]	[411]	[897]
Adult ageⓘ	38.11	38.70	−0.59	**Health**			
	(0.35)	(0.31)	(0.46)	Sick previous month(=1)ⓘ	0.485	0.472	0.013
	[996]	[852]	[1,848]		(0.020)	(0.023)	(0.030)
Home Characteristics					[1,707]	[1,445]	[3,152]
Homeowner (=1)	0.932	0.941	−0.009	Fungus, parasites skin	0.140	0.170	−0.030
	(0.017)	(0.014)	(0.022)	infections (=1)ⓘ	(0.015)	(0.016)	(0.022)
	[486]	[411]	[897]		[1,701]	[1,444]	[3,145]
Number of rooms	2.35	2.38	−0.03	**Schooling (Ages 5-17)**			
	(0.064)	(0.067)	(0.091)	School enrollment (=1)ⓘ	0.956	0.957	−0.001
	[487]	[413]	[900]		(0.009)	(0.012)	(0.015)
Cement roof+	2.17	2.21	−0.04		[496]	[402]	[898]
cement walls +	(0.076)	(0.055)	(0.093)	Absenteeism previous	0.188	0.175	0.013
hard floor $[0-3]$	[483]	[411]	[894]	month (=1)ⓘ	(0.018)	(0.027)	(0.032)
Bathroom inside	0.542	0.577	−0.035		[421]	[322]	[743]
house (=1)	(0.043)	(0.036)	(0.055)	**Public Safety**			
	[483]	[411]	[894]	Burglary in past	0.109	0.113	−0.004
Water connection	0.414	0.467	−0.053	12 months (=1)	(0.016)	(0.015)	(0.022)
inside house (=1)	(0.055)	(0.036)	(0.065)		[483]	[410]	[893]
	[486]	[412]	[898]	Vehicle stolen or	0.069	0.020	0.049
Tap water	0.777	0.789	−0.012	vandalized (12 months)	(0.036)	(0.019)	(0.040)
connection in lot(=1)	(0.050)	(0.044)	(0.066)		[65]	[46]	[111]
	[486]	[412]	[898]	Feels safe walking	0.619	0.612	0.007
Sewerage (=1)	0.851	0.877	−0.026	in street at night	(0.031)	(0.031)	(0.043)
	(0.034)	(0.032)	(0.047)	(=1)	[478]	[410]	[888]
	[486]	[412]	[898]	**Business Units**			
Electricity (=1)	0.978	0.971	0.007	Number of employees	1.78	1.56	0.22
	(0.005)	(0.017)	(0.017)	(including owner)	(0.13)	(0.10)	(0.16)
	[485]	[412]	[897]		[102]	[123]	[225]
Garbage collection (=1)	0.526	0.597	−0.071	Log sales	7.72	7.62	0.10
	(0.055)	(0.061)	(0.081)		(0.14)	(0.12)	(0.19)
	[486]	[413]	[899]		[102]	[123]	[225]
Cleanliness of street (=1)	0.37	0.46	−0.09	Log expenditures	7.19	7.01	0.18
	(0.06)	(0.07)	(0.09)		(0.17)	(0.15)	(0.23)
	[474]	[406]	[880]		[98]	[117]	[215]
Gas truck delivery	0.948	0.914	0.034	Log profits	6.89	6.89	0.00
service (=1)	(0.020)	(0.025)	(0.032)		(0.13)	(0.13)	(0.18)
	[487]	[411]	[898]		[94]	[107]	[201]

ⓘ denotes individual-level outcomes. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Individual variables regarding labor outcomes for individuals aged 18+. Some variable definitions: number of rooms (excluding kitchen, unless it is also used for sleeping). Government welfare programs include: Liconsa, Progresa-Oportunidades, DIF, etc. Significance levels reported only for *Diff*: * significant at 10%; ** significant at 5%; *** significant at 1%.

A.3 Item non-response

Table A3: Item Non-Response Rates

Variable	ITT=1	ITT=0	*Diff.*
Consumption			
Monthly log per	0.019	0.015	0.004
capita expenditure	(0.005)	(0.008)	(0.009)
	[470]	[408]	[878]
Monthly log sum of	0.009	0.002	0.007
itemized expenditures	(0.004)	(0.002)	(0.005)
per capita	[478]	[410]	[888]
Durable goods (0-6)	0	0	0
	–	–	–
	[487]	[413]	[900]
Vehicles (car/truck/motorcycle)	0	0	0
(0-3)	–	–	–
	[487]	[413]	[900]
Home improvements	0	0	0
(0-11)	–	–	–
	[487]	[413]	[900]
Bought materials	0.004	0.007	−0.003
for home improvement (=1)	(0.003)	(0.004)	(0.005)
	[487]	[412]	[899]
Credit			
Collateral-based	0.142	0.134	0.007
credit (=1)ⓘ	(0.012)	(0.012)	(0.017)
	[1215]	[1089]	[2304]
Collateral-based	0.142	0.134	0.007
credit amountⓘ	(0.012)	(0.012)	(0.017)
	[1215]	[1089]	[2304]
Non-collateral	0.142	0.134	0.007
based credit (=1)ⓘ	(0.012)	(0.012)	(0.017)
	[1215]	[1089]	[2304]
Non-collateral based	0.142	0.134	0.007
credit amountⓘ	(0.012)	(0.012)	(0.017)
	[1215]	[1089]	[2304]
Credit from family	0.142	0.134	0.007
and friends (=1)ⓘ	(0.012)	(0.012)	(0.017)
	[1215]	[1089]	[2304]
Informal private	0.142	0.134	0.007
credit (=1)ⓘ	(0.012)	(0.012)	(0.017)
	[1215]	[1089]	[2304]
Credit (continued)			
Credit card (=1)	0.008	0	0.008
	(0.005)	–	(0.005)
	[484]	[410]	[894]
Bank account (=1)	0.004	0	0.004
	(0.003)	–	(0.003)
	[483]	[410]	[893]
Labor and Transportation			
Weekly hours	0.331	0.343	−0.011
workedⓘ	(0.022)	(0.023)	(0.031)
	[743]	[653]	[1396]
Monthly log labor	0.378	0.345	0.033
income ⓘ	(0.023)	(0.025)	(0.034)
	[650]	[585]	[1235]
Plans to migrate	0.056	0.041	0.015
in search of work (=1)	(0.009)	(0.011)	(0.014)
	[456]	[386]	[842]
Cost of taxi to	0.002	0.006	−0.004
city center	(0.002)	(0.005)	(0.005)
	[483]	[409]	[892]
Time to city center	0	0.003	−0.003
(minutes)	–	(0.003)	(0.003)
	[487]	[413]	[900]
Housing			
Log owner estimate	0.099	0.087	0.012
of house value	(0.026)	(0.016)	(0.030)
	[316]	[304]	[620]
Log professional	0.052	0.052	−0.001
appraisal property	(0.021)	(0.017)	(0.026)
	[312]	[266]	[578]
Log professional	0.052	0.052	−0.001
appraisal land	(0.021)	(0.017)	(0.026)
	[312]	[266]	[578]
Nearest paved street	0	0.005	−0.005
(street blocks)	–	(0.005)	(0.005)
	[487]	[413]	[900]

Columns ITT=1 and ITT=0 report item non-response in 2009. *Diff.* column is the estimate of the coefficient on ITT=1 in a regression with dependent variable=1 if the question was not answered. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Individual variables regarding credit and labor outcomes for individuals aged 18+. Variable definitions: Nearest paved street (distance in blocks from the dwelling to the nearest paved street); collateral-based credit (mortgages, home-equity lines, and collateralized bank loans); non-collateralized credit (appliance- and furniture-store credit, bank-card credit, vehicle loans, and *casas de crédito popular*); informal credit (credit from informal lenders); credit card (bank account): indicator that someone in the household has a credit card (bank account); monthly log per capita expenditure (based on total self-reported expenditure in the household); monthly log sum of itemized expenditures per capita (based on the sum of household expenditures on food, phone, gas, electricity, education, rent/mortgage, clothes, and entertainment); durable goods (sum of indicators for refrigerator, washing machine, microwave oven, air conditioning, video player, and computer); vehicles (sum of indicators for car, truck, and motorcycle); home improvements (sum of indicators for improvements in flooring, walls, roofing, sewerage connection, plumbing, toilets, electrical installations, room construction, remodeling, security measures, and improvements to house front); materials purchased for home improvements (in the previous 6 months); weekly hours worked (in the previous week); plans to migrate in search of work (if someone in the household is expected to migrate in search of work); cost of taxi to city center (self-reported cost of a taxi from home to city center); time to city center (self-reported time to commute from home to city center). Significance levels reported only for *Diff*: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A4: Impacts of Pavement (Other Outcomes)

Variable	ITT	2SLS	Mean Control (2009)	Variable	ITT	2SLS	Mean Control (2009)
Home Characteristics				Government welfare	−0.003	−0.004	0.033
Homeowner (=1)	−0.011	−0.019	0.954	program participant	(0.012)	(0.019)	(0.009)
	(0.009)	(0.015)	(0.014)		[897]	[897]	[411]
	[897]	[897]	[411]	**Health**			
Number of rooms	−0.009	−0.015	2.43	Sick previous month(=1)ⓘ	−0.005	−0.009	0.523
	(0.085)	(0.139)	(0.079)		(0.025)	(0.040)	(0.017)
	[900]	[900]	[413]		[3,152]	[3,152]	[1,445]
Cement roof+	−0.010	−0.016	2.25	Fungus, parasites skin	0.006	0.010	0.167
cement walls +	(0.036)	(0.059)	(0.047)	infections (=1)ⓘ	(0.022)	(0.037)	(0.017)
hard floor $[0-3]$	[894]	[894]	[411]		[3,145]	[3,145]	[1,444]
Bathroom inside	0.009	0.014	0.561	**Schooling (Ages 5-17)**			
house (=1)	(0.037)	(0.060)	(0.037)	School enrollment (=1)ⓘ	0.018	0.029	0.841
	[894]	[894]	[411]		(0.020)	(0.033)	(0.016)
Water connection	0.015	0.024	0.522		[898]	[898]	[402]
inside house (=1)	(0.035)	(0.056)	(0.038)	Absenteeism previous	0.039	0.064	0.132
	[898]	[898]	[412]	month (=1)ⓘ	(0.035)	(0.056)	(0.023)
Tap water	0.015	0.024	0.793		[743]	[743]	[322]
connection in lot(=1)	(0.030)	(0.047)	(0.035)	**Public Safety**			
	[898]	[898]	[412]	Burglary in past	0.030	0.050	0.060
Sewerage (=1)	−0.004	−0.007	0.930	12 months (=1)	(0.019)	(0.033)	(0.012)
	(0.026)	(0.042)	(0.022)		[893]	[893]	[410]
	[898]	[898]	[412]	Vehicle stolen or	0.005	0.007	0.094
Electricity (=1)	0.014	0.023	0.967	vandalized (12 months)	(0.055)	(0.072)	(0.044)
	(0.012)	(0.020)	(0.019)		[111]	[111]	[46]
	[897]	[897]	[412]	Feels safe walking	0.029	0.048	0.623
Garbage collection (=1)	0.015	0.025	0.707	in street at night	(0.043)	(0.067)	(0.028)
	(0.055)	(0.088)	(0.053)	(=1)	[888]	[888]	[410]
	[899]	[899]	[412]	**Business Units**			
Gas truck delivery	−0.031	−0.051	0.940	Number of employees	−0.03	−0.05	1.67
service (=1)	(0.024)	(0.044)	(0.024)	(including owner)	(0.14)	(0.22)	(0.10)
	[898]	[898]	[411]		[248]	[248]	[125]
Cleanliness of street (=1)	0.11***	0.19***	0.73	Log sales	−0.09	−0.14	7.71
	(0.04)	(0.06)	(0.03)		(0.16)	(0.25)	(0.13)
	[880]	[880]	[406]		[247]	[247]	[124]
Labor				Log expenditures	0.09	0.15	7.19
Works (=1) ⓘ	−0.015	−0.024	0.64		(0.15)	(0.24)	(0.13)
	(0.022)	(0.035)	(0.017)		[243]	[243]	[124]
	[1,929]	[1,929]	[911]	Log profits	−0.05	−0.07	6.85
Unemployedⓘ	−0.002	−0.004	0.075		(0.15)	(0.22)	(0.11)
	(0.019)	(0.031)	(0.014)		[207]	[207]	[105]
	[1,127]	[1,127]	[532]				

ⓘ denotes individual-level outcomes. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Individual variables regarding labor outcomes for individuals aged 18+. Some variable definitions: number of rooms (excluding kitchen, unless it is also used for sleeping). Government welfare programs include: Liconsa, Progresa-Oportunidades, DIF, etc. Significance levels reported only for 2SLS and TOT: * significant at 10%; ** significant at 5%; *** significant at 1%.

A.4 Testing for complementarities using 2SLS

Table A5: Testing for complementarities using 2SLS

	$\frac{log(C)}{log(H)}$	$\frac{V}{log(H)}$	$\frac{V}{log(C)}$
$\widehat{b_i}$	0.008	0.010*	0.011*
	(0.012)	(0.006)	(0.007)
N	[381]	[395]	[864]

Bootstrapped standard errors (200 replications) clustered at the pavement-project level in parentheses. Models include a constant and the baseline outcome. Significance levels: * significant at 10%; ** significant at 5%; *** significant at 1%.

A.5 Multiple Testing

In any experimental evaluation with multiple outcomes, significant effects may emerge simply by chance. The larger the number of tests, the easier it is to make the mistake of thinking that there is an effect when there is none, i.e., "Type I" error. The problem is well-known in the theoretical literature (Romano and Wolf, 2005), and it has recently received some attention in the policy evaluation literature (Kling, Liebman, and Katz, 2007; Anderson, 2008).

Multiple-testing correction procedures adjust the individual p-values for each outcome to keep the overall error rate to less than or equal to the user-specified p-value cutoff or error rate. The default correction procedure is the Benjamini and Hochberg False Discovery Rate (Benjamini and Hochberg, 1995). It is the least stringent among the standard corrections, such as Bonferroni or Holm (1979), and provides a good balance between discovery of statistically significant outcomes and limitation of false positive occurrences. We have computed False Discovery Rates (FDR) using all three adjusted p-values.[26]

Our Benjamini and Hochberg p-values show significant ITT and 2SLS effects on appraised home value, appraised land value, distance to the nearest paved street, cleanliness of the street, and satisfaction with the local government. All of these measures survive the Holm (1979) and Bonferroni corrections. Note that the Bonferroni correction is the most stringent test of all, offering the most conservative approach to control for false positives. However, it does so at the cost of a very high rate of false negatives (outcomes are not statistically affected by the experiment when in reality they are). The fact that we find statistically significant effects even under the most stringent multiple-testing corrections suggests that the statistical significance levels of the reported impacts are not due to "Type I" errors.

[26] Given R outcomes and their unadjusted p-values, p_r for each $r = \{1, ..., R\}$, Bonferroni adjusted p-values are calculated as $Bp_r = min\{R \cdot p_r, 1\}$. Holm adjusted p-values are computed by ordering the unadjusted p-values for the R outcomes $p_1 < p_2 < ... < p_R$ and calculating $Hp_1 = min\{R \cdot p_1, 1\}$, $Hp_2 = min\{max\{Hp_1, (R-1) \cdot p_2\}, 1\}$, $Hp_3 = min\{max\{Hp_2, (R-2) \cdot p_3\}, 1\}$, etc. Finally, Benjamini and Hochberg p-values also order p-values ($p_1 < p_2 < ... < p_R$) and are calculated as $BHp_R = p_R$, $BHp_{R-1} = min\{BHp_R, \frac{R}{(R-1)} \cdot p_{R-1}\}$, $BHp_{R-2} = min\{BHp_R, \frac{R}{(R-2)} \cdot p_{R-2}\}$, etc.