The microeconomic effects of infrastructure: Experimental evidence from street pavement *

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Abstract

We design an infrastructure experiment in Mexico to evaluate the impact of first-time asphalting of inhabited residential streets on property values and household outcomes. We find substantial impacts on housing values (+16%) and land values (+54%). At the household level, street paving increased the use of collateral-based credit, and had strong effects on vehicle ownership (+43%), durable goods (+12%) and home improvements (+100%). We present a model to disentangle wealth effects from complementarities between vehicles and home improvements with street pavement. The results show that vehicles and street pavement are complementary goods, and are consistent with street pavement and home improvements being complements in the production of housing services. Finally, we provide a lower bound for the benefits of street pavement which represent 109% of the construction costs.

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1 Introduction

Today's developing countries are urbanizing at a much more rapid pace than that of their predecessors (Henderson, 2002). Large portions of the population in metropolitan areas throughout the developing world do not benefit from basic urban equipment such as piped water, electricity, sewerage lines, and asphalted roads (UN-Habitat, 2003). Because of the substantial opportunity costs involved in providing infrastructure, it is crucial to understand what the effects and benefits of infrastructure actually are.

While there is an extensive literature assessing the effects of infrastructure on economic outcomes, both at the macro (Haughwout, 2002; Donaldson, 2010) and micro level (Dinkelman, 2010; Duflo and Pande, 2007), none of the previous studies provides experimental evidence. The goal of this paper is twofold: first, to identify the causal effects of first-time asphalting of streets in inhabited residential neighborhoods[1] by means of a randomized experiment. Second, to shed light on how public infrastructure provision affects household behavior. A visual exposition of treatment is given in Figures 1 and 2 which show the same street before and after it has been paved.

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. From the public works office's set of 56 candidate street asphalting projects, we randomly selected half to be treated with pavement, which allows us to address the problem of selection bias inherent in infrastructure placement: A simple comparison of places with and without infrastructure in observational data is misleading, since infrastructure is normally allocated to places that provide the highest returns (Duflo and Pande, 2007; Van de Walle, 2002).

Our experimental estimates show that street pavement generated substantial increases in the value of properties abutting paved streets: according to professional appraisals, residential property values increased by 16%, with a corresponding increase in land values of

[1]Also known as road surface or pavement.

54%. According to homeowners' estimates, street pavement increased housing values by 25%. We corroborate the validity of these measures with two other pieces of market-value information.[2] These results quantify for the first time the effect of street pavement on property values in a context in which neither property tax rates nor valuations were updated as a result of the intervention. This allows us to interpret the estimates as measuring the willingness to pay for properties on paved streets, which contrasts with Cellini, Ferreira and Rothstein (2010), who have shown that, when local public goods are financed with property taxes, the marginal effect of public-good provision on property values is, in the optimum, zero.

At the household level, we find that pavement had important positive effects on credit use, durable-goods, vehicle ownership, and home improvements. Collateral-based credit use from financial institutions more than doubled, and loan size went from an average of 135 pesos to 1,508 pesos per adult.[3] Durable goods increased by 12%, while vehicle ownership increased by more than 40%. Finally, home improvements doubled. Non-durable good consumption, labor supply and labor income did not change.

Why did households living along paved streets change their consumption decisions? In the second part of the paper we use a standard economic model to help us understand how households should react to street pavement. The two main forces operating in the model are: the wealth effect due to property value increases and the complementarities of vehicle ownership and home improvements with street pavement. Our model has three predictions regarding the marginal rates of substitution between consumption, housing and vehicle services. If we assume a Cobb-Douglas utility function, testing the model amounts to running three regressions of three ratios (consumption to housing, vehicles to housing, and vehicles to consumption) on a street pavement indicator. The model's predictions are confirmed in

[2]Rents increased by 31% in paved streets; and for the houses that were purchased between the baseline and follow-up surveys ($N = 29$) the price paid was 85% higher on paved streets, although the coefficient is insignificant ($p = 0.49$) on account of the limited number of observations.

[3] The 2009 PPP exchange rate was 8.5 pesos to the U.S. dollar, and the nominal February 2009 exchange rate was 14.6 pesos to the U.S. dollar. Other forms of credit, including credit from family and friends or the government, did not respond to pavement provision.

the data: the mean consumption-housing ratio is the same for households in paved and unpaved streets, while the mean vehicle-housing and vehicle-consumption ratios are higher for households in paved streets; indicating that vehicle and street pavement are complements. Additionally, under the assumptions of the model, the increase in home improvements we observe can be understood as reflecting its complementarity with street pavement in the production of housing, and not a wealth effect.

Our paper concludes with a lower bound for the benefits of street pavement obtained 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 benefits to cost ratio of 1.09. This suggests that 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.

The structure of paper is as follows. Section 2 describes the design and context of the study. Section 3 presents the experimental results. Section 4 lays out the model and tests its predictions. Section 5 provides a cost-benefit analysis. Finally, Section 6 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. For example, Figure 1 shows how the plots along experiment streets were fully occupied by houses before the intervention. This situation is common throughout Mexico and other Latin American 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. Furthermore, non-payment of the property tax is not subject to prosecution.

2.2 The Experiment

The intervention consists of first time paving 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). 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. Fieldwork confirmed that congestion was not a concern - something we expected given the residential nature of the streets. Given its benefits, a valid question is why the market does not provide street pavement to begin with. The 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 the administration had an interest in paving. These were all contiguous street segments that connected with the existing city pavement grid. To be considered eligible, a street had to be unpaved. Pavement projects the administration had an interest in were characterized by relatively high population densities and ranged from 160 to 1,000 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. Figure 3 shows the location of those streets assigned to the treatment group ($Z = 1$) and those assigned to the control group ($Z = 0$). Crucially, the pavement program was not accompanied by other government programs.

It is important to bear in mind that every municipal administration in Acayucan allocates a portion of its budget to street paving. This means that any unpaved street has a positive probability of being paved, although it may mean waiting many years – 15 was the median in our sample. Because the municipality did not announce to the population the list of experimental street projects, the expectations of the control group as to the likelihood of being selected in the near future were unaffected.

Furthermore, the identity of the street projects to be paved was not immediately revealed to the population because budgeting is performed annually, whereas the study would take place over three years. 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. Participation in the program was thus revealed to neighbors with the arrival of measurement teams and eventually construction crews and machinery. This eliminates potential biases from anticipation effects in the housing values at baseline. Finally, streets not selected for pavement did not receive any form of compensation.

By February 2009, right before our follow-up survey, 17 of the streets in the treatment group had been completely paved, two projects were under way, and nine had yet to begin. The municipal government attributed the delays to foul weather and various technical difficulties. On the other hand, the administration did fulfill the requirement of not paving those streets assigned to the control group. We deal with the implications of this for our estimates in the identification section below.

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.[4] 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.

The target population of the survey consisted of all occupied residential structures on the streets that were selected for the experiment.[5] The baseline survey was administered to 1,231 households living in 1,193 dwellings, with a response-rate of 94%.[6] In 2009, 1,083 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.[7]

The household questionnaire collects detailed information for each individual in the

[4] A full description of the ASLS can be found in Gonzalez-Navarro and Quintana-Domeque (2010). We also obtained information on business units from a brief business census conducted in 2006 and 2009.

[5] We created a sampling frame from all inhabited residential dwellings in January 2006. As Deaton (1997) recognized, 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.

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

[7] 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, 2009). We sampled at a rate of 70% in the intent-to-treat (ITT) group and at a rate of 50% in the control group.

household (age, sex, educational attainment, labor supply, etc.) and characteristics at the household level (per capita expenditure, durable goods, vehicle ownership, home investments, etc.). 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.

Participants in the study (household respondents and the professional appraiser) were not aware of the ultimate objective of the survey/appraisals.[8] 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 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 taxes.[9] Even for properties registered as having been sold, in many cases there is a substantial lag between the date of the transaction and the date of registry. It seems that it is often the case that an individual buys and moves into a house and only later pays the transaction taxes and registers as its owner. 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 ob-

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

[9] Our conversations with local public notaries and municipal authorities revealed that since valuation for property-tax purposes is set as the maximum between the last declared transaction price and the very conservative property-tax assessed value, individuals do not usually report the actual amount paid for the property.

tained 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 who works for local banks and is also a real-estate agent in the city. We used the services of the same agent in 2006 and 2009 in order to minimize heterogeneity of assessment practices. 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, on account of budgetary constraints.[10]

The second main source of property valuation we obtained were homeowners' self-reports. Self-reported home values have long been used for research on housing economics (in developed countries see Kish and Lansing, 1954; Kain and Quigley, 1972; Goodman and Ittner, 1992; Kiel and Zabel, 1999; Bucks and Pence, 2006; Davis 2010; in developing countries see Jimenez, 1982). This literature concludes that the evolution of self-reported housing prices 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) have shown 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.[11] This suggests the professional appraiser's

[10]The appraiser did not enter the properties because piloting revealed that non-participation would be extremely high and previous research has shown that entering the property does not increase accuracy (Kish and Lansing, 1954).

[11] In the 2009 sample the mean difference between log appraised value and log homeowner valuation is

valuation is a better indicator of market value.

Finally, we validate the professional appraisals and self valuations with the limited number of market transactions data in our sample by asking recent buyers – those arriving between baseline and follow-up – how much they paid for the property. We also ask recent renters how much they pay per month in rent. This last measure has the advantage that we can check for balance of the rent being paid on the same properties at baseline.

2.3.2 Credit and Consumption Measures

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). 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.[12] Consumption of durable goods is measured according to two indices: one of vehicle ownership (the sum of automobile, truck, and motorcycle) and one of household durables (the sum of refrigerator, washing machine, microwave oven, air conditioning, video player, and computer).

−0.39 for the whole sample, and only −0.04 for short-tenure homeowners.

[12] Interviewers were asked to perform a consistency check permitting them to verify that total reported expenditures were at least as large as the sum of itemized expenditures.

2.3.3 Other Measures

The ASLS also contains information on labor supply (households respondents were asked, for instance, to specify the number of hours each working adult worked per day/per week and the income earned there from), 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 (symptoms of poor health among household members over the previous year), and children's schooling (school enrollment and absenteeism).

2.4 Identification

In line with the established impact-evaluation literature (e.g., Kling, Liebman, and Katz, 2007), we estimate intent-to-treat (ITT) and treatment-on-treated (TOT) effects. Let Y_{2009} be the outcome of interest in 2009 and let Z be an indicator for pavement-group assignment. We estimate

$$Y_{2009} = \alpha_0 + \alpha_1 Z + \alpha_2 Y_{2006} + \epsilon_1 \quad (1)$$

where Y_{2006} is included to improve precision. The ITT parameter is α_1 in equation (1).

We estimate the TOT effect using pavement group assignment as an instrumental variable for the street being paved, so Z is the excluded instrument for an indicator D of being paved in the two stage least squares (2SLS) estimation:

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

The TOT parameter β_1 is equal to the ITT parameter divided by the regression-adjusted compliance rate – the fraction of observations that were finally paved among those originally selected to be paved. We interpret these 2SLS estimates as treatment-on-treated estimates because paving was not applied to streets in the control group. In other words, there is only one-sided non-compliance (Bloom, 1984; Angrist, Imbens, and Rubin, 1996). The TOT is *identified* under the assumption that there was no average effect of pavement-group assignment on those households living on streets that were not finally paved (we provide evidence below that this assumption holds). We cluster standard errors at the street-pavement-project level and use survey weights.[13] Given that attrition was uncorrelated with treatment, the analysis for household outcomes refers to families interviewed both at baseline and followup. The appendix contains a detailed analysis of family migration to and from experiment streets.

3 Experimental Evaluation

3.1 Baseline Balance

Table 2 presents average baseline characteristics by treatment status for our main outcome variables.[14] The table shows that randomization was successful in balancing pre-treatment characteristics across both the intent-to-treat and the control groups.[15] We assessed a total of 50 variables and found evidence of balanced characteristics across the groups. The only variables that were significantly different were labor income and non-collateral based credit amount at the 10% significance level.

3.2 Pavement Impacts on Property Values

We begin by presenting, in the three columns of Table 2, our main experimental estimates for the effect of street pavement on home and land values. ITT and TOT 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. Using the professional-appraisal measures, we find that pavement

[13] 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.

[14] Additional outcomes can be found in Table A5 in the Appendix.

[15] 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.

increased home values by 16% and land values by 54%. Using homeowners' valuations, we estimate that street pavement increased property values by 25%. The fact that two independent measures of property value move in the same direction suggests that paving accounts for substantial increases in value.[16]

To our knowledge, these are the first experimental estimates of the impact of this type of basic urban infrastructure on property values. With respect to the literature on hedonic estimates of the value of amenities (see Rosen, 1974; the identification criticisms in Brown and Rosen, 1982; and Kanemoto, 1988), our estimates have an advantage in that we randomized the amenity at stake, meaning that we can estimate the increase in capital gains of homeowners due to street pavement. This represents the amount the owners would be willing to pay for the change: the simplest case for a hedonic welfare measurement (Palmquist, 2005).

The previous interpretation requires a clarification: Homeowners who obtained pavement did not see an increase in their property taxes. This scenario contrasts with a situation in which identical neighbors vote on the amount of public goods to be financed by property taxes: in equilibrium, a marginal increase in the public good has no effect on average property values because the public good's valuation is exactly offset by the tax increase (Cellini, Ferreira and Rothstein, 2010).[17]

Table 2 also shows that rents on treated streets were 31% higher than rents on control streets, controlling for rent paid by the previous renter for the same property, and that the prices paid for recently purchased houses on ITT and paved (TOT) streets are 44% and 85% higher than in the control group, although it must be conceded that, because the sample was small, this estimate is very imprecise. However, both supplemental indicators corroborate the estimates obtained from appraisals and homeowner valuations, and even suggest that the most conservative impact estimates are those given by the appraiser.

[16] The difference in the magnitudes of the estimated impacts is not statistically significant. The standard error of the estimated impact using homeowners valuation is three times higher than the one corresponding to the estimated impact using the professional-appraisal valuation.

[17] See also Brueckner (1982) and Haughwout (2002).

3.3 Pavement Impacts on Household Outcomes

3.3.1 Credit and Consumption

We now turn to the effects of street-pavement on household outcomes, focusing first on credit use and consumption. Table 3 shows that pavement increased 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 1,508 pesos among the treated: a more than tenfold increase. While this is an important finding, we do not have the information needed to determine whether the increase in collateral-based credit use is due to an increase in either the demand or the supply of credit.[18] Note that 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. We also find a seven-percentage-point increase in bank accounts over a control-group rate of 16%. The effect is close to being statistically significant.

Consumption effects are reported in Table 4. Street-pavement provision had a strong positive effect on the number of durable goods owned by the household: out of six durable goods, control households had an average of 2.4 goods, while the mean for households on paved streets was 2.7 goods (12% higher). There was 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. In terms of recent home investments, we find 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

[18] See Field and Torero (2004).

to flooring, plumbing, electrical installations, toilets, room remodeling, and air conditioning. Furthermore, 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 23% among the treated group).

Finally, we find that the provision of street pavement had no 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.[19]

3.3.2 Transportation and Labor

We measure the impact of street pavement on transportation costs in Table 5 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 nor statistically significant.

Similarly, we find no effect on labor outcomes either in terms of labor supply or earnings (in the appendix 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.

3.4 Robustness

3.4.1 The role of expectations and indirect effects on housing value impact estimates

Identification of the TOT effect is based on the assumption that there was no average effect of pavement-group assignment on those households living in places that were not finally paved. One potential concern is that non-compliers, i.e., people living along the ITT streets that were not finally paved, knew that their street had been selected and modified their

[19] Note that we do not have liquid saving measures.

estimates of their property's value even before the streets were paved. The appraiser could have updated his estimations of such properties as well. In any 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". Table 6 shows that there was no increase 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 prices did not change. This is also consistent with the local government's not announcing which streets were in the ITT group.

Our estimates provide lower bounds of the effect on housing if there are any indirect treatment effects. As long as distance to the nearest paved street is a determinant of housing value, households in the control group may have benefitted from the pavement provided to the treated group. This would be the case if a pavement project in the treatment group reduced the distance to the nearest paved street for some homes in the control group. In fact, we observe an average reduction of 0.68 street blocks among the control group between baseline and follow-up, and increases in home value among the control group that are correlated with the reduction in distance to the nearest paved street (Figure 4). This suggests that there is a downward bias to our estimates of the impact on property values.

In an attempt to assess the importance of indirect-treatment effects, we estimate the following model for the group of homes in the control group (ITT=0):

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

where Y_{2009} is appraisal's home valuation in 2009, and Δd is the change in distance to the nearest paved street between 2006 and 2009. The indirect-treatment effect is captured with γ_1. Our estimate for γ_1 is -0.034 (se=0.026). A decrease of one street block in the distance between a given house and the pavement grid was correlated with a 3% higher housing value.

3.4.2 Heterogenous effects on property values by pavement network intensity

In our data, there is heterogeneity in the proportion of paved streets surrounding the street projects. In particular, we noted before that in the central area of the city, as opposed to the outskirts, most streets were paved. In Table 7 we present separate estimates for the impact of pavement on home values for these two groups. We find that the effect of pavement is higher in the areas with a relatively low proportion of paved streets; i.e., when the surrounding streets are less likely to be paved, the impact of street pavement is higher. This suggests that the marginal private benefit of paving a street is higher in a low-pavement area than in a high-pavement area. This fact may be relevant for distributional concerns in pavement-allocation decisions. However, note that this does not address the question of what the socially optimal allocation of pavement in this context is. For one thing, policymakers would like to know if social returns are higher when paving some streets in all neighborhoods rather than all streets in some neighborhoods.

3.4.3 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.[20]

Our Benjamini and Hochberg p-values show significant ITT and TOT effects on appraised home value, appraised land value, distance to the nearest paved street, and cleanliness of the street. 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 impacts reported in this study are not due to "Type I" errors.

4 Infrastructure, Home Values and Household Outcomes

While the previous analysis is crucial to assess the causal effects of street pavement on distinct outcomes, a fundamental question remains: what can we learn from our experimental estimates? This section presents a simple economic model to understand what drives the observed changes in household behavior, namely significant increases in home improvements and vehicle acquisition, but negligible (if any) effects on per capita expenditure (non-durable consumption). The starting point in the model is the sizeable effect that street pavement has on home values. Because the primary residence typically constitutes the single most important depository of wealth for homeowners (e.g., Campbell and Cocco, 2007), changes in its asset value can be expected to have important consumption effects. This view is

[20] 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.

consistent with the observation in previous literature that the principal beneficiaries of public infrastructure investment are property owners (Haughwout, 2002).

What are the key mechanisms to be analyzed in the model? Street paving caused a raise in values of abutting properties, which could have two possible effects: (1) households are wealthier, and, to the extent that they can borrow against their housing wealth, increase their "consumption", broadly defined; (2) the return on investment in durable inputs (materials for home improvements) or durable goods (vehicle ownership) complementary with street-paving increased.

Before presenting the model formally, let us summarize briefly its main elements. A representative household derives utility from (non-durable) consumption, housing services, and vehicle services. Housing services is the fraction of housing wealth consumed by the household, which depends positively on private investments and street pavement. Vehicle services is a function that depends positively on the number of vehicles and street pavement. We allow for complementarities (substitutabilities) between street pavement and vehicles or private investments.

The household can decide to consume all its housing wealth in the form of housing services, and then its budget constraint remains the same after street pavement, or only part of it and selling/renting the other fraction (from currently renting a room to selling the entire house in the future), in which case the income to be spent on (non-durable) consumption, home improvements and vehicles increases.

The model provides three propositions regarding the marginal rates of substitution between consumption, housing and vehicle services. First, the marginal rate of substitution between consumption and housing services does not depend on street pavement. Second, the marginal rates of substitution between vehicle and housing services/consumption depends on street pavement as long as the marginal utility of vehicle services depends on street pavement. Finally, the price of private investment equals the value of the marginal product of private investment: production and consumption housing decisions are separable.

If we assume a Cobb-Douglas utility function, testing the model amounts to running three regressions of three ratios (consumption to housing, vehicles to housing, and vehicles to consumption) on a street pavement indicator. The Cobb-Douglas functional form just buys us the ratio part, while randomization of street pavement addresses identification concerns. In particular, our model predicts that the mean consumption-housing ratio is the same for households in paved and unpaved streets, while the mean vehicle-housing and vehicle-consumption ratios are higher for households in paved streets if vehicle and street pavement are complements.

4.1 Model

Let the representative homeowner maximize a utility function $U(C, \widehat{H}, V)$ that depends positively on (non-durable) consumption C, housing services $\widehat{H}$, and vehicle services V. Strict concavity and twice differentiability of the utility function is assumed.

It is conceptually important to distinguish current housing consumption choice - which may vary from period to period - from the asset value of the house the family owns. Pavement has a large immediate impact on the asset value of the house, and hence on the family's wealth position. And although the increased housing services may be all consumed in the short run, this is not necessarily the case in the long run. For example, a family may optimally increase current consumption through dissaving or debt and after a few years sell the house to pay off debts and realize the capital gains. Only in case the homeowner decides to live in the same higher value house forever would we expect no impact on other types of consumption. In other words, localized property value increases have very different implications for housing wealth effects than generalized property value increases, as in Muellbauer (2007). We implement the separation between asset value of the house and current housing consumption decision by assuming that the amount housing services *consumed* is a fraction of housing wealth: $\widehat{H} = (1 - \mu)rH$. The fraction μ is spent on non-housing goods. 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) is a function of vehicle ownership v and public investment D, so that $V = V(v; D)$.[21]

The household budget constraint is given by:

$$m + rp_h H = C + p_i i + p_v v + (1 - \mu) rp_h H \tag{2}$$

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, vehicles v, and housing services $(1 - \mu)rH$. We abstract from depreciation because it plays no role in the analysis. Given that housing is both an asset and a consumption good, (2) simplifies to:

$$m + \mu rp_h H = C + p_i i + p_v v \tag{3}$$

The Lagrangian for the household problem is:

$$\mathcal{L}(C, i, v, \mu, \lambda) = U(C, (1 - \mu)rH(i; D), V(v; D)) + \lambda[m + \mu rp_h H(i; D) - C - p_i i - p_v v] \tag{4}$$

The first-order conditions are given by:

$$\frac{\partial U(C^*, (1 - \mu^*)rH(i^*; D), V(v^*; D))}{\partial C} = \lambda^* \tag{5}$$

$$\frac{\partial U(C^*, (1 - \mu^*)rH(i^*; D), V(v^*; D))}{\partial \widehat{H}} = p_h \lambda^* \tag{6}$$

[21] For expositional purposes, we will treat D as a continuous parameter whenever it is convenient.

$$\frac{\partial U(C^*,(1-\mu^*)rH(i^*;D),V(v^*;D))}{\partial \widehat{H}}\frac{\partial H(i^*;D)}{\partial i}(1-\mu^*)r = \lambda^*\left[p_i - \mu^* r p_h \frac{\partial H(i^*;D)}{\partial i}\right] \quad (7)$$

$$\frac{\partial U(C^*,(1-\mu^*)rH(i^*;D),V(v^*;D))}{\partial V}\frac{\partial V(v^*,D)}{\partial v} = \lambda^* p_v \quad (8)$$

$$m + \mu^* r p_h H(i^*;D) = C^* + p_i i^* + p_v v^* \quad (9)$$

The marginal rate of substitution (MRS) between housing services $r\widehat{H}$ and consumption C is given by (5) and (6):

$$MRS_{\widehat{H},C} = -\frac{\frac{\partial U(C^*,(1-\mu^*)rH(i^*;D),V(v^*;D))}{\partial \widehat{H}}}{\frac{\partial U(C^*,(1-\mu^*)rH(i^*;D),V(v^*;D))}{\partial C}} = -p_h \quad (10)$$

So that the MRS in equilibrium does not depend on D.

By the same token, MRS between housing services and vehicle ownership services is given by (6) and (8):

$$MRS_{\widehat{H},V} = -\frac{\frac{\partial U(C^*,(1-\mu^*)rH(i^*;D),V(v^*;D))}{\partial \widehat{H}}}{\frac{\partial U(C^*,(1-\mu^*)rH(i^*;D),V(v^*;D))}{\partial V}} = -\frac{p_h}{p_v}\frac{\partial V(v^*;D)}{\partial v} \quad (11)$$

In contrast, the MRS between $\widehat{H}$ and V in equilibrium depends on public infrastructure if D changes the marginal utility of vehicle services. To see this, take the derivative of the RHS of (11) with respect to D:

$$\frac{\partial MRS_{H,V}}{\partial D} = -\frac{p_h}{p_v}\frac{\partial^2 V(v^*;D)}{\partial v \partial D} \quad (12)$$

where $\frac{\partial^2 V^*}{\partial v \partial D} > 0$ if complements, and < 0 if substitutes.

Similarly, the MRS between consumption and vehicle services is given by equations (5) and (8):

$$MRS_{C,V} = -\frac{\frac{\partial U(C^*,(1-\mu^*)rH(i^*;D),V(v^*;D))}{\partial C}}{\frac{\partial U(C^*,(1-\mu^*)rH(i^*;D),V(v^*;D))}{\partial V}} = -\frac{1}{p_v}\frac{\partial V(v^*;D)}{\partial v} \quad (13)$$

Again, the MRS between consumption and vehicle services depends on public infrastructure if vehicle ownership and street pavement are "complements"

Finally, note that combining (6) and (7) we obtain

$$p_i = p_h r \frac{\partial H(i^*; D)}{\partial i} \tag{14}$$

This is the efficiency condition for housing investments. It is interpreted as pinning down the level of private investment i^* such that the marginal cost of an additional unit of i equals its marginal benefit. This optimality condition depends on the technology used to produce housing services but not on household characteristics. Totally differentiating this expression with respect to i and D yields:

$$\frac{di}{dD} = -\frac{\frac{\partial^2 H}{\partial i \partial D}}{\frac{\partial^2 H}{\partial i^2}} \tag{15}$$

This condition provides a clear interpretation for our experimental finding that pavement generates an increase in housing investments. According to equation (15), the increase in housing investment is not due to a wealth effect, but rather to pavement increasing the marginal rate of return to investments in the house. In other words, our model suggests that the increase in home investments is a result of the complementarity between street pavement and home improvements. This new finding can be relevant for future work in light of recent studies which typically ignore the complementarity between public and private investments in housing production models (e.g. Rossi-Hansberg, Sarte and Owens III, 2010) .

4.2 Empirical implementation

Our model is tested on the basis that the household's utility function has a Cobb-Douglas form:

$$U(C, \widehat{H}, V) = C^{\alpha} \widehat{H}^{\beta} V^{1-\alpha-\beta} \tag{16}$$

where α, $\beta > 0$ and $\alpha + \beta < 1$. Hence, the optimality conditions imply the following ratios at the optimizing bundle:

$$\frac{C^*}{\widehat{H}^*} = \frac{\alpha}{\beta} p_h \tag{17}$$

$$\frac{V^*}{\widehat{H}^*} = \left[\frac{1-\alpha-\beta}{\beta}\right]\left[\frac{p_h}{p_v}\right]\frac{\partial V(v^*;D)}{\partial v} \tag{18}$$

$$\frac{V^*}{C^*} = \left[\frac{1-\alpha-\beta}{\alpha}\right]\left[\frac{1}{p_v}\right]\frac{\partial V(v^*;D)}{\partial v} \tag{19}$$

In our application, D is a discrete binary variable. Hence, if we compare these ratios for a household in two different scenarios, with and without pavement, we have:

$$\left.\frac{C^*}{\widehat{H}^*}\right|_{D=1} - \left.\frac{C^*}{\widehat{H}^*}\right|_{D=0} = 0 \tag{20}$$

$$\left.\frac{V^*}{\widehat{H}^*}\right|_{D=1} - \left.\frac{V^*}{\widehat{H}^*}\right|_{D=0} = \left[\frac{1-\alpha-\beta}{\beta}\right]\left[\frac{p_h}{p_v}\right]\left[\frac{\partial V(v^*;1)}{\partial v} - \frac{\partial V(v^*;0)}{\partial v}\right] \tag{21}$$

$$\left.\frac{V^*}{C^*}\right|_{D=1} - \left.\frac{V^*}{C^*}\right|_{D=0} = \left[\frac{1-\alpha-\beta}{\alpha}\right]\left[\frac{1}{p_v}\right]\left[\frac{\partial V(v^*;1)}{\partial v} - \frac{\partial V(v^*;0)}{\partial v}\right] \tag{22}$$

where (21) and (22) are positive if and only if $\left[\frac{\partial V(v^*;1)}{\partial v} - \frac{\partial V(v^*;0)}{\partial v}\right] > 0$, i.e., D and v are complements in V. In other words, for a Cobb-Douglas utility function (or *any* quasi-homothetic utility function), our model predicts that the ratio between housing and non-durable consumption should not be affected by pavement. Instead, the vehicle to housing and vehicle to non-durable consumption ratios should increase if vehicle marginal utility raises with pavement.

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

$$\frac{C_{h,s}}{\widehat{H}_{h,s}} = a_1 + b_1 D_s + \varepsilon_{h,s,1} \tag{23}$$

$$\frac{V_{h,s}}{\widehat{H}_{h,s}} = a_2 + b_2 D_s + \varepsilon_{h,s,2} \tag{24}$$

$$\frac{V_{h,s}}{C_{h,s}} = a_3 + b_3 D_s + \varepsilon_{h,s,3} \tag{25}$$

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. ie: $a_1 = E\left[\frac{\alpha}{\beta}p_h\right]$,
$b_2 = E\left[\left[\frac{1-\alpha-\beta}{\beta}\right]\left[\frac{p_h}{p_v}\right]\left[\frac{\partial V(v^*;1)}{\partial v} - \frac{\partial V(v^*;0)}{\partial v}\right]\right]$,
and $b_3 = E\left[\left[\frac{1-\alpha-\beta}{\alpha}\right]\left[\frac{1}{p_v}\right]\left[\frac{\partial V(v^*;1)}{\partial v} - \frac{\partial V(v^*;0)}{\partial v}\right]\right]$,
under random assignment of D.

Given this background, we test the following set of:

Qualitative Predictions:

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

2. $b_2 > 0$ The mean ratio of vehicles to housing services is *higher* for households on paved streets (complementarities).[22]

3. $b_3 > 0$ The mean ratio of vehicles to consumption is *higher* for households on paved streets (complementarities).[23]

Quantitative Predictions:

1. $a_1 = \frac{b_2}{b_3}$

2. $a_1 = \frac{a_2}{a_3}$

3. $\frac{b_2}{b_3} = \frac{a_2}{a_3}$

4. $a_1 = \frac{b_2}{b_3} = \frac{a_2}{a_3}$

To test these predictions, we first estimate equations (23)-(25) by means of seemingly unrelated regressions (SUR), and test our qualitative predictions using standard t-tests, as well as our quantitative predictions, using Wald-tests. However, since the test-statistic for

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

[23] Lower if vehicle and street pavement are substitutes, the same if they are neither complements nor substitutes.

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 (23)-(25) by SUR subject to the constraints implied by our quantitative predictions. Afterwards, we perform a Likelihood ratio test of the restricted model against the unrestricted one. We use appraised house value as our proxy for housing services.

Table 8 provides the corresponding tests of our model. The top panel displays the results corresponding to the SUR estimates of equations (23)-(25). The first column shows that the estimate for b_1 is virtually zero (0.006) 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 second qualitative prediction of our model in the presence of complementarities between vehicle 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 0.019 with a p-value<0.1. This last column confirms our third and last qualitative prediction. As a robustness check, in the appendix (Table A4), we present 2SLS estimates where assignment to treatment is the instrument for pavement following the approach in section 3, obtaining very similar results.

The second panel in the table contains the estimates of the relevant ratios, namely $\frac{b_2}{b_3}$ and $\frac{a_2}{a_3}$, and the Wald tests block. Remarkably, we cannot reject any of the constraints individually (p-values always greater than 0.66), nor jointly (p-value=0.8386). All of our quantitative predictions are satisfied in the data. Finally, we also report the Likelihood ratio test for the null hypothesis that the constrained model, i.e., the model subject to $a_1 = \frac{b_2}{b_3} = \frac{a_2}{a_3}$, is nested in the unconstrained model. We cannot reject that the restricted model is nested in the unrestricted one (p-value=0.6239).[24]

These results clearly point to public infrastructure having important effects on household consumption patterns by changing the marginal utility of some goods, such as vehicles. Another main takeaway is that under reasonable assumptions, the increase in housing investment that accompanies street pavement can be understood as reflecting the complementarity between private investment and public infrastructure in the production of housing, and not wealth effects. Finally, from a methodological standpoint, our parsimonious model illustrates how a basic economic structure combined with a randomized intervention generates clear but not obvious testable implications, which are confirmed in the data.

4.3 Is there a role for credit constraints?

The model we proposed made the implicit assumption that households can borrow against housing wealth; in other words, credit constraints did not play any role. Whether this assumption is sensible or not is an empirical issue. In this subsection we inquire about the validity of this assumption by looking at (potential) heterogenous pavement effects on household outcomes in 2009 by income class in 2006. If credit constraints were playing a role, we would expect to see heterogenous effects of pavement with little or no impact among the poorer households, and most of the changes at the higher end of the income distribution.

The approach here is quasi-experimental, in the sense that although we use instrumental variables (e.g., interactions of income classes with intent-to-treat indicators), we are stratifying our sample ex-post randomization. Table 9 presents 2SLS estimates of the following equation:

$$Y_{2009} = \pi_1 DI_1 + \pi_2 DI_2 + \pi_3 DI_3 + \pi_4 I_1 + \pi_5 I_2 + \pi_6 I_3 + \pi_7 Y_{2006} + \epsilon_5 \qquad (26)$$

where Y_{2009} is a household outcome in 2009, $I_j = 1$ if the household is in the j^{th} ter-

[24]This p-value is a conservative one because is based on estimating the constrained and unconstrained models at the cluster-mean level, N=54. Using household data without accounting for clustering, N=474, the p-value is 0.7985.

cile of the per capita expenditure (PCE) distribution in 2006, and we use pavement-group assignment Z and its interaction with PCE tercile indicators in 2006 (ZI_1, ZI_2, ZI_3) as instrumental variables for DI_1, DI_2, and DI_3. Note that we define income class in 2006 using PCE, which is common practice in developing countries because reported income is more noisily measured (Deaton, 1997).

The Table presents the impacts of pavement on consumption (PCE), durable goods, home improvements, vehicle ownership and having a bank account. We interpret this last variable as a proxy, albeit rather crude, for the household's ability to borrow against housing wealth. The assumption being that households without a bank account are more likely to be credit constrained, while those with a bank account are less likely to be so. Several findings in this table deserve especial attention. First, the positive effect of pavement on household acquisition of durable goods is concentrated among the poorer households in 2006. Second, the positive effects of pavement on household home improvements follow a U-shape: with higher effects among both the poorer and richer households in 2006. Finally, pavement had an impact on households having a bank account in 2009, but only among those households who were poorer in 2006. The bottom line of this table is that the poor did not benefit less from street pavement, in terms of higher durable goods acquisition and home improvements, and actually seem to have benefitted more in terms of access to credit. These results are not consistent with credit constraints, at least if these are more likely to be binding for the poorer households.

In Table 10 we introduce additional heterogenous pavement effects by household banking access in 2006, our proxy for credit constraints in 2006. Hence, we estimate 2SLS regressions of the following form:

$$Y_{2009} = \kappa_1 DI_1 + \kappa_2 DI_2 + \kappa_3 DI_3 + \kappa_4 DB + \kappa_5 I_1 + \kappa_6 I_2 + \kappa_7 I_3 + \kappa_8 B + \kappa_9 Y_{2006} + \epsilon_6 \quad (27)$$

where B is an indicator for the household having a bank account in 2006, and we use pavement-group assignment Z and its interaction with PCE tercile indicators in 2006 and bank account in 2006 (ZI_1, ZI_2, ZI_3, ZB) as instrumental variables for DI_1, DI_2, DI_3 and DB.

All the estimates are essentially the same as those in Table 9. The only differential pavement effect dependent on banking access in 2006 is found for vehicle ownership in 2009: households who had a bank account in 2006 were more affected by pavement than their counterparts. More remarkable is the finding that it was exactly the poor in paved streets who increased the most their banking account status by 2009. Taken altogether these findings suggest that our model assumption about no-credit constraints is, if anything, sensible. The evidence indicates that not only poor people benefitted from pavement in terms of durable goods and home improvements, but also in terms of relaxing their credit constraints, if these were present at all.

5 Cost Benefit Analysis

With zero marginal price for street use, the benefits of a paved street are defined as the increase in consumers' surplus 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. In this section, we obtain an estimate of consumer surplus for the group of individuals living on properties adjacent to paved streets, being unable to obtain estimates those living in other streets. Although this calculation provides a lower bound on the benefits of street pavement, a remark is in order: In our context of a circular city with a paved core and unpaved outer borders, in which improvements connect unpaved outer sections to the paved core, the downward bias generated by ignoring the benefits to users not living along the paved streets is expected to be much less important than in other road contexts, such as the case of a road connecting two towns in which there may be very few users living along the paved road.

Our 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] It is important to highlight that any price effect of the intervention on houses abutting streets already paved before the experiment took place through general equilibrium effects constitute a pecuniary externality, and hence should be ignored for welfare analysis (Mohring, 1993).

We measure construction costs as the sum of municipal expenditures on each street that got paved. 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 exercise. There were 814 plots on streets that got paved. The average plot on these streets was valued at 27,844 pesos. Multiplying this value by the estimated impact of street pavement (54%) gives an average benefit per plot of 15,081 pesos, for a total private benefit of 12,275,585 pesos. The last column shows that the increases in land values represent 109% of construction costs. Taking into account that the benefits we estimate are a lower bound, the estimated benefit to cost ratio of around one is relevant for two different reasons: first, it 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.[26] Second, it points towards the feasibility of property taxation or improvement taxes as a viable source of financing urban infrastructure in a developing country context. Descriptions of how this can be done in practice (and its challenges) are carefully discussed by Diamond (1983).

6 Conclusion

This paper analyzes the microeconomic effects of urban infrastructure, and it relies on two pillars: A randomized evaluation of first-time asphalting of streets in inhabited residential

[25] Alternative strategies to obtain consumers' surplus can be found in Kaufman and Quigley (1987) or in the transportation literature.

[26] See Warlters and Auriol (2005).

neighborhoods, and a simple economic model which allows us to understand why household behavior changes after street pavement is provided.

The experiment buys the identification of causal impacts. We find that street pavement had sizeable positive effects on property values. We also document significant effects at the household level, namely increases in collateral credit use, durable goods, home improvements, and vehicle acquisition.

Why did households living along paved streets change their consumption decisions? We present a parsimonious model in which two main forces are at stake: the wealth effect due to property value increases and complementarities between vehicle ownership and home improvements with street pavement. The main predictions in the model are taken to the data and confirmed. We learn that infrastructure can change the marginal utility of private goods and hence affect household consumption behavior. In particular, vehicles become more valuable with street pavement. The model also allows us to interpret the increase in home improvements as arising from a complementarity between public infrastructure and private investment in the production of housing services.

By focusing on property values and household behavior, our results not only complement and contribute to the existing literature on infrastructure, but may have important policy implications. Indeed, we estimate the benefits to costs of street pavement to be around 1.09, which justifies serious consideration of cost sharing mechanisms in the provision of localized benefit public goods.

Perhaps the most important limitation of this study is that it was conducted in just one city. This may raise standard concerns about external validity arising from extrapolating the results to other contexts. However, with a bit of economic structure, we are able to derive some context-independent implications which can be easily tested due to the randomization. Quite surprisingly, our predictions are confirmed, indicating that what we learn from the experiment appears to be more general than the conclusions derived from the randomized evaluation on its own.

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Figures

Figure 1: Before Pavement

Figure 2: After Pavement

Figure 3: Acayucan Street Projects

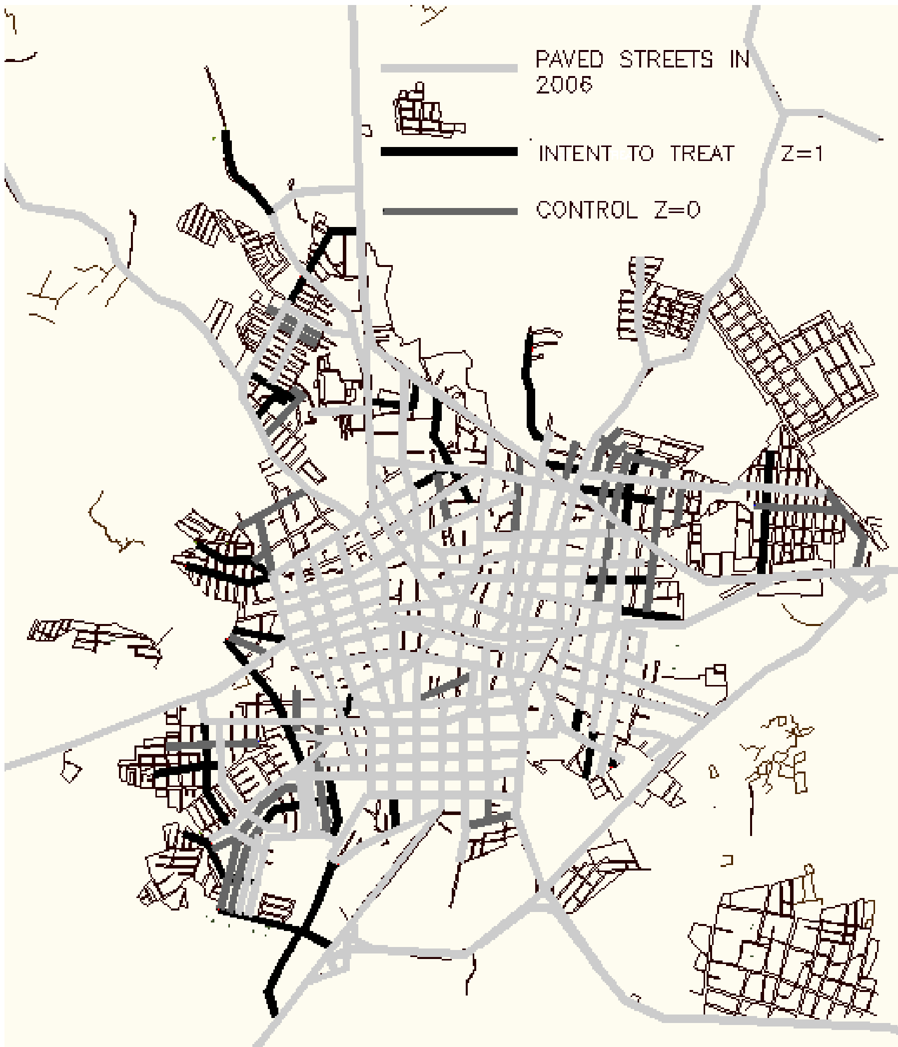
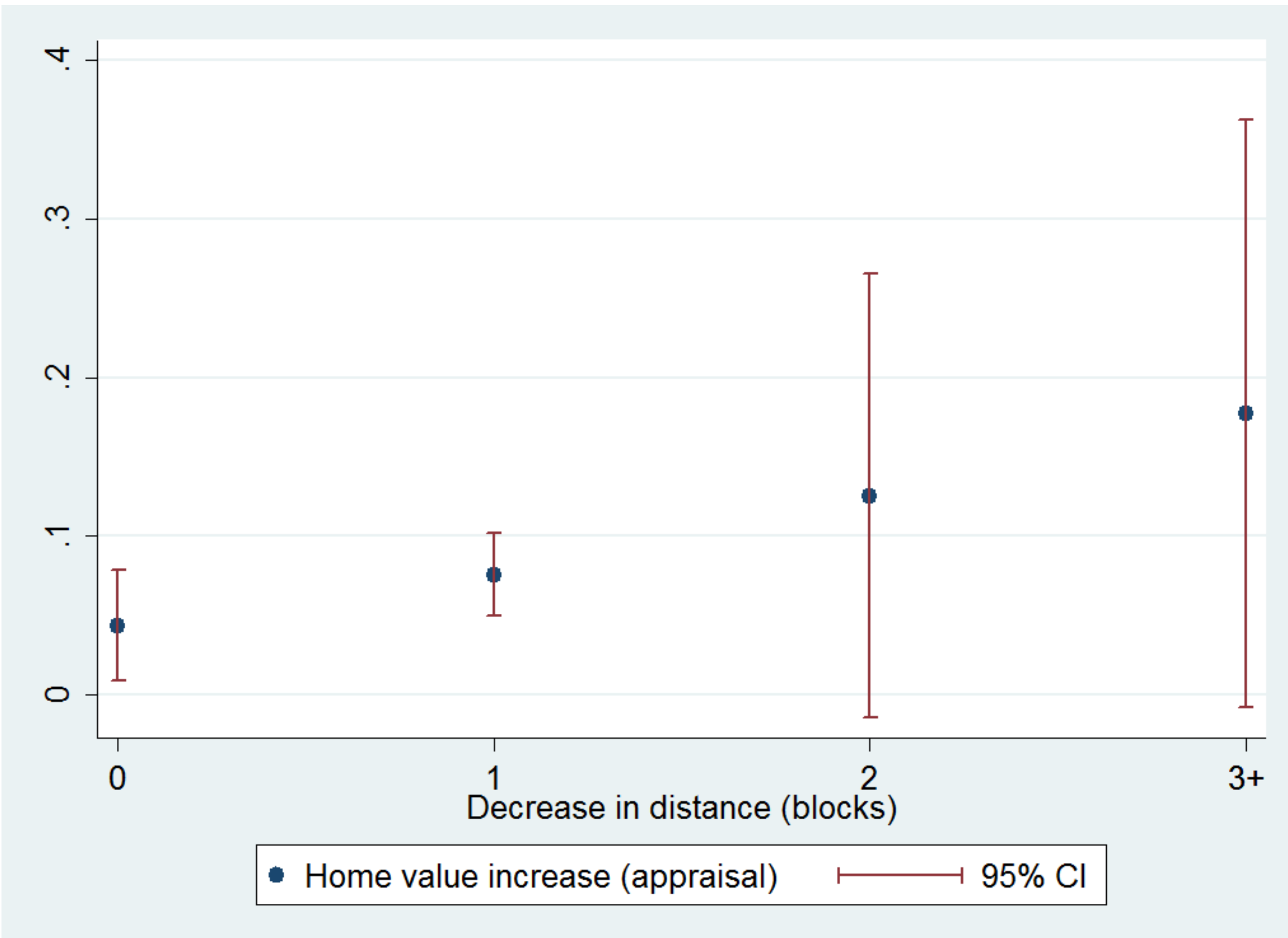


Figure 4: Change in House Value in Control Group



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.

Tables

Table 1: Pre-Intervention Balance in Means

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

ⓘ denotes individual-level outcomes. Means use survey weights. 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 crdito 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); sum of 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); sick previous month (self-reported symptoms: cough, flu, diarrhea, etc.), absenteeism previous month (if child missed at least one school day in the previous month). Significance levels reported only for *Diff*: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 2: Effect of Street Pavement on Property Values

	ITT	TOT	Mean Control (2009)
Log professional appraisal of property price	0.09***	0.16***	11.52
	(0.03)	(0.04)	(0.06)
	[548]	[548]	[253]
Log professional appraisal of land price	0.32***	0.54***	10.07
	(0.06)	(0.10)	(0.06)
	[548]	[548]	[253]
Log owner estimate of property price	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]

ITT column uses assignment to pavement as independent variable. TOT column instruments pavement with assignment to pavement. 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. Number of observations in brackets. Rent in 2009 includes as baseline control the rent paid by the family previously living in the same house in 2006. Transaction price is price paid by new homeowners (arriving between baseline and follow up). This last regression does not include the corresponding lagged dependent variable. Significance levels reported only for ITT and TOT: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Effect of Street Pavement on Credit Use

	ITT	TOT	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. TOT column instruments pavement with assignment to pavement. 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. Number of observations in brackets. Significance levels reported only for ITT and TOT: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Effect of Street Pavement on Consumption

	ITT	TOT	Mean Control (2009)
Sum of durable	0.166*	0.274*	2.36
goods (0-6)	(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. TOT column instruments pavement with assignment to pavement. 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. Number of observations in brackets. Significance levels reported only for ITT and TOT: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Effect of Street Pavement on Transportation and Labor

Transportation and Labor	ITT	TOT	Mean Control (2009)
Cost of taxi to	−0.360	−0.587	18.14
city center	(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
	(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]

textcircledi denotes individual-level outcomes. ITT column uses assignment to pavement as independent variable. TOT column instruments pavement with assignment to pavement. 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. Number of observations in brackets. Significance levels reported only for ITT and TOT: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Possible Anticipation Effects on Housing Value

Log professional appraisal of property price	
Assigned to treatment but unpaved	0.007
	(0.036)
	[344]
Log professional appraisal of land price	
Assigned to treatment but unpaved	0.112
	(0.089)
	[344]
Log owner estimate of property price	
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 lagged dependent variable. 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 7: Heterogenous Effects by Pavement Network Intensity

	Central District Pavement Projects		**Non-Central District Pavement Projects**	
	ITT	TOT	ITT	TOT
Log professional appraisal of property price	0.034	0.050	0.122***	0.216***
	(0.044)	(0.058)	(0.035)	(0.053)
	[163]	[163]	[385]	[385]
Log professional appraisal of land price	0.269**	0.404***	0.355***	0.634***
	(0.111)	(0.155)	(0.081)	(0.136)
	[163]	[163]	[385]	[385]

Central-district pavement projects are surrounded by paved streets, whereas the other pavement projects lie on the outskirts of the city and are for the most part surrounded by unpaved streets. ITT column uses assignment to pavement as independent variable. TOT column instruments pavement with assignment to pavement. 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. Number of observations in brackets. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: Testing the Model

	$\frac{log(C)}{log(H)}$	$\frac{V}{log(H)}$	$\frac{V}{log(C)}$
$\widehat{b_i}$	0.006	0.011*	0.019*
	(0.008)	(0.006)	(0.010)
$\widehat{a_i}$	0.585***	0.019***	0.033***
	(0.005)	(0.003)	(0.005)
N		[447]	
		Ratios	
$\frac{\widehat{b_2}}{\widehat{b_3}}$		0.593***	
		(0.036)	
$\frac{\widehat{a_2}}{\widehat{a_3}}$		0.588***	
		(0.009)	
		Wald Tests	
$H_0 : a_1 = \frac{b_2}{b_3}$		$\chi^2(1)$=0.05 p =0.8307	
$H_0 : a_1 = \frac{a_2}{a_3}$		$\chi^2(1)$=0.19 p =0.6602	
$H_0 : \frac{b_2}{b_3} = \frac{a_2}{a_3}$		$\chi^2(1)$=0.01 p =0.9137	
$H_0 : a_1 = \frac{b_2}{b_3} = \frac{a_2}{a_3}$		$\chi^2(2)$=0.35 p =0.8386	
		Likelihood Ratio Test	
H_0 : Constrained model nested in unconstrained		$\chi^2(2)$=0.94 p =0.6239	

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 9: Heterogenous Pavement Effects on Household Outcomes by Income Class at Baseline

	log(PCE)	Durable goods index	Home improvements index	Vehicle index	Bank account
Pavement × PCE_1	0.122	0.511**	0.348*	0.081	0.191***
	(0.116)	(0.207)	(0.198)	(0.065)	(0.014)
Pavement × PCE_2	0.021	0.145	0.253	0.024	0.015
	(0.086)	(0.210)	(0.288)	(0.089)	(0.062)
Pavement × PCE_3	0.222	0.028	0.489*	0.109	−0.011
	(0.162)	(0.229)	(0.255)	(0.091)	(0.074)
PCE_1	6.41***	0.889***	0.297***	0.031	0.026
	(0.041)	(0.097)	(0.068)	(0.026)	(0.019)
PCE_2	6.75***	1.13***	0.381***	0.153***	0.082***
	(0.034)	(0.122)	(0.093)	(0.037)	(0.025)
PCE_3	7.04***	1.38***	0.335***	0.219***	0.201***
	(0.072)	(0.123)	(0.079)	(0.044)	(0.039)
Baseline outcome	−−	0.606***	0.138***	0.503***	0.229***
		(0.034)	(0.039)	(0.068)	(0.048)
N	864	878	878	878	870

2SLS estimates. Standard errors clustered at the pavement-project level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 10: Heterogenous Pavement Effects on Household Outcomes by Income Class and Banking Access at Baseline

	log(PCE)	Durable goods index	Home improvements index	Vehicle index	Bank account
Pavement × PCE_1	0.141	0.487**	0.351*	0.057	0.191***
	(0.116)	(0.201)	(0.200)	(0.064)	(0.063)
Pavement × PCE_2	0.023	0.086	0.239	−0.028	0.015
	(0.092)	(0.225)	(0.297)	(0.093)	(0.058)
Pavement × PCE_3	0.263	−0.190	0.492*	−0.057	−0.011
	(0.177)	(0.269)	(0.253)	(0.109)	(0.089)
Pavement × Bank Account	−0.071	0.552	0.052	0.399***	−0.001
	(0.139)	(0.366)	(0.293)	(0.155)	(0.125)
PCE_1	6.39***	0.880***	0.292***	0.039	0.026
	(0.040)	(0.099)	(0.069)	(0.025)	(0.020)
PCE_2	6.72***	1.12***	0.374***	0.162***	0.082***
	(0.035)	(0.124)	(0.090)	(0.040)	(0.024)
PCE_3	6.95***	1.44***	0.304***	0.266***	0.201***
	(0.076)	(0.138)	(0.072)	(0.045)	(0.044)
Bank account	0.278***	−0.192	0.127	−0.136**	0.229***
	(0.061)	(0.218)	(0.136)	(0.075)	(0.048)
Baseline outcome	−−	0.617***	0.129***	0.530***	−−
		(0.034)	(0.038)	(0.058)	
N	858	872	872	872	870

2SLS estimates. Standard errors clustered at the pavement-project level in parentheses. * significant at 10%; ** significant at 5%; *** significant at 1%.

Table 11: Land-Value Increases on Paved Streets and Construction Costs

	Plots	Average value	Impact of Pavement	Gains per plot	Total gains	Gain/Cost ratio
Paved	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 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 3. 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)

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 (outmigrants); 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 experiment streets. We need to determine whether the attrition in our panel caused by the baseline households that moved out is random. If it is, experimental estimates based on stayers will be consistent but imprecise; if it is not, the estimates will be inconsistent.

The top panels in Table A2 and Table A3 show that neither out-migration nor immigration was affected by treatment status.[27] Moreover, the lower panel in Table A2 shows that when it comes to per capita expenditure, durable goods, and vehicle ownership there was no difference between out-migrants from control streets and those from paved streets. Similarly, the lower panel in Table A3 shows that there was no difference between immigrants into treatment streets and those into non-treatment streets.[28]

Given the short-term nature of the experimental evaluation, we cannot prove that migration to and from experimental streets was (mean) independent of treatment status. We therefore do not rule out the possibility that over the long term the paving of a neighborhood's streets would cause a recomposition of that neighborhood.

Having shown that our estimates are not inconsistent on account of out-migrant-based-attrition and that differential immigration flows to paved and unpaved streets had no masked effects, the paper focuses on stayers.

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

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

Table A1: Non-Response and Recontact

	2006 Dwellings		2009 Households
Eligible selected	1,275	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.

Table A2: Out-migration

Out-migration rate				Household Out-migrated (=1)					
				ITT	TOT	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)			Sum of Durable Goods			Vehicle Ownership		
	ITT	TOT	Mean Control (2006)	ITT	TOT	Mean Control (2006)	ITT	TOT	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. TOT 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 TOT: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A3: Immigration

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

Immigrant Characteristics	Log(PCE)			Sum of Durable Goods			Vehicle Ownership		
	ITT	TOT	Mean Control (2009)	ITT	TOT	Mean Control (2009)	ITT	TOT	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. TOT 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 TOT: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A4: Qualitative Predictions of the Model: 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%.

Other Outcomes

We found no evidence that pavement has an impact on health.[29] We investigated this issue because we had observed that Acayucan's lack of street pavement facilitates the accumulation of garbage, generates a dusty environment during the dry season, and during the rainy season provides a fertile breeding ground in the form of stagnant water for the *Aedes Aegypti* mosquito, which transmits dengue fever.[30] We asked individuals whether they had suffered any symptoms of disease, such as coughing, fever, and diarrhea; the self-reported results of our inquiry indicated that there was no correlation between disease and pavement. Similarly, human-capital accumulation measured by enrollment in school and absenteeism among children did not vary according to whether the children lived on paved or unpaved streets (Table A6).

A few of the properties on streets in the experiment were small businesses. We attempted to assess the impact of pavement on these businesses by means of a short survey instrument regarding the number of employees, sales, and expenditures. Table A6 (business units section) reports outcomes along the intensive margin at the firm level. The results show that pavement status had no effect on the number of employees, sales, expenditures, or profits.[31] Table A7 investigates whether there was an impact along the extensive margin and reports the sum of business units in 2006 and 2009 by treatment status. Although there was a greater increase in the number of business units in ITT projects than in control projects, both in absolute and in percentage terms, these differences were not borne out in terms of total employment.

[29] For evidence of the impact on health of the introduction of hard floors into homes in Mexico, see Cattaneo et al. (2009).

[30] There were 41,867, cases of dengue fever reported in Mexico in 2009, up from 27,479 in 2008. Of these, 7,898 were of the hemorrhagic type. Acayucan is located in Veracruz, the state with the largest number of cases of dengue fever.

[31] To determine if positive results were being masked by a temporary negative effect in streets recently paved (due to street blockages during construction), we performed tests of differences in sales, expenditures, and profits according to an indicator for paving having taken place within the previous six months and prior to the previous six months and found no such differences.

Table A5: 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)		(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. Means use survey weights. 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%.

Table A6: Impact of Pavement (Other Outcomes)

Variable	ITT	TOT	Mean	Variable	ITT	TOT	Mean
			Control (2009)				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)		(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. Means use survey weights. 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 ITT and TOT: * significant at 10%; ** significant at 5%; *** significant at 1%.

Table A7: Business-Unit Results

Extensive Margin (Total Units)						
	All Business Units		Difference	All Employees		Difference
	2006	2009		2006	2009	
ITT= 1	102	123	+21	182	202	+20
ITT=0	123	125	+2	192	209	+17

Data from a short census of all business units in the study projects.