ISSN 1471-0498

DEPARTMENT OF ECONOMICS

DISCUSSION PAPER SERIES

Paving Streets for the Poor: Experimental Analysis of Infrastructure Effects

Marco Gonzalez-Navarro and Climent Quintana-Domeque

Number 757
September 2015

Manor Road Building, Oxford OX1 3UQ

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

Marco Gonzalez-Navarro
University of Toronto and J-PAL

Climent Quintana-Domeque
University of Oxford and IZA

September 2015

Abstract

We provide the first experimental estimation of the effects of the supply of publicly financed urban infrastructure on property values. Using random allocation of first-time street asphalting of residential streets located in peripheral neighborhoods in Mexico, we show that within two years of the intervention households are able to transform their increased property wealth into significantly larger rates of vehicle ownership, household appliances, and home improvements. Increased consumption is made possible via both credit use and less saving. A cost-benefit analysis indicates that the valuation of street asphalting as capitalized into property values is about as large as construction costs.

JEL Classification Codes: C93, H41, O12, O18

Keywords: development, infrastructure, credit use, wealth effect, randomized controlled trial

*We would like to thank Anne Case, Angus Deaton, Alan Krueger, David Lee, Adriana Lleras-Muney, Christina Paxson, and Cecilia Rouse for their support and advice in this research project. The collaboration of the Acayucan 2005-2007 and 2008-2010 Municipal administrations is gratefully acknowledged. We also recognize the effort of José Luis Palma, Luz Uribe and Monica González at INSAD (`insad.com.mx`) who were in charge of the survey, and José Luis Reyes who provided the house value assessments. We also thank Carlos Bozzoli, Pierre-André Chiappori, Lucia Corno, Gilles Duranton, Frederico Finan, Paul Gertler, Larry Katz, Patrick Kline, Harry Krashinsky, Edward Miguel, Sonia Oreffice, four anonymous referees, and various seminar and conference participants at Alacant, Berkeley, CEMFI, Columbia, DIW-Berlin, IEB, Oxford, Princeton, RAND, Universitat Autònoma de Barcelona, Universidad de Navarra (NCID), University of Essex, U.S. Treasury, Tinbergen Institute, Toronto, ASSET Alacant, APPAM, BREAD-MIT, EEA-Glasgow, ESEM-Málaga, INSIDE-IAE, LILP-Quito, NEUDC-MIT, Pacific Development Conference, RES-London, and World Bank Conference on Infrastructure and Development for helpful comments and suggestions. Gonzalez-Navarro: University of Toronto, 121 St. George Street Toronto, ON M5S 2E8, Canada; marco.gonzalez.navarro@utoronto.ca; Quintana-Domeque: University of Oxford, Department of Economics, Manor Road Building, Manor Road, Oxford OX1 3UQ, United Kingdom; climent.quintana-domeque@economics.ox.ac.uk. The study protocol was approved by Princeton University Institutional Review Panel (Protocol 3104). Financial aid and support from Princeton University, Princeton Woodrow Wilson Scholars, Princeton Industrial Relations Section, Princeton Research Program in Development Studies, Robert Wood Johnson Scholars in Health Policy Research Program, Berkeley Economics, Universitat d'Alacant, the Lincoln Institute of Land Policy and the Spanish Ministry of Science and Innovation (ECO2011-29751) is gratefully acknowledged. Any errors contained in the paper are our own.

1 Introduction

Two concomitant facts about the developing world merit attention. The first is that urbanization is proceeding rapidly in many developing countries (Henderson, 2002). The second is that a large proportion of the urban poor throughout the developing world do not benefit from basic urban equipment such as piped water, electricity, sewerage lines, and asphalted roads (UN-Habitat, 2003). Given these two widespread phenomena, the dearth of empirical studies on the effect of infrastructure for the living conditions of the urban poor is remarkable. Our aim in this paper is to help fill this gap.

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

We overcome these two obstacles by combining a randomized infrastructure experiment (provision of first-time asphalting of streets in inhabited residential neighborhoods) with the collection of data from a dedicated survey before (2006) and after (2009) the intervention.[1] The intervention takes place in Acayucan (Mexico), where the city expands its pavement grid over time via "street asphalting projects", each defined as a contiguous set of unpaved street segments connecting to the existing pavement grid. Figures 1 and 2 illustrate the intervention under analysis. From the public works office's set of 56 candidate street asphalting projects, we randomly selected half of them to be treated with pavement using simple randomization.[2]

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

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

The first contribution of this paper is to provide the first experimental estimation of the effects of the supply of publicly financed urban infrastructure on property values.[3] We document substantial property value gains among properties lining newly paved streets compared to control streets. Using professional appraisals, we find that plots increased in value by 0.54 log points (or 72%)[4], representing a 17% increase in property values (the sum of land and structure). According to homeowner valuations, we estimate an effect of street pavement on property values of 28%. Two other pieces of market-value information point in the same direction: Rents raised by 36% in paved streets; and for the houses that were purchased between 2006 and 2009 the price paid was 85 log points (or 134%) higher on paved streets, although the coefficient on this last measure is not statistically significant on account of the limited number of transactions. Our experimental findings contribute to the literature on the property value effects of infrastructure, which typically relies on observational studies using difference-in-differences strategies (Billings, 2011; Gibbons and Machin, 2005).

From an economic development perspective, our second contribution is to show the fact that the provision of infrastructure, in our case first-time asphalting of streets, had sizeable positive effects on households' acquisition of household appliances, motor vehicle ownership, and home improvements. Indeed, we find that motor vehicle ownership went from one in every four households to one in every three, the number of home appliances owned by the household increased by 12%, and home improvements doubled. All these changes point to a reduction in material poverty.

Our third contribution is to offer an explanation for the observed changes in household behavior, namely significant increases in household appliances, the number of home improvements, and the acquisition of motorized vehicles. We investigate the role of credit use in explaining durable goods consumption by means of a correlational analysis. Our focus on

[3] While there is an extensive literature assessing the effects of infrastructure on economic outcomes, both at the macro level (Haughwout, 2002; Donaldson, forthcoming; Duranton and Turner, 2012) and at the micro level (Van de Walle, 2002; Davis, 2011; Dinkelman, 2011; Duflo and Pande, 2007), none of the previous studies provides experimental evidence.

[4] $exp(0.54) - 1 = 0.72$

credit is motivated by several observations. First, the increase in property values. Second, the fact that homeowners represent 95% of the households in our sample. Third, the doubling of collateral-based credit use from financial institutions.[5] Fourth, the absence of effects on transportation costs or labor market outcomes. Finally, the fact that banks in Mexico do offer home equity lines of credit.[6]

One possibility is that the increase in property values led to *credit constraint relaxations*. We indeed observe a correlation between credit use and durable good acquisition, but this *only* happens for households that were already using credit at baseline. For households not using credit at baseline, the effects are present only for household appliances, but these are precisely the goods less likely to be financed via secured loans (and more likely to be financed through dissaving and/or non-secured loans). Hence we interpret the overall evidence as pointing to a *wealth effect* rather than to a relaxation of credit constraints as the main force behind the increase in durable goods consumption.

In the last part of the paper, we perform a cost-benefit analysis by summing up the increases in land values of plots along paved streets using our most conservative estimate of the effect on land values. The comparison of benefits to construction costs reveals that street paving had a benefit to cost ratio of 1.09, and we cannot reject that it is equal to 1. According to the public finance literature on the valuation of public goods based on property value capitalization (Cellini, Ferreira and Rothstein, 2010), our estimated ratio is consistent with an efficient level of public good provision.

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

[5] Loan size across all adults went from virtually zero to 1,643 pesos (200 U.S. dollars at 2009 PPP exchange rate), equivalent to 2 months per capita expenditure.

[6] "Loans with real estate collateral" offer up to 50% of the assessed property value.

2 Experimental Design

2.1 Institutional Context

Acayucan is one of Mexico's 56 metropolitan areas encompassing three municipalities with a combined population of 105,000 (INEGI, 2007). The city has a central core where most streets have been paved, and outer sections where street pavement is gradually rolled out. Residences are built and inhabited long before streets are paved, as shown in Figure 1. This situation is common throughout Mexico and other Latin American countries (Fernandes, 2011), suggesting that the results from our analysis are potentially relevant for many other countries. Compared to other localities of similar size in Mexico, housing indicators in Acayucan are practically identical to the average. However, there is less manufacturing in Acayucan than in the average city and incomes are lower.[7]

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. Furthermore, cadastral property valuations are very low and rarely updated.

2.2 The Experiment

The intervention consists of first-time asphalting of residential non-arterial streets, varying in width from 8 to 15 meters, and allowing for two lanes of vehicular traffic and one or two lanes for parking. The pavement material used is either hot-mix asphalt concrete or portland cement reinforced concrete. Like most infrastructure, the lion's share of costs are

[7]Comparisons based on the 2005 Mexican Census - CONTEO. See Gonzalez-Navarro and Quintana-Domeque (2015).

borne initially: The transportation literature estimates annual cost of maintenance to be only 1.5% of construction costs (BITRE, 1978), or 0.3%-0.7% using the cost estimates in Chen, Lin and Luo (2003). After a street is paved, maintenance is a municipal responsibility and is funded from general revenues.

Street pavement in an urban context provides multiple services: It facilitates vehicle, pedestrian and cyclist movement and access, provides accessible space for vehicle parking, allows commercial vehicles to deliver goods, and has a significant impact on the visual appearance of the area. Fieldwork confirmed that congestion was not a concern − as expected given the residential nature of the streets. A valid question is then why the market does not provide street pavement to begin with. One reason is that residential street pavement is a pure public good (non-rivalrous and non-excludable), and hence, free rider incentives prevent private provision.[8]

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

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

[8]While the nature of the roads that were paved –residential and arterial– meant that we never observed congestion, this does not take away from the more general phenomenon that this type of policy may lead to more congested roads in the downtown areas.

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

By February 2009, right before our follow-up survey, 17 of the streets in the treatment group had been completely paved, and the other 11 were under way (the municipal government attributed the delays to foul weather and various technical difficulties).[9] However, and most importantly, the administration did fulfill the requirement of *not* paving those streets assigned to the control group.

2.3 Data Sources

The data for this study come from pre- and post-intervention rounds of a dedicated household survey (the Acayucan Standards of Living Survey, ASLS) and professional appraisals of residential-property values.[10] Importantly for our purposes, the part of the ASLS questionnaire focused on consumption and income is very detailed, following the Mexico National Survey of Household Income and Expenditure (ENIGH, Encuesta Nacional de Ingresos y Gastos de los Hogares). The baseline survey was fielded in February-March 2006, and the follow-up survey was fielded in February-March 2009. Professional appraisals were performed

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

[10] A description of the ASLS can be found in Gonzalez-Navarro and Quintana-Domeque (2015).

immediately after the survey work in each round.[11]

The target population of the survey consisted of all occupied residential structures on the streets that were selected for the experiment.[12] The baseline survey was administered to 1,231 households living in 1,193 dwellings, with a response-rate of 94%.[13] 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 (online appendix) details survey response rates.[14]

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

We did not inform participants in the study (household respondents and the professional appraiser) about the ultimate objective of the survey/appraisals.[15] 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

[11] A very short business census was applied to all business units whose main entrance faced one of the street projects in 2006 and 2009. Mobile business units were excluded from analysis (for example, a seller on a motorcycle, or a water distributor going around on a truck). Results based on these data (available in Table A6 of the online appendix) do not reveal any effects on businesses.

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

[13] 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.

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

[15] The data collected for this study underwent the approval process of the Institutional Review Panel at Princeton University (Research Protocol 3104). At the end of the experiment, we debriefed the appraiser about the purposes of the study.

Henry effects) was minimized.

2.3.1 Measuring Property Values

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

To compensate for the small number of transactions to be expected in our setting, we obtained two independent measures of property value: Professional appraisals and homeowner valuations. The fact that professional appraisals are used by banks to determine property values, and hence the size of mortgages, indicates that they are a reliable source of market valuation. In our case, we used the services of a professional appraiser contracted by local banks who is also a real-estate agent in the city.[16] 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."[17] We obtained professional appraisals of residential property (and land) value for

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

[17] Section A2 (online appendix) includes a copy of one of the assessment forms used by the assessor. It includes a set of "boxes to tick" and "paved street" is one of them.

half of the successfully interviewed households (578 properties), on account of budgetary constraints.[18]

The second main source of property valuation we obtained were homeowners' self-reports. There is an established literature in economics using self-reported home values (see Davis (2011) for a recent example). The reliability of self-reported home values has long been assessed in the housing economics literature (in developed countries see Kish and Lansing, 1954; Kain and Quigley, 1972; Goodman and Ittner, 1992; Kiel and Zabel, 1999; Bucks and Pence, 2006; Banzhaf and Farooque, 2012; in developing countries see Jimenez, 1982). This literature concludes that the evolution of self-reported housing values generally mimics that of actual prices. In the ASLS, we asked: "Approximately how much money do you think this house could sell for nowadays?" For the 2006 ASLS sample, Gonzalez-Navarro and Quintana-Domeque (2009) show that owners overestimate the value of their homes relative to the appraiser but that the bias is explained by long tenure: Short-tenured homeowners provide value assessments that are on average the same as professional appraisals.[19] This suggests the professional appraiser's valuation is a better indicator of market value.

Finally, to circumvent the drawbacks of our previous measures due to homeowner self-reported biases (e.g., priming effects) and potentially mechanical assessor models for valuing properties, we complement homeowners' self-reports and assessor valuations with data on transaction prices paid by recent buyers and housing rental rates. Data on recent transactions are obtained by asking recent buyers – those arriving between baseline and follow-up – how much they paid for the property. Housing rental rates are obtained by asking renters how much they pay per month in rent. This last measure has the advantage of overcoming the worry regarding the forward-looking behavior of house price data, i.e., that the control group

[18] From the household survey we conducted, we simply ordered all households we interviewed and selected for appraisal 1 out of every 2. We did not have enough funds to appraise all properties. Doing one out of every two also guaranteed we had observations in all clusters. The appraiser did not enter the properties because piloting revealed that non-participation would be extremely high, which would have compromised our entire evaluation.

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

was effectively treated (at least with some non-zero probability, and some discounting for the fact that the treatment is delayed) too.

2.3.2 Consumption and Credit Measures

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

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

To understand our credit measures it is important to have a sense of the institutional background on how access to loans works in Acayucan. In this regard, we note that four of the five Mexican banks with national coverage were present in Acayucan from before the time of the study (BBVA, Banamex, HSBC, and Santander). These banks do offer home equity lines of credit, which are informally called "*crédito liquidez*" and formally known as "*crédito con garantía hipotecaria*" - "loans with real estate collateral". These credit lines offer up to 50% of the assessed property value. In addition to secured-credit, non-secured credit is quite common in Mexico: Employees are offered a multiple of their monthly wage by

the bank with which they have direct deposit, major home appliances are commonly offered on weekly payment schemes by major retailers such as Elektra. It is also worth noting that major players in microfinance, such as "*Compartamos*", and savings cooperatives, such as "*Caja de Ahorro Popular Mexicana*", are also present in the city.

2.3.3 Other Measures

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

3 Empirical Strategy

3.1 Baseline Balance, Reduced-Form and 2SLS estimates

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

Note that we present balance tests for variables with different units of observation: Dwellings, households, and individuals (sometimes partitioned into adults and children).[20]

[20]Variable definitions: Nearest paved street (distance in blocks from the dwelling to the nearest paved street); collateral-based credit (mortgages, home-equity lines, and collateralized bank loans); non-collateralized credit (appliance- and furniture-store credit, bank-card credit, vehicle loans, and *casas de crédito popular*); credit card (bank account): indicator that someone in the household has a credit card (bank account); household appliances (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,

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

The table shows that randomization was successful in balancing pre-treatment characteristics across the intent-to-treat and the control groups.[21] We assess a total of 53 variables and find evidence of balanced characteristics across the groups; see Table A2 (online appendix). Only two variables are individually significantly different across the groups: Labor income and non-collateral based credit amount, both at the 10% significance level.

We then proceed to estimate reduced-form (RF) effects:

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

where Y_{2009} is the outcome of interest in 2009, Z is the intent-to-treat indicator, and Y_{2006} is the outcome at baseline, included to improve precision. Hence, unless otherwise indicated, all of our estimates use the two rounds and not only the follow-up cross-section. The ITT parameter is α_1 in equation (1). We cluster standard errors at the street-pavement-project level (56 clusters).[22] We also use survey weights.[23]

plumbing, toilets, electrical installations, room construction, remodeling, security measures, and improvements to house front); materials purchased for home improvements (in the previous 6 months); time to city center (self-reported time to commute from home to city center).

[21] The average difference in characteristic X is computed as the coefficient of a regression of X on the binary indicator Z, and its associated standard error is clustered at the pavement-project level. 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. See Deaton (2009) for further discussion. In addition, we perform a joint test for the randomization check using seemingly unrelated estimation and then testing the null hypothesis that the pre-intervention means of the variables that come from our household survey are the same for both the intent-to-treat ($Z = 1$) and control ($Z = 0$) groups. The corresponding adjusted Wald Test has an $F_{21,34}$ distribution under the null. We obtain an F-statistic of 1.58, so we cannot reject the null hypothesis (of no mean differences overall) at conventional levels of statistical significance.

[22] Given that our experiment took place in a context of different sized clusters due to differing street project pavement lengths, we also use the wild cluster bootstrap method (MacKinnon and Webb, 2014). Our clustered p-values are very similar to the wild cluster bootstrap ones.

[23] 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

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

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

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

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

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

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

3.2 Interpreting 2SLS estimates as TOT effects

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

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.

To assess condition **C2**, whether anticipation effects are negligible when estimating the effects of street pavement, the most natural outcome to look at is home value, which is likely to adjust in anticipation of street pavement provision. For example, McMillen and McDonald (2004) detect a house price adjustment, in anticipation of the opening of a new transit line in Chicago, up to 6 years before the actual opening of the line, which coincided with the *announcement* of the route for the proposed transit line. Here, a potential concern (although there was no announcement) is that non-compliers, i.e., people living along the ITT ($Z = 1$) streets that were not finally paved, learned that their street had been selected –by observing the arrival of measurement teams– and modified their estimates of property value in 2009. In that case, non-compliers would be affected by owning a house on a street selected for paving in the near future, and "being in a selected project" could not be used as an instrument for "being paved". The appraiser could have updated his estimations of such properties as well. Note, however, that rents have the advantage of overcoming the worry regarding the forward-looking behavior of house price data.

We examine the presence of anticipation effects on housing values by focusing on the subsample of households living in streets assigned to pavement but finally unpaved ($Z = 1$, $D = 0$) and those living in streets assigned to the control group ($Z = 0$). We run a regression of home value in 2009 on an indicator variable that takes the value 1 if ($Z = 1$, $D = 0$), and 0 if ($Z = 0$), controlling for the home value in 2006. Table 2 shows that there is no change in home values for those homes in the intent-to-treat group that were not finally paved, suggesting that the expectations of non-compliers regarding home values did not change. This evidence supports condition **C2**.[24]

Finally, we need to assess condition **C3**, whether paving has an effect on units in the

[24]Two other possible types of bias due to anticipation effects are (i) the possibility that members of the control group see their chances of getting pavement go up (if they had access to the list of projects considered for potential pavement) and the possibility that members of the treatment group expected pavement anyway, prior to the program. However, we do not think these are serious concerns in our context, since to our knowledge the list of projects considered for pavement was an internal document to the administration, and the short time horizon of the administrations means there is a lot of uncertainty regarding what the politician will actually deliver.

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

To investigate the potential effects of paving on property market sorting, i.e., whether changes in the desirability and price of treated properties affect the market price of control properties through neighborhood recomposition, we investigate whether the intervention under analysis affected either the rate or composition of movers in/out of the paved neighborhood. Column 1 in the top panel of Table 3 shows that out-migration is uncorrelated with intent-to-treat. Similarly, column 2 indicates that out-migration is unrelated to pavement status.[25] The bottom panel in Table 3 shows no statistically significant differences in the means of per capita expenditure, household appliances, and vehicle ownership between out-migrants from control streets and those from paved streets.[26] Regarding immigration flows, in 18% of the 2006 dwelling sample we found a new family in 2009.[27] The top panel in Table 4 shows that the likelihood of new households arriving to the experimental streets between 2006 and 2009 is not affected by either intent-to-treatment or pavement status of the street. In addition, the lower panel in the table shows that there is no statistically significant difference in average (socioeconomic) characteristics of immigrants to intent-to-treatment (or paved) streets and those arriving to control (or unpaved) streets. Hence, the evidence reported in Tables 3 and 4 fails to support that paving affected the rate or composition of movers in/out of the paved neighborhood.[28]

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

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

[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 than immigration rate.

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

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

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

where Y_{2009} is appraised home value in 2009, and Δd is the change in distance to the nearest paved street between 2006 and 2009 (measured in street blocks). The connectivity effect is captured by γ_1. Our estimate for γ_1 is -0.034 (se=0.025), so that a decrease of one street block in the distance between a given house and the pavement grid is correlated with a 3% higher housing value. Given that the average reduction in distance to the nearest paved street among the control group was 0.66 street blocks (from 1.35 in 2006 to 0.69 in 2009), the estimated downward bias is around 2.2 percentage points, which is not statistically significant, and relatively small compared to the RF and 2SLS estimates reported below.[29]

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

[29] In Table A4 (online appendix) we present an analysis of spillovers, not only for home values, but for transportation and labor supply outcomes: Cost of taxi, time to city center, weekly hours of work, and monthly log labor income. Spillovers onto the control group across these additional dimensions are either statistically insignificant or economically irrelevant.

as potential lower bounds for the TOT effects.[30]

4 Experimental Effects of Street Pavement

4.1 Effects on Property Values

We begin by presenting, in the three columns of Table 5, our main experimental estimates for the effects of street pavement on home and land values. RF and 2SLS estimated effects are presented in the first two columns and the mean of the corresponding variable for the control group in 2009 in the third. Using the professional-appraisal measures, we find that pavement increases home values by 17% and land values by 54 log points (or 72%). According to homeowners' valuations, we estimate that street pavement raises property values by 28%. The fact that two independent measures of property value move in the same direction suggests that paving accounts for substantial rises in home values. Note that the difference in the magnitudes of the estimated impacts with the two measures is not statistically significant.[31]

Table 5 also shows that rents on treated streets are 36% higher than rents on control streets, and that the amount paid for recently purchased houses on intent-to-treat and paved streets are 55% and 85 log points (134%) higher than in the control group, although it must be conceded that this last estimate is imprecise on account of the small sample size and the fact that we cannot control for the outcome at baseline. Nevertheless, it is reassuring to corroborate the qualitative findings obtained from appraisals and homeowner valuations

[30] If we compare the mean characteristics of units on $Z = 1$ streets that were paved by 2009 ("compliers") with those on $Z = 1$ streets that remained unpaved by 2009 ("non-compliers"), the only significant differences between "compliers" and "non-compliers" arise in terms of distance to the downtown in minutes, credit card prevalence, bank account prevalence and rent: For compliers it takes less time to commute to the city center than for non-compliers, they pay higher rents, and are more likely to have a credit card and a bank account. This suggests that pavement was first rolled out in streets closer to the downtown. Note that the interpretation we give to our 2SLS estimate is the effect of moving from a situation in which there is a positive but small probability of getting paved to one in which it is one. This is different from the estimate of assigning a probability of zero to getting paved and then getting it, but we believe the former provides an estimate of the policy relevant parameter.

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

with these supplemental indicators. The similarity of the effects on owners' self-reported prices (a stock variable) and rental rates (a flow variable) is remarkable, and corroborates our previous results on the absence of anticipation effects. We note that the absence of anticipation effects described in Table 2 is consistent with the small differences reported in Table 5 when comparing the estimated effects on self-reported house value measures (RF: 0.16, 2SLS: 0.25) against the estimated effects on house rental rates (RF: 0.18, 2SLS: 0.31).

We interpret these estimates as pointing to local infrastructure affecting homeowners through land value increases, which is consistent with previous findings documented by Brueckner (1982) and Haughwout (2002), among others.

4.2 Effects on Consumption and Credit

We now turn to estimate the effects of street pavement on consumption of durable and non-durable goods. Results are reported in Table 6. Street pavement provision has a strong positive effect on the number of household appliances owned by the household: Out of six household appliances, control households have an average of 2.4 goods, while the mean for households on paved streets is 2.7 goods (12% higher). There is also a significant effect on the rate of ownership of a motorized vehicle (motorcycle, car, or truck). Whereas the household-vehicle index is 0.25 in the control group, in the treated it is 0.35, corresponding to a 43% increase.

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

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

Credit effects are reported in Table 7. We find that pavement increases the percentage of individuals who use collateral-based credit from close to 2% among the control group to nearly 5% among the treated. The increased use of collateral-based credit is also reflected in the average loan size, on average 135 pesos among the control group and 1,643 pesos (equivalent to 2 months per capita expenditure) among the treated: A more than tenfold increase.[32] While this is an important finding, we do not have the required information to determine whether the increase in collateral-based credit use is due to an increase in either the demand or the supply of credit (Field and Torero, 2004). For all other types of credit, such as non-collateral based, credit from family and friends, credit from government entities, and credit from informal sources, we do not observe any changes either in the number of individuals using credit or in the extent of the credit.[33]

4.3 Effects on Transportation and Labor

We measure the effects of street pavement on transportation costs in Table 8 in terms of money and time: The cost of a taxi to the city center and the time it takes to go to the city center by one's usual means of transportation. We find that in both respects the savings for those who benefit from pavement over the control group are neither large (0.6 pesos and less than 1 minute) nor statistically significant. In addition, field visits did not reveal new bus routes in these neighborhoods after pavement was provided.

Similarly, we find no effect on labor outcomes either in terms of labor supply or earnings (in Table A5 (online 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

[32] Note that the average secured loan conditional on having a loan is 22,422 pesos, sufficient to cover the cost of a used car in Mexico.

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

migrate in search of work, which fell from 47% to 37% as a result of treatment.

Finally, and regarding other outcomes, Table A5 (online appendix) shows no significant effects on either school attendance or self-reported health for either adults or children (see Cattaneo et al. (2009) for health impacts of providing cement floors in Mexico).

5 Understanding Pavement Effects on Household Outcomes

While the previous analysis is crucial to assess the causal effects of street pavement on the lives of the poor, it does not tell us how pavement affects household behavior. In this section we seek to understand what drives the observed changes in household behavior, namely significant increases in household appliances, the number of home improvements, and the acquisition of motorized vehicles.

Given the increase in property values, that homeowners represent 95% of the households in our sample, the doubling of collateral-based credit use from financial institutions,[34] together with the fact that we do not find effects on transportation costs or labor market outcomes, we investigate the role of credit use in explaining durable goods consumption by means of a correlational (descriptive) analysis.

In particular, we investigate the role of credit use in explaining the durable goods consumption.[35] In order to accomplish this we regress the indices of household appliances (0-6), home improvements (0-11) and motorized vehicles (0-3) on the amount of (collateralized) credit in 2006, in 2009, and the interaction of the two, controlling or not for the corresponding lagged dependent variable.

Table 9 displays the results of these regressions. The coefficient on the credit 2009 variable can be interpreted as the correlation between credit use and the relevant index for households with *no* credit in 2006. For the household appliances index this coefficient is

[34] Loan size across all adults went from virtually zero to 1,643 pesos (200 U.S. dollars at 2009 PPP exchange rate), equivalent to 2 months per capita expenditure.

[35] Because the primary residence typically constitutes the single most important depository of wealth for homeowners, changes in its asset value can be expected to have important consumption effects (Campbell and Cocco, 2007) as long as households can borrow against housing wealth or dissave (Muellbauer, 2007).

positive and significant. This suggests that among households with no credit in 2006, those that experienced an increase in credit use in 2009 (used a credit line in 2009) were also more likely to buy durable goods. While this may be consistent with the *credit constraint relaxation channel*, this significant correlation is not present for the home improvements and motorized vehicles indices. However, what is positive and significant across the board is the interaction between credit in 2006 and credit in 2009. This suggests that the increase in the consumption of durable goods (broadly defined) was concentrated mostly among households that already had access to credit in 2006. For households not using credit at baseline, the effects are present only for household appliances, but these are precisely the goods less likely to be financed via secured loans (and more likely to be financed through dissaving and/or non-secured loans). Hence we interpret the overall evidence as pointing to a *wealth effect* rather than to a relaxation of credit constraints as the main force behind the increase in durable goods consumption.[36]

6 Cost-Benefit Analysis

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

Our consumers' surplus estimate is obtained by summing up the increases in land value

[36] Of course, part of the increase in vehicle ownership might just reflect an increase in the marginal utility of having a motorized vehicle due to street pavement.

over plots on treated streets.[37] A similar approach is used in Jacoby (2000).[38] Construction costs are measured as the sum of municipal expenditures on each street that was treated with pavement. Specifically, the municipality reported that the total cost of paving the streets in this study amounted to $11,304,642$ pesos.

Table 10 reports the results of this cost-benefit analysis. There are 814 plots on treated streets. The average plot on these streets is valued at 27,844 pesos. Multiplying this value by the estimated effect of street pavement on land value (0.54) gives an average benefit per plot of 15,081 pesos, for a total benefit of 12,275,585 pesos. The last column shows that the increases in land values represent 109% of construction costs and we cannot reject that the ratio is equal to 1. Our cost-benefit analysis indicates that the economic returns to street pavement in this context are about as large as the construction costs, even if we consider the typical deadweight losses generated by taxation in developing countries (Auriol and Warlters, 2012). Furthermore, it is worth noting that according to the public finance literature on the valuation of public goods based on property value capitalization (Cellini, Ferreira and Rothstein, 2010), a ratio of one is consistent with an efficient level of public good provision.

Of course, the cost-benefit analysis conducted in this section provides a benchmark. It is valid as long as markets are complete and perfect, so that the pavement amenity is perfectly capitalized into land values. The higher the degree of market imperfections, the higher the deviation from our benchmark and the "real" benefits of the intervention.

7 Conclusion

What can be learned from this paper? First, provision of urban infrastructure gets capitalized into property values. First-time street asphalting of residential streets in poor neigh-

[37] When performing the cost-benefit analysis there are two ways to proceed. One is to focus on home values, in which case we clearly need to account for the costs of home improvements. Alternatively, we can focus on land values. This is actually the path we follow here. According to the assessor (and consistent with the valuations), home improvements are not included in the valuation of the land, so focusing on land values sidesteps this issue.

[38] A more structural strategy to estimate consumers' surplus can be found in Kaufman and Quigley (1987).

borhoods in developing countries leads to substantial property appreciation effects which directly benefit owners of properties lining those infrastructure projects. Second, the increased property wealth is transformed quite rapidly (within two years) into tangible benefits: The consumption of household appliances, home improvements and vehicle ownership increase significantly. Because incomes are not significantly affected by this policy, the durable goods are financed through increased credit use, although most of this occurs among households that already had access to credit. We interpret these results as suggestive evidence of street pavement generating wealth effects rather than easing credit constraints.

References

Angrist, J. D., G. W. Imbens, and D. Rubin (1996): "Identification of Causal Effects Using Instrumental Variables," *Journal of the American Statistical Association*, 91(434), 444–472.

Auriol, E., and M. Wartlers (2012): "The Marginal Cost of Public Funds and Tax Reform in Africa," *Journal of Development Economics*, 97(1), 58–72.

Banzhaf, H., and O. Farooque (2012): "Interjurisdictional Housing Prices and Spatial Amenities: Which Measures of Housing Prices Reflect Local Public Goods?," *NBER Working Paper*, 17809.

Billings, S. B. (2011): "Estimating the Value of a New Transit Option," *Regional Science and Urban Economics*, 41(6), 525–536.

BITRE (1978): "Discussion Paper on Residential Streets," *Australian Bureau of Infrastructure, Transport and Regional Economics.*

Bloom, H. S. (1984): "Accounting for No-Shows in Experimental Evaluation Designs," *Evaluation Review*, 8(2), 225–246.

Brueckner, J. K. (1982): "A Test for Allocative Efficiency in the Local Public Sector," *Journal of Public Economics*, 19(3), 311–331.

Bucks, B., and K. Pence (2006): "Do Homeowners Know Their House Values and Mortgage Terms?," *FEDS Working Paper*, (2006-03).

Campbell, J., and J. Cocco (2007): "How do House Prices Affect Consumption? Evidence from Micro Data," *Journal of Monetary Economics*, 54(3), 591–621.

Case, K. E., and R. Shiller (1987): "Prices of Single-Family Homes Since 1970: New Indexes for four Cities," *New England Economic Review*, (Sep), 45–56.

Cattaneo, M. D., S. Galiani, P. J. Gertler, S. Martinez, and R. Titiunik (2009): "Housing, Health, and Happiness," *American Economic Journal: Economic Policy*, 1(1), 75–105.

Cellini, S. R., F. Ferreira, and J. Rothstein (2010): "The Value of School Facility Investments: Evidence from a Dynamic Regression Discontinuity Design," *Quarterly Journal of Economics*, 125(1), 215–261.

Chen, D.-H., D.-F. Lin, and H.-L. Luo (2003): "Effectiveness of Preventative Maintenance Treatments Using Fourteen SPS-3 Sites in Texas," *Journal of Performance of Constructed Facilities*, 17(3), 136–143.

Davis, L. W. (2011): "The Effect of Power Plants on Local Housing Prices and Rents," *Review of Economics and Statistics*, 93(4), 1391–1402.

Deaton, A. (1997): *The Analysis of Household Surveys. A Microeconometric Approach to Development Policy*. John Hopkins University Press.

——— (2009): "Instruments of Development: Randomization in the Tropics, and the Search for the Elusive Keys to Economic Development," *Keynes Lecture in Economics: Proceedings of the British Academy*, 162, 123–160.

Dinkelman, T. (2011): "The Effects of Rural Electrification on Employment: New Evidence from South Africa," *American Economic Review*, 101(7), 3078–3108.

Donaldson, D. (forthcoming): "Railroads of the Raj: Estimating the Impact of Transportation Infrastructure," *American Economic Review*.

Duflo, E., and R. Pande (2007): "Dams," *Quarterly Journal of Economics*, 122(2), 601–646.

Duranton, G., and M. A. Turner (2012): "Urban Growth and Transportation," *Review of Economic Studies*, 79(4), 1407–1440.

Fernandes, E. (2011): *Regularization of Informal Settlements in Latin America*. Policy Focus Report, PF023, Lincoln Institute of Land Policy.

Field, E., and M. Torero (2004): "Do Property Titles Increase Credit Access Among the Urban Poor?," *Working Paper*.

Gibbons, S., and S. Machin (2005): "Valuing Rail Access Using Transport Innovations," *Journal of Urban Economics*, 57(1), 148–169.

Gonzalez-Navarro, M., and C. Quintana-Domeque (2009): "The Reliability of Self-Reported Home Values in a Developing Country Context," *Journal of Housing Economics*, 18(4), 311–324.

——— (2015): "Description of the Acayucan Standards of Living Survey," *Working Paper*.

Goodman, J., and J. Ittner (1992): "The Accuracy of Home Owners' Estimates of House Value," *Journal of Housing Economics*, 2(4), 339–357.

Haughwout, A. F. (2002): "Public Infrastructure Investments, Productivity and Welfare in Fixed Geographic Areas," *Journal of Public Economics*, 83(3), 405–428.

Henderson, V. (2002): "Urbanization in Developing Countries," *World Bank Research Observer*, 17(1), 89–112.

INEGI, CONAPO, SEGOB, and SEDESOL (2007): *Delimitación de las zonas Metropolitanas de México 2005*.

Jacoby, H. G. (2000): "Access to Markets and the Benefits of Rural Roads," *Economic Journal*, 110(465), 713–737.

Jimenez, E. (1982): "The Value of Squatter Dwellings in Developing Countries," *Economic Development and Cultural Change*, 30(4), 739–752.

Joanis, M. (2011): "The Road to Power: Partisan Loyalty and the Centralized Provision of Local Infrastructure," *Public Choice*, 146(1), 117–143.

Kain, J., and J. Quigley (1972): "Note on Owner's Estimate of Housing Value," *Journal of the American Statistical Association*, 67(340), 803–806.

Kaufmann, D., and J. M. Quigley (1987): "The Consumption Benefits of Investment in Infrastructure: The Evaluation of Sites-and-Services Programs in Underdeveloped Countries," *Journal of Development Economics*, 25(2), 263–284.

Kiel, K. A., and J. E. Zabel (1999): "The Accuracy of Owner-Provided House Values: The 1978-1991 American Housing Survey," *Real Estate Economics*, 27(2), 263–298.

Kish, L., and J. Lansing (1954): "Response Errors in Estimating the Value of Homes," *Journal of the American Statistical Association*, 49(267), 520–538.

Kling, J. R., J. B. Liebman, and L. F. Katz (2007): "Experimental Analysis of Neighborhood Effects," *Econometrica*, 75(1), 83–119.

List, J. A., S. Sadoff, and M. Wagner (2011): "So you Want to Run an Experiment, Now What? Some Simple Rules of Thumb for Optimal Experimental Design," *Experimental Economics*, 14(4), 439–457.

MacKinnon, J. G., and M. D. Webb (2014): "Wild Bootstrap Inference for Wildly Different Cluster Sizes," Working Papers 1314, Queen's University, Department of Economics.

McMillen, D. P., and J. McDonald (2004): "Reaction of House Prices to a New Rapid Transit Line: Chicago's Midway Line, 1983-1999," *Real Estate Economics*, 32(3), 463–486.

Muellbauer, J. (2007): "Housing, Credit and Consumer Expenditure," *Federal Reserve Bank of Kansas City Jackson Hole Economic Policy Symposium*, pp. 267–334.

Tiebout, C. (1956): "A Pure Theory of Local Expenditures," *Journal of Political Economy*, 64(5), 416–424.

Van de Walle, D. (2002): "Choosing Rural Road Investments to Help Reduce Poverty," *World Development*, 30(4), 575–589.

Welch, B. L. (1947): "The Generalization of 'Student's' Problem when Several Different Population Variances are Involved," *Biometrika*, 34(1/2), 28–35.

Figures and Tables

Figure 1: Before Pavement

Figure 2: After Pavement

Figure 3: Acayucan Street Projects

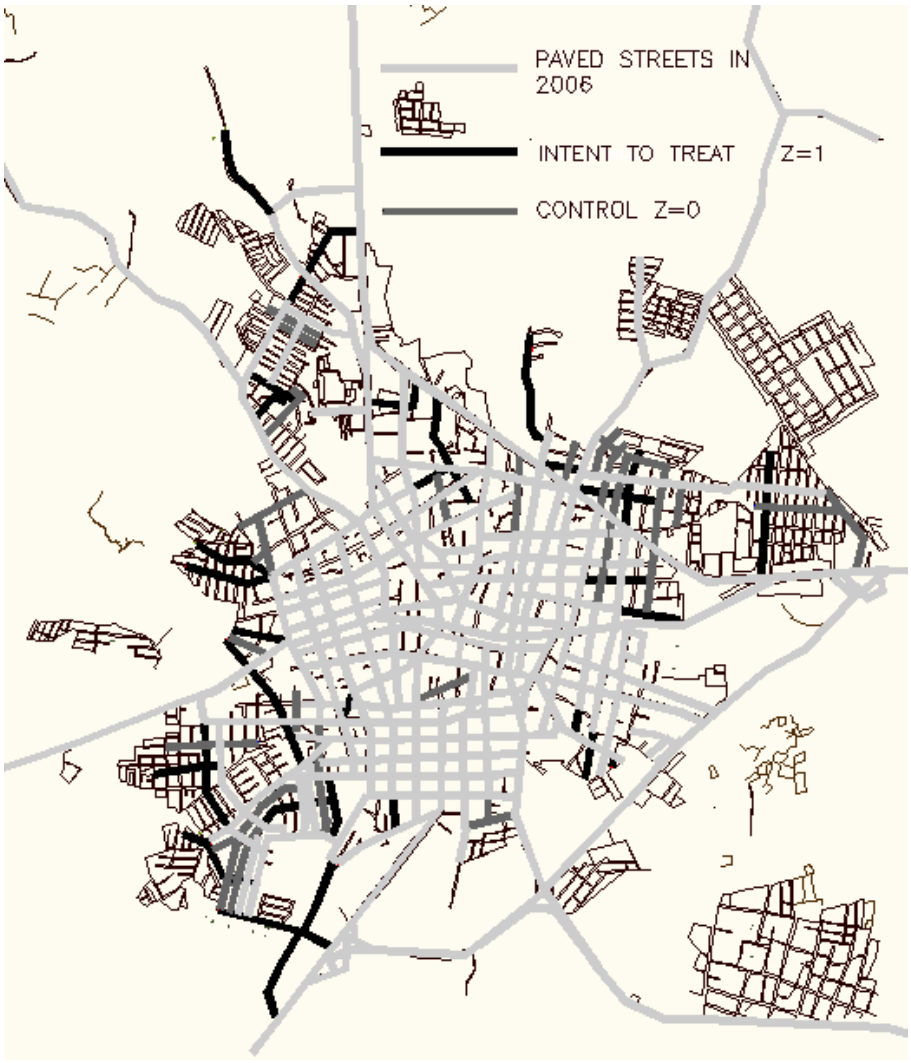
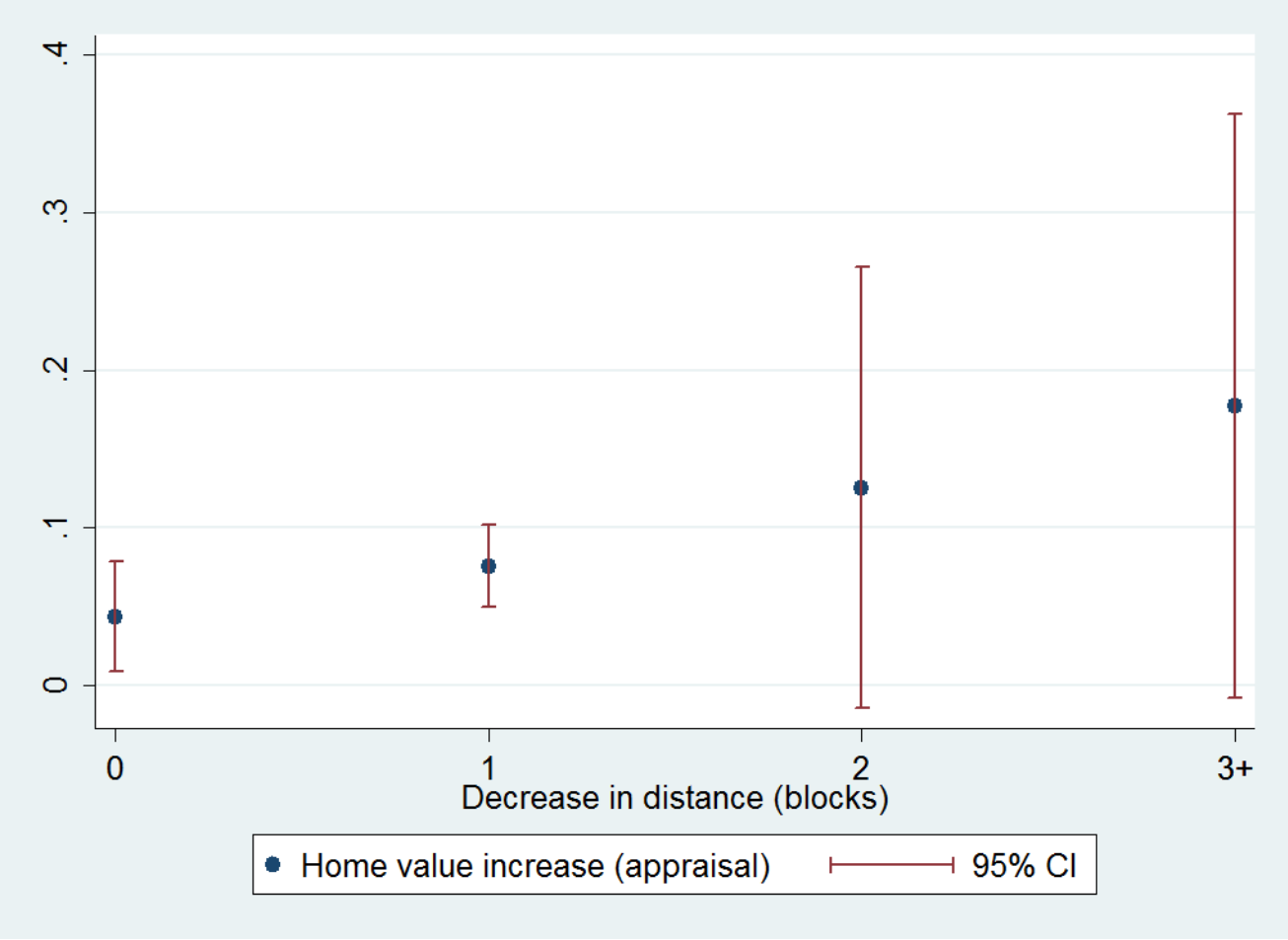


Figure 4: Change in House Value in Control Group



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

Table 1: Pre-Intervention Balance in Means

Variable	$Z=1$	$Z=0$	*Diff.*	Variable	$Z=1$	$Z=0$	*Diff.*
Consumption				**Credit (continued)**			
Monthly log per	6.77	6.69	0.08	Credit card	0.097	0.087	0.010
capita	(0.073)	(0.050)	(0.087)	(= 1)	(0.026)	(0.012)	(0.028)
expenditure	[461]	[403]	[864]		[480]	[410]	[890]
Monthly log sum of	6.60	6.49	0.11	Bank account (=1)	0.154	0.166	−0.012
itemized expenditures	(0.079)	(0.045)	(0.090)		(0.030)	(0.018)	(0.035)
per capita	[474]	[409]	[883]		[481]	[410]	[891]
Household appliances	2.12	2.04	0.08	**Labor and Transportation**			
(0-6)	(0.163)	(0.075)	(0.178)	Weekly hours	48.45	47.59	0.86
	[487]	[413]	[900]	worked(i)	(1.43)	(1.19)	(1.84)
Vehicles (car/truck/	0.203	0.226	−0.023		[498]	[429]	[927]
motorcycle)(0-3)	(0.050)	(0.033)	(0.059)	Monthly log labor	7.97	7.80	0.17*
	[487]	[413]	[900]	income (i)	(0.082)	(0.051)	(0.095)
Home improvements	0.541	0.474	0.067		[408]	[382]	[790]
(0-11)	(0.048)	(0.054)	(0.071)	Plans to migrate	0.410	0.418	−0.008
	[487]	[413]	[900]	in search of work (=1)	(0.030)	(0.022)	(0.037)
Bought materials	0.254	0.220	0.034		[431]	[370]	[801]
for home	(0.022)	(0.020)	(0.029)	Cost of taxi to	20.66	20.21	0.45
improvement (=1)	[485]	[409]	[894]	city center	(0.909)	(0.820)	(1.21)
Credit					[482]	[407]	[889]
Collateral-based	0.029	0.027	0.002	Time to city center	19.90	20.86	−0.96
credit (=1)(i)	(0.006)	(0.007)	(0.009)	(minutes)	(0.947)	(0.890)	(1.29)
	[1,047]	[937]	[1,984]		[487]	[412]	[899]
Collateral-based	658	429	229	**Housing**			
credit amount(i)	(272)	(152)	(308)	Log owner estimate	11.75	11.81	−0.06
	[1,047]	[937]	[1,984]	of house value	(0.12)	(0.10)	(0.15)
Non-collateral	0.050	0.034	0.016		[269]	[262]	[531]
based credit (=1)(i)	(0.008)	(0.006)	(0.010)	Log professional	11.64	11.60	0.04
	[1,047]	[937]	[1,984]	appraisal property	(0.08)	(0.05)	(0.10)
Non-collateral based	496	237	259*		[295]	[253]	[548]
credit amount(i)	(134)	(75)	(152)	Log professional	10.27	10.14	0.13
	[1,047]	[937]	[1,984]	appraisal land	(0.07)	(0.05)	(0.09)
Credit from family	0.006	0.004	0.002		[295]	[253]	[548]
and friends (=1)(i)	(0.003)	(0.003)	(0.004)	Log rent	6.48	6.53	−0.05
	[1,047]	[937]	[1,984]		(0.13)	(0.10)	(0.16)
Informal private	0.003	0.007	−0.004		[34]	[22]	[56]
credit (=1)(i)	(0.001)	(0.003)	(0.003)	Nearest paved street	1.49	1.35	0.14
	[1,047]	[937]	[1,984]	(street blocks)	(0.16)	(0.15)	(0.22)
					[487]	[411]	[898]

Note: (i) denotes individual-level outcomes. For each characteristic X, the average difference (*Diff*) is computed as the coefficient of a regression of X on the binary indicator Z, and its associated standard error (in parentheses) is clustered at the pavement-project level. Estimation takes survey weights into account. Number of observations in brackets. Individual variables regarding credit and labor outcomes for individuals aged 18+. Significance levels reported only for *Diff*: *** p-value<0.01, ** p-value<0.05, * p-value<0.1

Table 2: Anticipation Effects on Housing Value

Log professional appraisal of property value		
Assigned to treatment but unpaved	0.007	
	(0.036)	
	[344]	
Number of clusters	39	
p-value	Clustered	Wild Boostrap
	0.854	0.800
Log owner estimate of property value		
Assigned to treatment but unpaved	0.067	
	(0.167)	
	[338]	
Number of clusters	38	
p-value	Clustered	Wild Boostrap
	0.690	0.732
Log transaction price recent purchase		
Assigned to treatment but unpaved	1.20	
	(1.26)	
	[29]	
Number of clusters	16	
p-value	Clustered	Wild Boostrap
	0.355	0.652

Note: "Assigned to treatment but unpaved" is a dummy for observations from street projects assigned to pavement but unpaved by the time of the second survey. Sample consists of street projects assigned to control and assigned to treatment but unpaved. Regressions include a constant and the corresponding dependent variable at baseline. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Wild Boostrap p-values computed after 500 replications. *** p-value<0.01, ** p-value<0.05, * p-value<0.1

Table 3: Experimental Effects of Street Pavement on Out-migration Decision and Out-migrant Characteristics

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

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

Note: RF column uses assignment to pavement (Z) as independent variable. 2SLS column instruments pavement status (D) with assignment to pavement (Z). 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 RF and 2SLS: *** p-value<0.01, ** p-value<0.05, * p-value<0.1

Table 4: Experimental Effects of Street Pavement on Immigration Decision and Immigrant Characteristics

Immigration rate	Household Immigrated (=1)		
	RF	2SLS	Mean Control (2009)
	−0.007	−0.012	0.175
	(0.024)	(0.040)	(0.017)
	[1,083]	[1,083]	[497]

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

Note: RF column uses assignment to pavement (Z) as independent variable. 2SLS column instruments pavement status (D) with assignment to pavement (Z). 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 RF and 2SLS: *** p-value<0.01, ** p-value<0.05, * p-value<0.1

Table 5: Experimental Effects of Street Pavement on Property Values

	RF	2SLS	Mean Control (2009)
Log professional appraisal of property value	0.09***	0.16***	11.52
	(0.03)	(0.04)	(0.06)
	[548]	[548]	[253]
Log professional appraisal of land value	0.32***	0.54***	10.07
	(0.06)	(0.10)	(0.06)
	[548]	[548]	[253]
Log owner estimate of property value	0.16*	0.25*	12.01
	(0.09)	(0.15)	(0.08)
	[531]	[531]	[262]
Log rent	0.18*	0.31**	6.54
	(0.08)	(0.13)	(0.11)
	[56]	[56]	[22]
Log transaction price recent purchases ¶	0.44	0.85	10.82
	(0.65)	(1.22)	(0.38)
	[29]	[29]	[8]

Note: ¶Transaction price is amount paid by new homeowners (arriving between baseline and follow up), hence the dependent variable at baseline is not included. RF column uses assignment to pavement (Z) as independent variable. 2SLS column instruments pavement status (D) with assignment to pavement (Z). Regressions include a constant and the corresponding dependent variable at baseline. For log rent, we use as baseline control the rent paid by the family previously living in the same house in 2006. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Significance levels reported only for RF and 2SLS: *** p-value<0.01, ** p-value<0.05, * p-value<0.1

Table 6: Experimental Effects of Street Pavement on Consumption

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

Note: RF column uses assignment to pavement (Z) as independent variable. 2SLS column instruments pavement status (D) with assignment to pavement (Z). Regressions include a constant and the corresponding dependent variable at baseline. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Significance levels reported only for RF and 2SLS: *** p-value<0.01, ** p-value<0.05, * p-value<0.1

Table 7: Experimental Effects of Street Pavement on Credit Use

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

Note: ⓘ denotes individual-level outcomes. RF column uses assignment to pavement (Z) as independent variable. 2SLS column instruments pavement status (D) with assignment to pavement (Z). Regressions include a constant and the corresponding dependent variable at baseline. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Significance levels reported only for RF and 2SLS: *** p-value<0.01, ** p-value<0.05, * p-value<0.1

Table 8: Experimental Effects of Street Pavement on Transportation and Labor

	RF	2SLS	Mean Control (2009)
Cost of taxi to	−0.360	−0.587	18.14
city center (pesos)	(0.487)	(0.767)	(0.697)
	[889]	[889]	[407]
Time to city center (minutes)	−0.598	−0.989	19.04
	(0.920)	(1.52)	(0.789)
	[899]	[899]	[412]
Distance to nearest paved street	−0.46***	−0.75***	0.67
(street blocks)	(0.10)	(0.13)	(0.08)
	[898]	[898]	[411]
Weekly work hours ⓘ	2.31	3.77	47.29
	(1.42)	(2.46)	(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]

Note: ⓘ denotes individual-level outcomes. RF column uses assignment to pavement (Z) as independent variable. 2SLS column instruments pavement status (D) with assignment to pavement (Z). Regressions include a constant and the corresponding dependent variable at baseline. Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. Number of observations in brackets. Significance levels reported only for RF and 2SLS: *** p-value<0.01, ** p-value<0.05, * p-value<0.1

Table 9: Assessing Mechanisms

Dependent variable:	**Household Appliances (0-6)**		**Home Improvements (0-11)**		**Vehicles (0-3)**	
	(1)	(2)	(3)	(4)	(5)	(6)
Credit 09	0.026***	0.009**	0.003	0.003	0.002	−0.000
	(0.004)	(0.003)	(0.002)	(0.002)	(0.001)	(0.001)
Credit 06 × Credit 09	0.023***	0.012***	0.004***	0.005***	0.006***	0.002**
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Credit 06	0.027***	0.014***	0.008	0.006	0.014***	0.006
	(0.008)	(0.005)	(0.006)	(0.006)	(0.003)	(0.004)
Household appliances 06		0.646***				
		(0.038)				
Home Improvements 06				0.162***		
				(0.048)		
Vehicles 06						0.584***
						(0.078)
Observations	824	824	824	824	824	824

Note: Estimation takes survey weights into account. Standard errors clustered at the pavement-project level in parentheses. *** p-value<0.01, ** p-value<0.05, * p-value<0.1

Table 10: Cost-Benefit Analysis

	Plots	Average value	Effect of Pavement	Gains per plot	Total gains	Gain/Cost ratio
	814	27,844***	0.54***	15,081***	12,275,585***	1.09***
Standard error		(1,508)	(0.10)	(3,006)	(2,446,579)	(0.22)
95% Confidence interval						[0.66, 1.51]

Note: Plots column reports the number of plots abutting streets that were paved (residential, non-residential, and vacant). The average value of a plot is estimated by means of professional appraisals. Effect coefficient is taken from Table 5. 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. *** p-value<0.01, ** p-value<0.05, * p-value<0.1