Public Infrastructure, Private Investment and Residential Property Values: Experimental Evidence from Street Pavement

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Abstract

We design an infrastructure experiment in Mexico to evaluate the impact of street pavement on residential property values and private residential investment. We find that the provision of street pavement increases housing values by 21-25% according to homeowners, and 14-15% according to professional appraisals. Private investment responded too: Households on paved streets invest more in housing improvements than do those in the control group. A simple model allows us to test for the absence of complementarities between public and private investment in the production of housing. Since our non-complementarity tests do not reject, we conclude that the increase in private investment is due to a wealth effect. Finally, we estimate the magnitude of spillover effects to streets that did not get paved and use our estimates to provide a cost-benefit analysis of public investments in street pavement.

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1 Introduction

What is the price effect of street pavement on residential property?[1] The extensive literature on hedonic models has sought to answer this type of question ever since Rosen's (1974) seminal work. However, traditional hedonic estimates of the value of amenities as reflected in house prices have been subject to multiple identification criticisms (e.g., Brown and Rosen, 1982; Kanemoto, 1988). In this study we avoid the main shortcomings of past work by using random assignment of street pavement to identify the effect of this important type of publicly provided infrastructure on residential property values.

Between 2006 and 2009, in cooperation with the municipal government of the city of Acayucan, a city in southeast Mexico, we used random allocation to determine which street pavement projects would take place. Acayucan is one of Mexico's 56 metropolitan areas, with a population of 105,500 (INEGI, 2007). The neighborhoods in question lacked paved streets but were fully occupied by private residences, a situation common in many developing countries. We find that street pavement increased housing values by 21-25% according to homeowners, and 14-15% according to professional appraisals.[2]

Our investigation sheds light on the longstanding controversy in economics regarding the effects of public infrastructure expenditure on private investment. Aschauer's (1989) and Munell's (1990) pioneering empirical studies provided evidence consistent with the hypothesis that public investment in infrastructure induces an increase in the rate of return to private capital and thereby stimulates private investment expenditure. The first to raise doubts about these findings were Gramlich (1994) and Holtz-Eakin (1994), who pointed out identification issues. This prompted subsequent researchers to pay closer attention to these concerns, such as Fernald (1999) and, more recently, Michaels (2008). Even so, selection bias in the placement of infrastructure raises concerns about studies using observational

[1] We use the American term ≪street pavement≫ to refer to asphalt/concrete laid down for vehicular traffic. The British English equivalent is ≪road surface.≫

[2] This is consistent with Haughwout (1997) who showed that cities with more infrastructure have higher land values.

data that are hard to overcome (Duflo and Pande, 2007). Our experimental estimates show that the provision of street pavement increased private residential investment by 0.10 to 0.24 standard deviations with respect to the control group.

In order to understand the economic mechanism behind this causal relationship, we use a model of consumption, housing, and both public and private investment. Our main purpose is the derivation of testable implications regarding the *complementarity* between public and private investments in the production of housing. None of our tests reject non-complementarities in the production of housing. This leaves the wealth effect obtained from the raise in property value as the only plausible mechanism explaining the increase in housing investment.

Because our experiment took place in a single urban area, the provision of street pavement exogenously changed the distance to the nearest paved street for some of the properties in the control group, thereby allowing us to identify spillover effects. For every street block by which the distance to the nearest paved street was reduced, we observe an increase in the value of the properties on unpaved streets of 3%. This indicates that our experimental estimates are slightly downward biased.

Finally, in an approach reminiscent of those of Jacoby (2000) and of Rossi-Hansberg, Sarte and Owens III (2010), we measure the gains from street pavement as the sum of increases in property values. We estimate the gain to properties on paved roads to be 141% of pavement costs. Moreover, once we account for treatment spillovers to properties on unpaved streets, the total gains increase to 180% of street-pavement costs.

Our work is related to the core infrastructure literature: water, electricity, transportation, and communications appear to be necessary for the basic functioning of an economy. Galiani, Gertler and Schargrodsky (2005) document significant declines in child mortality as a consequence of water improvements in Argentina. Duflo and Pande (2007) find that the effects of irrigation dams in India depend on the population's location relative to the dam: downstream, agricultural production increases and rural poverty falls; upstream it increases.

Dinkelman (2008) shows that rural electrification in South Africa led to a reduction in the time women spend collecting firewood, allowing them to allocate more time to labor-market activities. Donaldson (2008) provides evidence that railroad infrastructure increased trade between Indian districts, not only increasing income but also reducing its volatility. Jensen (2007) established that the introduction of mobile phones among fishermen in India led to a drastic reduction in geographical price dispersion of fresh fish and thereby improved both producers' and consumers' welfare.

The structure of the paper is as follows. Section 2 describes the experimental design. Section 3 provides estimates of the increase in property value due to street pavement. Section 4 shows the impact of street pavement on residential investment and explores the complementarity between private and public investment in housing production. Section 5 inquires about the magnitude of spillovers to properties on unpaved streets. Section 6 contains a cost-benefit analysis, and section 7 presents our conclusions.

2 Experimental Design

2.1 Institutional Context

Municipal governments in Mexico are responsible for local public infrastructure, including sewerage, street pavement, and sidewalks. Each three-year administration has ample leeway as to budgetary allocations. The municipal budget consists mainly of transfers from general funds (revenues from the petroleum company, value added, and income taxes). Less than 10% derives from local taxes (property and business permit taxes). Property-tax receipts are very limited because cadastral property valuations are rarely updated and the city government does not legally prosecute non-payers.

As is the case in many other developing countries, many of the streets in the city's periphery were unpaved. The administration gave priority to the paving of streets in the city's more densely populated neighborhoods. The public-works office provided us with the plans for a set of 56 proposed street-pavement projects throughout the city. The administration was responsible for selecting and defining those projects. The street projects consisted of sets of contiguous city blocks that connected to the existing pavement grid.

Given that the administration would not be able to provide infrastructure for the 56 pavement projects, the mayor and the city council authorized us to select at random those streets to be paved, reasoning that it was in their interest to have a third-party evaluate their public-works program, and that this would be a fair and transparent allocation mechanism. We selected at random 28 out of the 56 proposed street projects in the summer of 2006. Figure 1 shows the location of experimental areas throughout the city: streets assigned to the treatment group ($Z = 1$) and streets assigned to the control group ($Z = 0$).

By March 2009, 17 of the streets in the treatment group had been completely paved, four projects were under way, and seven had yet to begin. The municipal government attributed the delays to bad 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. This situation, of one-sided noncompliance, permitted us to estimate the average effect of the treatment on the treated (in our case, the average effect of pavement on the paved), by means of Bloom's IV formula (1984). It is well known that the utility provided by a public good is in part determined by the degree of congestion (e.g., Inman, 1978). In our study, we did not find evidence of vehicle or pedestrian congestion either in paved or unpaved streets. Figure 2 presents an example of a street before and after pavement.

It is important to note that the Acayucan administration did not increase property taxes (by increasing either cadastral property values or tax rates) as a consequence of the paving of streets. Hence, the increase in property values we document reflects the market valuation of this public service.[3]

[3] In 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 (See Brueckner, 1982 or Haughwout, 2002.)

2.2 Data Sources

Transactions recorded in property registries have been the main data source in the study of property-value dynamics (e.g., Case and Shiller, 1987). However, the annual rate of residential property transactions is only about 5% of the stock (Goodman and Ittner, 1992). This means that only at the metropolitan level or higher do such transactions data serve as an accurate gauge of property-price dynamics. At the disaggregated level of the Acayucan street-pavement projects, transaction data are too sparse to permit the study of property-price changes.

Moreover, prices recorded in Mexican property registries are not reliable indicators of transaction prices. 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 property-tax assessed value, individuals do not usually report the actual amount paid for the property. Indeed, our examination of the local property-registry data revealed that transactions were not routinely registered immediately after the sale had taken place (only 40% of the transactions registered in a given year had taken place in that year), and more than 50% of residential transfers of property were registered as "gifts, donations, or inheritances." For these reasons, the best sources of property value that we could use were a) estimates by homeowners and b) professional property-value appraisals.

Self-reported housing values have long been used for research on the housing market. The Panel Study of Income Dynamics, the American Housing Survey, and the Survey of Consumer Finances all obtain a self-reported measure of housing value. The reliability of these measures has been well documented in multiple contexts (Kish and Lansing, 1954; Kain and Quigley, 1972; Jimenez, 1982; Goodman and Ittner, 1992; Kiel and Zabel, 1999; Bucks and Pence, 2006; Gonzalez-Navarro and Quintana-Domeque, 2009). The literature concludes that the evolution of self-reported housing prices generally mimics that of actual prices.

We obtained self-reported house valuations in March 2006 (before pavement) and March 2009 (after pavement) by means of two household surveys. (See Gonzalez-Navarro and Quintana-Domeque, 2010a for a full description of the survey.) We also obtained professional appraisals, which were performed by a single company throughout the city. Each appraisal requires a visit by the expert to the property and a careful evaluation of the approximate sale price. The fact that these professional appraisals are also used by banks to determine mortgages is an indicator of their reliability. We obtained professional appraisals of residential property value for half of the successfully interviewed households.[4]

The municipality did not announce to the population the existence of this study. Moreover, participants in the study (household respondents and the professional appraiser) were not aware of the objective of the survey. 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.

Descriptive statistics are provided in Table 1. Our sample consisted overwhelmingly of homeowners (95%), who lived in houses with an average of 2.4 rooms. In 2006, distance to the nearest paved street was on average 1.4 street blocks (a block being around 200 meters in length). Table 1 shows that in 2009 the average distance had decreased to 0.47 street blocks.

We asked heads of household for an estimate of property value. Although the non-response rate for this question, 38%, is quite high, Gonzalez-Navarro and Quintana-Domeque (2009) have shown that the probability of non-response is uncorrelated with professionally appraised values of these same properties. The median house value estimate is 200,000 Mexican pesos (13,700 2009 US dollars).[5] The median professional appraisal was 98,000 Mexican pesos (6,700 2009 US dollars). Self-reported housing values are on average higher than transactions prices (Goodman and Ittner, 1992), but this bias is uncorrelated with education of the household head or income. In Gonzalez-Navarro and Quintana-Domeque

[4]We did not obtain appraisals from all interviewed homes for budget reasons.

[5]23,500 PPP-adjusted 2009 US dollars.

(2009) the difference is only correlated with length of tenure.

To assess private residential home investment, we used a set of questions about home improvements made during the previous 6 months. Following Kling, Liebman, and Katz (2007), we create an index of private investment in housing consisting of the average of eleven areas in which improvements were measured: flooring, walls, roofing, sewerage connection, plumbing, bathrooms, electrical installation, room construction, house front, home security, and other remodeling. The summary index is defined as the equally weighted average of z-scores of its components. The z-scores are calculated by subtracting the control-group mean and dividing by the control-group standard deviation. Table 1 shows that the index of home improvements has an average value of 0.07. As a robustness check we also used "having bought construction material for the house in the past 6 months" as a proxy for housing investment; 17% of households reported having done so.

2.3 Identification

Although assignment to treatment was random, some streets thus assigned had not been paved by the time of the follow-up survey. Fortunately, the fact that all streets assigned to the control group remained unpaved, as promised by the municipality, creates a situation of one-sided noncompliance, enabling us to estimate the ATET by means of IV, Bloom (1984).[6] The IV estimate is obtained by regressing the outcome of interest on the treatment, where the latter is instrumented by assignment status.

We provide evidence in Table 2 that randomization successfully balanced subjects' characteristics across the intent-to-treat ($Z = 1$) and control ($Z = 0$) groups in 2006, before treatment. Indeed, as shown in the third column of the Table, none of the variables present significant differences.[7] Given the lack of systematic differences in mean (observable) characteristics between the intent-to-treat and control groups before the intervention, we have a

[6] To borrow the terminology of Angrist, Imbens, and Rubin (1996), there are no always-takers.

[7] For a description of balance in means for all survey variables see Gonzalez-Navarro and Quintana-Domeque (2010b).

valid instrument to identify the ATET.[8]

Had we obtained perfect compliance, the average effect of treatment on the treated would have been the same as the average effect of the treatment on a randomly chosen street (ATE, or average treatment effect). However, we provide two types of evidence suggesting that noncompliance in our experiment was random, and thus that our experimental estimates identify the ATE.

First, we show that the paved ($e = 1$) and the unpaved ($e = 0$) groups have the same mean pretreatment characteristics, a situation consistent with no selection on pretreatment characteristics. The last column in Table 2 shows that there were no significant differences in any of the variables across paved and unpaved groups before the intervention.

Second, under random noncompliance we should not find statistical differences between the OLS (ATE) and the IV (ATET) estimates. This can be formally tested using the Durbin-Wu-Hausman (DWH) test, a regression-based form of the Hausman test designed to detect systematic differences between OLS and IV estimates (Hausman 1978, 1983; Wooldridge, 2002). The DWH test produces a robust test statistic, even under heteorskedastic errors (Davidson, 2000).[9] We note that the results from this test, which are reported in the tables on experimental impacts explained in what follows, are consistent with random noncompliance.

Overall, our baseline balance findings suggest that: (i) we have a valid instrument to identify the ATET, (ii) ATET=ATE, and (iii) both OLS and IV estimates should provide similar estimated effects.

Finally, we note that the intention-to-treat effect (ITTE) could be easily obtained by regressing the observed outcome of interest on a constant and Z. However, in this context ITTE is not of interest because we are not concerned about the impact of being assigned to the pavement group in the experiment. Rather, we are interested in the effects of providing street pavement, i.e., the ATE or the ATET.

[8]The assumption being that if there are no mean differences in observable characteristics, there will be no mean differences in unobservable characteristics.

[9]The test consists of including the residuals from the first stage in the second stage regression and testing that the coefficient on the residuals is zero.

3 The Impact of Street Pavement on Residential Property Value

We present our main experimental estimates for the effect of street pavement on property values in Table 3. In the top panel, the dependent variable is self reported house value, while in the lower panel the dependent variable is professional appraisals. In the first column, we report the OLS estimate from equation (1), where H_j is (log) housing value in year j, and e is an indicator for being paved:

$$H_{2009} = \alpha_1 + \beta_1 e + \gamma_1 H_{2006} + \epsilon_1 \tag{1}$$

Adding the lagged outcome variable as a control variable is standard in the impact-evaluation literature (Imbens and Angrist, 1994; Duflo, Glennerster, and Kremer, 2007; Kling, Liebman, and Katz, 2007) because it reduces the standard error on the coefficient of interest.

In column (IV) we estimate the effect of pavement on the paved. We use Z - being randomly assigned to receive pavement - as an instrument for e:

$$H_{2009} = \alpha_2 + \beta_2 e + \gamma_2 H_{2006} + \epsilon_2 \tag{2}$$

$$e = \alpha_3 + \beta_3 Z + \gamma_3 H_{2006} + \epsilon_3 \tag{3}$$

The last column provides the mean of the outcome variable for the control group in 2009. All regressions use the survey weights, and standard errors are clustered at the street project level to account for intra-street correlation.

The top panel in the table shows that pavement increased housing values by 21% according to the OLS regression, and by 25% according to the IV regression. The DWH test that the coefficients are the same is not rejected ($p = 0.71$). The lower panel, which uses the professional appraisals, indicates that pavement generated an increase in property values of 14% (OLS) and 15% (IV). Again, according to the DWH test, the coefficients are not statistically different ($p = 0.66$), suggesting that OLS identifies the ATE.

It is noteworthy that our experimental estimates vary according to the measure of house value being used. With self-reported valuations street pavement increases property values by 21-25%, whereas according to the professional appraiser, housing values increase by 14-15%. To be on the safe side, we will use the estimate based on the professionally appraised measure in the cost-benefit analysis.

Before proceeding to the next section, a note of caution is warranted. The existence of general equilibrium effects may affect our experimental estimates. For example, let us assume that there are two distinct markets for housing in the city: one for houses on unpaved streets and one for houses on paved streets. In equilibrium, the latter commands higher prices than than does the former. The paving of streets increases the citywide supply of houses on paved streets and decreases the citywide supply of houses on unpaved ones. The general equilibrium effect is to reduce the price of paved houses and increase the price of unpaved ones (though not sufficiently to reverse the ordering). Thus, our estimates of impact on housing value are expected to be downward biased because of the general equilibrium effect. On the other hand, if these general equilibrium effects only become apparent to the appraiser and the homeowners over time, the estimates we obtained may be upward biased. Unfortunately, we cannot identify the magnitude of this bias with our data.

We now turn to the question of whether public investment in the form of street pavement induced an increase in private residential investment.

4 Private Residential Investment and Public Infrastructure

4.1 Does Private Residential Investment Respond to Public Infrastructure?

To analyze the effects of public investment on private residential investment, we present a simple model that captures the main economic forces at play in our experimental setting (For recent work on housing supply elasticity, see Glaeser, Gyourko, and Saks, 2006; and Saiz, 2010). The representative household seeks to maximize a utility function $U(H, C)$ that depends positively on housing H and consumption C. We assume strict concavity and twice differentiability of the utility function. Housing is a function of residential private investment i and public investment e, where we think of e as indicating whether the street is paved or not.[10] Our study explores the impact of an *exogenous* manipulation of e for a set of households located in houses on unpaved streets. In this sense, we can think of e as a parameter, so that $H = H(i; e)$.

An important consideration in our context is that households are for the most part home owners (95%).[11] Thus, not only is the house part of the family's wealth, for homeowners it normally constitutes the single most important depository of wealth (e.g., Campbell and Cocco, 2007). For the purposes of our study, it makes sense to account for the fact that the principal beneficiaries of public-infrastructure investment are property owners (Haughwout, 2002). To make matters as stark as possible, we assume that the household's wealth is equal to H. The budget constraint is: $r \cdot H = C + p \cdot i$. The household spends a fraction r of wealth and uses it either to consume C or to invest in home improvements i.

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

[11]Furthermore, for the analysis using self-reported housing values, we restrict attention to homeowners.

The first-order condition for optimization of the household's problem is given by:

$$\frac{1}{p}\frac{\partial H}{\partial i}\left[\frac{\partial U}{\partial H}+r\frac{\partial U}{\partial C}\right]=\frac{\partial U}{\partial C} \tag{4}$$

The left-hand side can be interpreted as the marginal benefit of investing an additional dollar on i. An additional dollar on i buys $1/p$ units of i, which produces $\partial H/\partial i$ units of H. The extra unit of housing not only produces utility $\partial U/\partial H$ (housing services) but also provides a return of $r \cdot \partial U/\partial C$, a wealth effect. When H increases, the budget constraint expands by $r \cdot dH$. On the right-hand side of the equation is the marginal cost of investing a dollar in i, which is the utility lost by a reduction of a unit of consumption.

The optimality condition (4) together with the budget constraint defines i^* and C^*. Solving for $\frac{di^*}{de}$ yields an expression of ambiguous sign. The intuition for the ambiguity is quite clear though. In the *argmax* the household chooses i to achieve the desired level of housing services. When the government provides an increase in e, this translates into a higher level of H than was originally desired, so the household has an incentive to cut down on H by reducing i. This is the case, for example, in the model by Rossi-Hansberg, Sarte and Owens III (2010), in which an increase in a neighborhood's land value causes a decrease in private investment. The major difference in our case is that there is a wealth effect working in the *opposite* direction, increasing desired housing services. These two opposite effects generate an ambiguous sign for the effect of infrastructure on private investment for homeowners. In addition, there is the issue of whether i and e are complements in the production of $H = H(i; e)$. If $\frac{\partial^2 H}{\partial i \partial e}$ is positive, an increase in e generates higher returns to private investment and thus a shift towards higher private investment. The question of how private investment is affected by public infrastructure, however, can be given an empirical answer that relies on the identification provided by the *randomization*.

We provide experimental evidence of the impact of street pavement on private residential investment in Table 4. In the first column, we report the OLS estimate from equation (5), where i_j is the private-residential-investment index in year j, and e is an indicator for having pavement:

$$i_{2009} = \alpha_4 + \beta_4 e + \gamma_4 i_{2006} + \epsilon_4 \quad (5)$$

In column (IV) we estimate the effect of pavement on the paved. We again use Z − being randomly assigned to receive pavement − as an instrument for e:

$$i_{2009} = \alpha_5 + \beta_5 e + \gamma_5 i_{2006} + \epsilon_5 \quad (6)$$

$$e = \alpha_6 + \beta_6 Z + \gamma_6 i_{2006} + \epsilon_6 \quad (7)$$

The top panel shows that households on paved streets invested 0.10 standard deviations (p=0.121) more than households on unpaved streets according to the OLS estimate. The IV estimate in the second column suggests that pavement increased private residential investment by 0.24 standard deviations (p=0.037). The estimates indicate the position of the mean of the treatment group in the distribution of the control group in terms of standard-deviation units.[12]

The lower part of the panel provides a robustness check for these results using a measure of the purchase of materials for home improvement in the past 6 months. If respondents declare that they have invested in the property, we should find a corresponding difference in the purchase of construction materials. Table 4 shows that this is indeed the case. Households on paved streets bought between 25 and 50% more construction materials in the previous 6 months than did those on unpaved streets. Whether this increase is in part due to a complementarity effect between private and public investment is investigated in the next subsection.

[12]See Gyourko and Saiz (2004) for evidence that private residential investment responds positively to neighborhood property values.

4.2 Testing for Complementarities between Public and Private Investment in Housing Production

Rearranging the optimality condition (4) we obtain:

$$\frac{\frac{\partial U}{\partial H}}{\frac{\partial U}{\partial C}} = \frac{p - r\frac{\partial H}{\partial i}}{\frac{\partial H}{\partial i}} \tag{8}$$

Equation (8) must hold regardless of the value of e. However, note that if e and i are not complements, $\frac{\partial^2 H}{\partial i \partial e}$=0, then the right hand side of the optimality condition does not change with a change in e. This suggests two independent tests of complementarities of e and i in the production of H.

In order to test for complementarities, we need an *identification* assumption; namely, the relationship between consumption and housing must be independent of income. This is achieved by assuming quasi-homotheticity.[13] As Deaton and Muellbauer (1980) point out, aggregation over agents requires that the preferences of each agent be at least quasi-homothetic.[14] For this type of utility functions, the linear relationship between consumption and housing can be identified in a *cross section* for given prices, interest rate, and level of public investment e. Figure 3 shows the relationship between per capita expenditure and self reported housing value in 2006. It depicts both a scatter plot and a Lowess curve. The fact that the slope of the Lowess curve is fairly constant provides support to our identifying assumption.

If there were no complementarities between e and i in the production of H, the introduction of street pavement e would not change the ratio on the right-hand side of equation (8). Because quasi-homotheticity implies a linear relationship between C and H for all utility levels, the coefficient on a linear regression between C and H in the cross section would be unchanged with changes in e. On the other hand, with complementarities between e and i

[13]Quasi-homothetic preferences makes the Engel curves (income-consumption paths for fixed prices) linear, but does not require them to pass through the origin; whereas ordinary homotheticity does.

[14]A well-known example of quasi-homothetic utility function is the Stone-Geary utility function which is the quasi-homothetic version of the Cobb-Douglas (see Gorman 1959, 1961).

in the production of H, the introduction of street pavement e would increase the right-hand side, and thereby alter the equilibrium relationship between C and H across households with different levels of utility.

Our first test therefore compares the cross-sectional relationship between per capita consumption and housing value by street-pavement status in 2009. Given that e was exogenously allocated, we can split the sample into paved $e = 1$ and unpaved $e = 0$ without worrying about sample selection. In practice, we estimate the following equation system:

$$C = \alpha_7 + \beta_7 \cdot H + \epsilon_7 \quad \text{if } e{=}1$$

$$C = \alpha_8 + \beta_8 \cdot H + \epsilon_8 \quad \text{if } e{=}0$$

Under the null hypothesis of no complementarities, $\beta_7 = \beta_8$. If we reject, this is evidence of complementarities in the production of H.[15]

The first three columns in Table 5 present the results of the estimation. The self-reported housing value appears in the top panel, the appraised home value in the lower. As indicated in the third column, the estimation shows that the positive relationship between housing and consumption is almost identical in the paved and unpaved groups, regardless of the measure of housing value (p=0.85, p=0.97 respectively). We therefore cannot reject non-complementarities between public and private investment in the production of housing.

The second test uses data on the relationship between private residential investment and housing value. Under the assumption of complementarities, the marginal return to private investments increases when there is street pavement. Under no complementarities, the marginal return is unchanged when there is street pavement.[16] Our null hypothesis is:

$$H_0 : \quad \frac{\partial H}{\partial i}\Big|_{e=1} = \frac{\partial H}{\partial i}\Big|_{e=0} \tag{9}$$

where i is the home improvement index. Again we split the sample into paved and unpaved

[15] Or substitutability, which is statistically possible, but economically unexpected.

[16] See footnote 15.

groups and estimate:

$$H = \alpha_9 + \beta_9 \cdot i + \epsilon_9 \quad \text{if } e\text{=1}$$

$$H = \alpha_{10} + \beta_{10} \cdot i + \epsilon_{10} \quad \text{if } e\text{=0}$$

and test the null hypothesis $\beta_9 = \beta_{10}$. The results of the estimation are presented in the last three columns in Table 5, which shows that we cannot reject the hypothesis that the relationship between home investments and housing value is the same across the paved and unpaved groups (p=0.41, p=0.96 respectively).

Further evidence of lack of complementarities in the production of housing comes from directly regressing housing value in 2009 on e, i, and their interaction. Specifically, we estimate:

$$H_{2009} = \alpha_{11} + \beta_{11}e + \gamma_{11}i_{2009} + \phi_{11}e \times i_{2009} + H_{2006} + \epsilon_{11} \tag{10}$$

As shown by Imai, Keele and Yamamoto (2010), unbiased estimation with mechanism i as a regressor requires sequential ignorability. However, here we are only interested in the significance of the interaction term. As Table 6 shows, the interaction is not significantly different from zero for either measure of housing value, which is consistent with non-complementarities.

These findings suggest that while the provision of pavement did cause an increase in private residential investment, it did not change the marginal return to private investment in the production of housing. Once we discard the complementarity channel, our theoretical model suggests that the observed increase in private investment due to street pavement is in fact explained by a wealth effect.

Finally, it is worth mentioning that in the last three columns of Table 5, although the coefficients do not change with variations in e, the coefficients of private residential investments on professionally assessed housing value are a third to half of those on self-assessed housing value. This is partly due to the fact that many of the home-improvement questions relate to measures which were unobservable to the professional appraiser, such as electrical installation, sewerage connection, etc.

5 Accounting for Treatment Spillovers

Households in the control group may have benefitted from the pavement provided to the treated group. This may happen whenever a street-pavement project in the treatment group reduces the distance to the nearest paved street for some homes in the control group. This is actually what we observe: a reduction of 0.76 street blocks on average among the control group. As long as distance to the nearest paved street affects home value, our previous experimental estimate of the impact of street pavement on home value will be downward biased.

Figure 4 shows that among unpaved streets, there was a larger increase in home value the larger the drop in distance to the nearest paved street. In fact, the relationship seems linear, although the 95% confidence interval becomes quite large as the reduction in distance to pavement increases (due to the reduction in the number of observations).[17]

We obtain a measure of the spillover effect by estimating:

$$H_{2009} = \alpha_{12} + \beta_{12}\Delta d + \gamma_{12} H_{2006} + \epsilon_{12} \tag{11}$$

for the control group, where Δd is the change in distance to the nearest paved street between 2006 and 2009. Note that β_{12} is identified because, within the control group, the change in distance to the nearest paved street is *exogenous*.

The estimation is presented in the first column of Table 7, which shows that a decrease by the length of one street block in the distance between a given house and the pavement grid was correlated with a 3.2% (p =0.115) higher housing value, albeit the effect is imprecisely

[17]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.

estimated. In the second column of the Table, we provide an estimate of the impact of a reduction in the distance to the nearest paved street using the full sample and controlling for whether the street was actually paved or not. In this case we estimate a 2.4% ($p = 0.102$) higher housing value for every street block that is reduced in terms of distance to the nearest paved street. This second regression gives us the average effect of distance reduction in the treatment and control groups.

Given that in the control group the reduction in distance to the nearest paved street was on average 0.7 street blocks, our preferred estimate from the first column suggests that the experimental estimate of the impact of street pavement on housing values of 14-15% for the appraiser and 21-25% for home owners is downward biased by 2 percentage points. In conclusion, we find some evidence of treatment spillovers in the control group. Being close to a paved-street project increased housing values by 3% for every street block by which the distance was reduced.

6 Cost-Benefit Analysis

Costs are measured as the sum of municipality expenditures on each street paved. Specifically, the municipality reported that total pavement expenditures amounted to $11,304,642$ Mexican pesos. Following Jacoby (2000) and Rossi-Hansberg, Sarte, and Owens III (2010), we calculate the benefits as the sum of estimated impacts on housing values.[18] We compute them separately for properties that benefitted directly (being on streets that got paved) and for those, on control streets, that benefitted indirectly (because of spillover effects).

Table 8 reports the results of our cost-benefit analysis. The top row presents results for the group of 587 properties on streets that got paved. We estimate that houses in this group would have been worth an average of 159,250 Mexican pesos had the streets not gotten paved. Multiplying this base value by the estimated impact of street pavement gives an average benefit per property of 27,072 Mexican pesos. The last column shows that the gain

[18] An alternative strategy can be found in Kaufman and Quigley (1987).

to the directly affected group represents 141% of the construction costs.

In the lower row of Table 8, we present the gains of the 1,624 properties on streets that did not get paved. They experienced an average increase in value of 2%, or 2,741 Mexican pesos, due to the reduction in their distance to the nearest paved street. Thus, even houses on streets that did not get paved benefitted, indirectly, from the pavement program. The last column shows that the spillover benefits represent 39% of construction costs.

Summing up both direct and indirect benefits provides an estimate of total gains to costs equal to 1.80. This finding suggests there is a positive return to street pavement, and that accounting for positive spillovers has a substantial impact on the cost-benefit analysis.[19]

7 Conclusion

Basic infrastructure is necessary for the adequate functioning of an economy. Despite an extensive effort in economics to analyze the effects of many types of infrastructure, endogeneity bias has always been a concern; it is difficult to overcome the argument that comparisons of places with and without infrastructure using observational data can be misleading.

We designed a unique street-pavement experiment in Mexico, the first to solve the selection bias inherent in street-pavement placement by using random assignment, and estimated positive experimental effects on home values and residential investment. Whether the increase in residential investment was in part due to a complementarity between public and private investment in the production of housing could be tested by simply assuming a linear relationship between consumption and housing at different utility levels. The fact that we were unable to reject non-complementarities between public and residential investment, demonstrates that the main force at stake was a wealth effect.

We also documented that the provision of pavement reduced the distance to the nearest

[19] Alternatively, we could have used the self-reported measures to perform the cost-benefit analysis. This would have led to an even larger benefits-to-cost ratio. Note that given that private investment was uncorrelated with appraised housing value, we did not have to correct for this in the cost-benefitted analysis presented here.

paved street for some of the properties in the control group. This treatment-spillover effect could then be measured by estimating the increase in home values due to the reduction in distance for the control group. We show that residential property values were 3% higher per street block that was reduced in distance to the nearest paved street, indicating that the experimental effect from the treatment-control groups comparison was slightly downward biased. Correcting for the downward bias, we find that housing values increased by 23-27%, according to homeowners, or by 16-17% according to a professional appraiser.

Summing up the benefits for properties on both paved and unpaved streets and given the municipality expenditures on each street pavement project, we obtain an estimated total-gains-to-costs ratio equal to 1.80. This positive cost-benefit ratio persuades us that the financing of local public infrastructure using property taxation is a fertile field for future research.

In this paper we have shown how a research collaboration with government can allow for an experimental analysis of public infrastructure provision. Our findings encourage the replication of experimental infrastructure evaluations in other contexts to obtain multiple internally valid estimates. This would in turn reveal whether the observed causal effects in Acayucan are both internally and externally valid. If they are, the implications for public policy at the municipal level are of worldwide significance.

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Tables and Figures

Table 1: Descriptive Statistics (2009)

Variable	Obs.	Mean	Median	SD	10%	90%
Homeowner (=1)	897	0.95	1	0.23	0	1
Rooms	900	2.42	2	1.19	1	4
Nearest paved street (street blocks)	893	0.47	0	0.74	0	1
Owner estimate of house price	531	297,667	200,000	351,766	40,000	600,000
Professional appraisal of house price	395	155,161	98,000	369,684	52,000	280,400
Home improvements index (6 months)	900	0.07	−0.19	0.64	−0.19	0.64
Bought materials (=1) (6 months)	894	0.17	0	0.38	0	1

Mean calculation takes survey weights and clusters into account.

Rooms is the number of rooms in the house excluding kitchen, unless it is also used for sleeping.

House value estimate and *Professional appraisal* in 2009 Mexican pesos.

Home improvements index is a sum of standardized indicators for improvements in: flooring, walls, roofing, sewerage connection, plumbing, toilets, electrical, room construction, remodeling, security measures, and improvements to house front.

Table 2: Pre-intervention Balance in Means (2006)

Variable	ITT ($Z = 1$)	Control ($Z = 0$)	*Diff.*	Paved ($e = 1$)	Unpaved ($e = 0$)	*Diff.*
Homeowner (=1)	0.93	0.94	−0.009	0.93	0.94	−0.010
	(0.017)	(0.014)	(0.022)	(0.019)	(0.013)	(0.022)
	[486]	[411]	[897]	[299]	[598]	[897]
Rooms	2.35	2.38	−0.03	2.42	2.35	0.07
	(0.06)	(0.07)	(0.09)	(0.08)	(0.06)	(0.10)
	[487]	[413]	[900]	[300]	]600]	[900]
Nearest paved	1.49	1.33	0.15	1.44	1.38	0.06
street (street blocks)	(0.16)	(0.15)	(0.21)	(0.20)	(0.13)	(0.23)
	[486]	[407]	[893]	[295]	[598]	[893]
Log owner estimate	11.74	11.81	−0.06	11.84	11.77	0.07
of house price	(0.12)	(0.10)	(0.15)	(0.16)	(0.08)	(0.18)
	[269]	[262]	[531]	[169]	[362]	[531]
Log professional	11.64	11.65	−0.009	11.70	11.63	0.066
appraisal of house price	(0.07)	(0.06)	(0.09)	(0.10)	(0.05)	(0.11)
	[210]	[185]	[395]	[130]	[265]	[395]
Home improvements index	0.036	0.00	0.036	0.031	0.01	0.02
(6 months)	(0.023)	(0.024)	(0.033)	(0.026)	(0.02)	(0.03)
	[487]	[413]	[900]	[300]	[600]	[900]
Bought materials (=1)	0.25	0.22	0.03	0.27	0.22	0.05
(6 months)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.03)
	[485]	[409]	[894]	[300]	[594]	[894]

Standard errors in parenthesis clustered at the pavement project level. Number of observations in parenthesis. Estimation takes survey weights into account.

Rooms is the number of rooms in the house excluding kitchen, unless it is also used for sleeping. *House value estimate* and *Professional appraisal* in 2009 Mexican pesos. *Home improvements index* is a sum of standardized indicators of improvements in: flooring, walls, roofing, sewerage connection, plumbing, toilets, electrical, room construction, remodeling, security measures, and improvements to house front.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 3: Effect of Street Pavement on Housing Value

	Log owner estimate of house price		
Variable	OLS	IV	Mean Control (2009)
Street pavement (=1)	0.212**	0.250*	12.01
	(0.105)	(0.145)	(0.074)
Observations	531	531	262
DWH test ($\beta_{OLS} = \beta_{IV}$)	F(1,53)=0.14 (p-value=0.71)		
	Log professional appraisal of house price		
Street pavement (=1)	0.139***	0.152***	11.57
	(0.038)	(0.048)	(0.067)
Observations	395	395	185
DWH test ($\beta_{OLS} = \beta_{IV}$)	F(1,53)=0.19 (p-value=0.66)		

Regressions include a constant and the lagged outcome. Standard errors in parenthesis clustered at the street pavement project level. Estimation takes survey weights into account.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 4: Effect of Street Pavement on Private Residential Investment

	Home improvement index		
Variable	OLS	IV	Mean Control (2009)
Street pavement (=1)	0.105	0.241**	0.00
	(0.066)	(0.116)	(0.03)
Observations	900	900	413
DWH test ($\beta_{OLS} = \beta_{IV}$)	F(1,54)=1.97 (p-value=0.17)		
	Bought material for home improvements		
Street pavement (=1)	0.047*	0.086*	0.15
	(0.026)	(0.046)	(0.02)
Observations	894	894	409
DWH test ($\beta_{OLS} = \beta_{IV}$)	F(1,54)=1.22 (p-value=0.27)		

Regressions include a constant and the lagged outcome. Standard errors in parenthesis clustered at the street pavement project level. Estimation takes survey weights into account.

Home improvement index is a sum of standardized indicators of improvements in: flooring, walls, roofing, sewerage connection, plumbing, toilets, electrical, room construction, remodeling, security measures, and improvements to house front during the previous 6 months.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 5: Testing for Complementarities between Public and Private Investment in Housing Production

	Log owner estimate of house price						
	Log(PCE)				Log(H)		
Variable	Paved	Unpaved	Diff.		Paved	Unpaved	Diff.
Log(*H*)	0.216***	0.230***	−0.013	Home improvement	0.295**	0.176***	0.118
	(0.062)	(0.032)	(0.068)	index	(0.135)	(0.054)	(0.142)
Observations	167	356	523	Observations	167	356	523
Mean	6.94	6.75		Mean	12.26	12.01	
	(0.101)	(0.042)			(0.154)	(0.074)	
	Log professional appraisal of house price						
	Log(PCE)				Log(H)		
Variable	Paved	Unpaved	Diff.		Paved	Unpaved	Diff.
Log(*H*)	0.178	0.173***	0.005	Home improvement	−0.090	−0.097	0.007
	(0.112)	(0.049)	(0.120)	index	(0.126)	(0.078)	(0.144)
Observations	128	258	386	Observations	128	258	386
Mean	6.95	6.72		Mean	11.76	11.56	
	(0.81)	(0.054)			(0.106)	(0.054)	

Standard errors in parenthesis clustered at the street pavement project level. Estimation takes survey weights into account.

PCE is household per capita expenditure.

Home improvement index is a sum of standardized indicators of improvements in: flooring, walls, roofing, sewerage connection, plumbing, toilets, electrical, room construction, remodeling, security measures, and improvements to house front during the previous 6 months.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 6: Testing for Complementarities between Public and Private Investment in Housing Production

	Log owner estimate of house price				**Log professional appraisal of house price**		
	(1)	(2)	(3)		(4)	(5)	(6)
Street pavement (=1)	0.212**	0.190*	0.179	Street pavement (=1)	0.138***	0.139***	0.145***
	(0.105)	(0.108)	(0.109)		(0.042)	(0.042)	(0.042)
Home improvement		0.211***	0.193***	Home improvement		−0.009	−0.000
index		(0.043)	(0.053)	index		(0.014)	(0.014)
Street pavement×home			0.073	Street pavement×home			−0.047
improvement index			(0.103)	improvement index			(0.045)
Observations	531	531	531	Observations	370	370	370

Standard errors in parenthesis clustered at the street pavement project level. Estimation takes survey weights into account.
Home improvement index is a sum of standardized indicators of improvements in: flooring, walls, roofing, sewerage connection, plumbing, toilets, electrical, room construction, remodeling, security measures, and improvements to house front during the previous 6 months.
The estimations control for housing value in 2006.
* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 7: Spillovers: Reduction in Distance to the Nearest Paved Street and Housing Values

Variable	$log(H)$	$log(H)$
	Control Group	Full Sample
Δd	-0.032	-0.024
	(0.020)	(0.014)
Paved		0.110**
		(0.043)
Observations	262	389

Standard errors in parenthesis clustered at the street pavement project level. Estimation takes survey weights into account.

Δd is the change in distance to the nearest paved street before and after the experiment ($\Delta d \leq 0$).

The estimations control for housing value in 2006.

* significant at 10%; ** significant at 5%; *** significant at 1%.

Table 8: Cost-Benefit Analysis

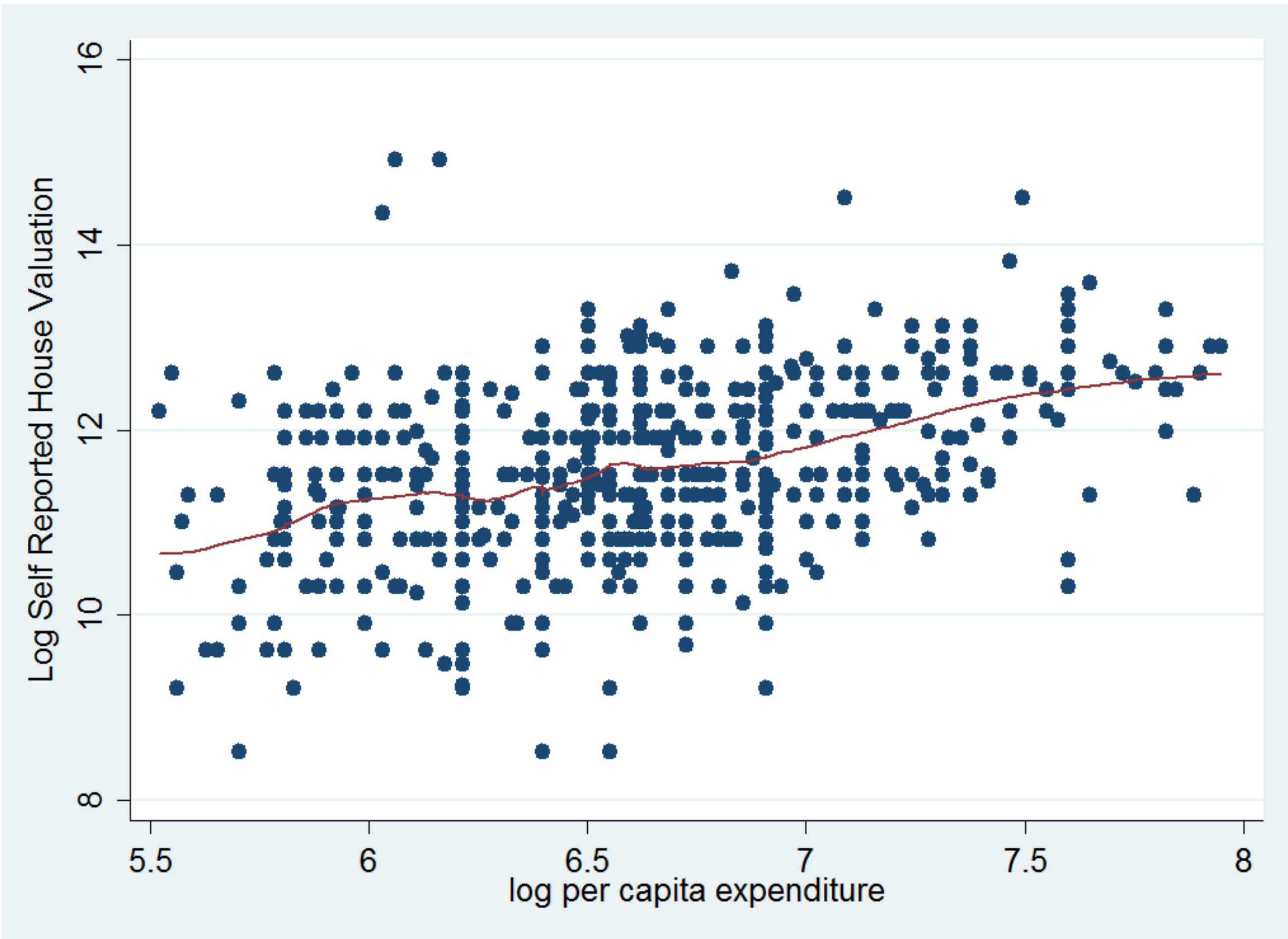
	Properties	Average value	Impact	Gains per property	Gains	Gain/Cost ratio
Paved	587	159,250	0.17	27,072	15,891,264	1.41
Unpaved	1,624	137,094	0.02	2,741	4,451,384	0.39
Total					20,342,648	1.80

Figures in 2009 Mexican pesos. 2009 PPP exchange rate was 8.5 pesos per US dollar. Nominal February 2009 exchange rate was 14.6 Mexican pesos per US dollar.

Figure 1: Acayucan Street Projects

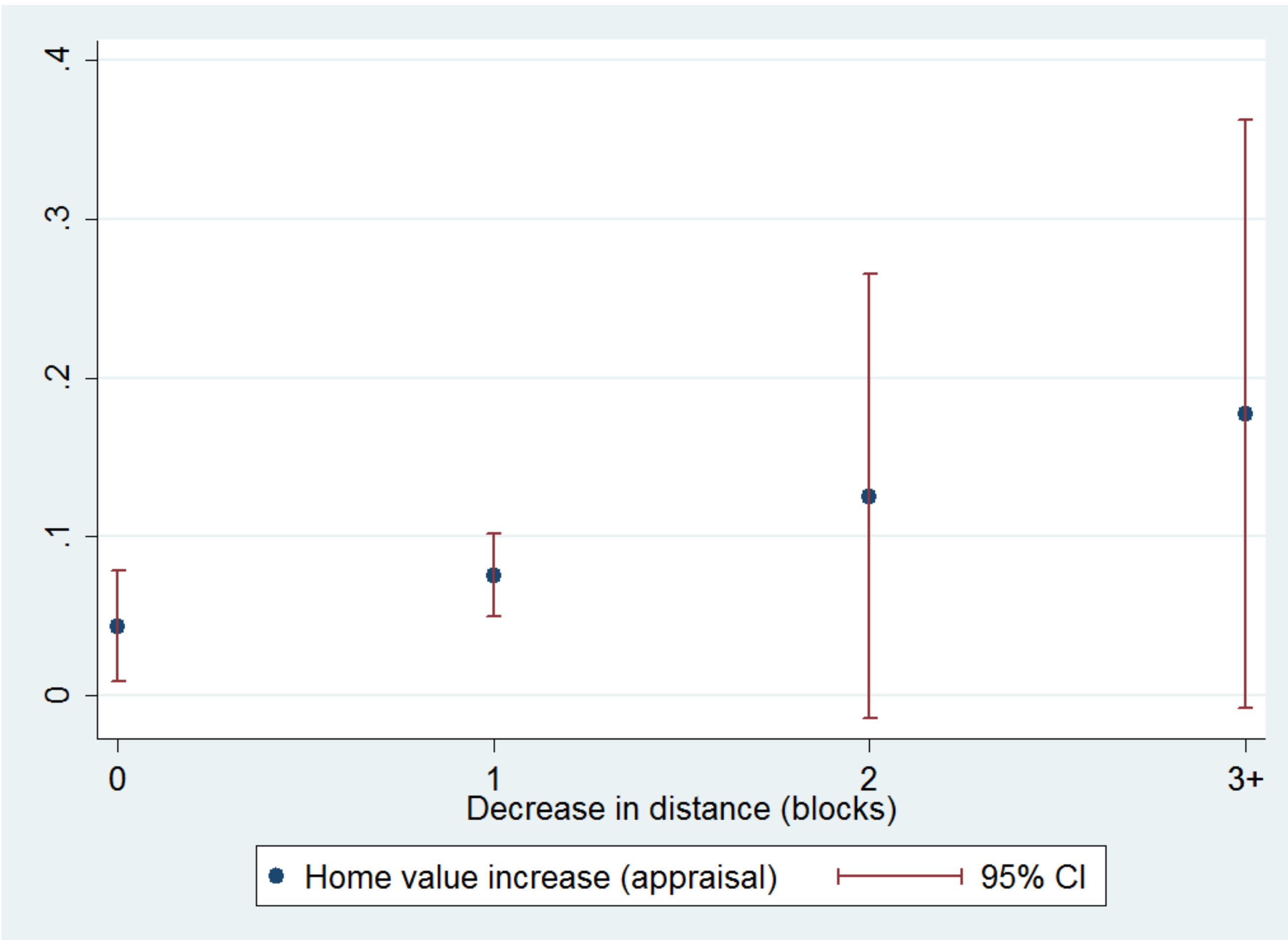
Figure 2: Before and After Example

Figure 3: The Relationship Between Consumption and Housing in 2006



Lowess curve with bandwith=0.3.

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.