

IDB WORKING PAPER SERIES N° 01248

# Do Slum Upgrading Programs Impact School Attendance?

Wladimir Zanoni  
Paloma Acevedo  
Diego Guerrero

# Do Slum Upgrading Programs Impact School Attendance?

Wladimir Zanoni  
Paloma Acevedo  
Diego Guerrero

Cataloging-in-Publication data provided by the  
Inter-American Development Bank

Felipe Herrera Library

Zanoni López, Wladimir, 1975-

Do slum upgrading programs impact school attendance? / Wladimir Zanoni, Paloma Acevedo, Diego Guerrero.

p. cm. — (IDB Working Paper Series ; 1248)

Includes bibliographic references.

1. Slums-Social aspects-Uruguay. 2. Urban renewal-Uruguay. 3. School attendance-Uruguay. I. Acevedo, Paloma. II. Guerrero, Diego. III. Inter-American Development Bank. Housing and Urban Development Division. IV. Inter-American Development Bank. Education Division. V. Inter-American Development Bank. Office of Strategic Planning and Development Effectiveness. VI. Title. VII. Series.

IDB-WP-1248

Keywords: Slum Upgrading, school absences, regression discontinuity

JEL Codes: I25; C54; D04; O18

<http://www.iadb.org>

Copyright © 2021 Inter-American Development Bank. This work is licensed under a Creative Commons IGO 3.0 Attribution-NonCommercial-NoDerivatives (CC-IGO BY-NC-ND 3.0 IGO) license (<http://creativecommons.org/licenses/by-nc-nd/3.0/igo/legalcode>) and may be reproduced with attribution to the IDB and for any non-commercial purpose, as provided below. No derivative work is allowed.

Any dispute related to the use of the works of the IDB that cannot be settled amicably shall be submitted to arbitration pursuant to the UNCITRAL rules. The use of the IDB's name for any purpose other than for attribution, and the use of IDB's logo shall be subject to a separate written license agreement between the IDB and the user and is not authorized as part of this CC-IGO license.

Following a peer review process, and with previous written consent by the Inter-American Development Bank (IDB), a revised version of this work may also be reproduced in any academic journal, including those indexed by the American Economic Association's EconLit, provided that the IDB is credited and that the author(s) receive no income from the publication. Therefore, the restriction to receive income from such publication shall only extend to the publication's author(s). With regard to such restriction, in case of any inconsistency between the Creative Commons IGO 3.0 Attribution-NonCommercial-NoDerivatives license and these statements, the latter shall prevail.

Note that link provided above includes additional terms and conditions of the license.

The opinions expressed in this publication are those of the authors and do not necessarily reflect the views of the Inter-American Development Bank, its Board of Directors, or the countries they represent.



# Do Slum Upgrading Programs Impact School Attendance?

Wladimir Zanoni\*  
Paloma Acevedo\*\*  
Diego Guerrero\*\*\*

\* Inter-American Development Bank, [wladimirz@iadb.org](mailto:wladimirz@iadb.org)

\*\* Inter-American Development Bank, [palomaa@iadb.org](mailto:palomaa@iadb.org)

\*\*\* University of Tennessee–Knoxville, [guerrero@utk.edu](mailto:guerrero@utk.edu)

## Table of contents

Abstract .....	3
Acknowledgements .....	3
1. Do Slum Upgrading Programs Impact School Attendance? .....	4
2. A Theory of Change .....	8
2.1. The SUP programs in Uruguay .....	10
3. Data .....	12
4. Identification Strategy .....	15
5. Results .....	18
5.1. Validity tests: adding covariates .....	21
5.2. Validity tests: placebo tests .....	23
5.3. Validity test: manipulation around the threshold .....	25
5.4. Heterogeneous effects .....	26
6. Discussion .....	27
References .....	30
Appendix .....	36
Appendix 1. Normalizing the time frame in the data .....	36
Appendix 2. SUPs and parental expectations .....	37
Tables and Figures .....	38

## Abstract

This paper analyzes how slum upgrading programs impact elementary school children's attendance in Uruguay. We take advantage of the eligibility rule that deems slums eligible for a SUP program if they have 40 or more dwelling units. Using a fuzzy regression discontinuity estimator, we find that students exposed to SUPs are between 23 and 63 percent less likely to be at the 90th percentile of the yearly count of school absences. That effect appears to impact boys and girls similarly, irrespective of the age and time since the program started. We discuss some critical urban and education policy implications of our findings.

## Acknowledgements

Thanks to Veronica Adler and Marcelo Pérez Alfaro for encouraging and supporting this study.

Thanks to the Ministry of Housing, Territorial Planning and Environment (MVOTMA) for sharing the data on urban settlements, and thank you to its Evaluation Division, particularly Pablo Cruz for his strong collaboration and guidance in urban issues.

Thanks to Norbert Schady, Allen Blackman, Sebastian Gallegos, Nora Ruth Libertun de Durden, Ana Maria Cuesta, Celeste Carruthers, and participants in the workshops of the IADB's Housing and Urban Development Division and the MVOTMA for their comments and suggestions.

Thanks to the National Administration of Public Education (ANEP), particularly the General Direction of Early Childhood and Primary Education for sharing the data on school absences. Oscar Montañés was of great help to understand the database. Juan Bogliaccini and Juan Pereira provided excellent early data management work.

Finally, we also thank Carola Alvarez and Tatiana Gallego for their support.

## 1. Do Slum Upgrading Programs Impact School Attendance?

The rapid growth of Latin American cities is a widespread phenomenon consistently accompanied by the expansion of informal settlements or “slums.” Slums are urban areas generally characterized by a lack of access to basic services (such as water, sanitation, and electricity) and a lack of property rights on the lands dwellers occupy. Slum-dwellers lag in many socioeconomic dimensions. They are poorer and less educated than residents in the formal city, and they are also more likely to be subject to discrimination because of their ethnic, racial, or migrant origins. Combined, these elements of precariousness lead households in slums to fall into intergenerational poverty traps. Data from the World Bank indicates that in 2018 nearly 20.8 percent of the urban population of Latin America (LATAM) lived in such slums.

For the last five decades, governments in Latin America have carried out slum upgrading programs (SUP's hereafter), aimed at integrating slums into the social and urban fabric of the formal areas of cities. While the most conspicuous objective of SUPs is addressing deficiencies in basic infrastructure services, as explained in next section, they are also implemented as investments in social development, urban integration (in the sense of social inclusion of its population), and the formalization of property rights. Multilateral Development Institutions are the main financiers of these programs. For instance, in the period 1992-2005, the World Bank and the Inter-American Development Bank invested USD 11.7 billion (about USD 1 billion a year) in Latin America, almost half of which (USD 5 billion) was invested by the Inter-American Development Bank (Bahl, Linn and Wetzel, 2013).

Since SUPs are comprehensive interventions that address several triggers of poverty traps (including inadequate infrastructure, poor social capital, and lack of property rights), one would expect that they have direct positive impacts on several social outcomes. In fact, there is some evidence in impact evaluations, though scarce, of components associated with SUPs programs being effective at decreasing the incidence of malnutrition and waterborne diseases (Galiani and Schargrotsky, 2004; Galiani, Gonzalez-Rozada and Schargrotsky, 2009; Cattaneo et al., 2009; Moraes et al., 2003; Field, 2003, 2005; Fort, 2008; Aiga et al., 1999), and increasing hours of work and employment (Field, 2007) and overall quality of life, as reflected in higher property values (Acevedo, Hobbs and Martinez, 2017; Gonzalez Navarro and Quintana-Domeque, 2016, 2010).

In this paper, we contribute to the broad research agenda that seeks to answer whether SUPs programs impact children's educational outcomes. To that end, we address whether children who reside in urban slums change their school attendance rates when SUPs intervene in their neighborhoods. From a scientific perspective, our research helps close a knowledge gap in the understanding of the mechanisms through which deprived neighborhoods in developing countries influence human capital accumulation. There is little empirical evidence of the impact of all SUPs' components on educational outcomes (Galiani and Schargrotsky, 2010) and inconclusive evidence of their impact on absenteeism (Gonzalez Navarro and Quintana-Domeque, 2010; Goytia and Dorna, 2019), even though SUPs can improve both the transaction costs associated

with attending school and the returns to parents of investing in their children's education. From a policy perspective, we provide evidence on the impact of urban policies on social outcomes, and on the effectiveness of SUPs on absenteeism. This is relevant because children's failure to attend school regularly is consistently observed across developing countries, and poor school attendance is empirically linked to poor school performance, dropout, and several outcomes in adult life<sup>1</sup>. We offer a key input to broaden the policy discussion about the long-term cost-efficiency of SUP programs. This discussion persistently revolves around the high costs of these interventions, while only considering a subset of their short-term local benefits.

To answer this research question, we present a case from Uruguay and evaluate the impact of the SUP program there on the daily attendance of elementary school children. Despite its wealth (Uruguay is the second highest income country in Latin America), the country is not exempt from the regional pattern in slum growth: As its cities grow, so do its slums. In 2018, Uruguay had 607 slums encompassing 165,000 inhabitants in 50,000 households<sup>2</sup>. As it is typical in the region, many Uruguayan slums lack paved roads and sewage systems and have insecure access to water and electricity services. Homes built in these slums have been financed (and often constructed) by the slums' occupants on occupied land<sup>3</sup>. Uruguayan slum dwellers are also socioeconomically disadvantaged compared to inhabitants of the formal city. As Figure 1 reveals, slum dwellers are younger, form bigger households, exhibit higher income-to-poverty ratios, and have fewer years of education than their counterparts who do not live in slums<sup>4</sup>.

---

<sup>1</sup> That attendance predicts school failure is documented in Goldman (2014); Gershenson, Jacknowitz and Brannegan (2014); Connolly and Olson (2012); Allensworth and Easton (2007); Gottfried (2009); Nichols (2003); Romero and Lee (2007); Ready (2010); Neild and Balfanz (2005); Ginsburg, Jordan and Chang (2014). Poor attendance also predicts longer-term outcomes such as poor health outcomes (Eaton, Brener and Kann, 2008; Allison, Attisha and on School Health, 2019), and a variety of other outcomes because it heavily loads into non-cognitive skills (Kautz and Zanoni, 2015; Heckman and Kautz, 2012; Almlund et al., 2011).

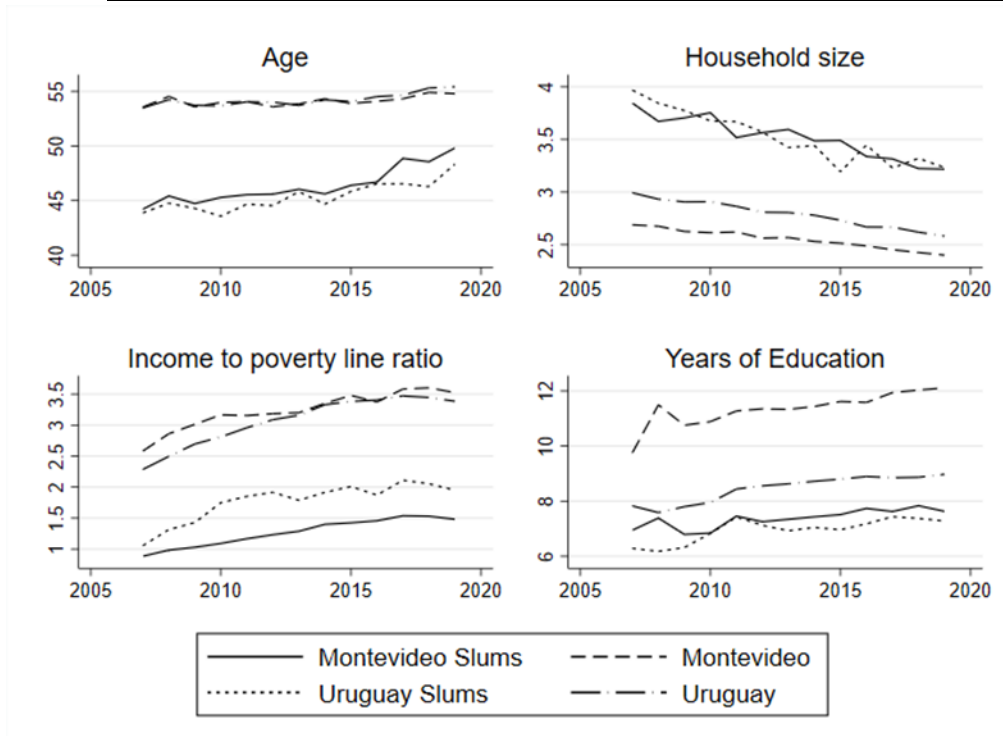
<sup>2</sup> The Uruguayan census defines informal settlement geography as that which joins 10 or more dwellings built on public or private land without authorization from the owner, under unregulated conditions, in noncompliance with urban development regulations.

<sup>3</sup> There is an informal property rights housing market where informal rights on the dwellings are often traded and/or leased. See: [http://pmb.mvotma.gub.uy/sites/default/files/asentamientosrecientesuruguay\\_0.pdf](http://pmb.mvotma.gub.uy/sites/default/files/asentamientosrecientesuruguay_0.pdf)

<sup>4</sup> This pattern is observed in both Montevideo (Uruguay's capital) and the rest of the country.



**Figure 1**  
**Selected household characteristics. Montevideo vs. the rest of the country.**



Notes: The variables “age” and “years of education” refer to the head of household.  
 Source: Encuesta Continua de Hogares by Uruguay’s National Institute of Statistics (INE).

The government of Uruguay has invested in SUPs since 1999. By 2018, those programs had intervened in 104 slums (representing more than 11,000 households, or 46,000 individual beneficiaries), 43 of which were in Montevideo, the country’s capital<sup>5</sup>. SUPs in Uruguay built urban infrastructure and provided public services, while promoting initiatives to strengthen human and social capital and helping slum dwellers obtain formal property rights.

Our research is motivated by the hypothesis that school attendance patterns among Uruguayan children living in slums have been affected by those SUP programs through at least two pathways that we summarize here (and explain in more detail later in the document). First, infrastructure improves mobility (through paved roads, sewerage, public lighting, etc...) and health (through the provision of clean water and sanitation that reduces water borne diseases) lowering the transaction costs of going to school and increasing attendance. Second, because of SUPs’ comprehensive interventions (including infrastructure, social investments and land titling), parents may have changed their expectations of the returns from investing in their children’s human

<sup>5</sup> Twenty-eight of those projects had been finalized, eight were in the construction stage, and the rest were in the planning stage.

capital and invested more in education (reflected in improved school attendance)<sup>6</sup>.

The eligibility rules for the assignment of slums to SUP programs provide for an opportunity to identify the causal effects of these programs on school attendance. The eligibility rules require slums to have at least 40 houses. We classified students who lived in slums according to their settlements' eligibility, based on the number of dwellings, for a SUP intervention. We also classified students by whether SUPs had intervened in their places of residency. Assignment to SUPs based on the number of dwellings was not perfect, with some smaller settlements receiving SUPs and some bigger ones not getting them. Given this setup, we implemented a fuzzy regression discontinuity (FRD) estimator where the number of dwellings is the running variable, and 40 dwellings is its cutoff. We computed local average treatment effects of the Uruguayan SUP program on school attendance.

To carry out the empirical analysis, we combined several data sets, including: 1) administrative records of students attending public, early childhood and elementary schools between 2013 and 2018; 2) geographical data on the characteristics and location of slums in the country; 3) data on the location, timing, and characteristics of the SUP interventions implemented, and; 4) data from household surveys aggregated at the neighborhood level.

School attendance is a variable that counts the occurrence of a behavior, and it is precisely the cumulative pattern in that behavior what triggers impacts on educational outcomes. To capture this cumulative process, we classified students according to whether they fall in the 90th percentile of the school absences distribution. We call that pattern "recurrent truancy." This 90th percentile threshold follows administrative standards adopted by some public-school districts in the USA<sup>7</sup> to classify students who fail to attend classes regularly and be subject to disciplinary sanctions or interventions.

In this study, we found that students who resided in intervened slums improved their school attendance patterns: they were between 23 and 63 percent less likely to be recurrent truants that what they would have been in the absence of those interventions. We did not find heterogeneities in the impacts of the program on attendance. In particular, improvements in attendance appear to impact boys and girls similarly, irrespective of the age and time since the program started.

Those FRD estimates are robust to the choices of bandwidths around the eligibility threshold, local polynomial model specification, and the type of kernel weight assigned to the estimator. The results are also robust to including covariates and to placebo testing on the threshold. There is no evidence of strategic manipulation of the threshold.

---

<sup>6</sup> This last mechanism would be triggered by the fact that now (with SUPs) parents see opportunities for social mobility because their communities are recognized by the state and provided with basic infrastructure, while their access to social services has improved, and they are granted property rights to their homes.

<sup>7</sup> The Chicago Public Schools District, among those (see: CPS, 2007).

To our knowledge, our research marks the first study employing a causal inference framework to study how SUPs affect the educational outcomes of low-income children. We expect these results to highlight the relevance that SUP programs have in contributing to accumulating human capital in low income children. SUP programs appear to be fostering an opportunity for the younger generations of slum residents to break out of their poverty traps.

The document is organized as follows. First, we develop a theory of change that explains the mechanisms linking school absences with SUPs. The next section describes the analytical database and variables. Third, we present the FRD framework and provide the context for it with reference to SUPs in Uruguay. Section IV shows our general results. We then describe in detail results for subsamples grouped by gender, students' ages, and the stages of the SUP intervention. Finally, we discuss the policy implications and highlight the role of SUPs in affecting human capital accumulation.

## 2. A Theory of Change

This section lays out our hypothesis of how SUPs affect elementary school students' daily attendance. The empirical evidence on how SUPs impact children's attendance is scarce (Jaitman and Brakarz, 2013; Soares, Soares et al., 2005; Magalhães et al., 2016). However, several studies in the economics of human development, urban economics, and neighborhood effects provide building blocks of evidence linking the types of investments that SUPs make with the development of children's human capital.

The three components (or groups of investments) that characterize SUPs are:

- 1) infrastructure: delivering paved roads, sidewalks, street lighting, water and sanitation, and improving public spaces in the neighborhood;
- 2) social: bringing to the neighborhood social service workers who interact with the inhabitants during the programs, linking social needs with existing programs (such as job training and drug abuse programs) and promoting community networks to strengthen social capital, and;
- 3) property rights: facilitating access to formal property rights for occupied spaces and homes.

Empirical evidence suggests, with regard to the infrastructure component, that investments that improve mobility, such as paving roads and constructing sidewalks, reduce the costs (in terms of time and money) of taking children to school (Lenhoff, Singer and Cook, 2020; Gottfried, 2017). Although a randomized control trial found no significant effects on school attendance after paving streets in Mexico (GonzalezNavarro and Quintana-Domeque, 2010), other factors besides paving may reduce mobility costs. Neighborhoods' physical appearance (mostly the provision of street lighting) has also been associated with reductions in crime (Chalfin et al., 2019; Blattman et al., 2017; Farrington and Welsh, 2002; Doleac and Sanders, 2015; Kondo et al., 2016; Branas et al., 2018), and safer streets have been found to impact school attendance (Burdick-Will, Stein and

Grigg, 2019; Burdick-Will et al., 2019; Burdick-Will, 2018; Curran, 2019; Sanfelice, 2019; McMillen, SarmientoBarbieri and Singh, 2019). Furthermore, Goytia and Dorna (2019) find evidence of a statistical association between SUPS, reduction in floods and school absenteeism. Enhanced mobility may also lead to the integration of public transportation in the slum, and public transportation access can lower absenteeism (Gottfried, 2017)<sup>8</sup>.

Also related to the infrastructure component is access to basic services, such as water and sanitation. Such access is consistently found to have an effect on children's health, with positive consequences for school attendance. SUPs can reduce school absenteeism by successfully providing water and sanitation infrastructure that makes kids and parents healthier. Healthier parents face fewer barriers in getting their children to school, and healthier children are less likely to be absent from classes. (Trinies et al., 2016; Freeman et al., 2014; Sclar et al., 2017; Galiani, Gonzalez-Rozada and Schargrotsky, 2009; Aiga et al., 1999; Moraes et al., 2003; Ashraf et al., 2017; Aiga and Umenai, 2002).

The social component of SUPs facilitates access to social service and employment programs, such as those that provide workforce job training and skills development. These programs have been found to positively affect labor market outcomes (Card, Kluve and Weber, 2018; Urzuá and Puentes, 2010; González-Velosa, Rosas and Flores, 2016; Ibarrarán and Rosas Shady, 2009; Attanasio, Kugler and Meghir, 2008; Ibarrarán et al., 2019) and, in the short term, expectations about the future (Acevedo et al. 2020). The Becker and Lewis (1973) quantity-quality theory of human capital investment suggests that these improved labor market outcomes for parents, including wages, and longer life expectancy lead to better investments in the human capital of children<sup>9</sup>.

With regard to the property rights component, evidence from Latin America also suggests that formal property rights acquisition leads to improved educational outcomes in children from low-income families (Galiani and Schargrotsky 2010). Acquisition of land titles appears to positively influence expectations of upward social mobility, promoting greater investments in children's human capital. Whether improved school attendance is the mechanism driving these effects is still unknown. Nonetheless, land titling has positive effects on health and fertility (Galiani and Schargrotsky, 2004; Field, 2003, 2005; Fort, 2008) which may also contribute to improvements in school attendance.

---

<sup>8</sup> There is, however, conflicting evidence on transportation costs and education in slums, suggesting the relationship might be negative (Lenhoff, Singer and Cook, 2020). This might be so because, in areas with high poverty, the means of transportation are heterogeneous. For instance, surveys in Mumbai indicate that although only 44 percent of the population walks to work, 63 percent of the poor do so walking (Baker et al., 2005). Meanwhile, the literature on urban economics shows that the poor benefit from public transportation and concentrate at a certain distance from the city center depending on the means of transportation available (Glaeser, Kahn and Rappaport, 2008).

<sup>9</sup> There is empirical evidence supporting this theory: as life time income increases, the number of children per family decreases, and the investment in each child's quality increases (Aizer and Cunha, 2012; Li, Zhang and Zhu, 2008; Angrist, Lavy and Schlosser, 2005).

Overall, we identify two potential pathways through which SUPs may affect school attendance. First, the provision of infrastructure decreases mobility costs for dwellers and improves their health: Once the physical infrastructure is built, it becomes easier to transit roads and sidewalks, streetlights make streets safer, sewage systems lower the probability of flooding during the rains, and access to water and sanitation improves health. Together, all these improvements lead to lower transaction costs for attending school for both parents and children. Second, SUPs improve parents' expectations regarding the returns from their children's education. This leads them to improve the quantity and quality of the investments they make in their children's human capital development (including taking them more regularly to school). To the extent that comprehensive SUP interventions lead parents to foresee better life and labor market prospects for them and their children, investments today in their children's human capital have higher expected returns. These changes in parental expectations as a result of SUPs interventions would improve investments in their children's human capital development.

In Appendix A.A2, we develop a simple microeconomic model that summarizes the elements described here. This model can guide our economic intuition regarding how interventions, like SUPs, affect investments in the human capital of children. The basic intuition of the model is that the ratio between the marginal decrease in absences and a marginal decrease in mobility costs derived from a SUP intervention will equal the ratio of the marginal benefit of consumption to the parent's marginal benefit from the accumulation of human capital. Therein, to the extent that parents foresee that reductions in transaction costs have higher relative marginal utility from investing time in their children's attendance than from income, they would send their kids more regularly to school.

## 2.1. The SUP programs in Uruguay

The government of Uruguay has invested in SUP programs since 1999. By 2018, these programs had been implemented in 104 slums (representing more than 11,000 households or 46,000 individual beneficiaries), 43 of which were in Montevideo (the country's capital)<sup>10</sup>.

The projects generally aim to improve the living conditions of the resident population in slums and other degraded areas. The specific goal is to promote urban integration through the provision of basic infrastructure, adequate social and urban services, a guarantee of tenure security with regard to property and an improvement in social capital. The programs are usually organized around three pillars or components.

First, an infrastructure component includes design and construction activities: (i) to expand or improve the infrastructure and basic urban services; (ii) improve the environment; (iii) reduce the vulnerability of the population settled in high risk areas; and (iv) provide or rehabilitate urban

---

<sup>10</sup> Twenty-eight of those projects had been finalized, eight were in the construction phase, and the rest were in the planning stage.

equipment and furniture. The interventions are comprehensive in nature and generally implemented in stages. They are included as eligible investments: (i) basic urban infrastructure networks (potable water, sanitary sewerage, storm drainage, road paving, electrification and public lighting); (ii) in some cases, a basket of materials to facilitate individual connections for toilets and sanitation networks; (iii) construction, rehabilitation or conditioning of social infrastructure (community centers, nurseries, healthcare centers and / or child and family care centers, among others); (iv) relocation of families, when necessary; and (v) initiatives taken together with citizens for the protection and / or recovery of the environment. The specific initiatives in each settlement are defined in the urbanization plans drawn up in preparation for the program or throughout its execution.

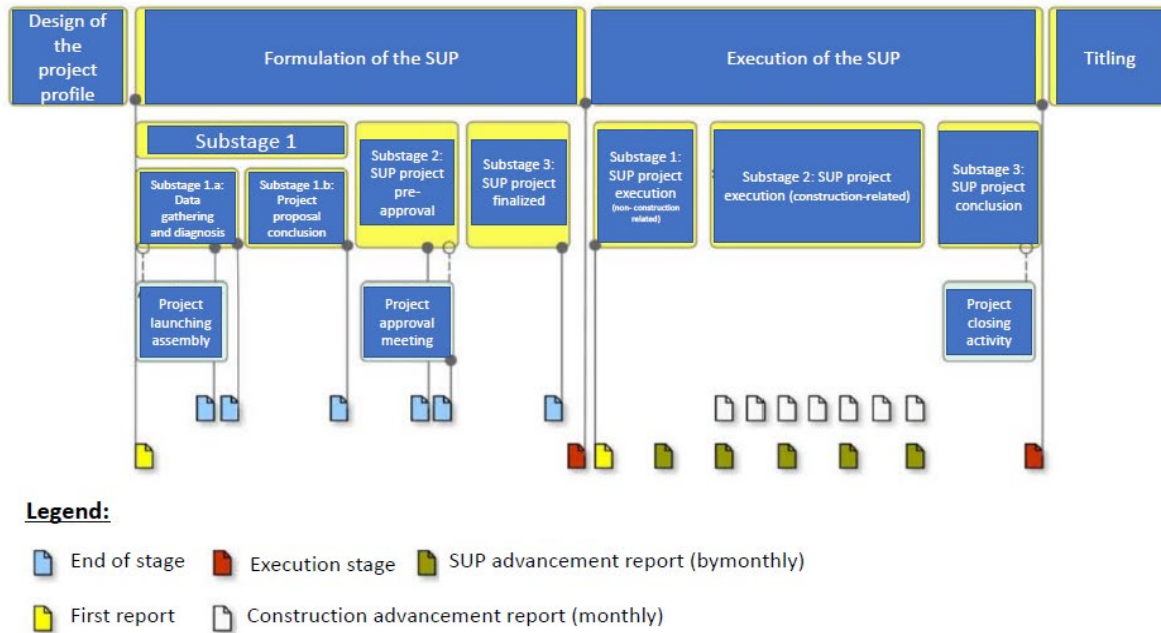
Second, a social and *community development* component supports project implementation and urban integration through a series of workshops and courses taught to slum dwellers. These workshops and courses include: (i) participatory diagnostics to assess population needs (including the needs of older adults, children, youth, single women, mothers, and others); (ii) guidance and referrals aimed at improving access to available social services; (iii) environmental and health education (instruction on how to better use sanitation equipment, as well as on drainage maintenance, garbage collection and disposal, reforestation, and home repairs and maintenance); and (iv) projects stemming from neighborhood initiatives. The workshops and courses are tailored to needs identified in each slum at the diagnostic stage and during execution.

A third component seeks to support families to *formalize property rights*. It finances technical and legal assistance for the transfer of ownership of the lands in the settlements. SUPs pay fees to lawyers and can cover the costs of land registration and other transaction costs associated with the process of formalizing property rights.

SUPs last between three and five years and are implemented in stages. The programs' life cycle can illustrate what mechanisms play a role in the theory of change. As depicted in Figure 2, SUPs have four main stages: Design of the project profile, formulation, execution, and titling. The first two stages (project profile and formulation) correspond to the planning stages. This is where social teams enter the neighborhoods. It is where meetings between the authorities and the inhabitants are held to define the specific type of investments to be made and set the organizational infrastructure intended to ease communication between the involved parties. Once the planning stage has been concluded, the execution stage begins. This is where the hard infrastructure works are implemented. Once execution is completed, the final stage focuses on the acquisition of formal property titles. Note that even though the titling stage is the last stage, inhabitants can count on legal security with regard to their properties from the beginning of the project. That, is to say, they cannot be evicted.

If impacts occurred at the beginning of the project, they would likely be created by expectations. However, if they were seen later on, when the infrastructure was completed, they might also be due to the reduction in mobility costs from the enhancement of the infrastructure.

**Figure 2**  
**Project cycle of SUPs in Uruguay**



Source: MOMTVA

### 3. Data

The analytic database employed in this research links four databases. The first data source combines all students from early childhood education to sixth grade who attended public schools registered by the Ministry of Education of Uruguay between 2013 and 2018. That administrative database contains the date of birth, sex, grade, and school attended, as well as students' addresses. The data also includes the outcome variable in our study: monthly records of the number of days a student was absent during each academic year enrolled.

The second data source comes from the Mayor's Office of Montevideo and maps all the informal settlements in 2006. That data identifies the neighborhoods where the settlements are located, along with some of the settlements' physical attributes, such as their area in square meters, their populations, and their number of dwellings.

A third database comes from the Uruguayan Ministry of Urban Development (MVOTMA)<sup>11</sup>, the agency responsible for the SUPs execution. The MVOTMA shared the information on their geographic information systems. This information identifies all the informal settlements intervened

<sup>11</sup> Ministerio de Vivienda Ordenamiento Territorial y Medioambiente

by the SUP program by 2018. That database also details when those SUP interventions occurred. Finally, the fourth and last data source is the Uruguayan Household Survey of 2006. Matching through geographies, we used census tract-level data from this survey to impute sociodemographic characteristics to each settlement<sup>12</sup>. This extrapolation allowed us to recover information about the settlements' income, poverty, and household composition. In Appendix A1, we fully describe the imputation method.

Our data focuses on the country's capital city, Montevideo, which encompasses 40 percent of the country's population. The capital is also the place for which we had comprehensive and reliable data from schools and SUPs. Our analytic database takes a subset of all students attending public schools in Montevideo between 2013 and 2018 whose addresses geographically overlapped the city's informal settlements in 2006. We classified students into a treatment and a comparison group, according to whether by 2018 they matched an informal settlement intervened by a SUP. We selected those addresses of students that overlapped within the geographic polygon defined by an SUP (a point-to-polygon match where the point is the student's address and the polygon is the slum).

In Table 1, we present descriptive statistics to familiarize the reader with the database and variables used in this paper. The table shows means and standard deviation (SD) of selected variables for two populations of students who in 2006 resided in slums by the "size" of the slum, with a threshold set at 90 dwelling units that separates our analytic database (the subset of data from which we estimate the treatment effects) from the rest of the data.

We split the variables in table 1 into three panels from top to bottom. The top panel shows the slums' sociodemographic characteristics, the middle panel describes the students' attributes, and the bottom one shows the distribution of the variable that records the number of student absences in a year. The p value of a t-test for the differences in the mean of each variable across the two populations is presented in the right column labelled "Diff" (the standard errors are clustered at the slum level). Some patterns in the table are worth noticing. For instance, the slums encompass very poor people with yearly incomes of around 10,000 to 11,000 Uruguayan pesos in 2006 (approximately USD 350-400). On average, families of four members inhabit homes where at least one family member is a child under 14. About 28 percent of the children in the sample attend preschool (for kids ages 3-5). On average, there were between 36 and 37 absences per year.

---

<sup>12</sup> Census tracts are called "segmentos censales" by the National Institute of Statistics in Uruguay.



**Table 1**  
**Descriptive statistics**

	Size of the slum						Diff
	90 dwellings or more			Less than 90 dwellings			
	Obs	Mean	SD	Obs	Mean	SD	
<b>Slum attributes (2006)</b>							
Number of dwellings	74	236.53	203.88	148	36.10	22.71	-200.426***
Proportion below poverty line	64	0.74	0.19	127	0.79	0.19	0.051
Rental value	63	1604.84	397.85	127	1508.73	405.82	-96.113
Income	64	17044.22	2658.93	127	17045.30	3753.83	1.079
Home has been in flood	64	10991.29	2992.71	127	9913.17	3123.83	-1,078.122**
HH members	64	0.21	0.15	127	0.23	0.23	0.020
HH members less than 14	64	3.83	0.69	127	3.83	0.97	0.001
<b>Students attributes (2018)</b>							
Boys	3910	0.52	0.50	1788	0.51	0.50	-0.005
Attends PreK 3-5 years old	3910	0.28	0.45	1788	0.26	0.44	-0.023*
Attends First Grade	3910	0.14	0.35	1788	0.14	0.35	-0.001
Attends Second Grade	3910	0.12	0.32	1788	0.14	0.35	0.025***
Attends Third Grade	3910	0.13	0.33	1788	0.11	0.32	-0.012
Attends Fourth Grade	3910	0.11	0.31	1788	0.11	0.32	0.006
Attends Fifth Grade	3910	0.11	0.31	1788	0.12	0.32	0.008
Attends Sixth Grade	3910	0.11	0.32	1788	0.11	0.31	-0.003
Total Absences	3910	36.43	27.81	1788	36.63	27.21	0.196

**Notes:** The table shows means and SD of selected variables for two populations of students who in 2006 resided in slums by the "size" of the slum (measured in terms of the number of dwelling units). The p-value of a t-test for the differences in the mean of each variable across the two populations is presented in the right column labelled Diff. The stars next to the coefficients denote statistical significance as follows: \* 0.1; \*\* 0.05; \*\*\*0.01

Notice also from Table 1 that there are no statistically significant differences in most of the characteristics presented in the table related to the size of the slum. Whenever those differences are significant (in some cohort sizes by grade), they are qualitatively very small. That there are strong similarities across the analytic sample and the rest of the data is important for our study because, as we will show, the FRD estimator renders local internal validity on the smaller subset of slums. However, because socioeconomic and

student characteristics are similar across the subpopulations in the table, further research may analyze the external validity of our results.

## 4. Identification Strategy

According to the criteria set by the MOMTVA, the settlements must have 40 or more dwellings to participate in Uruguay's SUP program. These criteria lend themselves to a fuzzy regression discontinuity (FRD) design. The outcome variable in our analysis is the probability that a student falls in the 90th percentile of the school absences distribution, and, therefore, is classified as a "recurrent truant". The running variable is the number of dwellings in a settlement. The 40 dwellings measure sets a known "cut off" that assigns settlements to receive treatment (the SUPs) if they are above that cutoff. Given that the probability of treatment increases at the cutoff, we can exploit variation in the neighborhood around 40 dwellings to learn about the causal nature of the impact of the SUP program on student absences.

The FRD estimator is similar to the instrumental variables estimator used in the context of encouragement designs where participants are randomly invited to participate in a program, but can decide whether to receive the treatment<sup>13</sup>. Let  $Z_i$  be an indicator for whether student  $i$  lives in a settlement classified above or below the 40 dwellings cutoff  $c_{40}$ , and  $X_i$  be the number of dwellings in a settlement, such that:

$$(1) \quad Z_i = 1\{X_i > c_{40}\}$$

The FRD estimator identifies a local average treatment effect (LATE) among compliers with the instrument. Identification requires that the standard IV assumptions of monotonicity and exclusion restrictions hold. The IV should also exert statistical power, so that units change treatment status in a way that is behaviorally consistent and statistically significant with changes in the value of the instrumental variable. Under those assumptions, the FRD estimator ( $\delta_{FDR}$ ) is the ratio of the difference in outcomes to the difference in the probability of participation among compliers at the cutoff:

$$(2) \quad \delta_{FDR} = \frac{E[A_i|X_i=c_{40}] - E[A_i|X_i=c_{40}]}{E[D_i|X_i=c_{40}] - E[D_i|X_i=c_{40}]}$$

---

<sup>13</sup> See West and Mullen (2008) for a reference on encouragement designs.

Where  $D_i$  is an indicator for actual participation in the SUP. The estimated  $\delta_{FDR}$  can be approximated by:

$$(4) \quad \widehat{\delta_{FDR}} = \frac{E[X_i=c_{40}] - E[X_i=c_{40}]}{E[X_i=c_{40}] - E[X_i=c_{40}]}$$

Equation (4) motivates estimation in two steps. In the first step, we select bandwidths to the right and left of the cutoff to join observations to compute the probability of treatment  $D_i$  using the cutoff  $Z_i$  as an instrumental variable. Let's functions  $f(\cdot)$  and  $g(\cdot)$  represent a local polynomial, due to the non-linear distribution in the bandwidths around the cutoff. Calling  $\tilde{X}_i = X_i - c_{40}$ , the first-stage estimating equation is:

$$(5) \quad D_i = \gamma_0 + \gamma_1 f(\tilde{X}_i) + \gamma_2 g(\tilde{X}_i) + \pi Z_i + \kappa_1 f(\tilde{X}_i) Z_i + \kappa_2 g(\tilde{X}_i) Z_i + \mu_i$$

In the second step, we estimate  $\delta_{FDR}$  using the following regression:

$$(6) \quad A_i = \alpha + \beta_1 f(\tilde{X}_i) + \beta_2 g(\tilde{X}_i) + \delta_{FRD} \hat{D}_i + \lambda_1 f(\tilde{X}_i) \hat{D}_i + \lambda_2 g(\tilde{X}_i) \hat{D}_i + \epsilon_i$$

In our research, the FRD estimator assumes that settlements around the eligibility threshold (40 dwellings) are comparable, and, if they are sufficiently close to that threshold, their eligibility act as a conditionally random mechanism that assigns some of those to SUP interventions. Figure B1 in the appendix provides evidence that slums composed of more than 40 dwelling units differed in their probability of treatment. Because eligibility based on slum sizes affected the actual probability of being subject of an SUP intervention, some eligible slums were, in fact, renewed by SUP programs. Our FRD estimator produces a semi parametric mean difference in the likelihood of recurrent truancy between students at each side of the 40 dwellings cutoff. This difference is interpreted as the local average treatment effect of SUPs on attendance.

When studying how to implement the FRD estimator, we found several ways to average the differences in students' outcomes at each side of the 40 dwellings cutoff. We also found various methods to determine how far students should be from that cutoff, so that we could claim they were observationally equivalent. Because those choices could have affected the value and precision of our local estimates, we compared the sensitivity of the estimates across the following three criteria:

- 1) While the estimator was based on local linear regressions, we compared results obtained with alternative exponential polynomial series in those regressions (we tested a linear, and quadratic exponential series).
- 2) When computing the semiparametric differences in outcomes using the FRD estimator, we studied whether the estimates changed with alternative choices for

kernel weights (uniform, triangular, and Epanechnikov) that assign different importance to observations according to their distance from the cutoff.

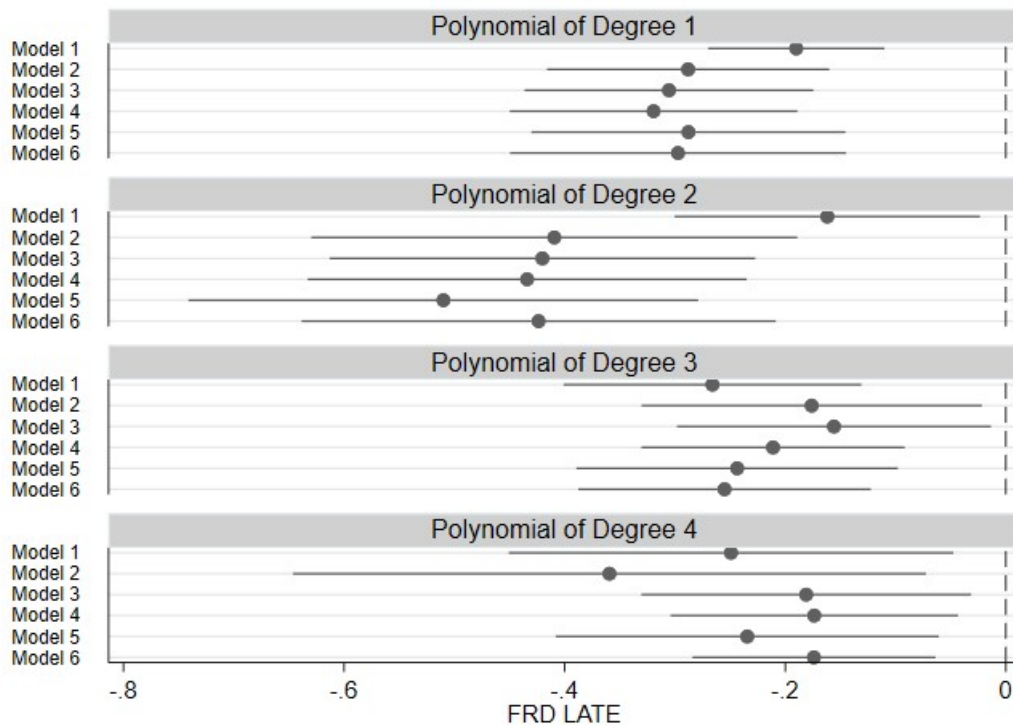
- 3) We used the mean squared error optimal bandwidth selector suggested by the literature (see the above references). We evaluated the sensitivity of the estimates when we employed the same or two different bandwidths to join observations at each side of the cutoff (one for above and another for below the cutoff).

We compared estimates across those specification choices to assess the results' consistency and the sensitivity of our estimates to decisions on how to specify the FRD model. As we will show, our results were highly consistent irrespective of the exponential series, kernel weights and bandwidth selector we chose.

## 5. Results

In Figure 3, we present 24 FRD estimates of the effects of SUP programs on school attendance. We computed those estimates across the four polynomial series, three weighting schemes, and two kinds of bandwidths described above. The figure shows estimates in four horizontal panels labelled “Polynomial of Degree 1” to “Polynomial of Degree 4” which correspond to FRD estimates computed with linear, quadratic, cubic, and quartic local polynomial series. In Figure 3, the round solid dots are the FRD effects of the SUP program on school attendance (the magnitudes are expressed in the horizontal axis). The solid lines at each side of the dots represent 95 percent confidence intervals for the mean estimates.

**Figure 3**  
**Fuzzy Regression Discontinuity Estimates of the Effects of the SUP Program on School Absences**



**Figure Notes:**

The graph shows FRD estimates of the effects of the SUP program on school absences computed without covariates and for various choices of Kernel weights and bandwidth selection algorithms. The dots are the FRD effects of the SUP program on school attendance with the magnitudes indicated in the horizontal axis. The solid lines are 95 percent confidence intervals. The FRD specifications are as follows:

*Model 1:* Uniform Kernel weights with Equal Bandwidths at both sides

*Model 2:* Uniform Kernel weights with Unique Bandwidths at each side  
*Model 3:* Triangular Kernel weights with Equal Bandwidths at both sides  
*Model 4:* Triangular Kernel weights with Unique Bandwidths at each side  
*Model 5:* Epanechnikov Kernel weights with Equal Bandwidths at both sides  
*Model 6:* Epanechnikov Kernel weights with Unique Bandwidths at each side

In each of the four panels of Figure 3, we show six different estimates of the SUP effects on attendance (those are denoted “Model 1” to “Model 6” in the vertical axis of the figure). “Model 1” shows estimates computed with uniform Kernel weights and equal bandwidths at both sides of the cutoff. Model 2 indicates estimates computed with a uniform kernel weight and unique bandwidths at each side of the cutoff. The specifications for “Model 3” have triangular kernels and equal bandwidth at both sides of the cutoff. We computed “Model 4” estimates using a triangular kernel weight with a unique bandwidth at each side, and “Model 5” estimates with an Epanechnikov kernel weight and considering equal bandwidths at both sides of the cutoff. Finally, we computed the estimates labelled “Model 6” in Figure 3 using Epanechnikov kernel weights with unique bandwidths at each side of the 40 dwellings eligibility cutoff.

Figures B2 to B5 in the Appendix study in more detail each of the estimates in Figure 3 by presenting regression discontinuity plots that show how the outcome variable behaves at each side of the cutoff. In addition, we also show first stage equation results to analyze how the location of the students across the cutoff explains their probability of treatment. Figure B2 extends the results in the upper panel of Figure 3, and Figures B3, B4, and B5 do the same for the subsequent panels from top to bottom of Figure 3.

We make two key observations with regards to Figure 3. First, the estimates show remarkable consistency in sign and precision, as all are signed negative, and they also are statistically significant at conventional precision levels (95 percent or higher). Taken as a whole, these results suggest that the students who lived in neighborhoods intervened by SUPs are less likely to be recurrent truants than other students.

Second, the magnitude of the estimates in Figure 3 vary. The estimates range from negative -0.16 (with a 2nd order polynomial, a uniform kernel, and unique bandwidth) to over -0.50 percent (with a 2nd order polynomial, a Epanechnikov kernel, and unique bandwidth). However, there are not statistically significance differences across all estimates. The causality from SUPs to school absences is unambiguously negative, with SUP exposure reducing absences.

From the estimates in Figure 3, we choose one based on several criteria. To explain our choice, let's first direct attention to the Regression Discontinuity plots that we present in Appendix B to highlight that school absences exhibit nonlinearities in the neighborhood of the cutoff point. To allow for a flexible, functional form that accommodates such nonlinearities, we singled out those estimates computed with a quadratic polynomial series (as we indicated, we provide details of the estimations based on a quadratic polynomial series in Figure B3).<sup>14</sup> Also, because of those conspicuous nonlinearities, we choose a Triangular weighting scheme for the kernel to give more weight to units closer, and less importance to units farther, from the threshold. In addition, we selected those impacts computed with bandwidths that minimized the mean squared error in the data's fit on each side of the cutoff.

To be consistent with our hypothesis, we expected that the instrumental variable coefficient in the first stage (an indicator turning one if the student is to the right of the cutoff, and zero otherwise) would show a positive sign. We also expect that such an instrument would exert statistical power that motivates meaningful differences in the probability of treatment assignment. Notice that all our estimates give results with estimates where the instrument as a function of SUP intervention in the first stage had a positive sign (Figures B2 to B5).

Our preferred estimate was computed using Triangular weights for the kernel, a second polynomial series in the local regression, and optimal bandwidths independently chosen at each side of the eligibility threshold. As we present in Column 4 of Table 2, the impact of the SUP program on student attendance using that estimator is -0.434 (se: 0.101).<sup>15</sup> We interpret this result as robust causal evidence that low income children who reside in settlements intervened by SUP programs have that lower probability of failing to regularly attend school compared with what their attendance would be if the program had not been implemented.

---

<sup>14</sup> Despite the estimates not being very different, we follow Gelman and Imbens (2019) suggestion to avoid high-order polynomials. Notice, however, that all estimates are within the same confidence intervals and they all convey the same information.

<sup>15</sup> The standard errors vary if we cluster them at the settlement/year, or at the school /year levels. However, the relevant estimates in the first and second stages of the FRD estimator do not lose statistical significance at conventional levels by either type of clustering.

**Table 2**  
**Regression Discontinuity estimates with a Polynomial of Degree 2. Various Kernel Weights and Bandwidth Selection Algorithms. No Covariates.**

	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.1619*	-0.4094***	-0.4202***	-0.4340***	-0.5099***	-0.4237***
	(0.0706)	(0.1124)	(0.0984)	(0.1015)	(0.1179)	(0.1097)
First-Stage Est	1.35	0.69	0.58	0.56	0.47	0.6
First-Stage SE	0.06	0.03	0.00	0.00	0.01	0.01
Obs Left	454	566	997	997	997	781
Obs Right	906	1509	1649	1649	1649	1649
Bandwidth	Unique	Different	Unique	Different	Unique	Different
Kernel	Uniform	Uniform	Triangular	Triangular	Epanechnikov	Epanechnikov
# Slums Left	5	7	14	14	14	11
# Slums Right	9	15	16	16	16	16

**Notes:** The table shows FRD estimates of the effects of the SUP program on school absences (standard errors in parentheses). The effects were computed using local polynomials of second degree. Specifications do not include covariates. The details of the model specifications (1 to 6) in the top file are as follows: *Model 1*: Uniform Kernel weights with Equal Bandwidths at both sides; *Model 2*: Uniform Kernel weights with Unique Bandwidths at each side; *Model 3*: Triangular Kernel weights with Equal Bandwidths at both sides; *Model 4*: Triangular Kernel weights with Unique Bandwidths at each side; *Model 5*: Epanechnikov Kernel weights with Equal Bandwidths at both sides; *Model 6*: Epanechnikov Kernel weights with Unique Bandwidths at each side of the cutoff.

### 5.1. Validity tests: adding covariates

The validity of our estimates depends on the assumption that, within the selected bandwidths, the settlements where students reside are randomly assigned to the SUP interventions. The eligibility of a settlement for an SUP intervention should only affect student absences via the effect it has on the likelihood that the settlement is in fact intervened by an SUP program.

It is possible, however, that some differences across students regarding factors, such as the timing of the start of SUP interventions, socioeconomic disparities across the eligible settlements, differences in the ages of children exposed to the program, and children's gender, might interact with eligibility to affect the attendance outcomes directly. If that is the case, the exclusion restriction on which the estimator hinges could be violated.

The specific controls added included fixed effects for the age, quadratic age, gender, and fixed effects for the years of the outcome when we measure their school attendance



outcome. To hold constant differences in the idiosyncratic conditions (economic, political, etc.) at those times. We also hold constant poverty in the settlement in 2006 to account for potential differences in wealth that could jointly influence the probability of receiving a SUP intervention and children’s school outcomes. In addition, we include the distance from the home to the school. Finally, because the interventions were at different stages during the study’s time window, we controlled for the number of years that passed between when the intervention started and when we observed the attendance outcomes.<sup>16</sup>

As we can see by comparing Tables 2 and 3, the addition of covariates changes the magnitude of the estimates, however there are no statistically significant differences between the estimates in both tables. As expected, however, the estimates are more precise when covariates are added than when they are not included.

**Table 3**  
**Regression Discontinuity Estimates with a Polynomial of Degree 2. Various Kernel Weights and Bandwidth Selection Algorithms. Includes Covariates.**

	(1)	(2)	(3)	(4)	(5)	(6)
RD Estimate	-0.2286*** (0.0508)	-0.2357*** (0.0647)	-0.2704*** (0.0762)	-0.2648*** (0.0632)	-0.2925*** (0.0808)	-0.2229*** (0.0576)
First-Stage Est	0.87	1.16	0.9	0.9	0.84	1.12
First-Stage SE	0.01	0.03	0.02	0	0.02	0.01
Obs Left	318	566	732	997	732	781
Obs Right	848	848	1261	906	1261	906
Bandwidth	Equal	Different	Equal	Different	Equal	Different
Kernel	Uniform	Uniform	Triangular	Triangular	Epanechnikov	Epanechnikov
# Slums Left	3	7	9	14	9	11
# Slums Right	6	6	12	9	12	9

**Notes:** The table shows FRD estimates of the effects of the SUP program on school absences (standard errors in parentheses). The effects were computed using local polynomials of fourth degree. Specifications control for covariates. The details of the model specifications (1 to 6) in the top file are as follows: *Model 1*: Uniform Kernel weights with Equal Bandwidths at both sides; *Model 2*: Uniform Kernel weights with Unique Bandwidths at each side; *Model 3*: Triangular Kernel weights with Equal Bandwidths at both sides; *Model 4*: Triangular Kernel weights with Unique Bandwidths at each side; *Model 5*: Epanechnikov Kernel weights with Equal Bandwidths at both sides; *Model 6*: Epanechnikov Kernel weights with Unique Bandwidths at each side of the cutoff.

<sup>16</sup> Controlling for years since the intervention started required a time normalization exercise that we describe in Appendix C.

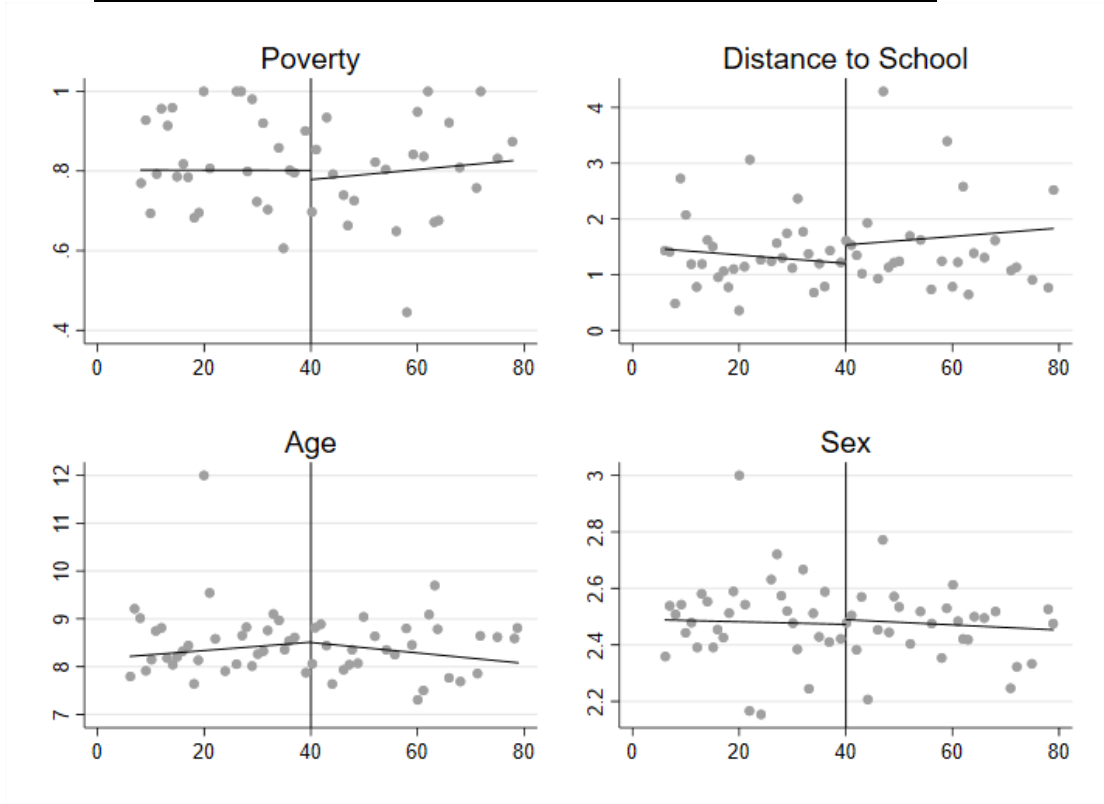
## 5.2. Validity tests: placebo tests

One way to further validate our results is by implementing a “placebo” cutoff test to evaluate whether there are local effects around “invalid or fake” cutoffs using the FRD estimator. If our FRD estimates are correct, the treatment status should be constant around these “fake” cutoffs, and we should not find impacts of the SUP program on absences.

To implement those tests, we replaced the true 40 dwellings cutoff value with a series fake cutoffs in the running variable ranging from 20 to 60 dwelling units, where we expected to find no discontinuities in the outcome, as eligibility criteria were non-binding there. We computed FRD estimates around those placebo cutoffs without using covariates, and employing a local quadratic polynomial series estimator with a Triangular kernel weights (as we did with the estimates shown in Table 2).

The FRD estimates that we present in Figure B6, provide evidence that in the vast majority of cases SUPs had no effect on attendance when we employed cutoffs ranging from 20 to 60 dwellings. This result suggests that, contrary to the 40 dwellings cutoff, natural experiments did not occur inside the windows that join students around those false cutoffs.

**Figure 4**  
**Regression Discontinuity Plots with Placebo Outcomes**



**Notes:** The figure shows RD plots for three placebo outcomes measured at baseline (average income in a settlement, population size of the settlement, and whether the settlement would have flooded in the past). We used local polynomials of first degree and employed Triangular Kernel weights. Specifications do not control for covariates.

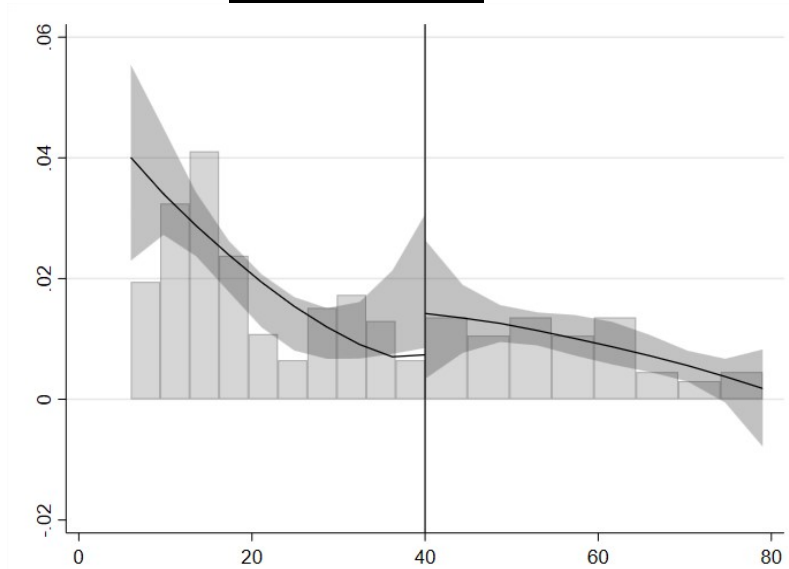
An alternative way we validated our results was using placebo tests to evaluate whether there were discontinuities in covariates around the cutoff value. We run placebos at the true cutoff on four covariates that, because those were measured at baseline (2006), they should not have been affected by the SUP programs. The placebo outcomes that we studied are the poverty, distance to school, age, and sex. Figure 4 shows prototypical RD plots for these three outcomes. None of the outcomes described there have significant changes at the cutoff value, suggesting that the cutoff value is only binding to exert changes in the probability of SUP interventions on our outcome of interest (school attendance).

### 5.3. Validity test: manipulation around the threshold

Another approach we take to validate our results is to advance a manipulation test where we look for evidence that program participants would have manipulated the size of their settlements to be considered eligible for an SUP intervention. We hypothesize that, because the unit of intervention is the settlement, individuals find it very costly to manipulate the position of a settlement around the cutoff, for that would require adding more dwelling units to a settlement. To validate that hypothesis, we plot in Figure 5 the frequency in the number of dwelling units per settlement in our sample. As can be seen, the data patterns do not show evidence of manipulation, as there is no sizeable “jump” in the number of settlements to the right of the cutoff. This test rejects the null hypothesis of no manipulation.

We also conducted a formal test to evaluate the differences in the density of dwelling units around the threshold. Beyond the graphic interpretation in Figure 5, we formally evaluated the null hypothesis that the distribution of the number of dwellings units is continuous around the 40 dwelling units threshold (Cattaneo et al., 2020). Using first and second order polynomial series, we found no evidence of discontinuities in those density distributions.<sup>17</sup>

**Figure 5**  
**Manipulation Testing Using Local Polynomial Density Estimation at the Settlement Level**



<sup>17</sup> The p-values for the differences in the density of dwelling units at the cutoff were 0.64 and 0.30 using a linear or quadratic polynomial series, respectively.

#### 5.4. Heterogeneous effects

Consistent with the theory of change presented previously, we estimate the FRD effects of the SUP programs on attendance by gender, by students' ages (when the programs started), and by different stages throughout the duration of the SUP programs. Studying heterogeneity of effects across these dimensions helped us better understand the mechanisms underlying the effects of SUP programs on children's school attendance.

**Table 5**  
**Fuzzy Regression Discontinuity Estimates by Subgroups of Relevance**

	Boys	Girls	Age 3-8	Age > 8	Duration 0-5yrs	Duration > 5yrs
RD Estimate	-0.4471*** (0.1225)	-0.4142** (0.1548)	-0.5161** (0.1675)	-0.4659* (0.1823)	-0.3733* (0.1467)	-0.5208** (0.1989)
First-Stage Est	0.61	0.67	0.42	0.54	0.74	0.36
First-Stage SE	0.01	0.03	0.01	0.01	0.02	0.01
Obs Left	529	345	672	455	383	621
Obs Right	819	970	979	1381	1516	919

**Notes:** The table shows FRD estimates of the effects of the SUP program on school absences (standard errors in parenthesis) across relevant subgroups identified in the top row. Duration refers to time since SUP intervention started. The effects were computed using local polynomials of second degree. We employed Triangular Kernel weights with Different Bandwidths at each side of the cutoff.

In Table 5, we show fuzzy regression discontinuity estimates of the effects of the SUP program on school absences (standard errors in parentheses) across relevant subgroups identified in the top row. We computed those effects using local polynomials of second degree, and Triangular kernel weights. The specifications independently selected optimal bandwidths at each side of the cutoff. We present the number of observations in each bandwidth in the bottom panel of the table. In the first two columns of Table 5, we present FRD impacts of the SUP program on attendance for boys and girls separately. The results presented in the table indicate that SUPs would improve the attendance patterns of boys and girls.

We found it interesting to look at impact by age, because children develop agency with regards to school attendance as they age. This reality makes it relevant to examine whether the SUP program's effects on attendance are heterogeneous by the age children had when the program started. In Columns 3 and 4 of Table 5 we present FRD estimates

across two subgroups defined by the age of children. We found that SUP programs affect children similarly irrespective of their ages.<sup>18</sup>

Because SUP interventions last several years, we wanted to analyze whether the effects of the programs manifest during the earlier or later years following the programs' start. The evidence that we present in Columns 5 and 6 of Table 5 suggests that the effects of the SUP programs on school attendance manifest themselves across all years after the intervention has begun: SUPs reduce the likelihood of recurrent truancy dynamically. However, the local effects are bigger five years after the programs begin. While the average student's probability of becoming a recurrent truant falls by 37 percent during the first five years of the program, it falls 52 percent during the following years. This incremental nature in the dynamics of the effect speaks to the interaction between the margin accruing to reductions in transaction costs and that pertaining to changes in the expected returns from human capital investments. As explained in the theory of change, the SUP components may affect school outcomes through the decrease in transaction costs, and through the increase in expected returns to children's education. These results suggest that there is a first effect when SUP programs start (pathway of expectations), and, once the urban infrastructure is finalized, a second effect kicks in with the reduction in transaction costs from taking children to school.

## 6. Discussion

According to the United Nations Human Settlements Program, slums are areas that lack access to basic services (such as water and sanitation), and where people live in poor quality and overcrowded houses and do not have security of tenure. As we have already shown, slum dwellers are also socioeconomically disadvantaged. Education is recognized as a key instrument to foster the upward social mobility of vulnerable populations, such as those residing in slums (OECD, 2018). However, students in slums present high rates of absenteeism, and school dropout threatens their opportunities to break the poverty cycle through the acquisition of human capital with formal education.

This paper shows that an urban program that upgraded slums in Uruguay improved the attendance patterns of vulnerable children who resided in slums during elementary school years. School dropout is the final stage in the process of disengagement that manifests early as poor performance in the task of attending classes. Scholars have long recognized that school attendance in early grades is a strong predictor of dropout in subsequent

---

<sup>18</sup> Notice, however, that the difference in magnitudes of the coefficient is not statistically different from zero.

years, and that dropout has long lasting negative consequences for important adult outcomes, such as criminal activity, employment, income, and health.<sup>19</sup> Our paper contributes to the literature of the impact of SUPs on human development outcomes by highlighting that SUPs have implications for how children attend school. This reinforces the multidimensional effects of SUPs and enriches the policy dialogue on the impact of SUPs in improving skills acquisition in elementary school, with positive long-term consequences for adult life.

Our results suggest that when considering the economic efficiency of SUP programs, policy makers should monetize the present value of the future income stream of children. It is generally acknowledged that, from the perspective of infrastructure costs, it is more efficient to urbanize a planned and unpopulated territory than a slum. Although this is true, this study contributes new evidence of the impacts that SUPs could have on children, and the fact that such programs could also positively impact parental education, training, employment, and ultimately children's future income.

We also found that children's school attendance was affected by SUPs equally, regardless of their age. If, as we argue, children acquire the agency to control the behavior leading to absences as they age, this suggests that SUP programs affect both parents and children. SUPs induce parents to better perform the task of getting their children to school. At the same time, they impact the early engagement of young adolescents in risky behaviors that would prevent them from attending classes regularly.

As we suggested in section I, where we describe the theory of change, improved parental expectations and lower transaction costs would be the key drivers of the effects of SUPs on school attendance. By computing the effects before and after five years of implementation, we indirectly explored whether the SUP effects kicked in at the beginning (as a potential result of a rise in expectations) or when the project was finalized (as a potential reduction in transaction costs plus the earlier effect on expectations). We found improvements in school attendance both at the beginning and at the end of the project. Although we cannot disentangle these effects, evidence suggests that SUP programs first increase parental expectations, and once the projects end, expectations remain high, and families enjoy the benefits of the improved infrastructure. In other words, results are consistent with both of the pathways of our theory of change.

---

<sup>19</sup> Even more, attendance can affect a variety of soft skills that are important for life and coexistence with others, generating more patience, better focus on the achievement of goals, decreasing the probability of engaging in risky behaviors, and improving trust and social interaction. Oreopoulos and Salvanes (2011).

In the urban policy agenda, SUP interventions have long been recognized for their capacity to provide large infrastructure investments, provide basic services and help people secure property rights. However, our research underscores that those interventions may also affect the self-perception and expectations of slum inhabitants by providing implicit recognition of people's value as members of society with rights and duties. We show that SUPs have tangible impacts on human capital development, which grants those interventions an across-sectors importance in contributing to the fulfillment of several of the sustainable development goals (SDGs).

From the education policy perspective, our research indicates that some interventions that occur outside of schools can contribute to improving school attendance. We indirectly highlight the relevance of neighborhood conditions to influencing parents' human capital investments in their children. Moreover, we underscore the opportunity/necessity of coordinating urban and education policy for more effective results when it comes to investments for improving the school outcomes of low-income children.

Our study also presents some limitations. A longer term, and more detailed database (with different sources of exogenous variation), and/or an evaluation with an experimental design, would be needed to clearly disentangle the mechanisms behind the effects of the SUP program on attendance (over which we can only speculate in this paper). Also, even though the rigor of our study ensures internal validity, more studies are needed in different contexts to confirm whether we can extrapolate our results as generally valid. In spite of those limitations, we believe that our study is a stepping stone in the direction of more and better research about whether and how SUPs affect the educational outcomes of low-income children.



## References

- Acevedo, Paloma, Jason Hobbs, and Sebastian Martinez.** 2017. "The impact of upgrading municipal infrastructure on property prices: evidence from Brazil." Technical Note, at Interamerican Development Bank, (IDB-TN-1277
- Acevedo, P., C. Cruces, P. Gertler, and S. Martinez.** 2020. "Living up to expectations: how vocational education made women better off but left men behind." *Labor Economics*, 65, 101824..
- Aiga, Hirotsugu, and Takusei Umenai.** 2002. "Impact of improvement of water supply on household economy in a squatter area of Manila." *Social Science & Medicine*, 55(4): 627–641.
- Aiga, Hirotsugu, Yumiko Arai, Eiji Marui, and Takusei Umenai.** 1999. "Impact of improvement of water supply on reduction of diarrheal incidence in a squatter area of Manila." *Environmental Health and Preventive Medicine*, 4(3): 111–116.
- Aizer, Anna, and Flavio Cunha.** 2012. "The production of human capital: Endowments, investments and fertility." National Bureau of Economic Research.
- Allensworth, Elaine M., and John Q. Easton.** 2007. "What Matters for Staying On-Track and Graduating in Chicago Public High Schools. A Close Look at Course Grades, Failures, and Attendance in the Freshman Year." Consortium on Chicago School Research July, Chicago.
- Allison, Mandy A., Elliott Attisha, and AAP Council on School Health.** 2019. "The Link Between School Attendance and Good Health." *Pediatrics*, 143(2).
- Almlund, Mathilde, Angela Lee Duckworth, James Heckman, and Tim Kautz.** 2011. "Chapter 1 - Personality Psychology and Economics." In *Handbook of the Economics of Education*. Vol. 4 of *Handbooks in Economics*, , ed. Stephen Machin Eric A. Hanushek and Ludger Woessmann, 1–181. Elsevier B.V.
- Angrist, Joshua D, Victor Lavy, and Analia Schlosser.** 2005. "New evidence on the causal link between the quantity and quality of children." National Bureau of Economic Research.
- Ashraf, Nava, Edward Glaeser, Abraham Holland, and Bryce Millett Steinberg.** 2017. "Water, health and wealth." National Bureau of Economic Research.
- Attanasio, Orazio, Adriana Kugler, and Costas Meghir.** 2008. "Training disadvantaged youth in Latin America: evidence from a randomized trial." National Bureau of Economic Research.
- Bahl, Roy W, Johannes F Linn, and Deborah L Wetzel.** 2013. *Financing metropolitan governments in developing countries*. Lincoln Institute of Land Policy Cambridge, MA.

- Baker, Judy, Rakhi Basu, Maureen Cropper, Somik Lall, and Akie Takeuchi.** 2005. *Urban poverty and transport: the case of Mumbai*. The World Bank.
- Becker, Gary S, and H Gregg Lewis.** 1973. "On the Interaction between the Quantity and Quality of Children." *Journal of Political Economy*, 81(2, Part 2): S279–S288.
- Blattman, Christopher, Donald Green, Daniel Ortega, and Santiago Tobón.** 2017. "Place-based interventions at scale: The direct and spillover effects of policing and city services on crime." National Bureau of Economic Research.
- Branas, Charles C, Eugenia South, Michelle C Kondo, Bernadette C Hohl, Philippe Bourgois, Douglas J Wiebe, and John M MacDonald.** 2018. "Citywide cluster randomized trial to restore blighted vacant land and its effects on violence, crime, and fear." *Proceedings of the National Academy of Sciences*, 115(12): 2946–2951.
- Burdick-Will, Julia.** 2018. "Neighborhood violence, peer effects, and academic achievement in Chicago." *Sociology of Education*, 91(3): 205–223.
- Burdick-Will, Julia, Kiara Millay Nerenberg, Jeffrey Grigg, and Faith Connolly.** 2019. "Student Mobility and Violent Crime Exposure at Baltimore City Public Elementary Schools."
- Burdick-Will, Julia, Marc L Stein, and Jeffrey Grigg.** 2019. "Danger on the way to school: exposure to violent crime, public transportation, and absenteeism." *Sociological Science*, 6: 118–142.
- Card, David, Jochen Kluge, and Andrea Weber.** 2018. "What works? A meta analysis of recent active labor market program evaluations." *Journal of the European Economic Association*, 16(3): 894–931.
- Cattaneo, Mattias D, Michael Jansson and Xinwei Ma.** 2020. "Simple local polynomial density estimators." *Journal of the American Statistical Association*, 115(531): 1449–1455.
- Cattaneo, Matias D, Sebastian Galiani, Paul J Gertler, Sebastian Martinez, and Rocio Titiunik.** 2009. "Housing, health, and happiness." *American Economic Journal: Economic Policy*, 1(1): 75–105.
- Chalfin, Aaron, Benjamin Hansen, Jason Lerner, and Lucie Parker.** 2019. "Reducing crime through environmental design: Evidence from a randomized experiment of street lighting in New York City." National Bureau of Economic Research.
- Connolly, Faith, and Linda S Olson.** 2012. "Early Elementary Performance and Attendance in Baltimore City Schools's Pre-Kindergarten and Kindergarten." Baltimore Education Research Consortium March, Baltimore.
- CPS, Chicago Public Schools.** 2007. "Chicago public schools policy manual." Board Report 06-0222-PO2.
- Curran, F Chris.** 2019. "Does the Chicago Safe Passage program reduce reported crime around elementary schools? Evidence from longitudinal, geocoded crime data." *Criminal Justice Policy Review*, 30(9): 1385–1407.

- Doleac, Jennifer L, and Nicholas J Sanders.** 2015. "Under the cover of darkness: How ambient light influences criminal activity." *Review of Economics and Statistics*, 97(5): 1093–1103.
- Eaton, Danice K, Nancy Brener, and Laura K Kann.** 2008. "Associations of health risk behaviors with school absenteeism. Does having permission for the absence make a difference?" *The Journal of School Health*, 78(4): 223–229.
- Farrington, David P, and Brandon C Welsh.** 2002. "Improved street lighting and crime prevention." *Justice Quarterly*, 19(2): 313–342.
- Field, Erica.** 2003. "Fertility responses to urban land titling programs: The roles of ownership security and the distribution of household assets." Mimeo, Harvard University.
- Field, Erica.** 2005. "Property rights and investment in urban slums." *Journal of the European Economic Association*, 3(2-3): 279–290.
- Field, Erica.** 2007. "Entitled to work: Urban property rights and labor supply in Peru." *The Quarterly Journal of Economics*, 122(4): 1561–1602.
- Fort, R.** 2008. "The homogenization effect of land titling on investment incentives: evidence from Peru." *NJAS-Wageningen Journal of Life Sciences*, 55(4): 325–343.
- Freeman, MC, T Clasen, R Dreibelbis, S Saboori, LE Greene, B Brumback, R Muga, and R Rheingans.** 2014. "The impact of a school-based water supply and treatment, hygiene, and sanitation programme on pupil diarrhoea: a cluster-randomized trial." *Epidemiology & Infection*, 142(2): 340–351.
- Galiani, Sebastian, and Ernesto Schargrotsky.** 2004. "Effects of land titling on child health." *Economics & Human Biology*, 2(3): 353–372.
- Galiani, Sebastian, and Ernesto Schargrotsky.** 2010. "Property rights for the poor: Effects of land titling." *Journal of Public Economics*, 94(9-10): 700–729.
- Galiani, Sebastian, Martin Gonzalez-Rozada, and Ernesto Schargrotsky.** 2009. "Water expansions in shantytowns: Health and savings." *Economica*, 76(304): 607–622.
- Gelman, Andrew, and Guido Imbens.** 2019. "Why high-order polynomials should not be used in regression discontinuity designs." *Journal of Business & Economic Statistics* 37.3: 447-456.
- Gershenson, Seth, Alison Jackowitz, and Andrew Brannegan.** 2014. "Are student absences worth the worry in U.S. primary schools?"
- Ginsburg, Alan, Phyllis Jordan, and Hedy Chang.** 2014. "Absences add up: how school attendance influences student success." Attendance Works.
- Glaeser, Edward L, Matthew E Kahn, and Jordan Rappaport.** 2008.

- “Why do the poor live in cities? The role of public transportation.” *Journal of Urban Economics*, 63(1): 1–24.
- Goldman, Joshua.** 2014. “Flaking Out: Student Absences and Snow Days as Disruptions of Instructional Time.” NBER. Working Paper No. 20221.
- Gonzalez-Navarro, Marco, and Climent Quintana-Domeque.** 2010. “Urban infrastructure and economic development: Experimental evidence from street pavement.”
- Gonzalez-Navarro, Marco, and Climent Quintana-Domeque.** 2016. “Paving streets for the poor: Experimental analysis of infrastructure effects.” *Review of Economics and Statistics*, 98(2): 254–267.
- Gonzalez-Velosa, Carolina, David Rosas, and Roberto Flores.** 2016. “On the Job Training in Latin America and the Caribbean: Recent Evidence.” In *Firm Innovation and Productivity in Latin America and the Caribbean*. 137– 166. Palgrave Macmillan, New York.
- Gottfried, Michael A.** 2009. “Excused versus unexcused: How student absences in elementary school affect academic achievement.” *Educational Evaluation and Policy Analysis*, 31(4): 392–415.
- Gottfried, Michael A.** 2017. “Linking getting to school with going to school.” *Educational Evaluation and Policy Analysis*, 39(4): 571–592.
- Goytia, Cynthia, and Guadalupe Dorna.** 2019. “Evaluación del programa de mejoramiento de barrios.” Universidad Torcuato di Tella.
- Heckman, James J, and Tim Kautz.** 2012. “Hard Evidence on Soft Skills, (6580).
- Ibarrar´an, Pablo, and David Rosas Shady.** 2009. “Evaluating the impact of job training programmes in Latin America: evidence from IDB funded operations.” *Journal of Development Effectiveness*, 1(2): 195–216.
- Ibarrar´an, Pablo, Jochen Kluge, Laura Ripani, and David Rosas Shady.** 2019. “Experimental Evidence on the Long-Term Effects of a Youth Training Program.” *ILR Review*, 72(1): 185–222.
- Jaitman, Laura, and J Brakarz.** 2013. “Evaluation of Slum Upgrading Programs.” *Literature Review and Methodological Approaches. Inter-American Development Bank Technical Note No. IDBTN-604, Washington, DC.*
- Kautz, Tim, and Wladimir Zanoni.** 2015. “Measuring and Fostering NonCognitive Skills in Adolescence : Evidence from Chicago Public Schools and the OneGoal Program.”
- Kondo, Michelle, Bernadette Hohl, SeungHoon Han, and Charles Branas.** 2016. “Effects of greening and community reuse of vacant lots on crime.” *Urban Studies*, 53(15): 3279–3295.

- Lenhoff, Sarah Winchell, Jeremy Singer, and Walter Cook.** 2020. "School Transit and Accessing Public School in Detroit."
- Li, Hongbin, Junsen Zhang, and Yi Zhu.** 2008. "The quantity-quality tradeoff of children in a developing country: Identification using Chinese twins." *Demography*, 45(1): 223–243.
- Magalhães, Fernanda, Patricia Acosta Restrepo, Fernanda Lonardoni, and Roberto Moris.** 2016. "Slum upgrading and housing in Latin America." *New York: Inter-American Development Bank*.
- McMillen, Daniel, Ignacio Sarmiento-Barbieri, and Ruchi Singh.** 2019. "Do more eyes on the street reduce crime? Evidence from Chicago's Safe Passage Program." *Journal of Urban Economics*, 110: 1–25.
- Moraes, LRS, Jacira Azevedo Cancio, Sandy Cairncross, and Sharon Huttly.** 2003. "Impact of drainage and sewerage on diarrhoea in poor urban areas in Salvador, Brazil." *Transactions of the Royal Society of Tropical Medicine and Hygiene*, 97(2): 153–158.
- Neild, Ruth Curran, and Robert Balfanz.** 2005. "Unfulfilled Promise : The Dimensions and Characteristics of Philadelphia's Dropout Crisis , 2000-2005." Philadelphia Youth Network. John Hopkins University and University of Pensilvania.
- Nichols, Joe D.** 2003. "Prediction indicators for students failing the state of Indiana high school graduation exam." *Preventing School Failure*, 47(3): 112– 20.
- OECD, A.** 2018. "A broken social elevator? How to promote social mobility." *COPE Policy Brief*.
- Oreopoulos, Philip, and Kjell G Salvanes.** 2011. "Priceless: The nonpecuniary benefits of schooling." *Journal of Economic perspectives*, 25(1): 159–84.
- Ready, Douglas.** 2010. "Socioeconomic Disadvantage, School Attendance, and Early Cognitive Development: The Differential Effects of School Exposure." *Sociology of Education*, 83(4): 271–286.
- Romero, Mariajosé, and Young-sun Lee.** 2007. "A National Portrait of Chronic Absenteeism in the Early Grades." National Center for Children in Poverty. Columbia University/ October, New York City.
- Sanfelice, Viviane.** 2019. "Are safe routes effective? Assessing the effects of Chicago's Safe Passage program on local crimes." *Journal of Economic Behavior & Organization*, 164: 357–373.
- Sciar, GD, JV Garn, G Penakalapati, KT Alexander, J Krauss, MC Freeman, S Boisson, KO Medicott, and T Clasen.** 2017. "Effects of sanitation on cognitive development and school absence: a systematic review." *International Journal of Hygiene and Environmental Health*, 220(6): 917–927.
- Soares, Fabio, Yuri Soares, et al.** 2005. "The socio-economic impact of favelabairro: what do the data say?" *Inter-American Development Bank, Washington DC*.

- Trinies, Victoria, Joshua V Garn, Howard H Chang, and Matthew C Freeman.** 2016. “The impact of a school-based water, sanitation, and hygiene program on absenteeism, diarrhea, and respiratory infection: a matched– control trial in Mali.” *The American Journal of Tropical Medicine and Hygiene*, 94(6): 1418–1425.
- Urzu´a, Sergio, and Esteban Puentes.** 2010. “La evidencia del impacto de los programas de capacitaci3n en el desempe˜o en el mercado laboral.” *IDB DP*, 268.
- West, S. G., Duan N. Pequegnat W. Gaist P. Des Jarlais D. C. Holtgrave D. Szapocznik J. Fishbein M. Rapkin B. Clatts M., and P. D. Mullen.** 2008. “Alternatives to the randomized controlled trial.” *American Journal of Public Health*, 98: 1359–1366.

## Appendix

### Appendix 1. Normalizing the time frame in the data

We normalized the data identifying when the SUP interventions started to compare attendance patterns between treated and non-treated students. Notice that for students in the comparison group residing in settlements not affected by SUPs, there is no natural baseline date to carry out such a comparison.

To circumvent this limitation, we randomly assigned each settlement in the comparison group a baseline year between 2006 and 2018 (the years when we knew the SUP programs had started). We assigned the baseline years to students in the comparison group, so that the proportion of students in each student/baseline year combination was the same in the treatment and comparison groups.

#### Imputed attributes to the settlements based on the Uruguayan Household Survey

To better model the process that explains why the MOVTVA assigned some settlements (and not others) to SUPs, we imputed sociodemographic characteristics to the settlements using geography-matched data from the Uruguayan Household Survey of 2006. In summary, we first isolated all the census tracts overlapping each settlement and then computed averages of their attributes. The specific imputation procedure involved the following steps:

- 1) We first identified all census tracts that geographically overlapped with each informal settlement. Then we formed a geographic polygon for each settlement called a “cluster” that joins all those overlapping tracts.<sup>20</sup>
- 2) We computed averages of the clusters’ attributes, aggregating survey data reported by households residing in settlements across all the census tracts that form each cluster.
- 3) Each cluster-level characteristic is a weighted statistic of census tract attributes (mean). The weights are inversely proportional to the distance between the centroids of the settlements and the overlapping census tracts’ centroids.<sup>21</sup>

---

<sup>20</sup> This cluster-based imputation strategy is similar to that employed in the National Survey of Early Care and Education (NSECE) in the United States, which imputes cluster-level values from the American Community Survey to households interviewed by the NSECE <https://nsece.wordpress.com/>.

<sup>21</sup> Geographic centroids are the mean position of all the points that join a polygon in all of the coordinate directions. In our study, the polygons are settlements or census tracts.

The imputed data allowed us to better characterize the settlements in 2006 when the MOMOVTS determined what settlements would be eligible for SUP interventions. Our empirical strategy uses that data to control for observable differences across the informal settlements that predict what settlement is intervened by a SUP and factors that could predict school attendance.

To do this, we first isolated those informal settlements that, according to public records from the Mayor's Office of Montevideo, were identified as such by the authorities in 2007. Secondly, the geographic coordinates of the polygons formed by those settlements in Montevideo, were matched to the coordinates determined by census tracts in the Uruguayan Household Survey. Census tracts are called “segmentos censales” by the National Institute of Statistics in Uruguay. Thirdly, we produced census tract aggregate statistics (means, medians and standard deviations) for all such tracts in Montevideo. Those statistics characterize the attributes of inhabitants of informal settlements in each tract. Finally, we created variables that characterize the settlements in Montevideo in 2007 by extrapolating to each settlement the aggregate characteristics at the census tract level generated in the previous step, across all census tracts that overlapped with the polygon of a settlement. If more than one census tract overlapped with a settlement, aggregates were weighted inversely to the distance between the geographical centroids of the settlement and those centroids in each overlapping census tract.

## Appendix 2. SUPs and parental expectations

Let transportation costs  $T$  be a function  $T(c, S)$  of associated costs  $c$  and parental skills  $S$ . Working hours  $h$  are paid at a wage  $w$  with no leisure, so that the budget constraint of an agent is  $M = w(1 - T(c, S))$ .

Furthermore, introduce attendance  $A$  and conceptualize it as an indicator of performance in the task of, either taking a child from home to school, or pushing a child to attend school. In this model, attendance  $A$  is a function  $A(S, c)$  of parental skills  $S$ , and  $c$  the associated cost paid, or level of effort needed, to accomplish  $A$  due to infrastructure and mobility in the slum.

Assume that, in addition to consumption, parents receive utility from the school performance of their children, and that performance improves with  $A$ , so that children who attend classes more often do better in school. In other words, the stock of human capital of children  $k(A(c, S))$  is a decreasing function in absences. Parents' instantaneous utility function is:



$$U(w(1 - T(c, S), k(A(c, S))))$$

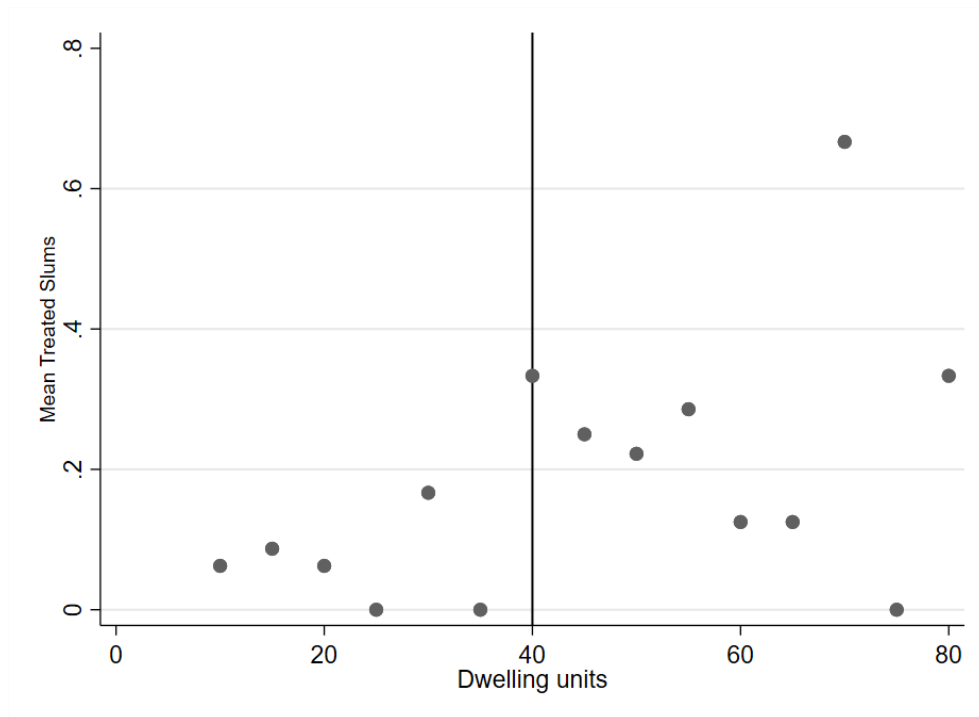
If SUPs have effects on transportation costs, the first order condition is:

$$-U_M w \frac{dT(c, S)}{dc} + U_k k' \frac{dA(c, S)}{dc} = 0$$

From this relationship, there is a confounding effect of the intervention through a reduction in the opportunity costs from mobility  $w \frac{dT(c, S)}{dc}$  and the effect in children's absences  $\frac{dA(c, S)}{dc}$ . The implication is that the ratio between the marginal decrease in absences and marginal decrease in transportation costs will equal the ratio of marginal benefit of consumption to parents' marginal benefit from the accumulation of human capital  $\frac{U_M}{U_k}$ . In an expected value setting, effects from reduced absenteeism  $k' \frac{dA(c, S)}{dc}$  accumulate in the child's human capital stock.

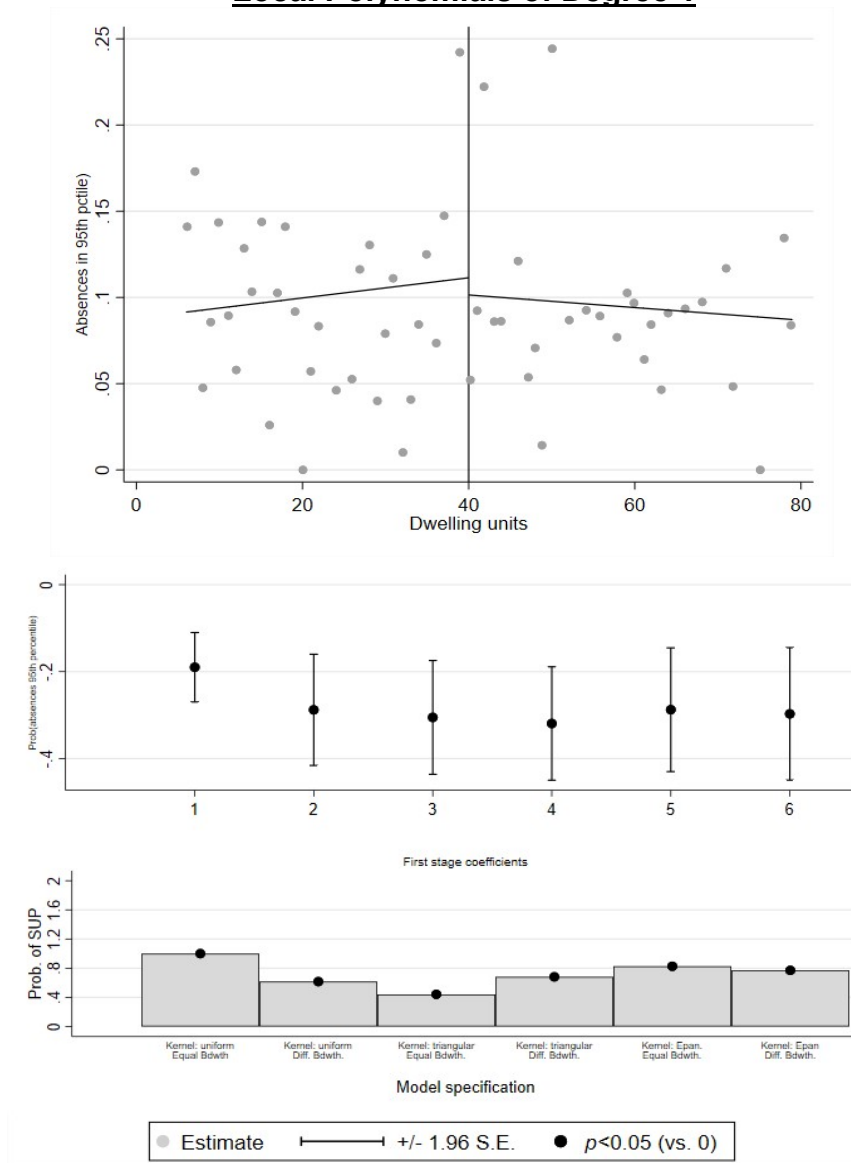
## Tables and Figures

**Figure B1**  
**Number of Dwellings and Unconditional Probability of SUP programs**



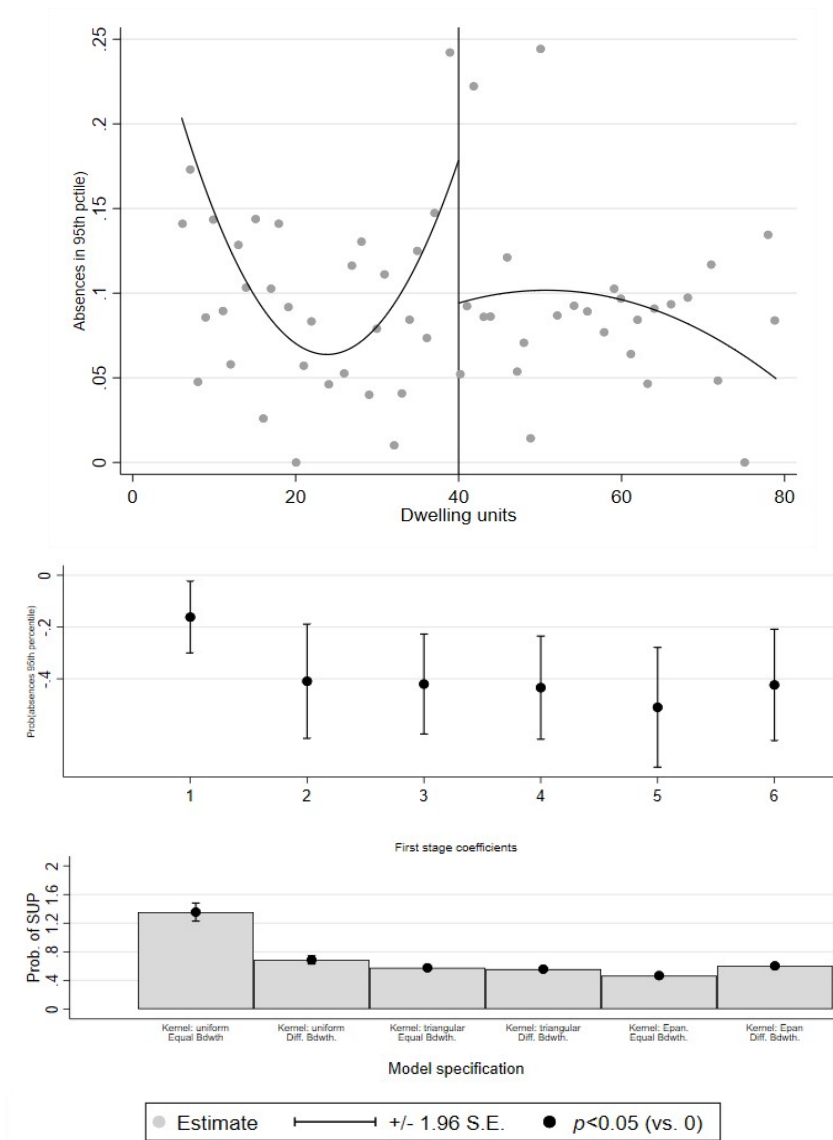
**Notes:** Dwellings are grouped in intervals of five. The vertical line represents the cutoff of 40 dwelling units.

**Figure B2**  
**FRD Effects of the SUP Program on School Absences Computed with**  
**Local Polynomials of Degree 1**



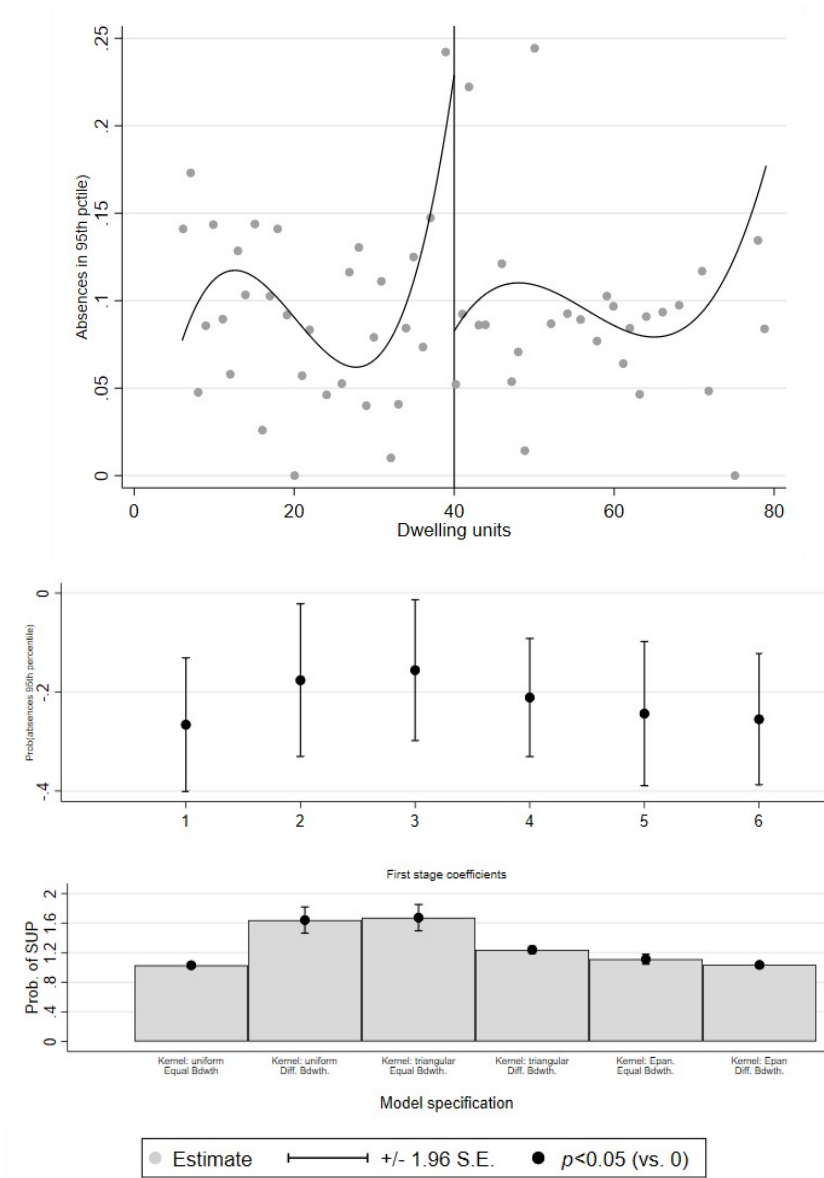
**Notes:** The *top graph* is a regression discontinuity plot computed with local linear polynomials. The *middle graph* shows FRD estimates of the effects of the SUP program on school absences (the mean effects are solid dots and the lines top and bottom of the dots are 95 percent confidence intervals). The effects were computed using local polynomials of first degree. Specifications do not include covariates. The details of the FRD are as follows: *Model 1*: Uniform Kernel weights with Equal Bandwidths at both sides; *Model 2*: Uniform Kernel weights with Unique Bandwidths at each side; *Model 3*: Triangular Kernel weights with Equal Bandwidths at both sides; *Model 4*: Triangular Kernel weights with Unique Bandwidths at each side; *Model 5*: Epanechnikov Kernel weights with Equal Bandwidths at both sides; *Model 6*: Epanechnikov Kernel weights with Unique Bandwidths at each side of the cutoff. The *bottom graph* shows the magnitude of the effect of the instrument (an indicator for what side of the cutoff a student is) on the probability of treatment. The instruments' coefficients are in bars, and the solid rounded dots indicate statistical significance at 95% or lower.

**Figure B3**  
**FRD Effects of the SUP Program on School Absences Computed with**  
**Local Polynomials of Degree 2**



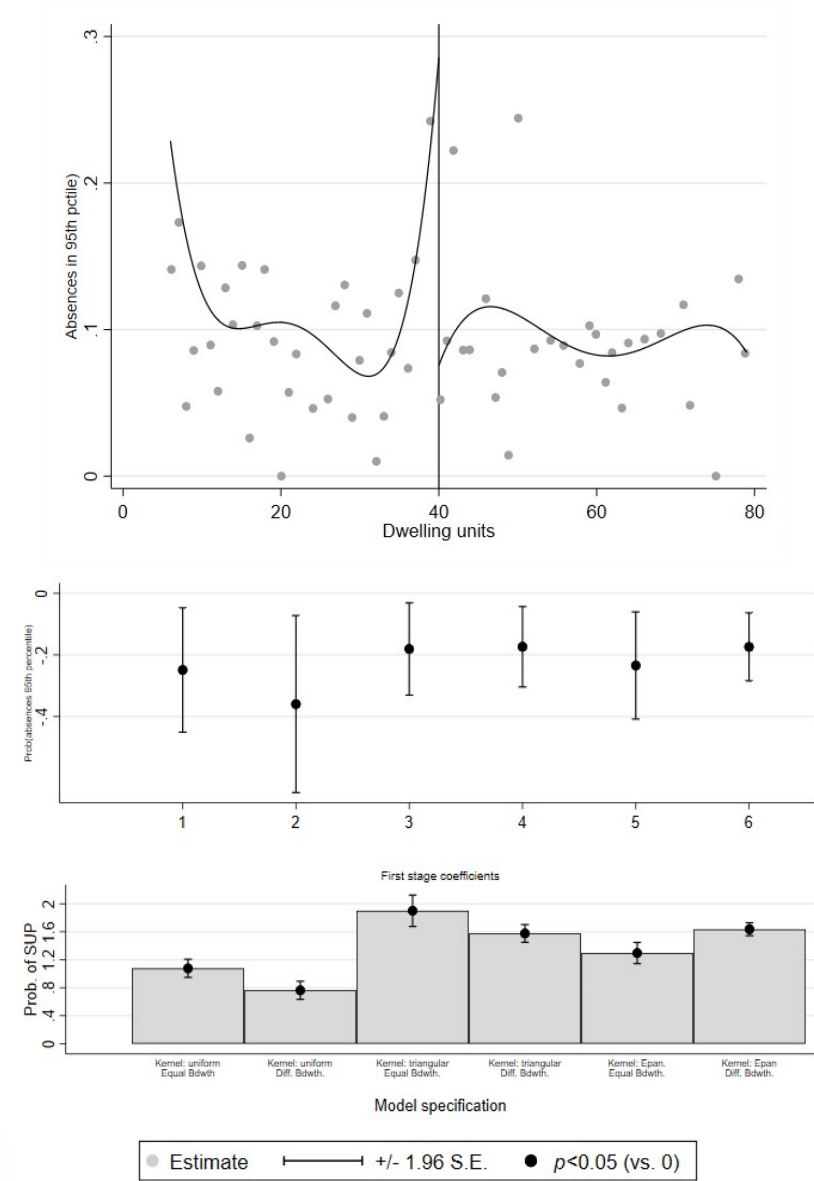
**Notes:** The *top graph* is a regression discontinuity plot computed with local second degree polynomials. The *middle graph* shows FRD estimates of the effects of the SUP program on school absences (the mean effects are solid dots and the lines top and bottom of the dots are 95 percent confidence intervals). The effects were computed using local polynomials of second degree. Specifications do not include covariates. The details of the FRD are as follows: *Model 1*: Uniform Kernel weights with Equal Bandwidths at both sides; *Model 2*: Uniform Kernel weights with Unique Bandwidths at each side; *Model 3*: Triangular Kernel weights with Equal Bandwidths at both sides; *Model 4*: Triangular Kernel weights with Unique Bandwidths at each side; *Model 5*: Epanechnikov Kernel weights with Equal Bandwidths at both sides; *Model 6*: Epanechnikov Kernel weights with Unique Bandwidths at each side of the cutoff. The *bottom graph* shows the magnitude of the effect of the instrument (an indicator for what side of the cutoff a student is) on the probability of treatment. The instruments' coefficients are in bars, and the solid rounded dots indicate statistical significance at 95% or lower.

**Figure B4**  
**FRD Effects of the SUP Program on School Absences Computed with**  
**Local Polynomials of Degree 3**



**Notes:** The *top graph* is a regression discontinuity plot computed with local third degree polynomials. The *middle graph* shows FRD estimates of the effects of the SUP program on school absences (the mean effects are solid dots and the lines top and bottom of the dots are 95 percent confidence intervals). The effects were computed using local polynomials of third degree. Specifications do not include covariates. The details of the FRD are as follows: *Model 1:* Uniform Kernel weights with Equal Bandwidths at both sides; *Model 2:* Uniform Kernel weights with Unique Bandwidths at each side; *Model 3:* Triangular Kernel weights with Equal Bandwidths at both sides; *Model 4:* Triangular Kernel weights with Unique Bandwidths at each side; *Model 5:* Epanechnikov Kernel weights with Equal Bandwidths at both sides; *Model 6:* Epanechnikov Kernel weights with Unique Bandwidths at each side of the cutoff. The *bottom graph* shows the magnitude of the effect of the instrument (an indicator for what side of the cutoff a student is) on the probability of treatment. The instruments' coefficients are in bars, and the solid rounded dots indicate statistical significance at 95% or lower.

**Figure B5**  
**FRD Effects of the SUP Program on School Absences Computed with**  
**Local Polynomials of Degree 4**



**Notes:** The *top graph* is a regression discontinuity plot computed with local fourth degree polynomials. The *middle graph* shows FRD estimates of the effects of the SUP program on school absences (the mean effects are solid dots and the lines top and bottom of the dots are 95 percent confidence intervals). The effects were computed using local polynomials of fourth degree. Specifications do not include covariates. The details of the FRD are as follows: *Model 1:* Uniform Kernel weights with Equal Bandwidths at both sides; *Model 2:* Uniform Kernel weights with Unique Bandwidths at each side; *Model 3:* Triangular Kernel weights with Equal Bandwidths at both sides; *Model 4:* Triangular Kernel weights with Unique Bandwidths at each side; *Model 5:* Epanechnikov Kernel weights with Equal Bandwidths at both sides; *Model 6:* Epanechnikov Kernel weights with Unique Bandwidths at each side of the cutoff. The *bottom graph* shows the magnitude of the effect of the instrument (an indicator for what side of the cutoff a student is) on the probability of treatment. The instruments' coefficients are in bars, and the solid rounded dots indicate statistical significance at 95% or lower.

**Table B1**  
**Balance around the Dwelling Threshold**

	P-value
Absences	.7165
Age	.4645
Sex	.0425
Flooding	.1598
Income	.8706
Year	.6186
Month of Birth	.0001
Year of Birth	.386
Program Start Year	.5852
Time Normalized	.4783

**Note:** We consider dwellings between 30 and 50 around the cutoff. Standard Errors for Flooding and Income are clustered at the settlement level.

**Figure B6**  
**Fuzzy Regression Discontinuity Estimates using Placebo Cutoffs**

