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Shelter from the storm: Upgrading housing infrastructure in Latin American slums☆

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ABSTRACT

This paper provides empirical evidence regarding the causal effects that upgrading slum dwellings has on the living conditions of the extremely poor. In particular, we study the impact of providing better houses *in situ* to slum dwellers in El Salvador, Mexico and Uruguay. We experimentally evaluate the impact of a housing project run by the NGO TECHO ("roof"), which provides basic pre-fabricated houses to members of extremely poor population groups in Latin America. The main objective of the program is to improve household well-being. Our findings show that better houses have a positive effect on overall housing conditions and general well-being: the members of treated households are happier with their quality of life. In two countries, we also document improvements in children's health; in El Salvador, slum dwellers who have received the TECHO houses also feel that they are safer. We do not find this result, however, in the other two experimental samples. There are no other noticeable robust effects in relation to the possession of durable goods or labor outcomes. Our results are robust in terms of both their internal and external validity because they are derived from similar experiments in three different Latin American countries.

1. Introduction

Rural-urban migration, combined with the intrinsic growth of the urban population, gave rise to a rapid urbanization process in the developing world during the last century (Henderson, 2002). During the period of time when this shift in settlement patterns was taking place, the United Nations adopted the Universal Declaration of Human Rights (1948), which identifies housing, along with food and clothing, as a basic requirement for achieving an adequate standard of living.[1] Nonetheless, by 2010, around 800 million people - 12% of humanity - were living in urban slums, i.e., overcrowded settlements which have poor-quality housing, inadequate access to safe water and sanitation, and insecurity of tenure (UNHabitat, 2010). While slums are usually associated with the worst face of poverty, the traditional "modernization theory" of slums (Frankenhoff, 1967 and Turner, 1969, among others) suggests that slums are not a lasting urban development problem, but instead a transitory phenomenon mainly present in fast-growing economies. According to this theory, as

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[1] United Nations, Universal Declaration of Human Rights, Article 25 (1948).

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developing economies approach a steady state, economic development progressively transforms informal settlements into formal neighborhoods.

In line with this theory, Glaeser (2011) argues that slums provide tremendous economic opportunities for the poor and that slum dwellers move there voluntarily, usually to escape subsistence-level rural poverty, and then improve their labor productivity by taking advantage of the benefits of agglomeration, economies of scale and networks offered by large cities. The income gains derived from their increasing labor productivity is said to allow the poor to gradually improve their living conditions and eventually to transform the slums into non-slum neighborhoods or to migrate out of the slums into formal housing within the city. In this view, cities are not making people poor but instead are attracting poor people; the emergence of slums is attributed to the willingness of the poor to live in substandard housing and hostile geographical environments if doing so also enables them to be close to employment opportunities.

Glaeser (2011) is aware of the potential poverty traps to be found in slums and recognizes that, while it would be a mistake to overlook the economic opportunities that slums provide for the world's poor (especially rural migrants who come from much poorer environments), it is also a mistake to idealize them. He warns us that cities require management and that, even though slums are places of opportunity, they are also places of public failure. Indeed, Glaeser (2011) calls for slum upgrading initiatives and better public goods for slum residents and argues that cities require an effective public sector to provide the basics, such as clean water and honest police, as well as better roads and means of transportation to connect slums with the more successful parts of their cities.

In fact, governments have taken multiple approaches to improving the quality of life of slum dwellers. Jaitman (2015) argues that during the 1970s a popular approach was to take families out of the slums and provide them with urban lots elsewhere – usually cheap land on the cities' outskirts – so that they could re-build their homes in a new, formal setting. These kind of "sites-and-services" strategies were widely criticized for being incomplete responses and for breaking up the geography of opportunities that slum dwellers had built up around the slum sites. Indeed, as the incoming masses of rural population groups set up their homes in cities, most of these new neighborhoods eventually became "new" slums. This raised the question as to whether governments should support the building of "new slums" for the "new poor" or instead upgrade the existing ones and progressively transform them into formal neighborhoods. This policy debate is still going strong today.

As a response to the explosive growth of new slums in large cities, during the 1980s *in situ* slum upgrading programs emerged. These programs are underpinned by a range of policies designed to improve the infrastructure and urban services available within slums, as well as to provide families with access to better housing and secure land tenure. Slum upgrading programs are viewed as a successful approach that integrates low-income communities into their larger urban contexts without disrupting the social networks that slum dwellers have built up in slum areas, some of which are of fundamental importance for optimizing the inhabitants' economic opportunities (see Jaitman, 2015 for a review of different implementation strategies for slum upgrading programs). However, there is surprisingly little evidence on the effect of *in situ* slum upgrading initiatives, in particular housing programs, on the quality of life of the poor. Questions that remain to be answered include, for example, whether the lack of proper housing in any way constrains the living standards of slum dwellers and what the effects of improved housing conditions would be in terms of their economic performance and well-being.

Adequate housing provides a number of benefits. First, families spend a large amount of time at home. Their houses are one of the few places available to families for rest and relaxation. As such, housing quality contributes substantially to well-being, quality of life and mental health. A proper house can induce a sense of dignity and pride (Sen, 1999). In fact, Cattaneo et al. (2009) and Devoto et al. (2012) have shown how specific housing improvements, such as better floors and access to better sanitation and clean water, have resulted in increased satisfaction with quality of life as well as better mental health. Second, adequate housing can promote physical health by providing protection against the ravages of the environment. Roofs and walls shelter from rain and from the cold. Clean water, sanitation and non-dirt floors protect against parasitic infestations and infections. Finally, housing may provide security and serve as a defense against crime, a major problem in slums (UNHabitat, 2010). Thus, proper housing may allow households to accumulate assets by freeing up time for use in more productive activities that would otherwise be devoted to protecting their existing assets (UNHabitat, 2003a).

This paper provides some of the first experimental evidence regarding the causal effects of upgraded dwellings in terms of the living conditions of extremely poor persons who reside in slums. We examine the impact of supplying inexpensive but sturdy houses constructed by TECHO, an NGO that provides basic prefabricated and transitional houses to slum dwellers in Latin America, regardless of whether they own the land on which they are living or not. TECHO targets the poorest informal slums and, within these slums, the families that live in the most substandard housing. TECHO houses are a significant improvement over existing housing units in terms of their flooring, roofs and walls, but they do not have indoor sanitation facilities, running water or kitchens.

We use experimentally generated variation to assess the effects of upgraded housing on the living conditions of slum dwellers in three Latin American countries: El Salvador, Mexico and Uruguay. Our findings show that the better structures have a positive effect on overall housing conditions and subjective well-being: members of treated households are more satisfied with the quality of their lives. This is a dimension of social policy that is often overlooked but is crucial to the "life experience" of poor people and, thus, should be taken into account whenever housing programs like the TECHO initiative are evaluated. Specifically, satisfaction with housing quality increases by between 0.5 to 0.63 standard deviations, while satisfaction with quality of life jumps by almost 0.4 standard deviations, on average. This is equivalent to 3.5 times the gap in subjective well-being between households below and above the median income level of our sample and is equal to twice the gap between people whose monthly incomes per capita differ by US$100 - a huge effect given that the average monthly income per capita in the control group at baseline is around US$60. In two countries, El Salvador and Mexico, we also document improvements in children's health, which are much larger than sanitation-only interventions and almost comparable to the effects found in programs that combine sanitation and water system upgrades (Duflo et al., 2015). In El Salvador, slum dwellers' perception of their safety and security also improves, but this has not translated into positive effects in terms of the possession of durable goods or employment outcomes.

Any causal study must overcome both internal and external threats to its validity (see Campbell, 1957 and Cook et al., 1979). Most research is focused on addressing threats to internal validity; i.e., on ensuring that the estimated effects are "causal" within the context of the study population. External validity, in contrast,

refers to the extent to which the estimated effects can be applied to other populations in different settings and at different times. Ultimately, external validity is established by replication in multiple datasets drawn from a variety of environments (Angrist, 2004).[2] Our results are unusually robust in terms of both their internal and external validity because they are derived from experimental assessments of the same intervention in three different Latin American countries, and we are therefore able to identify causal results that are robust across countries.[3]

Despite the importance of housing, however, very little evidence exists regarding the causal effects of housing programs. Our findings constitute a contribution to the small body of literature on this subject, as well as to our understanding of how *in situ* slum upgrading initiatives can affect the living standards of slum dwellers.[4] To the best of our knowledge, this study is the first randomized experiment undertaken to assess the impact of upgrading housing infrastructure in slums in the developing world.[5] Previous contributions include the evaluation of relocation initiatives conducted by Katz et al. (2001), who analyzed the results of the Moving to Opportunity (MTO) program, which randomly offered vouchers to residents in poor neighborhoods in the U.S. that allowed them to relocate to areas with lower poverty rates. Voucher recipients experienced improvements in some indicators of well-being, including safety and health, and a reduction in the prevalence of behavioral problems among boys. Kling et al. (2005) exploited the same experiment and found a reduction in arrests of young people for violent crimes and of young females for property crimes, but also found increased behavioral problems and property crime in the case of young males. This contrasts with the findings of Barnhardt et al. (2015) for a relocation program that randomly offered the participants an opportunity to move out of a slum and into improved housing on the outskirts of Ahmedabad, India. These authors find that, fourteen years after the program assignment, a third of the beneficiaries had never moved to the new sites. While beneficiaries have better housing conditions relative to control households, they do not show higher incomes, better health or improvements in child educational outcomes. Moreover, the program seems to have destroyed the social capital of beneficiaries by increasing their isolation from family and caste networks and by reducing their access to informal insurance.

In order to evaluate *in situ* upgrading interventions, Cattaneo et al. (2009) exploit a natural experiment to show that replacing dirt floors with cement floors in urban slums in Mexico has a positive impact on child health, maternal mental health and satisfaction with quality of life. Along the same lines, Devoto et al. (2012) study the effects of randomly offering credit to finance household hook-ups to the water distribution system in urban Morocco. While they do not find significant health effects, they do find a significant improvement in self-reported well-being. Finally, in a recent study by Duflo et al. (2015), the authors find that an integrated intervention that combines drinking water supply and sanitation facilities in slums in rural India decreased the incidence of diarrhea by between 30% and 50%.

The rest of this paper is organized as follows. In Section 2, we discuss who lives in slums and offer some insights into the formation of slums. In Section 3, we describe the TECHO intervention. Section 4 presents the experimental design. In Section 5, we introduce the econometric methods used in this study while, in Section 6, we present our empirical results. Finally, Section 7 concludes.

2. Who lives in slums

Conventional explanations attribute the emergence of slums to the fact that the poor are willing to live in substandard housing in polluted or flood-prone areas, on slopes or ridges and in other inhospitable geographical environments if this allows them to be close to employment opportunities in the city center (Glaeser, 2011).[6] This suggests that slum dwellers have a strong preference for being close to the labor market- so strong that it may offset any kind of disadvantage that living in an irregular settlement may entail.

Marx et al. (2013) argue that slums are the product of multiple market and policy failures (mainly governance and coordination problems) that obstruct slum dwellers' capital accumulation and human development opportunities. Indeed, most slum dwellers live in houses with dirt floors, poor-quality roofs and walls constructed out of waste materials such as cardboard, tin and plastic. These houses do not provide proper protection from inclement weather, are not secure and are not pleasant to live in. Many have insufficient access to services such as clean water, sanitation and electricity (UNHabitat, 2003b and Marx et al., 2013). Thus, life in the slums may constitute a form of poverty trap for a majority of the residents, most of whom find themselves stuck in slums for generations.

The potential effects that housing upgrading interventions can have on the living standards of slum dwellers should be analyzed against the backdrop of these two hypotheses, i.e., taking into account the complementarities and externalities that emerge from the interaction between slum dwellers' locational decisions, on the one hand, and their housing and income baseline conditions, on the other. How poor are slum dwellers compared with non-slum poor population groups in terms of income and housing? Are the slum dwellers who live in peripheral areas poorer than those who live in slums located closer to urban centers?

In this section, we provide some evidence to support the hypothesis that slum and non-slum dwellers have different income and housing preferences. In Appendix Tables A.9–A.14, we compare a large number of outcomes of interest in regard to the slum population using information from the national household surveys of El Salvador, Mexico and Uruguay on the non-slum poor population in the same geographical areas as our TECHO samples, i.e., households whose members were below the national poverty line in the year that the national survey was conducted[7]. These are typ-

[2] See Cruces and Galiani (2007) for an application of this idea in the context of a quasi-experiment on the effect of fertility on the female labor supply.

[3] While external validity is evaluated in terms of the direction and statistical significance of the effects of the intervention, the size of the effects could well be different across settings because the counterfactuals might also differ across settings.

[4] See Marx et al. (2013) for a survey on the economics of slums, Jaitman (2015) for a literature review on slum upgrading programs, and Duflo et al. (2012) on urban services.

[5] There are also a large number of cross-sectional observational studies that point to the existence of strong associations between poor housing and indicators of poor health (see Thomson et al., 2001 for a review).

[6] In fact, one of the reasons mentioned by Banerjee et al. (2008) for the rise of unemployment in South Africa after the end of apartheid in 1994 is the high cost of job searches for the black population, since the country's persistent geographical racial segregation has confined blacks to areas far away from the city center, which is also hard to reach due to the unavailability of good public transportation. The end of apartheid thus resulted in an increase in the labor supply among the black population that, in light of high job-search costs, could not find a match in labor demand.

[7] In the case of Uruguay, the national survey results enable us to distinguish between poor slum dwellers and poor groups not living in slum conditions. This is rather rare, since, in general, household surveys' coverage of slum settlements is very limited or non-existent (see, among others, Marx et al., 2013), and we there-

ically poor households whose members live in rented dwellings or have received housing subsidies that have enabled them to purchase their own properties in the formal sector; most of these people live on the outskirts of urban areas where land prices are lower. The first column of each table shows the mean of the variable of interest for the poor population and the second for the slum dwellers targeted by TECHO. The third column shows the difference across the poor and slum dweller groups. For El Salvador and Mexico, in the fourth column we also show what the differential is once we control for a dummy that indicates whether the household is in a rural or urban area. In those cases, our preferred estimate is the one shown in this last column of each table.

The first salient aspect of the comparison is that, in all three countries, slum dwellers are generally worse-off in terms of housing quality and assets than other poor populations. For instance, the share of rooms with good-quality floors is 14% among slum inhabitants compared to 61% for the poor population of El Salvador overall. In Mexico and Uruguay, the share of rooms with good-quality floors among the non-slum poor is 20 percentage points greater than it is for slum dwellers. Rates for water connections, access to toilets and sewerage systems, and possession of refrigerators and TV sets are all significantly higher for the average poor household of El Salvador and Mexico than for slum dwellers in the same country. In Uruguay, the differences are smaller - in part because the average rates are much higher among this highly urban population.

In Uruguay and Mexico, however, the incomes of slum dwellers are higher than the incomes of poor persons who do not live in slums. In Mexico, the slum dwellers included in our baseline survey earn, on average, US$108 per month per capita, while the average income for the poor population overall is US$86 a difference of 25%. In Uruguay, slum dwellers earn an impressive 71% more than poor persons not living in slums; the difference between men's and women's incomes is also significant in both countries. Not only are monthly incomes higher, but the wage incomes of slum dwellers are also significantly higher than those of the rest of the poor population. The difference amounts to approximately 40% in Uruguay and 30% in Mexico when we average out the wage differentials for both men and women.

Interestingly, in the case of Montevideo, Uruguay, the 2008 continuous household survey (which is representative at both the national and regional levels) enables us to distinguish between poor slum dwellers and poor groups not living in slum conditions at the city level. We find that non-slum poor households and slum households located in the city center and its immediate surroundings earn, on average, US$15 more per capita than their counterparts living on the outskirts - a statistically significant difference at the 5% level that accounts for around 30% of the difference between the monthly incomes per capita of slum and non-slum poor populations (not shown)[8]. Indeed, the difference increases further when comparing residents of central urban areas with other city residents. For example, slum dwellers who reside in the central area of a city earn, on average, US$53 and US$97 more than slum dwellers and non-slum poor households located in the immediately surrounding areas or on the outskirts, respectively. Importantly, using the same locational definition, we find that the proportion of slum households located in the city center and its immediate surroundings is significantly greater than the proportion of poor households located in the same area (39% vs 24%). Overall, this latter finding indicates that a considerable proportion of the income premium associated with residence in slums as compared with residence in a non-slum poor neighborhood is apparently explained by locational effects.

The case of El Salvador is different. In economic terms, the slum households in El Salvador are much more disadvantaged in all respects. In this case, the labor-market outcomes of slum dwellers are worse than those of the poor not living in slums. Indeed, the educational attainment of household heads and school enrollment rates for their children are also lower in slums. This may have to do with the fact that, in El Salvador, many people have moved to slums in order to escape violent civil conflict rather than in order to seek economic opportunities. Given that fact, the presence of poverty traps seems plausible here. If this is the case, then the main reason for living in slums would not have to do with different housing and income preferences, but rather with institutional and policy failures that have prevented slum dwellers from relocating to safe and economically more productive environments. Under these circumstances, the lack of economic opportunities ends up blocking slum dwellers' efforts to accumulate capital, leaving them stuck in an income and productivity trap.

In contrast, the results in Uruguay and Mexico seem to be consistent with the existence of poor groups with different preferences. We find that, while slum dwellers have clearly worse housing infrastructure than the rest of the poor population, they earn significantly more than poor people living in non-slum areas even though they have the same levels of human capital. There appears to be an intrinsic "selection" among the poor: those who prefer to have good access to the labor market in cities tend to gather in slums, while those who are less willing to do so live in better environments, although at a significant cost in terms of income. Consequently, the question that naturally arises is how to explain why slum dwellers earn more but live in much worse housing units.

A first explanation revolves around the lack of property rights. In the context of slums, where most residents do not hold legal title to their dwellings, housing upgrading initiatives have typically been seen as inefficient unless property rights - in particular land titles – are provided beforehand. Land titling programs would encourage slum dwellers to increase the value of their properties by investing in their existing housing and in environmental improvements (see Field, 2005, and Galiani and Schargrodsky, 2010). Nonetheless, as cities become denser, land prices increase, which not only raises the opportunity cost for local authorities of providing land titles to slum occupants, but also invites eviction by landowners (Jimenez, 1984; 1985 and Brueckner and Selod, 2009). A second explanation could be that slum dwellers incomes are too low for them to be able to afford significant housing upgrades. The costs of material improvements, transportation and expertise plus the opportunity costs in terms of the time and effort required to renovate housing may be so high that they discourage slum dwellers from implementing infrastructure enhancements.

In summary, low incomes and the absence of property rights, coupled with a higher risk of eviction, may prevent slum dwellers from investing in upgrades for their houses. While housing upgrading programs that offer lasting material improvements regardless of land tenure status may enhance slum dwellers' living standards, it can also increase the likelihood of eviction as the land value increases. If eviction takes place, then housing in-

fore use that national survey for the analysis in this section although we restrict it to the geographical areas covered in our study. For El Salvador and Mexico, however, the information on slum dwellers is drawn exclusively from our baseline survey.

[8] References to the city center and its immediate surroundings correspond to city center zones (CCZs) located in the first or second belt areas of Montevideo, excluding CCZ 11, which is predominantly rural; references to the periphery or to the outskirts of the urban area include CCZs located in the third belt of Montevideo along the border of Canelones Department (see de Montevideo, 2013).

Fig. 1. TECHO house.

vestments will be lost, and slum dwellers will be relocated, most probably, to locations in which they will have poorer living conditions than before. In this context, the provision of transitional housing with structures that can be dismantled, as is the case of the TECHO units, emerges as an intermediate solution that, on the one hand, avoids the obstacles to slum-dwelling enhancements and, on the other, allows slum residents to conserve the value of housing investments in the event of eviction, since they can take their houses with them to their new location. We describe these and other details of TECHO interventions in the next section.

3. Upgrading housing infrastructure

The TECHO program provides basic, pre-fabricated, transitional houses to extremely poor families living in informal settlements (slums) in Latin America regardless of whether or not they own the land on which they live. The aim of this program is to increase the well-being of these families. The program was started 19 years ago in Chile and now works in 19 Latin American countries. This NGO has built almost 100,000 houses with the help of an army of volunteers. Every year, more than 30,000 youths throughout Latin America volunteer to work with TECHO.[9]

The locations of the settlements in El Salvador are somewhat different than the sites in the other two countries. In El Salvador, TECHO works in poor areas scattered throughout the country, but not in the country's main urban center of San Salvador. In contrast, the TECHO intervention sites are concentrated closer to the largest urban centers in the other two countries. In Mexico, this includes slums in Estado de Mexico located adjacent to Mexico City and, in Uruguay, slums located in and around Montevideo.

TECHO targets the poorest informal settlements and, within these settlements, households that are lodged in very substandard dwellings. TECHO serves "irregular settlements," which are defined as communities in which a majority of the families are living on plots of land that they do not own. These settlements are plagued by a host of problems, including insufficient access to basic utilities (water, electricity and sanitation), significant levels of soil and water contamination, and overcrowding. The typical housing units in these informal settlements are no better than the surrounding dwellings, as they are rudimentary units constructed from discarded materials such as cardboard, tin and plastic, have dirt floors and lack connections to basic utilities such as water supply and sewerage systems.

The TECHO housing units are 18 m^2 (6 m by 3 m) in size. The walls are made of pre-fabricated, insulated pinewood or aluminum panels, and the roofs are made of tin to keep occupants warm and protect them from humidity, insects, and rain.[10] Floors are built on top of 15 stacks that raise them up to between 30 and 80 cm off the ground in order to reduce dampness and protect occupants from floods and infestations. Although these houses are a major improvement over the recipients' previous dwellings, the amenities that they offer are limited, as they do not include a bathroom or kitchen or plumbing, drinking water hook-ups or gas connections.

The houses are designed to be low in cost and easy to construct; they can be placed on a plot of land next to an existing house or as a new unit that replaces the existing one. Units are modular and portable, can be built with simple tools, and are set up by volunteers working in squads of from 4 to 8 members. The cost of a TECHO house is less than US$1000 with the bulk of the cost being accounted for by the acquisition, storage and transportation of the building materials, since there are essentially no labor costs. The beneficiary family contributes 10% of that amount (around US$100). In El Salvador, US$100 is approximately equivalent to 3.3 months' per capita baseline earnings, while in Mexico and Uruguay, it is roughly equivalent to 1.6 and 1.4 months, respectively (see Appendix Table 6). Fig. 1 shows examples of the TECHO houses built in El Salvador, Mexico and Uruguay. Importantly, added to the fact that the TECHO house is heavily subsidized, there are no exact substitutes of TECHO houses on the market that households could be investing in incrementally. TECHO do not offer the house in the market and only offer it to a group of selected slum dwellers that are in the poorest conditions within slums. Hence, even if households did not face credit constrains to get access to housing improvements, they could not get access to TECHO houses neither in the form nor at the price offered by

[9] While the program primarily focuses on building homes, over 3500 regular volunteers also commit at least one day a week to community organization and social inclusion initiatives. This second phase of the intervention aims at developing skills through the implementation of these projects. Our study focuses on evaluating the impact of the first phase of the program: the construction of transitional houses. We limit the evaluation sample frame to settlements that did not receive the services provided during the second phase of the intervention so that no intervention other than the construction of housing was involved during the period of analysis.

[10] In all three countries, the roofs of TECHO houses are made of aluminum. In El Salvador, the floors of TECHO houses are made of cement, and the walls are made of aluminum. In Mexico and Uruguay, the floors and walls are made of wood.

TECHO. This consideration is relevant for interpreting the results of our study.

Finally, the houses are also easy to disassemble and move to a new location. It is important for the houses to be movable because most of the families in these makeshift settlements do not have formal title to the land that they live on. TECHO managers were concerned that upgrading the value of the land by building permanent housing might induce both public and private owners to try to force residents to move in order to reclaim the improved land. However, making the housing mobile does away with that incentive.[11]

4. Experimental design

The TECHO programs budget and personnel constraints limit the number of housing units that can be built at any one time.[12] Under these constraints, TECHO opted to select beneficiaries through a lottery system that gives all eligible households in a pre-determined geographical area an equal opportunity to receive the housing upgrade in a given year. We exploit this experimental variability to assess the impact of improved housing conditions.

TECHO first selected a set of eligible settlements and then conducted a census to identify eligible households within each settlement (i.e., those poor enough to be given priority).[13] The eligible households were surveyed (baseline survey) and then randomly assigned to treatment and control groups within each settlement.[14, 15] In order to obtain truthful information from the households and to avoid creating any desirability bias in the treatment group, the data collection work was separated from the implementation of the intervention by contracting a highly respected survey firm in each country. The enumerators told the people whom they interviewed that they were collecting data for a study on living conditions and did not make any reference to the TECHO program either verbally or in written form. After randomization, treatment households were told about the program and its requirements by TECHO officials. Some of them accepted the program and some rejected it. Note that the control households were not told that they would receive the TECHO houses in the future, and so their behavior should not have been affected by the expectation of being treated in the next round, although they could have felt frustrated when they realized that they had lost the lottery. We discuss this point further in the section dealing with the results of the experiment.

Baseline surveys were conducted approximately one month before the start of the construction work in each settlement. Since the TECHO program did not have the capacity to work in all settlements at once, the program was rolled out in each country in two phases, and the follow-up surveys were therefore conducted between 15 and 27 months after the construction work.[16] All the surveys included modules on socioeconomic characteristics, the labor market, assets, security, health and self-reported measures of satisfaction. (See Tables A.2–A.4 in the Appendix for details on the variables covered in these analyses.)

Our sample includes 23 settlements in El Salvador, 39 settlements in Mexico and 12 in Uruguay. The total number of eligible households in these settlements was 2373, with the total being split more or less evenly across the three countries.[17, 18] Treatment was offered to 60% of the households in El Salvador, 51% in Mexico and 61% in Uruguay (see Table A.5 in the Appendix). In all, over 85% of the households in the intention-to-treat groups complied with the treatment assignment (the remaining 15% were unable to afford the required 10% copayment and hence did not receive a house), while the compliance rates for the non-intention-to-treat groups were practically perfect. Finally, we attempted to track all of the households that migrated out of the study settlements, but could find and interview only a fraction of them. Attrition rates from the sample were between 5.5% and 7% in the intention-to-treat group and between 6.3% and 8.7% in the non-intention-to-treat group. Though the attrition rates are about one percentage point higher in the non-intention-to-treat group in all three countries, the differences are not statistically significant at conventional levels. Finally, note that both non-compliance and attrition rates are pretty much the same across country samples; thus, potential differences in the causal effects should not be attributed to treatment or sample selection issues, but instead to baseline differentials between sites.[19]

4.1. Experimental group balance

Under randomization, the outcomes of the intention-to-treat and non-intention-to-treat groups should be equal, on average, prior to treatment. In Tables A.6 and A.7 in the Appendix, we present summary statistics separately for the intention-to-treat and non-intention-to-treat groups on a large set of pre-treatment variables grouped as socioeconomic characteristics, housing characteristics, assets, satisfaction with quality of housing and quality of life, security, education and health. We also report robust standard errors and test for the null hypothesis of no dif-

[11] A more comprehensive slum upgrading program would likely be preceded by a land titling program (see, among others, Field, 2005 and Galiani and Schargrodsky, 2010).

[12] This also constrained the size of the sample used in our study in each country.

[13] Eligible settlements are in slums where: (i) at least 50% of the residents do not have land title, and/or (ii) the slum lacks access to at least one of the following three basic services: electricity, drinking water and sanitation. Settlements where TECHO had intervened in the past were considered ineligible and were not included in the evaluation. In El Salvador, we first randomly selected states (excluding San Salvador), then randomly selected municipalities within each selected state, and then TECHO did a census of eligible settlements within each selected municipality. In the case of Mexico, we first randomly selected municipalities within Estado de Mexico, and then TECHO did a census of eligible slums within each selected municipality, all of which were considered in the sample. Finally, in the case of Uruguay, since most of the municipalities in Montevideo Department included settlements in which TECHO had already worked, the sampling was non-random and based on a census of settlements where TECHO had not implemented the program in the past (for a map of the regions where the settlements included in the study are located in each country, see Fig. 2.)

[14] In El Salvador and Uruguay, some settlements were randomly assigned a higher intensity-of-treatment level. However, due to the small number of clusters (settlements), for the most part we did not exploit this feature in our analysis.

[15] Within each settlement, every household had the same probability of being chosen for inclusion in the intention-to-treat group, but this was not necessarily the case across settlements, since the proportion of households that were treated varied from settlement to settlement.

[16] See Appendix Table A.1 and Fig. 3 for the dates of each phase and follow-up survey in each country.

[17] Note, however, that the number of individuals, as measured in the follow-up survey, increased in almost all groups and samples. Among the households interviewed in the follow-up survey, a large percentage of the new members were children under 2 years of age. The rest were mainly other children of the head of household who had not been not present at the time that the respective baseline survey was conducted.

[18] In general, the number of treatments represents a small proportion of all the households in each settlement. For example, around 40% of the settlements had fewer than 10% of households in the treatment group, and only in 22% of the settlements did the proportion of treated households exceed 30% of the population.

[19] Since compliance rates are very high, the intention-to-treat effect (ITT) resembles the local average treatment effect (LATE) and, inasmuch as compliance rates are very similar across countries, no differences in 2SLS estimates should be expected across them. Hence, we only report intention-to-treat effects.

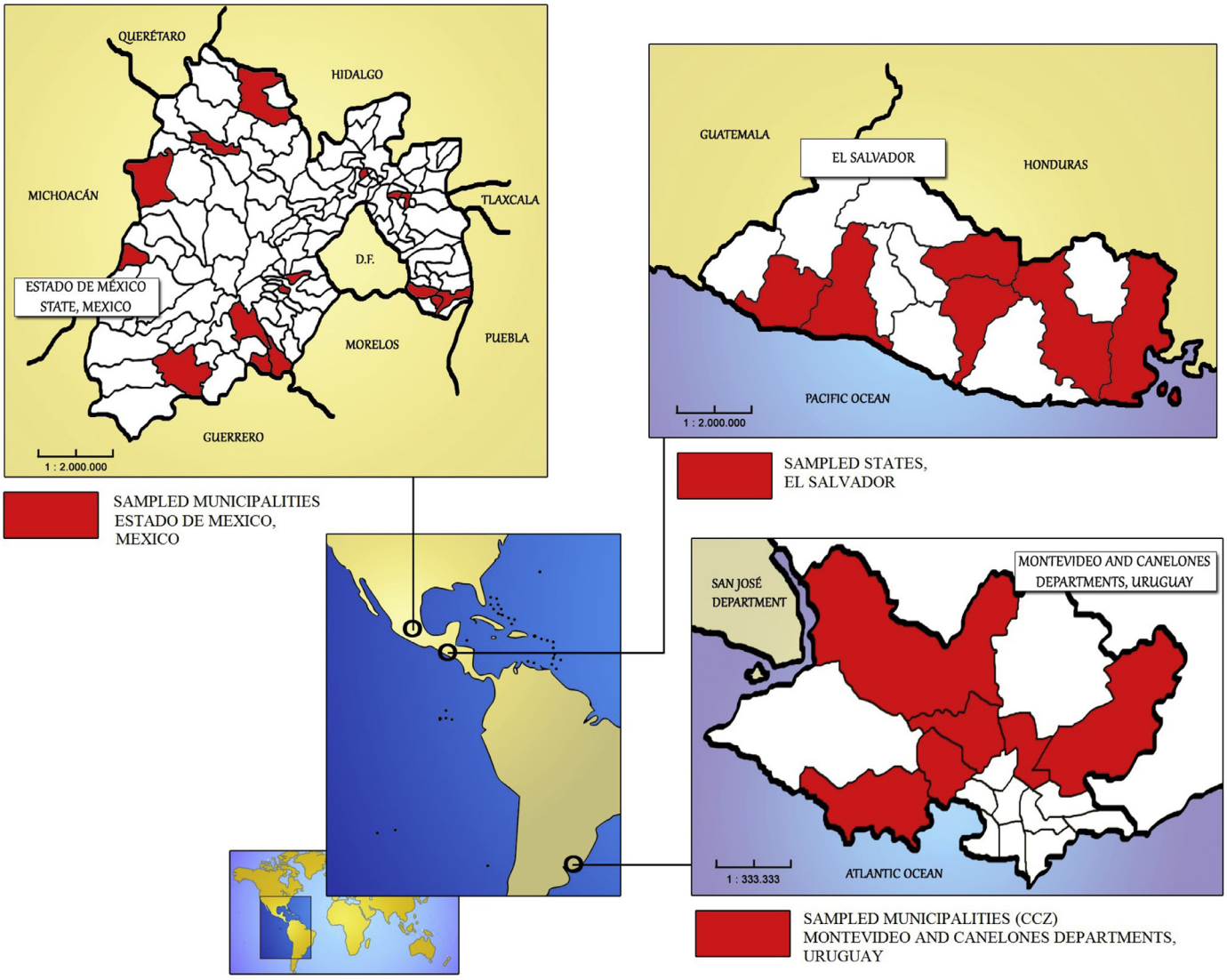

Fig. 2. Evaluation sites.

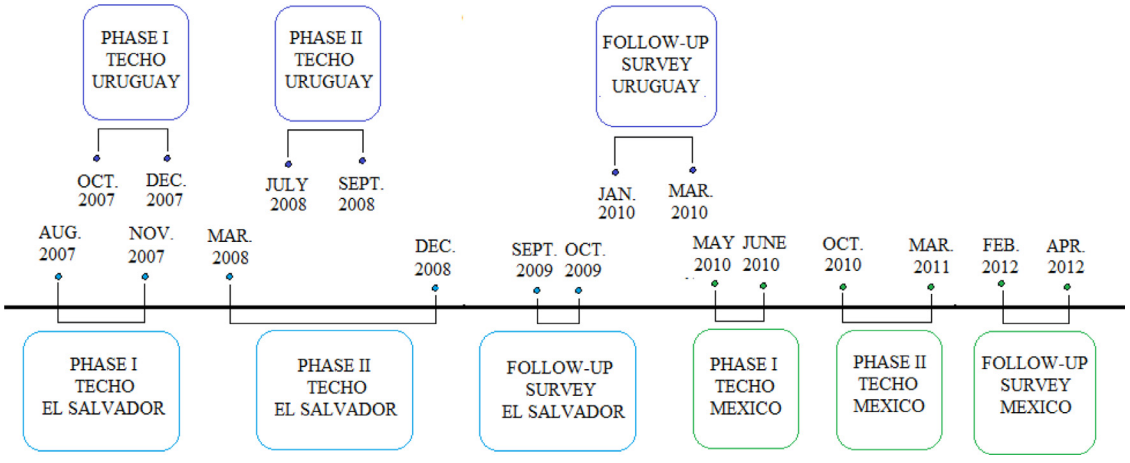

Fig. 3. Timeline of interventions.

ference between the mean values of each variable for each experimental group. Given that the randomization of units between experimental groups occurred within each settlement, we expect them to be well-balanced once we control for settlement fixed effects. Thus, when testing the null hypothesis of no differences between the two groups, we control for settlement fixed effects.

The analysis indicates that the design is well-balanced, since, in Mexico and El Salvador, only 2 out of 44 variables are unbalanced (and just one of them at a 5% significance level), while, in Uruguay, 6 variables appear to be unbalanced, but none of them at a 5% significance level. Finally, in the combined three experiments, while 3 out of 44 variables are statistically different between groups, only one of them is unbalanced at conventional levels (5%). This is about what would be expected to occur purely by chance.[20, 21]

4.2. Baseline cross–country housing differences

A major strength of this study is that it provides an evaluation of the same intervention in three different populations and environments. Mexico and Uruguay are much richer than El Salvador. The purchasing power parity (PPP) gross national income (GNI) per capita in 2007 was US$12,580 in Mexico and US$11,020 in Uruguay, compared to US$5640 in El Salvador. These differences are reflected in housing and, as such, influence the estimated impacts of dwelling upgrades on outcomes. Therefore, a comparison of the baseline housing characteristics is an important input for the interpretation of our results, as they provide the counterfactual estimates for the treatment effects.

In Table A.8 in the Appendix, we highlight a set of 11 housing characteristics measured at baseline in all of the countries and test the null hypothesis of no difference between the mean values of each variable by country. Baseline housing was, as is to be expected, substantially better in Mexico and Uruguay than in El Salvador. For example, in Mexico, 64.9% of the households had good-quality floors while, in Uruguay, the corresponding figure was 37.2% and, in El Salvador, it was only 14.4%. In Uruguay and Mexico, a large percentage of households had electricity (95.9% and 83.8%, respectively) and some type of water hook-up (91.3% and 51%, respectively), while, in El Salvador, only 39.1% of households had electricity and 21.5% of them had some sort of water hook-up on the property.

5. Methods

We report estimates of the average intention-to-treat effect for the outcomes of interest. Given the high compliance rate, these parameters are very close to average treatment effects. Operationally, we estimate the following regression model:

$$Y_{ij} = \alpha + \gamma Treat_{ij} + \beta X_{ij} + \mu_j + \varepsilon_{ij} \quad (1)$$

where i indexes households or individuals, j indexes settlements, Y_{ij} is any of the outcomes under study (measured at follow-up round), and γ is the parameter of interest (i.e., the coefficient associated to a dummy variable that equals 1 for the households or individuals that were experimentally allocated to treatment, and 0 otherwise) for the outcome under consideration[22]; X_{ij} is a vector of pre–treatment characteristics measured at baseline; μ_j is a settlement fixed effect; and ε_{ij} is the error term. The settlement fixed effects capture the average unobservable differences across settlements that may exist given that randomization was conducted within each settlement. Controlling for settlement fixed effects, we assume that the error terms are independent and report only robust standard errors throughout the empirical section of the analysis.[23, 24]

In studies with multiple outcomes, statistically significant effects may emerge simply by chance. The larger the number of tests, the greater the likelihood of incurring in a type I error. We correct for this possibility by using Bonferroni family-wise error rates (FWER) to adjust the p-values of the individual tests as a function of the number of outcome variables. We compute Bonferroni FWER corrections at the 10% level of statistical significance by dividing the desired size of the test (10%) by the number of outcome variables in conceptually similar blocks of outcomes grouped by table and by country experiment[25].

We also follow Kling et al. (2007) in constructing summary indices by family group. We first standardize each outcome variable by subtracting the within-settlement mean value of the control group and by dividing by its standard deviation. Then, for those observations that have information in at least one outcome of the family group but have missing information in other outcome variables of the family group, we impute missing standardized outcome variables using the within-settlement mean by intention-to-treat status. The summary index is computed as the sum of standardized outcome variables in the family group with the sign of each measure oriented so that more beneficial outcomes have higher scores divided by the number of outcome variables. These summary indices, aggregating information across related outcomes, are not only useful summary statistics but may also heighten the statistical power of the data for the detection of the effects of the intervention that are consistent across groups of outcomes when they have idiosyncratic variation.

[20] The analysis remains almost unchanged if we instead cluster the standard errors at the settlement level while still including settlement fixed effects. We find only 3 variables that are unbalanced in El Salvador, 4 in Mexico and Uruguay, and only 3 in the combined three experimental samples. These results are available upon request.

[21] Without controlling for settlement fixed effects, we find that, in Uruguay, only 2 variables appear to be statistically unbalanced; in Mexico, 6 variables are unbalanced, but in El Salvador as many as 8 variables are unbalanced at the 10% level of statistical significance. Overall, in the combined three samples, 6 variables are unbalanced at conventional levels of statistical significance. These results are available upon request.

[22] Some of the variables under study are limited dependent variables (LDVs). The problem posed by causal inference with LDVs is not fundamentally different from the problem of causal inference with continuous outcomes. If there are no covariates or the covariates are sparse and discrete, linear models are no less appropriate for LDVs than for other types of dependent variables. This is certainly the case in a randomized control trial where controls are included only in order to improve efficiency, but their omission would not bias the estimates of the parameters of interest.

[23] The statistical inference of the results reported in the next section are robust to clustering the standard errors at the settlement level in that rejection decisions of the null hypothesis of no effect remain the same at conventional levels of statistical significance. This result lends credibility to our assumption that the settlement fixed effect captures the systematic unobserved differences across slums. These results are available upon request.

[24] Note that the phasing design of the intervention is given at the settlement level, and so there is no within-settlement variation in phase. Thus, controlling for phase effects makes no sense, since phase and settlement fixed effects span the same subspace.

[25] See the notes to each table for the specific Bonferroni corrected *p*-value applied to each set of variables. For example, if there are 5 variables in the group, then the Bonferroni corrected *p*-value is 0.1/5 = 0.02. Thus, we would reject the null-hypothesis of no treatment effect for an outcome within a given group if the estimated coefficient is significant at the 2% level.

Finally, the use of standard statistical corrections to attempt to control for the type-I error rate of a test, such as the Bonferroni correction, are more helpful in the context of an experiment where there is little other information to be used in the analysis than the data on the randomization of treatment status. Our study departs from that paradigm in that it reports effects of three independent samples, and we can therefore rely on the information gleaned from these independent samples to confirm the validity of our inference. In the next section, greater emphasis will therefore be placed on the sets of results that have been obtained for all three samples.

6. Results

In this section we report the estimated effects of the delivery of TECHO houses on several outcome variables of interest, including dwelling quality, satisfaction with the house and with quality of life, security, assets, labor supply and child health. We report the results of estimating Eq. (1) for two different specifications–one with and one without a set of control variables that include the household head's years of schooling, gender and age, as well as the value of household assets per capita and monthly income per capita, all of which were measured during the baseline round. In each table, we first present the results for Model 1 (not controlling for baseline covariates) and Model 2 (controlling for baseline covariates) for each country separately and then present the estimates for the parameter of interest in these two models for a pooled sample that includes the three experiments. These estimates provide an informative "average" summary of the results across all three countries and also are likely to be more precise. At the bottom of each table, we report the effect on the aggregated summary index for all indicators. Finally, we report conventional significance levels in the traditional manner in the tables and the corresponding Bonferroni FWER adjusted *p*-value for each group in the table notes.

6.1. Housing

We begin by demonstrating that the provision of a TECHO house has an impact on the quality of housing. This is a necessary condition in order for this intervention to have any impact on the other outcomes. In addition, we test whether families invested further in their house. Better houses may also provide incentives to invest in further housing improvements, since such investments may be associated with other complementarities (see, among others, Banerjee and Duflo, 2011). Generally, we find that the delivery of a TECHO house has had a large positive effect on the quality of housing but no more than that.

In Table 1 we present the results for the program's effects on housing quality. As expected, the program resulted in substantial improvements in the quality of floors, walls and roofs, as well as in the percentage of rooms with windows. The TECHO program substantially improved overall housing as reflected in the program's effect on the housing quality summary index. Since baseline housing conditions were worse in El Salvador than in Uruguay and Mexico, the program's absolute effects are consistently larger in the first case than in the others. Still, in all cases, the effects are large both in absolute and in relative terms. All the estimated effects except those for the number of rooms remain significant after adjusting the p-values for multiple outcomes. Nevertheless, the increase in the number of rooms remains statistically significant in the case of Mexico and also in the combined analysis across the three experiments.

Table 2 reflects our findings regarding the question as to whether the improvement in housing triggered further investments by the beneficiary families. We find that the program did

Table 1
Regressions of housing quality on program dummy.

Dependent variable	El Salvador			Uruguay			Mexico			All		
	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2
Number of rooms	2.690	0.233	0.234	3.486	0.100	0.075	3.067	0.234	0.214	3.088	0.188	0.175
	(1.330)	[0.117]**	[0.115]**	(1.636)	[0.132]	[0.132]	(1.285)	[0.088]***	[0.086]**	(1.440)	[0.064]***	[0.064]***
		[0.047]	[0.043]		[0.453]	[0.572]		[0.008]	[0.014]		[0.004]	[0.006]
Share of rooms	0.165	0.284	0.288	0.317	0.197	0.197	0.706	0.111	0.110	0.442	0.182	0.183
with good quality	(0.274)	[0.027]***	[0.026]***	(0.415)	[0.033]***	[0.033]***	(0.355)	[0.022]***	[0.022]***	(0.426)	[0.016]***	[0.016]***
floors		[0.000]	[0.000]		[0.000]	[0.000]		[0.000]	[0.000]		[0.000]	[0.000]
Share of rooms	0.104	0.255	0.255	0.483	0.136	0.135	0.420	0.167	0.164	0.352	0.178	0.177
with good quality	(0.223)	[0.026]***	[0.026]***	(0.471)	[0.035]***	[0.035]***	(0.388)	[0.024]***	[0.024]***	(0.410)	[0.017]***	[0.017]***
walls		[0.000]	[0.000]		[0.000]	[0.000]		[0.000]	[0.000]		[0.000]	[0.000]
Share of rooms	0.283	0.231	0.235	0.312	0.188	0.188	0.599	0.099	0.096	0.427	0.161	0.161
with good quality	(0.385)	[0.030]***	[0.030]***	(0.414)	[0.033]***	[0.033]***	(0.374)	[0.022]***	[0.022]***	(0.416)	[0.016]***	[0.016]***
roofs		[0.000]	[0.000]		[0.000]	[0.000]		[0.000]	[0.000]		[0.000]	[0.000]
Share of rooms	0.192	0.233	0.235	0.607	0.111	0.115	0.303	0.183	0.178	0.364	0.171	0.171
with windows	(0.274)	[0.024]***	[0.025]***	(0.336)	[0.025]***	[0.025]***	(0.329)	[0.021]***	[0.021]***	(0.358)	[0.013]***	[0.013]***
		[0.000]	[0.000]		[0.000]	[0.000]		[0.000]	[0.000]		[0.000]	[0.000]
Housing quality	0.000	0.960	0.965	0.000	0.328	0.327	0.000	0.387	0.377	0.000	0.507	0.504
Summary index	(0.646)	[0.072]***	[0.071]***	(0.515)	[0.041]***	[0.041]***	(0.570)	[0.040]***	[0.040]***	(0.576)	[0.029]***	[0.029]***
		[0.000]	[0.000]		[0.000]	[0.000]		[0.000]	[0.000]		[0.000]	[0.000]
Sett. fixed-effects		√	√		√	√		√	√		√	√
Controls		×	√		×	√		×	√		×	√

Note: Responses regarding construction materials used in rooms were included only for those households that reported information for all rooms. All the regressions have a dummy by settlement. Model 1: No Controls; Model 2: Control for HH's Years of Schooling, HH's Gender, HH's Age, Assets – Value Per Capita (USD), Monthly Income Per Capita (USD), all measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. The Housing Quality Summary Index is defined as the average of the within-settlement z–scores of all the variables in the table, with the sign of each measure oriented so that the more beneficial outcomes have higher scores. Reported results: estimated coefficient, robust standard error, and *p*-value, in that order. For non-corrected *p*-value: *Significant at 10%. **Significant at 5%. ***Significant at 1%. For Bonferroni-corrected *p*-value, we contrast the *p*-value against 0.02 for a significance level of 0.1.

Table 2
Regressions of housing investment on program dummy.

Dependent variable	El Salvador Follow Up Control Mean (Std. Dev.)	El Salvador Model 1	El Salvador Model 2	Uruguay Follow Up Control Mean (Std. Dev.)	Uruguay Model 1	Uruguay Model 2	Mexico Follow Up Control Mean (Std. Dev.)	Mexico Model 1	Mexico Model 2	All Follow Up Control Mean (Std. Dev.)	All Model 1	All Model 2
Sink on room where food is prepared	0.016 (0.123)	−0.008 [0.010] [0.418]	−0.006 [0.010] [0.558]	0.335 (0.472)	−0.014 [0.037] [0.706]	−0.009 [0.037] [0.815]	0.020 (0.140)	−0.008 [0.010] [0.421]	−0.010 [0.010] [0.363]	0.112 (0.315)	−0.010 [0.013] [0.453]	−0.009 [0.013] [0.527]
On-site water supply	0.252 (0.434)	−0.062 [0.034]* [0.072]	−0.059 [0.034]* [0.086]	0.897 (0.304)	0.008 [0.022] [0.742]	0.001 [0.022] [0.976]	0.551 (0.498)	−0.010 [0.032] [0.744]	−0.014 [0.032] [0.657]	0.573 (0.494)	−0.017 [0.017] [0.336]	−0.019 [0.017] [0.265]
Electricity connection inside the house	0.496 (0.500)	−0.046 [0.042] [0.279]	−0.038 [0.042] [0.364]	0.933 (0.251)	0.024 [0.018] [0.191]	0.023 [0.018] [0.218]	0.903 (0.297)	−0.044 [0.022]* [0.058]	−0.049 [0.023]** [0.034]	0.800 (0.400)	−0.021 [0.015] [0.166]	−0.022 [0.015] [0.153]
Use gas or kerosene stove to cook	0.167 (0.373)	0.016 [0.032] [0.626]	0.022 [0.032] [0.508]	0.521 (0.500)	−0.014 [0.039] [0.724]	−0.022 [0.038] [0.572]	0.252 (0.434)	−0.051 [0.023]** [0.029]	−0.055 [0.022]** [0.017]	0.309 (0.462)	−0.022 [0.018] [0.233]	−0.023 [0.018] [0.211]
House with own toilet	0.516 (0.500)	−0.069 [0.042] [0.103]	−0.064 [0.041] [0.128]	0.730 (0.444)	−0.011 [0.035] [0.748]	−0.018 [0.034] [0.603]	0.392 (0.488)	0.012 [0.034] [0.727]	0.005 [0.034] [0.891]	0.527 (0.499)	−0.016 [0.021] [0.459]	−0.020 [0.021] [0.340]
Housing investment summary index	0.000 (0.425)	−0.065 [0.042] [0.123]	−0.054 [0.041] [0.194]	0.000 (0.539)	0.006 [0.042] [0.892]	−0.004 [0.042] [0.932]	0.000 (0.374)	−0.052 [0.029]* [0.071]	−0.063 [0.029]** [0.031]	0.000 (0.442)	−0.036 [0.021]* [0.095]	−0.038 [0.021]* [0.069]
Sett. fixed-effects		✓	✓		✓	✓		✓	✓		✓	✓
Controls		×	✓		×	✓		×	✓		×	✓

Note: All the regressions have a dummy by settlement. Model 1: No Controls; Model 2: Control for HH's Years of Schooling, HH's Gender, HH's Age, Assets–Value Per Capita (USD), Monthly Income Per Capita (USD), all measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. The Housing Investment Summary Index is defined as the average of the within-settlement z-scores of all the variables in the table, with the sign of each measure oriented so that the more beneficial outcomes have higher scores. Reported results: estimated coefficient, robust standard error, and *p*-value, in that order. For non-corrected *p*-value, *Significant at 10%. **Significant at 5%. ***Significant at 1%. For Bonferroni corrected *p*-value, we contrast the *p*-value against 0.02 for a significance level of 0.1.

not induce significant positive complementary investments among beneficiaries. In particular, there are no positive effects on access to water, electricity or sanitation. If anything, we find that two out of the five outcomes that were studied are negatively affected in the case Mexico at conventional levels of statistical significance. In one case, significance was lower when contrasted with the Bonferroni adjusted p-values. Finally, while there is a significant negative effect on the housing investment summary index for Mexico and the all-country samples, the effect ceases to be significant when compared with the Bonferroni adjusted p-values. This indicates that, if there is any negative effect on subsequent housing investments, it would be mostly due to the aggregate and spurious effect of adding multiple variables to the summary index, not to the intervention itself.

Our results are consistent with those of Cattaneo et al. (2009), who find that upgrading dirt floors for slum dwellers in Mexico did not trigger subsequent investments in sanitation facilities, the restoration of walls and ceilings, or housing expansion. Indeed, the self-reported rent and sale values of those houses remained the same after two to four years of treatment exposure. Moreover, using a difference-in-difference identification strategy, Field (2005) finds that strengthening property rights in urban slums in Peru has a significant effect on housing renovation investment. That author finds that the increase in housing investment was mainly financed without the use of credit, indicating that changes over time are due mostly to higher investment incentives related to a lower threat of eviction. Along the same lines, Galiani and Schargrodsky (2010) exploit a natural experiment in the allocation of land titles in shantytowns in Argentina and find that families with title to their land substantially increased their housing investments and that this effect was primarily channeled through the increased physical capital of title owners rather than, here again, through reduced credit constraints. In keeping with this literature, we hypothesize that the roughly null effects of the TECHO program on subsequent housing investment is due to the increasing value of beneficiary housing in the absence of land titles, as this increases the risk of eviction and thus neutralizes the incentives for TECHO households to continue investing in housing improvements.

6.2. Satisfaction with housing and quality of life

We report estimates of intention-to-treat effects for a number of self-reported measures of subjective well-being with respect to housing quality, as well as for a comprehensive subjective well-being (SWB) measure for quality of life. The measures are based on responses to the following question, each one highlighting the specific attribute evaluated: "How satisfied are you with (i) the quality of your floor; (ii) the quality of your walls; (iii) the quality of your roof; (iv) the extent to which your house is protected against water when it rains; and (v) your quality of life - Would you say you are "Unsatisfied", "Neither Unsatisfied nor Satisfied", "Satisfied" or "Very Satisfied"? These measures are good in the sense that they yield results that are comparable with the responses obtained for questions about general life satisfaction and provide a basis for global retrospective assessments of how people think their lives are going; they are also increasingly being used to assess the impact of social programs and public policy overall (Dolan et al., 2011).[26] For each of these measures, we assume that responses can

[26] On the metric of happiness measures, there are various studies that suggest that people have a common understanding of happiness and that numerical measures are effective in capturing feelings. For example, Van Praag (1991) argues that people seem to translate numerical happiness into verbal labels, and Diener and Lucas (1999) suggest that people are even able to predict the satisfaction levels of others. Nonetheless, Alwin (1992) suggests that there are diminishing returns to addi-

be represented on a linear scale. Thus, we estimate a linear probability model in which our dependent variable is a dummy that equals 1 if the respondent reports being "satisfied" or "very satisfied" and zero if not.[27]

Table 3 , Panel A, presents the program's effects on binary self-reported measures of satisfaction with the housing unit and with an overall SWB measure of quality of life. In all countries, all measures substantially increased. Families are happier with their houses and with their lives once they are in their new houses.[28] The gains are substantially larger in El Salvador[29] than in Mexico and Uruguay, which is consistent with the fact that the improvement in housing conditions is greater in the first case than in the other two.[30] The index that measures satisfaction with the quality of floors, for example, is over 200% higher in households in the treatment group with respect to the control group in El Salvador, while in Mexico the index is around 20% higher in the intention-to-treat households than in the control-group households, and in Uruguay the differential is around 39%. Similarly, satisfaction with quality of life is 41% higher in the intention-to-treat households in El Salvador, while in Mexico the figure is around 28%, and in Uruguay it is around 21%.

The relatively small effect on satisfaction with quality of life as compared with the sizable effects on satisfaction with housing quality should not be at all surprising. This suggests that housing quality is not the only consideration for respondents when assessing their quality of life. To the extent that a new house does not generate any other sizeable effect, it can be expected that no more than a moderate effect on our self-reported measure of quality of life will be found.

Thus far our analysis has considered a binary state of satisfaction, which fails to take into account any variance except that which exists between one half ("unhappy") and the other half ("happy") of the scale. Hence, as detailed in the methods section, we follow Kling et al. (2007) and Ludwig et al. (2012) to construct standardized satisfaction outcomes and a summary SWB index using all the values from the satisfaction scale. This is not only useful as a robustness check, but also for comparisons of the cost-effectiveness of the TECHO program with respect to other housing or relocation programs that have been shown to have a significant SWB impact in similar populations.

As can be observed in Table 3, Panel B, satisfaction with housing quality increases by between 0.5 to 0.63 standard deviations (SDs), while satisfaction with quality of life rises by almost 0.4 SDs, on average. This is equivalent to 3.5 times the gap in SWB between households below and above the median income and is equivalent to twice the gap between slum dwellers whose monthly incomes per capita differ by US$100- a huge effect given that the average monthly income per capita of the control group at baseline is around US$60. In other words, the effect of the TECHO program on SWB is roughly equivalent to three times the monthly income per capita of an average household. Considering that beneficiary households invested US$100 as a copayment to obtain the TECHO house, then on average their return on the housing investment was around 100% in terms of SWB. This means that an amount equal to 1.67 times their baseline average income yielded a SWB equivalent to the level of SWB that they would have obtained if they earned, on average, 3.33 times as much as their baseline income.

Interestingly, Ludwig et al. (2012) measure the long-run effects of the randomly assigned benefits of the Moving to Opportunity (MTO) program on SWB and find that 10 to 15 years after the intervention, a 1-SD decline in neighborhood poverty (13 percentage points) increased the SWB of MTO beneficiaries by an amount equal to two thirds of the gap in SWB between U.S. blacks and whites, or the equivalent of the gap between people whose annual incomes differ by around US$250 per month per capita; this is a large effect given that the annual income of the control group in that study is US$13,000, or around US$400 per capita per month. Note, however, that the effect of the MTO program on SWB is roughly equivalent to an increase in the monthly income per capita of an average household of 60%, which amounts to a smaller proportional effect than that of the TECHO intervention.[31] While the populations across studies are not comparable in terms of baseline income or housing conditions (TECHO beneficiaries are much poorer than their MTO counterparts, although their SWB measures are relatively similar), this evidence suggests that relocation to better neighborhoods might not always buy more happiness than *in situ* upgrading interventions.

tional response options for happiness scales. Indeed, for questions about feelings in the U.S., Andrews and Withey (1976) find that 3–point response scales may capture 80%-90% of the variation captured by 7–point scales. Finally, as Jacoby and Matell (1971) and Lehmann and Hulbert (1972) recommend, 3-point or higher scales are fine when the focus is on group averages, as is the case with our study of the results of the TECHO program.

[27] A limitation of linear models is the assumption of cardinality, i.e., that responses to the happiness question fall on a linear scale. However, as Ludwig et al. (2012) note, even if respondents differ in terms of the thresholds that they use to map experienced utility into happiness reports, this is not a problem for our analysis so long as the TECHO treatment itself does not affect the happiness thresholds because, in that case, the distribution of happiness thresholds would be similar across experimental groups by virtue of random assignment. Thus, to be on the safe side, we later relax the cardinality assumption and re–estimate Eq. (1) using an Ordered Probit model and obtain qualitatively similar results.

[28] Note that, for all variables considered in this section and all experimental samples, in no case did the average outcome for the control group decrease between the baseline and treatment measures, which indicates that being a lottery loser did not generate a frustration effect on the part of the control households. Moreover, we tested for heterogeneous treatment effects across households in slums in which a high proportion (greater or equal to 30%) versus a low proportion (lower to 30%) of the population was treated and found no significant differential effect. The effect is robust to higher proportions of treatment. Overall, this suggests that negative spillover effects from treated (winner) to untreated (loser) households were not present. These results are available upon request.

[29] Due to a problem with data collection in the follow-up survey in El Salvador, non-response to this question was differentially larger for the control group. Thus, to be on the safe side, we impute a value equal to 1 ("satisfied with quality of life") to 84 missing values in the control group observations; this reduces the non-response rate for this variable from 43% to 7%, which is the same as in the intention–to–treat group. Without performing this imputation, the coefficient is 0.479 for Model 1 and 0.480 for Model 2.

[30] As was mentioned before, qualitatively, the results for this section are robust to the estimation of an Ordered Probit model. The probability of being in the highest (or second-highest) satisfaction category increases with treatment in every case, and the marginal effect is also statistically significant at conventional levels in each case. These results are available upon request.

[31] Note also that this is a much higher return relative to the return afforded by similar interventions such as the Piso Firme program in Mexico, where an average beneficiary investment of US$150 per household to replace dirt floors with cement floors - which represents around 1.5 times the mean monthly income per capita of an average household - generates an average increase of 18% in SWB 2 to 4 years after treatment exposure. In contrast, a US$100 investment as a copayment to obtain a TECHO house in Mexico - around 1.6 times the monthly income per capita of an average household of the type targeted by TECHO - increases SWB by 29% 1 to 2 years after the intervention. In other words, a relatively similar level of investment (in terms of the share of total household income that it represents) in housing quality generates a 60% greater increase in SWB for TECHO program beneficiaries than it does for Piso Firme beneficiaries.

Table 3
Regressions of satisfaction on program dummy.

	El Salvador			Uruguay			Mexico			All		
Dependent variable	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2
Panel A. Binary outcomes												
Satisfaction with floors quality	0.163 (0.369)	0.387 [0.039]*** [0.000]	0.389 [0.040]*** [0.000]	0.314 (0.464)	0.121 [0.038]*** [0.002]	0.120 [0.038]*** [0.002]	0.551 (0.498)	0.108 [0.034]*** [0.002]	0.108 [0.034]*** [0.002]	0.374 (0.484)	0.180 [0.022]*** [0.000]	0.181 [0.021]*** [0.000]
Satisfaction with walls quality	0.132 (0.338)	0.477 [0.039]*** [0.000]	0.479 [0.040]*** [0.000]	0.267 (0.443)	0.142 [0.037]*** [0.000]	0.140 [0.037]*** [0.000]	0.439 (0.496)	0.149 [0.035]*** [0.000]	0.149 [0.035]*** [0.000]	0.303 (0.459)	0.226 [0.022]*** [0.000]	0.227 [0.021]*** [0.000]
Satisfaction with roofs quality	0.159 (0.366)	0.476 [0.038]*** [0.000]	0.477 [0.039]*** [0.000]	0.339 (0.474)	0.179 [0.037]*** [0.000]	0.176 [0.038]*** [0.000]	0.404 (0.491)	0.153 [0.034]*** [0.000]	0.157 [0.035]*** [0.000]	0.317 (0.465)	0.241 [0.021]*** [0.000]	0.242 [0.021]*** [0.000]
Satisfaction with protection against water	0.167 (0.373)	0.426 [0.038]*** [0.000]	0.427 [0.039]*** [0.000]	0.325 (0.469)	0.166 [0.038]*** [0.000]	0.158 [0.038]*** [0.000]	0.347 (0.476)	0.094 [0.034]*** [0.007]	0.098 [0.035]*** [0.005]	0.291 (0.454)	0.199 [0.021]*** [0.000]	0.200 [0.022]*** [0.000]
Satisfaction with quality of life	0.506 (0.501)	0.207 [0.045]*** [0.000]	0.211 [0.046]*** [0.000]	0.449 (0.498)	0.096 [0.039]** [0.015]	0.097 [0.039]** [0.015]	0.593 (0.491)	0.165 [0.032]*** [0.000]	0.166 [0.032]*** [0.000]	0.527 (0.499)	0.151 [0.022]*** [0.000]	0.153 [0.022]*** [0.000]
Satisfaction summary index	0.000 (0.692)	1.029 [0.086]*** [0.000]	1.031 [0.087]*** [0.000]	0.000 (0.720)	0.323 [0.062]*** [0.000]	0.317 [0.062]*** [0.000]	0.000 (0.708)	0.291 [0.052]*** [0.000]	0.295 [0.052]*** [0.000]	0.000 (0.706)	0.482 [0.038]*** [0.000]	0.485 [0.038]*** [0.000]
Panel B. Standardized outcomes												
Z-score satisfaction with floors quality	0.000 (0.952)	1.126 [0.109]*** [0.000]	1.261 [0.111]*** [0.000]	0.000 (0.964)	0.323 [0.082]*** [0.000]	0.317 [0.082]*** [0.000]	0.000 (0.950)	0.189 [0.070]*** [0.007]	0.191 [0.070]*** [0.006]	0.000 (0.954)	0.496 [0.050]*** [0.000]	0.499 [0.050]*** [0.000]
Z-score satisfaction with walls quality	0.000 (0.958)	1.565 [0.120]*** [0.000]	1.558 [0.122]*** [0.000]	0.000 (0.964)	0.379 [0.082]*** [0.000]	0.374 [0.082]*** [0.000]	0.000 (0.955)	0.292 [0.075]*** [0.000]	0.290 [0.074]*** [0.000]	0.000 (0.958)	0.632 [0.053]*** [0.000]	0.634 [0.053]*** [0.000]
Z-score satisfaction with roofs quality	0.000 (0.942)	1.421 [0.107]*** [0.000]	1.418 [0.109]*** [0.000]	0.000 (0.964)	0.413 [0.079]*** [0.000]	0.403 [0.080]*** [0.000]	0.000 (0.954)	0.345 [0.072]*** [0.000]	0.354 [0.072]*** [0.000]	0.000 (0.952)	0.630 [0.050]*** [0.000]	0.633 [0.050]*** [0.000]
Z-score satisfaction with protection against water	0.000 (0.952)	1.339 [0.111]*** [0.000]	1.331 [0.113]*** [0.000]	0.000 (0.971)	0.373 [0.083]*** [0.000]	0.357 [0.083]*** [0.000]	0.000 (0.955)	0.263 [0.072]*** [0.000]	0.267 [0.072]*** [0.000]	0.000 (0.958)	0.562 [0.051]*** [0.000]	0.563 [0.051]*** [0.000]
Z-score satisfaction with quality of life	0.000 (0.915)	0.627 [0.088]*** [0.000]	0.634 [0.089]*** [0.000]	0.000 (0.964)	0.298 [0.077]*** [0.000]	0.300 [0.078]*** [0.000]	0.000 (0.942)	0.323 [0.071]*** [0.000]	0.322 [0.071]*** [0.000]	0.000 (0.940)	0.389 [0.045]*** [0.000]	0.390 [0.045]*** [0.000]
Z-score satisfaction summary index	0.000 (0.744)	1.242 [0.088]*** [0.000]	1.241 [0.089]*** [0.000]	0.000 (0.732)	0.357 [0.062]*** [0.000]	0.350 [0.063]*** [0.000]	0.000 (0.711)	0.281 [0.052]*** [0.000]	0.283 [0.052]*** [0.000]	0.000 (0.726)	0.540 [0.039]*** [0.000]	0.542 [0.039]*** [0.000]
Sett. fixed-effects		√	√		√	√		√	√		√	√
Controls		×	√		×	√		×	√		×	√

Note: Panel A shows results for binary outcomes, i.e, dummy variables that equal 1 if the individual report being "Satisfied" or "Very Satisfied" and zero otherwise. Panel B shows results for standardized outcomes using all the values from the satisfaction scale. Outcome variables are standardized by subtracting the within-settlement mean value of the control group and by dividing by its standard deviation. All the regressions have a dummy by settlement. Model 1: No Controls; Model 2: Control for HH's Years of Schooling, HH's Gender, HH's Age, Assets–Value Per Capita (USD), Monthly Income Per Capita (USD), all measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. The Satisfaction Summary Indexes are defined as the average of the within-settlement z-scores of all the variables in each family of outcomes, with the sign of each measure oriented so that the more beneficial outcomes have higher scores. Reported results: estimated coefficient, robust standard error, and *p*-value, in that order. For non-corrected *p*-value, *Significant at 10%. **Significant at 5%. ***Significant at 1%. For Bonferroni-corrected *p*-value, we contrast the *p*-value against 0.02 for a significance level of 0.1.

Note that TECHO houses are heavily subsidized - families only pay US$100 of an investment that amounts to, on average, US$1000. If we assume that a TECHO house can be obtained in the market for US$1000 (which is not the case, since TECHO is the only producer of this type of house), slum dwellers would have to spend, on average, more than 3 times their total monthly income (around 16 times their monthly income per capita) to obtain a comparable housing upgrade. This is arguably not an incentive that would be compatible with the many other needs that poor families also have to satisfy on a daily basis (e.g., food, transportation, etc.), unless these families could finance the cost of the house through credit - something that, to the best of our knowledge, has not been implemented yet in the slums under study.[32]

Additionally, if the risk of being evicted is high (as is probably the case for most slum dwellers who lack title to the land on which they live), then the expected capital cost involved would be close to 100%, as slum dwellers will not be able to recover the housing investment when their houses are confiscated. This naturally discourages slum dwellers from investing a great deal in improving their housing.

Finally, it is important to make the distinction between the cost- effectiveness exercise made by the beneficiary and the one made by the policy maker. While the beneficiary considers the US$ 100 co-payment as the only cost of accessing the TECHO house, the policy maker should consider both the co-payment plus the subsidy (US$ 1000 in total). In that sense, as long as SWB is an outcome to be considered for policy decisions, the cost-effectiveness policy question would be whether an unconditional US$ 1000 cash transfer would have produced the same level of SWB than the one produced by TECHO program. The latter is naturally untestable given our experimental design. However, at least we know that a US$ 1000 housing program like TECHO yields to SWB gains on the order of 0.4 SDs, which is equivalent to an effect of 28% in terms of life satisfactions when using binary indicators. This is still a more cost-effective intervention than alternative infrastructure interventions in comparable contexts like Urban Morocco, where a program that costs on average US$ 940 per households and offered free interest rate credits to get access to piped water to poor households generated positive effects on life satisfaction on the order of only 8% (Devoto et al., 2012).

6.3. Security and safety

Security is one of the most important concerns of urban slum dwellers. Information from our baseline survey shows that, overall, 38% of the heads of household often or always felt unsafe and 54% felt unsafe when leaving their homes alone. In this sense, it could be argued that providing a better house could potentially make people feel safer. In Table 4 we present the results of the program in terms of several measures of security related to housing. We report the effect of the program on the perception of security: whether people feel safe inside the house, whether they feel that it is safe to leave the house alone, whether it seems safe to leave children alone in the house and whether the house has been burglarized. All the questions refer to the preceding year.

[32] Note that we are not considering the labor and transportation costs involved in buying and transporting the materials or the machinery, tools and opportunity costs of the time required to assemble the house, all of which may increase the overall cost even more.

Table 4
Regressions of perception of security on program dummy.

	El Salvador			Uruguay			Mexico			All		
Dependent variable	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2
Safe inside the house during the last 12 months	0.643 (0.479)	0.175 [0.040]*** [0.000]	0.178 [0.041]*** [0.000]	0.621 (0.486)	0.029 [0.038] [0.455]	0.025 [0.038] [0.506]	0.718 (0.450)	0.001 [0.031] [0.969]	0.002 [0.031] [0.944]	0.668 (0.471)	0.053 [0.021]** [0.013]	0.052 [0.021]** [0.014]
Safe leaving the house alone during the last 12 months	0.601 (0.490)	0.155 [0.043]*** [0.000]	0.159 [0.043]*** [0.000]	0.376 (0.485)	−0.066 [0.037]* [0.078]	−0.068 [0.037]* [0.071]	0.551 (0.498)	0.014 [0.035] [0.686]	0.018 [0.035] [0.609]	0.512 (0.500)	0.021 [0.022] [0.348]	0.022 [0.022] [0.326]
Safe leaving the kids alone in the house during last 12 months	0.248 (0.432)	0.141 [0.043]*** [0.001]	0.144 [0.043]*** [0.001]	0.170 (0.376)	0.001 [0.029] [0.986]	−0.002 [0.029] [0.945]	0.162 (0.368)	−0.007 [0.026] [0.806]	−0.005 [0.026] [0.857]	0.188 (0.390)	0.032 [0.018]* [0.085]	0.031 [0.018]* [0.092]
The house had been robbed in the last 12 months	0.031 (0.173)	0.023 [0.019] [0.229]	0.023 [0.019] [0.224]	0.268 (0.443)	0.013 [0.035] [0.705]	0.013 [0.035] [0.717]	0.065 (0.246)	0.002 [0.017] [0.931]	0.002 [0.017] [0.926]	0.116 (0.319)	0.011 [0.014] [0.466]	0.010 [0.014] [0.480]
Perception of security summary index	0.000 (0.617)	0.210 [0.059]*** [0.000]	0.215 [0.059]*** [0.000]	0.000 (0.629)	−0.029 [0.050] [0.555]	−0.035 [0.051] [0.485]	0.000 (0.547)	−0.017 [0.042] [0.688]	−0.014 [0.042] [0.743]	0.000 (0.591)	0.034 [0.029] [0.237]	0.034 [0.029] [0.238]
Sett. fixed-effects		√	√		√	√		√	√		√	√
Controls		×	√		×	√		×	√		×	√

Note: All the regressions have a dummy by settlement. Model 1: No Controls; Model 2: Control for HH's Years of Schooling, HH's Gender, HH's Age, Assets–Value Per Capita (USD), Monthly Income Per Capita (USD), all measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. The Perception of Security Summary Index is defined as the average of the within-settlement z–scores of all the variables in the table, with the sign of each measure oriented so that the more beneficial outcomes have higher scores. Reported results: estimated coefficient, robust standard error, and *p*-value, in that order. For non-corrected *p*-value, *Significant at 10%. **Significant at 5%. ***Significant at 1%. For Bonferroni-corrected *p*-value, we contrast the *p*-value against 0.025 for a significance level of 0.1.

Our estimates show that, in El Salvador, all self-reported measures of security improve substantially. The increase in the index for security inside the house is around 27% and the improvement is about 57% on the index that measures whether it is safe to leave children alone, but no such effect is detected in Uruguay or Mexico.

What are the mechanisms through which a better house could make families feel safe at home? On the one hand, stronger and better constructed houses could reduce the risk of burglary, making people feel safer inside of the house or when leaving it alone. On the other hand, a better house may also be more attractive to burglars and may thus generate a negative effect on perceptions of security. Hence, a positive result may be interpreted as reflecting cases in which the first effect prevails over the second one. In fact, however, we do not find that the program has any effect on crime, as there was no statistically significant reported change in the frequency of burglaries during the past year in any of the three countries; it is also true, however, that, in El Salvador and Mexico, burglary rates in the settlements in our sample were very low, and the exercise was therefore not very informative.

6.4. *Possession of durable goods*

There are different ways in which housing conditions can influence the possession of durable goods. For example, rising house prices may stimulate consumption by increasing households' perceived wealth or by relaxing borrowing constraints (Campbell and Cocco, 2007). Also, if a better house provides security to those who live in it, then it will also provide more security for the assets inside it. Thus, the persons living in such houses can invest more in durable goods. Having a better house can also increase the valuation of some types of durable goods and thus act as an incentive for their acquisition.

Table 5 depicts the performance of different variables corresponding to the possession of assets. We estimate the effect of the program on the possession of TV sets, fans, gas stoves, refrigerators and bicycles. The results show, however, that the program has had no effect on the possession of any of these assets. Despite the fact that the TECHO program increases housing quality and generates some effects in terms of perceptions of security (mainly in El Salvador), we do not find that the treated households have responded to the investment in their houses by increasing their own investments in supplementary durable goods. This is actually not surprising, however, since the program has null effects on income and labor earnings (see the next section), and the beneficiaries' consumption capacity is therefore the same before and after the program.

6.5. *Household structure and labor outcomes*

We first estimate whether the improved housing has had any effect on the number of members residing in each house and find no statistically significant effects on this front. We also investigate whether, in this limited period of time, there has been any effect on fertility by estimating whether the treatment has influenced the number of newborns in the housing units, but, here again, we do not identify any significant effects (see Table 6).[33]

[33] In Uruguay, and only for Model 2, the increase in the number of newborns in the last two years is statistically significant at conventional levels but the significance disappears once the test is contrasted against the adjusted p-value for multiple comparisons. We also tested whether treatment affected the age structure of the household, given that we have detected some changes in household size, by estimating Models 1 and 2 for the four members-by-age categories reported in Table A.2 in the Appendix. We did not find any significant effect at conventional levels. These results are available upon request.

Table 5
Regressions of durable goods on program dummy.

	El Salvador			Uruguay			Mexico			All		
Dependent variable	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2
T.V	0.434	−0.013	0.000	0.926	0.005	0.010	0.728	−0.034	−0.037	0.711	−0.016	−0.014
	(0.496)	[0.047]	[0.046]	(0.261)	[0.022]	[0.021]	(0.445)	[0.030]	[0.030]	(0.453)	[0.018]	[0.018]
		[0.786]	[0.995]		[0.821]	[0.649]		[0.272]	[0.225]		[0.397]	[0.451]
Fan	0.034	0.015	0.019	0.535	0.018	0.017	0.018	0.001	0.001	0.177	0.010	0.010
	(0.181)	[0.020]	[0.020]	(0.499)	[0.040]	[0.040]	(0.131)	[0.010]	[0.010]	(0.381)	[0.015]	[0.015]
		[0.458]	[0.342]		[0.656]	[0.677]		[0.934]	[0.993]		[0.516]	[0.535]
Kitchen or	0.404	0.000	0.008	0.768	−0.008	−0.008	0.451	−0.035	−0.041	0.534	−0.018	−0.019
gas stove	(0.491)	[0.044]	[0.043]	(0.423)	[0.034]	[0.035]	(0.498)	[0.030]	[0.030]	(0.499)	[0.020]	[0.020]
		[0.997]	[0.851]		[0.809]	[0.827]		[0.262]	[0.183]		[0.383]	[0.358]
Refrigerator	0.123	−0.028	−0.016	0.683	−0.017	−0.017	0.207	−0.005	−0.010	0.327	−0.014	−0.013
	(0.329)	[0.032]	[0.031]	(0.466)	[0.037]	[0.037]	(0.405)	[0.026]	[0.026]	(0.469)	[0.018]	[0.018]
		[0.385]	[0.605]		[0.661]	[0.649]		[0.861]	[0.715]		[0.454]	[0.479]
Bicycle	0.323	0.037	0.044	0.546	0.014	0.020	0.279	−0.029	−0.028	0.370	0.001	0.003
	(0.468)	[0.043]	[0.043]	(0.498)	[0.040]	[0.040]	(0.449)	[0.030]	[0.030]	(0.483)	[0.021]	[0.021]
		[0.400]	[0.317]		[0.726]	[0.611]		[0.347]	[0.361]		[0.967]	[0.901]
Assets	0.000	0.030	0.052	0.000	−0.009	−0.002	0.000	−0.035	−0.042	0.000	−0.012	−0.009
summary index	(0.477)	[0.050]	[0.048]	(0.541)	[0.047]	[0.047]	(0.472)	[0.034]	[0.034]	(0.494)	[0.025]	[0.024]
		[0.547]	[0.276]		[0.841]	[0.959]		[0.300]	[0.217]		[0.637]	[0.698]
Sett. fixed-effects		√	√		√	√		√	√		√	√
Controls		×	√		×	√		×	√		×	√

Note: All the regressions have a dummy by settlement. Model 1: No Controls; Model 2: Control for HH's Years of Schooling, HH's Gender, HH's Age, Assets–Value Per Capita (USD), Monthly Income Per Capita (USD), all measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. The Assets Summary Index is defined as the average of the within-settlement z–scores of all the variables in the table, with the sign of each measure oriented so that the more beneficial outcomes have higher scores. Reported results: estimated coefficient, robust standard error, and p-value, in that order. For non-corrected p-value, *Significant at 10%. **Significant at 5%. ***Significant at 1%. For Bonferroni-corrected p-value, we contrast the p-value against 0.02 for a significance level of 0.1.

Table 6
Regressions of demographic variables on program dummy.

	El Salvador			Uruguay			Mexico			All		
Dependent variable	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2
HH size	5.453	−0.031	−0.100	4.954	0.253	0.269	5.264	0.002	−0.035	5.223	0.079	0.070
	(2.513)	[0.273]	[0.261]	(2.657)	[0.220]	[0.216]	(2.595)	[0.175]	[0.171]	(2.596)	[0.124]	[0.122]
		[0.909]	[0.703]		[0.252]	[0.214]		[0.991]	[0.837]		[0.522]	[0.567]
Newborns (≤1)	0.116	0.011	0.010	0.124	−0.009	−0.008	0.110	0.028	0.027	0.116	0.011	0.012
	(0.321)	[0.031]	[0.032]	(0.351)	[0.028]	[0.028]	(0.320)	[0.025]	[0.025]	(0.330)	[0.016]	[0.016]
		[0.732]	[0.749]		[0.748]	[0.767]		[0.263]	[0.292]		[0.485]	[0.446]
Newborns (≤2)	0.229	−0.018	−0.022	0.262	0.053	0.066	0.239	0.023	0.023	0.243	0.023	0.027
	(0.429)	[0.041]	[0.041]	(0.515)	[0.041]	[0.040]	(0.477)	[0.036]	[0.035]	(0.476)	[0.022]	[0.022]
		[0.668]	[0.593]		[0.205]	[0.105]		[0.526]	[0.530]		[0.314]	[0.235]
Adults (>18)	2.791	−0.009	−0.027	2.376	−0.021	−0.025	2.633	−0.118	−0.134	2.599	−0.059	−0.068
	(1.418)	[0.146]	[0.138]	(1.175)	[0.092]	[0.091]	(1.374)	[0.096]	[0.092]	(1.338)	[0.062]	[0.060]
		[0.949]	[0.846]		[0.823]	[0.780]		[0.220]	[0.144]		[0.345]	[0.260]
Demographic summary index	0.000	0.026	0.008	0.000	0.063	0.069	0.000	0.037	0.023	0.000	0.043	0.040
	(1.068)	[0.120]	[0.117]	(0.954)	[0.078]	[0.078]	(1.020)	[0.078]	[0.077]	(1.013)	[0.051]	[0.051]
		[0.829]	[0.943]		[0.420]	[0.372]		[0.634]	[0.769]		[0.396]	[0.427]
Sett. fixed-effects		√	√		√	√		√	√		√	√
Controls		×	√		×	√		×	√		×	√

Note: All the regressions have a dummy by settlement. Model 1: No Controls; Model 2: Control for HH's Years of Schooling, HH's Gender, HH's Age, Assets–Value Per Capita (USD), Monthly Income Per Capita (USD), all measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. The Demographic Summary Index is defined as the average of the within-settlement z–scores of all the variables in the table, with the sign of each measure oriented so that the more beneficial outcomes have higher scores. Reported results: estimated coefficient, robust standard error, and *p*-value, in that order. For non-corrected *p*-value, *Significant at 10%. **Significant at 5%. ***Significant at 1%. For Bonferroni-corrected *p*-value, we contrast the *p*-value against 0.025 for a significance level of 0.1.

We then estimate whether the improved housing, either directly or indirectly, stimulates labor supply and earnings (in particular, the income per capita of the household and whether either the head of household or the spouse works more). As can be seen from Table 7, we do not detect significant effects in terms of any of these outcomes.[34] This is consistent with Galiani and Schargrodsky (2010), who find that the labor-market performance of slum dwellers with land titles in Argentina shows no improvements relative to the performance of neighbors without land property rights, even when the effects are measured 10 to 15 years after the land titles were provided. This contrasts with Field (2007), who reports significant positive effects of improved tenure security on slum dwellers' labor supply in Peru 1 to 3 years after the intervention; this may be explained by the indirect effects that land titles played in freeing up resources that had previously been used to protect household assets (such as time or investments in informal security policies).

While the TECHO program has some positive effect on the beneficiaries' perception of security, this is limited to El Salvador, and it is not clear that it can be associated with an increase in the security of land tenure. It may be the case that the physical improvements represented by TECHO houses allay fears of property theft by other residents (as can be inferred from the increase in the perception of security of "staying in the house" or "leaving the house alone") but do not necessarily ease the fear of eviction. If the TECHO program is ineffective in easing the tensions associated with a lack of property rights, then the null results for the labor supply should not be surprising at all.[35]

Last but not least, as we discussed in Section 2, it is likely that a considerable portion of the productivity gains has already been produced by locational effects. The potential income effects that housing improvements can have under this scenario seem to be small compared with the huge income gains produced by living close to economic opportunities.

6.6. Child health

The reasons why better housing can lead to an improvement in the health of the persons living in those houses are clear. For instance, dirt floors generally pose a serious threat to children's health. In the study carried out by Cattaneo et al. (2009) concerning the replacement of dirt floors with cement floors, the authors found a statistically significant reduction in the incidence of parasitic infections, diarrhea and the prevalence of anemia. Another way in which housing improvements can support health is by reducing indoor air pollution. Hanna et al. (2016) have shown that improper ventilation of houses and the use of substandard kitchen stoves can have significantly negative effects on respiratory - and

[34] We also explored whether treatment affected educational attainment, measured by the maximum years of schooling completed as reported in Table A.2 in the Appendix for children between the ages of 6 and 12 (primary school) and between the ages of 13 and 18 (secondary school). Overall, we did not find any significant effect. We detected a small negative effect in Mexico for children from 13 to 18 years of age, but this variable was unbalanced at baseline for this group of the sample in the same direction and magnitude as the detected effect. In contrast, in Uruguay, for the same age group, we did find a small positive statistically significant effect. These results are also available upon request.

[35] Note that the follow-up survey was conducted between 1.3 and 2.3 years after the implementation of the program, with the average time since treatment being 1.8 years. This is comparable to the length of treatment exposure reported in Field (2007) for the evaluation of the Peruvian land titling program, so we do not think that the null effects are associated with the amount of time that had elapsed since the intervention.

Table 7
Regressions of labor and income variables on program dummy.

Dependent variable	El Salvador			Uruguay			Mexico			All		
	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2
Monthly income	31.618	0.704	1.422	94.862	−3.371	−3.606	55.422	−0.422	0.240	59.572	−1.835	−2.278
per capita (USD)	(29.224)	[3.098]	[2.876]	(156.792)	[13.443]	[13.420]	(54.912)	[3.759]	[3.819]	(81.054)	[3.905]	[3.858]
		[0.820]	[0.621]		[0.802]	[0.788]		[0.911]	[0.950]		[0.638]	[0.555]
Hours worked	38.033	1.738	1.050	39.081	0.025	0.592	41.086	0.824	0.651	39.711	0.704	0.847
last week by	(17.351)	[2.072]	[2.074]	(19.877)	[1.821]	[1.836]	(19.498)	[1.616]	[1.574]	(19.154)	[1.055]	[1.039]
head of HH		[0.402]	[0.613]		[0.989]	[0.747]		[0.610]	[0.679]		[0.505]	[0.415]
Hours worked	35.500	4.974	4.412	39.353	−0.047	−0.140	28.250	−3.052	−1.656	34.194	−0.693	−0.399
last week	(25.995)	[5.418]	[5.870]	(19.561)	[2.661]	[2.670]	(18.867)	[3.026]	[3.151]	(20.903)	[1.883]	[1.887]
by spouse		[0.361]	[0.455]		[0.986]	[0.958]		[0.315]	[0.600]		[0.713]	[0.833]
Economic	0.000	0.089	0.089	0.000	−0.006	−0.001	0.000	0.065	0.072	0.000	0.047	0.051
summary index	(0.395)	[0.053]*	[0.052]*	(0.488)	[0.039]	[0.039]	(0.445)	[0.044]	[0.044]	(0.445)	[0.026]*	[0.026]*
		[0.093]	[0.090]		[0.881]	[0.985]		[0.135]	[0.102]		[0.071]	[0.050]
Sett. fixed-effects		✓	✓		✓	✓		✓	✓		✓	✓
Controls		×	✓		×	✓		×	✓		×	✓

Note: In the case of monetary variables, observations over the 99th percentile were excluded. With regard to the number of hours worked, cases in which more than 84 hours were reported were not considered. All the regressions have a dummy by settlement. Model 1: No Controls; Model 2: Control for HH's Years of Schooling, HH's Gender, HH's Age, Assets–Value Per Capita (USD), Monthly Income Per Capita (USD), all measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. The Economic Summary Index is defined as the average of the within-settlement z-scores of all the variables in the table, with the sign of each measure oriented so that the more beneficial outcomes have higher scores. Reported results: estimated coefficient, robust standard error, and p-value, in that order. For non-corrected p-value, *Significant at 10%. **Significant at 5%. ***Significant at 1%. For Bonferroni-corrected p-value, we contrast the p-value against 0.033 for a significance level of 0.1.

even general - health. The houses provided by the TECHO program provide better ventilation than most of the slum dwellings do and may therefore have a positive effect on overall health as well.

In Table 8 we test whether the upgraded houses result in an improvement in child health; the indicators used for this purpose are the prevalence of diarrhea and of respiratory disease. The estimated coefficients are mainly negative in both El Salvador and Mexico, suggesting that there may have been a decrease in the prevalence of those illnesses due to the intervention, but this is not the case in Uruguay. However, given our sample sizes, the estimated coefficients are imprecisely estimated and hence are not statistically significant at conventional levels. The point estimates, though, show a large decrease in diarrhea both in Mexico and in El Salvador.[36] As a result, the overall effect, pooling across countries, is still large (a decrease of approximately 18% with a p-value equal to 0.17).[37] If we assume that the effect is not present in Uruguay because, there, the experiment took place in a better, more urbanized environment where people have greater access to services, then the pooled effect in the other two countries, reported in the last two columns of the table, point to an even larger effect, of approximately 27%, which is statistically significant at the 10% level. In contrast, we do not find enough evidence to conclude that there is a large effect in terms of the reduction of the prevalence of respiratory diseases. Nevertheless, the health summary index is also statistically significant at the 5% level for those two countries, taken together.[38]

Overall, while the receipt of a TECHO house seems to reduce the frequency of diarrhea episodes, this effect is limited to children below 5 years of age who live in environments in which there is a lack of access to basic services such as clean water or sanitation. This raises the question as to whether the provision of such basic services are a more cost-effective way of enhancing health outcomes than the improvement of building materials would be.[39] Infrastructure-based solutions that connect housing up to sewerage systems have proved effective in urban environments when combined with improvements in water supply (see, for example, Cutler and Miller, 2006 and Galiani et al., 2005).

Nevertheless, Wolf et al. (2014) suggest that interventions promoting on-site sanitation without hook-ups to sewerage systems are not as effective as the installation of hook-ups to those systems are. Indeed, Clasen et al. (2014) and Patil et al. (2014) find that sanitation-only interventions (e.g., the construction of latrines in poor villages in India) have very little effect on latrine usage and no impact at all on health. Interestingly, Duflo et al. (2015) hypothesize that water and sanitation interventions have complementarities and externalities that may amplify the effects on health if they are implemented together. In fact, the authors estimate the impact

[36] In both cases, the percentage changes are larger than the one estimated by Cattaneo et al. (2009), though neither the treatments nor the compliance rates are comparable between studies.

[37] We also interacted the intention-to-treat dummy variable with a dummy indicating whether, in the samples for El Salvador and Uruguay, the settlement was randomized to a high-intensity treatment level. The interaction was in no case found to be statistically significant at conventional levels, regardless of whether the standard errors were clustered at the settlement level or not. These results are also available upon request.

[38] Since this analysis is based on a set of assumptions, we do not contrast these results with the adjusted p-values, though the effect on the summary index would remain significant under these more stringent conditions.

[39] See Duflo et al. (2012) for a complete description of the disease burden arising from unsanitary living conditions in slums.

Table 8
Regressions of health variables of children on program dummy.

	El Salvador			Uruguay			Mexico			All			El Salvador and Mexico		
Dependent variable	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2	Follow Up Control Mean (Std. Dev.)	Model 1	Model 2
Respiratory disease	0.690	−0.041	−0.034	0.175	−0.002	0.005	0.417	−0.047	−0.041	0.403	−0.029	−0.025	0.519	−0.047	−0.045
during last	(0.463)	[0.060]	[0.061]	(0.381)	[0.034]	[0.035]	(0.494)	[0.043]	[0.044]	(0.490)	[0.025]	[0.025]	(0.500)	[0.035]	[0.035]
4 weeks		[0.498]	[0.579]		[0.963]	[0.877]		[0.283]	[0.351]		[0.249]	[0.322]		[0.182]	[0.204]
Diarrhea episodes	0.168	−0.050	−0.046	0.158	−0.011	−0.004	0.135	−0.035	−0.031	0.151	−0.027	−0.024	0.147	−0.040	−0.038
during last	(0.374)	[0.042]	[0.043]	(0.365)	[0.034]	[0.033]	(0.342)	[0.028]	[0.028]	(0.358)	[0.019]	[0.019]	(0.354)	[0.023]*	[0.023]
4 weeks		[0.243]	[0.290]		[0.737]	[0.902]		[0.224]	[0.284]		[0.172]	[0.219]		[0.095]	[0.109]
Health	0.000	0.111	0.098	0.000	0.016	−0.002	0.000	0.102	0.090	0.000	0.068	0.059	0.000	0.106	0.102
summary index	(0.746)	[0.093]	[0.093]	(0.727)	[0.067]	[0.068]	(0.765)	[0.063]	[0.063]	(0.746)	[0.041]*	[0.041]	(0.757)	[0.052]**	[0.052]*
		[0.232]	[0.292]		[0.811]	[0.981]		[0.104]	[0.153]		[0.097]	[0.148]		[0.040]	[0.050]
Sett. fixed-effects		√	√		√	√		√	√		√	√		√	√
Controls		×	√		×	√		×	√		×	√		×	√

Note: All the regressions have a dummy by settlement. Model 1: Control for Age, Age Squared, Gender, and a dummy equal to 1 if the mother lives in the household at the time of the follow–up round; Model 2: Control for Age, Age Squared, Gender, a dummy equal to 1 if the mother lives in the household at the time of the follow–up round and also for HH's Years of Schooling, HH's Gender, HH's Age, Assets–Value Per Capita (USD), and Monthly Income Per Capita (USD) at the time of the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. The Health Summary Index is defined as the average of the within-country z–scores of all the variables in the table, with the sign of each measure oriented so that the more beneficial outcomes have higher scores. Reported results: estimated coefficient, robust standard error, and *p*-value, in that order. For non-corrected *p*-value, *Significant at 10%. **Significant at 5%. ***Significant at 1%. For Bonferroni-corrected *p*-value, we contrast the *p*-value against 0.05 for a significance level of 0.1.

of a US$60 per-household integrated water and sanitation improvement program in rural India that provided household-level water connections, latrines, and bathing facilities, and find that the program reduced treated diarrhea episodes by 30%–50%, a somewhat greater impact at a lower price than the effects of the TECHO program. While the samples across studies are not entirely comparable in terms of the age of the subjects, poverty levels or the prevalence of diarrhea, this evidence suggests that large improvements in housing materials such as those represented by TECHO houses can be almost as cost-effective in reducing episodes of diarrhea in poor settlements as low-cost holistic interventions that integrate water and sanitation facilities are.

7. Conclusion

This paper provides an analysis of the impact of providing better houses *in situ* to slum dwellers in El Salvador, Mexico and Uruguay. As expected, the quality of housing greatly improves after the intervention. Subsequently, satisfaction with housing and with the quality of life increases drastically. This is a very significant result, since it suggests that limited *in situ* improvements in the dwellings of poor families has a large effect on their well-being. This finding is consistent with those of Cattaneo et al. (2009) and Devoto et al. (2012) and highlights the importance of using subjective indicators to evaluate interventions such as housing improvement programs, where the main objective is to improve the quality of family and social interactions. Thus, we conclude that the quality of housing is an important input in a household's utility function irrespective of whether it affects other material outcomes. Our results show that, as in the case of the interventions analyzed by Cattaneo et al. (2009) and Devoto et al. (2012), improvements in housing conditions have a clearly positive effect on the satisfaction and well-being of poor slum dwellers.

On the policy side, a natural question behind this result is why do people do not invest in housing upgrades if the SWB returns are high. An obvious hypothesis is credit constraints. In fact, the market had not offered a relatively low-cost, low-risk, and high-quality housing solution until TECHO came in to the slums; TECHO houses are a unique and US$ 900 (90%) subsidized housing solution that people cannot buy in the market, and that is what explain the high take-up rate among intention-to-treat households. It is not that slum dwellers did not value housing improvements at all- people were able to invest 1.5 to 3.3 Monthly Income Per Capita in order to get the house - but they do not seem to be able to afford to pay US$ 1000 for them without access to credit. Therefore, TECHO is a potential technological breakthrough that slum dwellers should be able to acquire in the market so long it is sold for an accessible price if financing of it is available.

Additionally, also in line with Cattaneo et al. (2009), we find that improved housing conditions lead to large reductions in the incidence of diarrhea, at least in two of the three experiments. The one case in which these improvements do not seem to have had health effects is the one in which the experiment took place in a better, more urbanized environment in which services are more accessible.

The provision of better housing has virtually no other statistically significant effects. Perceptions of security and safety change for the better only in El Salvador, while there is no change in the other two countries. In all three countries, better housing has little or no effect on further housing investments to supplement the upgrading intervention, the possession of durable goods, household structure or labor outcomes. Do the null effects on labor supply and incomes means that slum dwellers are immersed in a poverty trap that TECHO housing cannot eliminate? In light of our results in Section 2, we believe that the bulk of the produc-

tivity gains have already been obtained from the location decision (living in a slum). Hence, providing better housing conditions to slum dwellers generates little if any additional benefits in terms of income, unless the housing improvement frees up resources that were previously used for less productive activities which is not the case. As Field (2007) and Galiani and Schargrodsky (2010) suggest, land titling programs play an essential role in reducing the costs involved in protecting people's land. Thus, a more holistic slum upgrading intervention that combines land titling with material improvements is potentially much more effective in tackling slum poverty traps than housing upgrading initiatives alone.

In this study we also compare slum dwellers to the rest of the poor population in the areas analyzed. When we consider the slum dwellers' situation within their national contexts, it becomes possible to shed some light on their housing decisions and the dynamics of slum formation. We find that slum dwellers have clearly worse housing infrastructure than poor non-slum dwellers. However, in the more urban areas, the slum dwellers earn significantly more than other poor households and have comparable levels of educational attainment and labor-market participation outcomes. These findings are consistent with the plausible explanation for slum formation as a consequence of some poor groups being more willing to trade off living conditions for better access to the labor market. These poor households choose to live in substandard dwellings in slum areas because they tend to be closer to production activities than other parts of urban conglomerates. At the same time, other poor people are less willing to do so and therefore live in better environments but at a significant cost in terms of their income. The existence of these two types of poor households with different preferences should be taken into account when designing housing policies.

These findings serve as inputs for the debate about slum upgrading initiatives. What emerges from our analysis is that the provision of the kind of *in situ* housing upgrade that we studied in this paper has some significant effects on the living conditions of slum dwellers but that those effects are perhaps not as large as society might wish or expect. At first glance, the conclusion to be drawn from this finding might be that *in situ* upgrading should be ruled out and priority should be given to geographic relocation policies. This conclusion could, however, be in error. First of all, the *in situ* intervention is fairly inexpensive and substantially increases life satisfaction, an effect that seems to be larger than the ones reported from evaluations of relocation interventions (Ludwig et al., 2012). What is more, in the two countries where we detect a reduction in the incidence of diarrhea, the effects are quite large, much larger than sanitation-only interventions and almost comparable to the effects found in programs that combine sanitation and water system upgrades (Duflo et al., 2015). Additionally, Cattaneo et al. (2006) analyzed the performance of the Mexican "Iniciamos Tu Casa" program, which provided new houses to poor inhabitants. These houses were located far from the city center. A year after the program had started, the authors found that a large proportion of the participants had abandoned the houses; moreover, those who remained in them mentioned that, although housing conditions were better, the new neighborhoods provided them with poor access to public goods and general infrastructure (Barnhardt et al., 2015 find results that point in the same direction).

Our results are also consistent with the evidence presented in Takeuchi et al. (2008) for Mumbai. These authors use a residential location model to assess the welfare of an *in situ* slum upgrade program and a slum relocation program and conclude that, at least for those households relocated to more remote locations, the disadvantages of changes in commute distance wipe out the housing benefits of the program and that the treated households would have been better off if they had been given access to the more limited housing improvements provided by the *in situ* intervention. In light of this evidence, added to the evidence that we present in Section 2, where we show that, at least in urban areas, poor households are willing to trade off housing conditions for better access to labor markets and, hence, higher earnings, *in situ* upgrading appears to remain a valid policy choice.

Last but not least, it is important to mention that our conclusions are limited to a very specific *in situ* upgrading intervention, one that provides a unique in-kind and heavily subsidized housing solution that certainly constitutes a very cost-effective (and thus attractive) housing improvement for poor and credit constrained households. A natural counterfactual to this experiment might be to offer a cash transfer that households could invest according to whatever their own preferences suggest, either on housing or any other material necessity that they may consider the top priority. Such experiment would allow us to evaluate the level of importance that slum dwellers attribute to housing upgrades compared to satisfying other necessities, and whether such an alternative intervention can yield slum dwellers to obtain comparable effects on other areas of development like health or security. Had an unconditional cash transfer resulted in higher levels of SWB and material development, the natural conclusion would be that TECHO program is subject to an implementation scheme that does not optimize the potential welfare that could be produced with the same level of resources. Future research oriented to discuss optimal implementation schemes of housing policies can be very valuable for the debate on the cost-effectiveness of different interventions in the area.

Appendix

Table A.1
Timeline of intervention and surveys.

	El Salvador	Mexico	Uruguay
Phase 1 - Construction	August - December, 2007	May - August, 2010	October - December, 2007
Phase 2 - Construction	March - August, 2008	November, 2010 - March, 2011	July - September, 2008
Follow-up survey	September - October, 2009	February - April, 2012	January - March, 2010

Note: Baseline surveys were conducted approximately one month before the start of each phase of construction. Given financial constraints, 5 out of 159 houses in El Salvador at Phase 2 were built in December 2008.

Table A.2
Description of variables and sample sizes. Follow-up survey.

Variable	Description	El Salvador		Uruguay		Mexico		All	
		Observations control	Observations treatment	Observations control	Observations treatment	Observations control	Observations treatment	Observations control	Observations treatment
Monthly income per capita (USD)	Monthly Income per capita in US dollars of July 2007. It is calculated as the sum of the monthly earnings of each household's member divided by the household size.	200	324	258	386	339	360	797	1070
Assets value per capita (USD)	Total Asset Value per capita reported by the head of household.	258	398	282	446	401	425	941	1269
Newborns (< 1)	Number of individuals below 1 year old by household.	258	398	282	446	401	425	941	1269
Newborns (< 2)	Number of individuals below 2 year old by household.	258	398	282	446	401	425	941	1269
Age	Age in years for all the individual.	1402	2215	1393	2320	2082	2231	4877	6766
Age in months	Age in months for children below 5 years old.	156	235	215	391	265	293	636	919
Head of HH's age	Age of head of household in years.	257	397	281	443	392	412	930	1252
Spouse's age	Age of the spouse of head of household in years.	180	292	174	250	291	314	645	856
Gender	Indicator equal to one if the individual is a man.	1407	2217	1397	2342	2111	2273	4915	6832
Head of HH's gender	Indicator equal to one if the head of household is a man.	258	397	282	446	401	425	941	1268
Years of schooling (6–12 years old)	Years of schooling if individual is between 6 and 12 years old.	214	366	286	472	367	430	867	1268
Years of schooling (13–18 years old)	Years of schooling if individual is between 13 and 18 years old.	226	337	176	315	273	327	675	979
Head of HH's years of schooling	Years of schooling of head of household equivalent to the higher level of education reached.	254	387	272	435	396	421	922	1243
Spouse's years of schooling	Years of schooling of the spouse or partner of head of household equivalent to the higher level of education reached.	178	287	168	242	293	321	639	850
Hours Worked last week by head of HH	Number of hours worked by the head of household at main and secondary job during the last week, conditioned on having worked during the last week.	160	265	240	388	299	320	699	973
Hours worked last week by spouse	Number of hours worked by the spouse or partner of head of household at main and secondary job during the last week, conditioned on having worked during the last week.	35	80	117	169	98	120	250	369
HH size	Number of individuals living in the house.	258	398	282	446	401	425	941	1269
Members per household (< 5)	Number of individuals below 5 years old living in the house.	258	398	282	446	401	425	941	1269
Members per household (6–12)	Number of individuals between 6 and 12 years old living in the house.	258	398	282	446	401	425	941	1269
Members per household (13–18)	Number of individuals between 13 and 18 years old living in the house.	258	398	282	446	401	425	941	1269
Members per household (> 18)	Number of individuals over 18 years old living in the house.	258	398	282	446	401	425	941	1269
Number of rooms	Number of rooms in the terrain (observed by the enumerator).	258	398	278	444	401	424	937	1266
Share of rooms with good quality floors	Proportion of rooms with floors made of good quality materials like cement, brick, or wood (observed by the enumerator).	258	398	278	444	401	424	937	1266
Share of rooms with good quality walls	Proportion of rooms with walls made of good quality materials like wood, cement, brick or zinc metal (observed by the enumerator).	258	398	282	446	397	424	937	1268
Share of rooms with good quality roofs	Proportion of rooms with roofs made of good quality materials like cement, brick, tile and zinc metal (observed by the enumerator).	258	398	279	444	401	424	938	1266
Share of rooms with windows	proportion of rooms with at least 1 window (observed by the enumerator).	258	398	282	446	400	424	940	1268
On-site water supply	Indicator equal to one if there is access to drinkable or not drinkable water in the terrain where the house is located (observed by the enumerator).	258	398	282	446	401	425	941	1269
House with own toilet	Indicator equal to one if there is a toilet inside or outside the house, but inside the terrain (observed by the enumerator).	258	398	282	446	401	425	941	1269

Table A.3
Description of variables and sample sizes. Follow-up survey (cont.).

Variable	Description	El Salvador		Uruguay		Mexico		All	
		Observations control	Observations treatment	Observations control	Observations treatment	Observations control	Observations treatment	Observations control	Observations treatment
Electricity connection inside the house	Indicator equal to one if there is a formal or informal connection to the electricity system inside the house (observed by the enumerator).	258	398	282	446	400	425	940	1269
Sink in room where food is prepared	Indicator equal to one if there is a sink inside the room where food is prepared (observed by the enumerator).	258	398	275	442	398	423	931	1263
Use gas stove or kerosene to cook	Indicator equal to one if the household reports the use of gas stove or kerosene to cook.	258	398	282	446	401	425	941	1269
Refrigerator	Indicator equal to one if the enumerator observes and the household reports having a refrigerator.	235	352	271	432	401	425	907	1209
T.V.	Indicator equal to one if the enumerator observes and the household reports having a television.	235	352	271	432	401	425	907	1209
Fan	Indicator equal to one if the enumerator observes and the household reports having a fan.	235	352	271	432	400	425	906	1209
Kitchen or gas stove	Indicator equal to one if the enumerator observes and the household reports having a kitchen or gas stove.	235	352	271	432	401	425	907	1209
Bicycle	Indicator equal to one if the enumerator observes and the household reports having a bicycle.	235	352	271	432	401	425	907	1209
Satisfaction with floor quality	Indicator equal to one if the respondent reports being satisfied or very satisfied with the quality of floors, measured by a Likert scale of 4 categories that goes from "unsitisfied" to "very satisfied".	258	398	277	441	401	424	936	1263
Satisfaction with wall quality	Indicator equal to one if the respondent reports being satisfied or very satisfied with the quality of walls, measured by a Likert scale of 4 categories that goes from "unsitisfied" to "very satisfied".	258	398	277	441	401	425	936	1264
Satisfaction with roof quality	Indicator equal to one if the respondent reports being satisfied or very satisfied with the quality of roofs, measured by a Likert scale of 4 categories that goes from "unsitisfied" to "very satisfied".	258	398	277	441	401	425	936	1264
Satisfaction with protection from water provided by house when it rains	Indicator equal to one if respondent reports being satisfied or very satisfied with the house's protection against water when it rains, measured by a Likert scale of 4 categories that goes from "unsitisfied" to "very satisfied".	258	398	277	441	401	425	936	1264
Satisfaction with quality of life	Indicator equal to one if respondent reports being satisfied or very satisfied with the quality of life of her family in that house, measured by a Likert scale of 4 categories that goes from "unsitisfied" to "very satisfied".	154	367	276	439	400	422	830	1228
Feel safe inside the house during the last 12 months	Indicator equal to one if respondent has never or rarely felt unsafe inside the house during the last 12 months, measured by a Likert scale of 5 categories that goes from "never unsafe" to "always unsafe".	258	398	282	446	401	425	941	1269
Feel safe leaving the house alone during the last 12 months	Indicator equal to one if respondent has never or rarely felt unsafe leaving the house alone during the last 12 months.	258	398	282	446	401	425	941	1269
Feel safe leaving the kids alone in the house during the last 12 months	Indicator equal to one if respondent feels safe or very safe leaving the kids alone in the house during the last 12 months, measured by a Likert scale of 5 categories that goes from "never unsafe" to "always unsafe".	258	398	282	446	401	425	941	1269
House has been robbed in the last 12 months	Indicator equal to one if respondent reports the house has been robbed during the last 12 months.	258	398	276	441	400	425	934	1264
Respiratory disease during last 4 weeks	Indicator equal to one if the mother reports that a child below 5 years old had a respiratory disease in the last four weeks.	155	229	211	374	259	283	625	886
Diarrhea episodes during last 4 weeks	Indicator equal to one if the mother reports that a child below 5 years old had diarrhea in the last four weeks.	155	229	209	374	259	277	623	880

Table A.4
Description of variables and sample sizes. Follow-up survey (cont.).

Variable	Description	El Salvador		Uruguay		Mexico		All	
		Observations control	Observations treatment	Observations control	Observations treatment	Observations control	Observations treatment	Observations control	Observations treatment
Housing investment summary index (z-score)	Equally weighted average of z-scores of Sink on Room where food is prepared, Room where food is prepared is also used as Bedroom, Water in Terrain, Electricity Connection inside the House, Use Gas Stove or Kerosene to Cook, and House with Own Toilet.	258	398	274	446	401	425	933	1269
Satisfaction summary index (z-score)	Equally weighted average of z-scores of Satisfaction with Floor Quality, Satisfaction with Wall Quality, Satisfaction with Roof Quality, Satisfaction with House Protection against Water when it rains, and Satisfaction of Quality of Life.	258	398	282	446	401	425	941	1269
Perception of security summary index (z-score)	Equally weighted average of z-scores of Safe inside the house during the last 12 months, Safe leaving the house alone during the last 12 months, Safe leaving the kids alone in the house during the last 12 months, and The house had been robbed in the last 12 months.	258	398	276	446	401	425	935	1269
Assets summary Index (z-score)	Equally weighted average of z-scores of Television, Fun, Kitchen or Gas Stove, Refrigerator, and Bicycle.	258	398	282	446	401	425	941	1269
Economic summary Index (z-score)	Equally weighted average of z-scores of Monthly Income Per Capita (USD), Hours worked last week by Head of HH, and Hours worked last week by Spouse.	258	398	282	446	401	425	941	1269
Demographic summary Index (z-score)	Equally weighted average of z-scores of HHSize, Newborns(< 1), and Newborns(< 2).	258	398	282	446	401	425	941	1269
Health summary Index (z-score)	Equally weighted average of z-scores of Respiratory Disease during last 4 weeks and Diarrhea during last 4 weeks.	155	229	208	374	259	283	622	886

Table A.5
General information. Intention-to-treat groups.

	El Salvador			Uruguay			Mexico			All		
	Observations treatment	Observations control	Mean differences	Observations treatment	Observations control	Mean differences	Observations treatment	Observations control	Mean differences	Observations treatment	Observations control	Mean differences
Number of households	421	277		478	301		457	439		1356	1017	
	60.3%	39.7%		61.4%	38.6%		51.0%	49.0%		57.1%	42.9%	
Number of individuals	2111	1363		2067	1259		2239	2152		6417	4774	
	60.8%	39.2%		62.2%	37.8%		51.0%	49.0%		57.3%	42.7%	
Attriters: number of households	23	19		32	19		32	38		87	76	
Attrition Rate	0.055	0.069	−0.014	0.067	0.063	0.004	0.070	0.087	−0.017	0.064	0.075	−0.011
	(0.011)	(0.015)	(0.018)	(0.011)	(0.014)	(0.018)	(0.011)	(0.013)	(0.017)	(0.006)	(0.008)	(0.010)
Number of households - Follow-up Sample	398	258		446	282		425	401		1269	941	
Phase I	221	67		224	129		166	120		611	316	
Phase II	177	191		222	153		259	281		658	625	
Number of individuals - Follow up Sample	2217	1407		2342	1397		2273	2111		6832	4915	
Compliers: number of households	349	257		383	280		368	401		1100	938	
	87.7%	99.6%		85.9%	99.3%		86.6%	100.0%		86.7%	99.7%	
Non compliance rate	0.123	0.004	0.119	0.141	0.007	0.134	0.134	0.000	0.134	0.133	0.003	0.130
	(0.016)	(0.003)	(0.016)***	(0.016)	(0.005)	(0.017)***	(0.016)	(0.000)	(0.016)***	(0.009)	(0.001)	(0.009)***
Movers: number of households	20	16		36	25		22	22		78	63	
	4.8%	5.8%		7.5%	8.3%		4.8%	5.0%		5.8%	6.2%	
Movers rate	0.048	0.058	−0.010	0.075	0.083	−0.008	0.048	0.050	−0.002	0.058	0.062	−0.004
	(0.010)	(0.014)	(0.017)	(0.012)	(0.015)	(0.019)	(0.010)	(0.010)	(0.014)	(0.006)	(0.007)	(0.009)

Note: The term "movers" refers to households whose members moved out of the original slum between the times that the baseline and the follow-up surveys were conducted. Some of these people were located and responded to the follow–up survey; those who were not located have been classified as attriters. *Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Table A.6
Differences in pre–treatment means. Intention-to-treat groups. Baseline survey.

Variables	El Salvador			Uruguay			Mexico			All		
	Mean treatment	Mean control	Mean differences	Mean treatment	Mean control	Mean differences	Mean treatment	Mean control	Mean differences	Mean treatment	Mean control	Mean differences
Income and assets												
Assets value per capita (US$)	45.397	53.578	6.059	45.369	47.694	−1.599	48.772	50.265	1.048	45.177	48.745	−0.311
	(5.539)	(8.126)	(11.900)	(3.558)	(4.677)	(6.452)	(4.527)	(4.111)	(6.104)	(2.365)	(2.764)	(3.911)
Monthly income per capita (US$)	29.940	30.463	−1.713	64.899	77.871	−15.626	56.281	67.969	−6.209	51.210	59.118	−6.453
	(1.413)	(1.893)	(2.855)	(4.179)	(6.834)	(9.275)*	(2.965)	(3.664)	(4.744)	(1.826)	(2.425)	(3.521)*
T.V.	0.453	0.412	−0.028	0.844	0.825	0.019	0.604	0.677	−0.039	0.643	0.651	−0.017
	(0.025)	(0.030)	(0.044)	(0.016)	(0.022)	(0.029)	(0.022)	(0.022)	(0.031)	(0.013)	(0.015)	(0.019)
Fan	0.043	0.050	0.004	0.291	0.264	0.037	0.033	0.023	0.005	0.127	0.101	0.016
	(0.010)	(0.013)	(0.022)	(0.021)	(0.025)	(0.034)	(0.008)	(0.007)	(0.010)	(0.009)	(0.009)	(0.013)
Kitchen or gas stove	0.455	0.527	−0.030	0.651	0.664	0.022	0.418	0.474	−0.027	0.511	0.544	−0.012
	(0.025)	(0.030)	(0.044)	(0.022)	(0.027)	(0.036)	(0.023)	(0.023)	(0.029)	(0.013)	(0.015)	(0.020)
Refrigerator	0.059	0.099	−0.018	0.495	0.510	0.011	0.204	0.187	0.014	0.263	0.259	0.006
	(0.011)	(0.018)	(0.026)	(0.023)	(0.029)	(0.039)	(0.018)	(0.018)	(0.024)	(0.012)	(0.013)	(0.018)
Bicycle	0.335	0.359	−0.014	0.453	0.462	−0.011	0.269	0.269	0.010	0.354	0.349	−0.003
	(0.023)	(0.029)	(0.041)	(0.023)	(0.029)	(0.039)	(0.020)	(0.021)	(0.029)	(0.013)	(0.015)	0.020)
Characteristics of the house												
Number of rooms	2.488	2.354	−0.146	2.912	2.837	0.105	2.803	2.825	−0.023	2.743	2.700	−0.010
	(0.056)	(0.069)	(0.095)	(0.068)	(0.087)	(0.117)	(0.061)	(0.059)	(0.085)	(0.036)	(0.041)	(0.058)
Share of rooms with good quality floors	0.145	0.142	−0.038	0.371	0.374	−0.020	0.661	0.636	0.012	0.398	0.423	−0.011
	(0.011)	(0.014)	(0.021)*	(0.020)	(0.025)	(0.033)	(0.017)	(0.018)	(0.024)	(0.011)	(0.013)	(0.016)
Share of rooms with good quality walls	0.110	0.107	−0.021	0.248	0.217	0.022	0.259	0.237	0.022	0.204	0.193	0.010
	(0.010)	(0.012)	(0.018)	(0.021)	(0.026)	(0.035)	(0.017)	(0.016)	(0.021)	(0.009)	(0.010)	(0.014)
Share of rooms with good quality roofs	0.101	0.149	−0.016	0.348	0.353	−0.023	0.502	0.468	−0.013	0.322	0.347	−0.017
	(0.012)	(0.019)	(0.023)	(0.019)	(0.025)	(0.033)	(0.019)	(0.019)	(0.027)	(0.011)	(0.013)	(0.016)
Share of rooms with windows	0.154	0.184	0.002	0.561	0.586	−0.026	0.294	0.253	0.015	0.345	0.333	−0.002
	(0.012)	(0.018)	(0.024)	(0.017)	(0.022)	(0.029)	(0.016)	(0.015)	(0.022)	(0.010)	(0.011)	(0.014)
On-site water supply	0.228	0.195	−0.033	0.916	0.907	0.016	0.501	0.519	0.015	0.563	0.546	0.004
	(0.020)	(0.023)	(0.030)	(0.012)	(0.016)	(0.021)	(0.023)	(0.023)	(0.028)	(0.013)	(0.015)	(0.015)
Sink in room where food is prepared	0.014	0.007	0.002	0.269	0.231	0.047	0.013	0.025	−0.011	0.103	0.081	0.012
	(0.005)	(0.005)	(0.010)	(0.020)	(0.024)	(0.033)	(0.005)	(0.007)	(0.009)	(0.008)	(0.008)	(0.012)
Electricity connection inside the house	0.394	0.386	−0.063	0.962	0.953	0.008	0.807	0.870	−0.041	0.734	0.763	−0.030
	(0.023)	(0.029)	(0.038)	(0.008)	(0.012)	(0.016)	(0.018)	(0.016)	(0.023)*	(0.012)	(0.013)	(0.014)**
Use gas or kerosene stove to cook	0.195	0.141	0.010	0.439	0.475	−0.017	0.276	0.280	−0.008	0.308	0.300	−0.007
	(0.019)	(0.020)	(0.030)	(0.022)	(0.028)	(0.037)	(0.020)	(0.021)	(0.023)	(0.012)	(0.014)	(0.017)
House with own toilet	0.506	0.448	−0.056	0.657	0.598	0.062	0.403	0.392	−0.011	0.524	0.468	0.003
	(0.024)	(0.029)	(0.042)	(0.021)	(0.028)	(0.036)*	(0.022)	(0.023)	(0.031)	(0.013)	(0.015)	(0.020)

Note: All the regressions control for settlement fixed effects. Responses regarding construction materials used in rooms were included only for those households that reported information for all rooms. In the case of monetary variables, observations over the 99th percentile were excluded. Robust standard errors are reported in parenthesis. *Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Table A.7
Differences in pre-treatment means. Intention-to-treat groups. Baseline survey (cont.).

Variables	El Salvador			Uruguay			Mexico			All		
	Mean treatment	Mean control	Mean differences	Mean treatment	Mean control	Mean differences	Mean treatment	Mean control	Mean differences	Mean treatment	Mean control	Mean differences
Satisfaction with quality of house and life												
Satisfaction with floor quality	0.133	0.116	0.018	0.164	0.196	-0.020	0.375	0.377	0.036	0.225	0.252	0.013
	(0.016)	(0.019)	(0.027)	(0.016)	(0.022)	(0.030)	(0.022)	(0.023)	(0.030)	(0.011)	(0.013)	(0.017)
Satisfaction with wall quality	0.095	0.083	0.004	0.117	0.130	-0.012	0.255	0.249	0.030	0.157	0.169	0.010
	(0.014)	(0.016)	(0.025)	(0.014)	(0.019)	(0.026)	(0.020)	(0.020)	(0.029)	(0.009)	(0.011)	(0.016)
Satisfaction with roof quality	0.117	0.091	0.008	0.176	0.157	0.000	0.212	0.229	0.002	0.163	0.176	0.003
	(0.015)	(0.017)	(0.026)	(0.021)	(0.016)	(0.028)	(0.019)	(0.020)	(0.028)	(0.010)	(0.011)	(0.016)
Satisfaction with house protection	0.103	0.090	–0.005	0.159	0.180	–0.006	0.190	0.176	0.038	0.152	0.154	0.013
against water when it rains	(0.014)	(0.017)	(0.025)	(0.016)	(0.022)	(0.029)	(0.018)	(0.018)	(0.025)	(0.009)	(0.011)	(0.016)
Satisfaction with quality of life	0.266	0.181	0.025	0.219	0.229	–0.020	0.354	0.339	0.036	0.279	0.263	0.015
	(0.021)	(0.023)	(0.033)	(0.019)	(0.024)	(0.032)	(0.022)	(0.022)	(0.032)	(0.012)	(0.013)	(0.019)
Perception of security												
Feel safe inside the house during	0.527	0.538	–0.045	0.615	0.595	0.029	0.713	0.708	0.013	0.621	0.628	0.004
the last 12 months	(0.024)	(0.030)	(0.043)	(0.022)	(0.028)	(0.037)	(0.021)	(0.021)	(0.031)	(0.013)	(0.015)	(0.020)
Feel safe leaving the house alone	0.435	0.419	–0.011	0.328	0.272	0.061	0.615	0.597	0.031	0.458	0.452	0.031
during the last 12 months	(0.024)	(0.029)	(0.043)	(0.021)	(0.025)	(0.035)*	(0.022)	(0.023)	(0.032)	(0.013)	(0.015)	(0.020)
Feel safe leaving the kids alone in the	0.147	0.166	–0.049	0.144	0.126	0.011	0.166	0.191	–0.034	0.153	0.165	–0.023
house during the last 12 months	(0.017)	(0.022)	(0.032)	(0.016)	(0.019)	(0.025)	(0.017)	(0.018)	(0.026)	(0.009)	(0.011)	(0.016)
House has been robbed in the last 12 months	0.079	0.036	0.053	0.273	0.283	–0.030	0.059	0.055	0.008	0.141	0.117	0.006
Last 12 months	(0.013)	(0.011)	(0.020)**	(0.020)	(0.026)	(0.033)	(0.011)	(0.010)	(0.015)	(0.009)	(0.010)	(0.013)
Sociodemographic characteristics												
HH size	5.014	4.921	–0.040	4.324	4.183	0.109	4.899	4.902	–0.099	4.732	4.694	–0.015
	(0.124)	(0.140)	(0.233)	(0.113)	(0.134)	(0.189)	(0.113)	(0.117)	(0.159)	(0.068)	(0.075)	(0.108)
Newborns (<1 year old)	0.114	0.123	–0.013	0.178	0.150	0.010	0.118	0.153	–0.040	0.138	0.144	–0.017
	(0.016)	(0.021)	(0.030)	(0.018)	(0.021)	(0.029)	(0.015)	(0.017)	(0.024)	(0.009)	(0.011)	(0.016)
Newborns (<2 years old)	0.214	0.220	–0.025	0.343	0.312	0.007	0.284	0.276	–0.008	0.283	0.271	–0.007
	(0.021)	(0.026)	(0.037)	(0.025)	(0.030)	(0.041)	(0.022)	(0.024)	(0.034)	(0.013)	(0.015)	(0.022)
Members per household	0.622	0.606	0.016	0.828	0.794	–0.007	0.622	0.606	–0.086	0.769	0.782	–0.035
(< 5 years old)	(0.036)	(0.046)	(0.068)	(0.044)	(0.055)	(0.074)	(0.036)	(0.046)	(0.063)	(0.024)	(0.028)	(0.040)
Members per household	1.043	0.993	–0.059	0.831	0.731	0.137	1.043	0.993	0.026	0.965	0.905	0.043
(6–12 years old)	(0.054)	(0.064)	(0.096)	(0.048)	(0.055)	(0.077)*	(0.054)	(0.064)	(0.074)	(0.030)	(0.033)	(0.047)
Members per household	0.660	0.675	–0.023	0.542	0.455	0.093	0.660	0.675	–0.013	0.650	0.636	0.020
(13–18 years old)	(0.044)	(0.051)	(0.080)	(0.038)	(0.046)	(0.064)	(0.044)	(0.051)	(0.065)	(0.024)	(0.028)	(0.040)
Members per household	2.437	2.350	0.076	1.856	1.947	–0.114	2.437	2.350	–0.029	2.172	2.213	–0.032
(> 18 years old)	(0.057)	(0.065)	(0.111)	(0.037)	(0.050)	(0.068)*	(0.057)	(0.065)	(0.075)	(0.029)	(0.032)	(0.047)
Head of HH's age	45.038	44.227	0.129	38.723	37.270	1.827	41.518	41.379	0.426	41.627	40.935	0.824
	(0.819)	(1.013)	(1.555)	(0.649)	(0.806)	(1.089)*	(0.747)	(0.697)	(0.999)	(0.430)	(0.479)	(0.673)
Head of HH's gender	0.798	0.769	0.028	0.498	0.545	–0.046	0.788	0.770	0.018	0.689	0.703	–0.001
	(0.019)	(0.025)	(0.036)	(0.022)	(0.028)	(0.038)	(0.019)	(0.020)	(0.028)	(0.012)	(0.014)	(0.019)
Head of HH's years of schooling	2.514	2.326	–0.053	5.828	5.877	0.121	4.144	3.850	0.305	4.237	4.026	0.157
	(0.147)	(0.170)	(0.245)	(0.135)	(0.183)	(0.237)	(0.151)	(0.151)	(0.203)	(0.091)	(0.105)	(0.131)
Spouse's age	38.909	37.900	0.274	33.623	33.036	0.595	37.110	37.731	0.065	36.727	36.514	0.270
	(0.852)	(1.047)	(1.609)	(0.754)	(0.927)	(1.263)	(0.744)	(0.757)	(1.045)	(0.460)	(0.519)	(0.725)
Spouse's years of schooling	2.210	1.921	0.127	6.023	6.229	–0.185	4.120	4.274	–0.320	4.019	4.161	–0.170
	(0.166)	(0.180)	(0.265)	(0.179)	(0.225)	(0.304)	(0.178)	(0.177)	(0.237)	(0.112)	(0.127)	(0.155)
Hours worked last week	41.278	40.963	1.373	38.610	40.258	–1.744	40.924	40.785	0.606	40.182	40.662	–0.046
by head of HH	(1.230)	(1.461)	(2.306)	(1.113)	(1.437)	(1.910)	(1.150)	(1.140)	(1.623)	(0.671)	(0.764)	(1.092)
Hours worked last week	34.261	26.340	4.137	37.159	37.438	0.267	28.122	28.113	–2.283	33.370	31.377	–0.250
by spouse	(2.872)	(3.035)	(4.392)	(1.845)	(1.775)	(2.759)	(1.864)	(1.865)	(2.699)	(1.225)	(1.225)	(1.786)
Years of schooling	1.594	1.601	–0.090	1.900	2.012	–0.044	2.494	2.401	0.055	1.999	2.053	–0.013
(6–12 years old)	(0.076)	(0.096)	(0.145)	(0.077)	(0.104)	(0.140)	(0.087)	(0.090)	(1.678)	(0.047)	(0.057)	(0.080)
Years of schooling	5.248	5.049	–0.134	5.373	5.535	–0.101	6.627	7.038	–0.366	5.795	6.088	–0.228
(13–18 years old)	(0.145)	(0.183)	(0.268)	(0.113)	(0.152)	(0.197)	(0.116)	(0.122)	(0.171)**	(0.076)	(0.093)	(0.118)*
Health (< 5 years old)												
Respiratory disease during last 4 weeks	0.669	0.635	0.042	0.351	0.352	–0.018	0.376	0.401	–0.022	0.444	0.439	–0.007
	(0.029)	(0.037)	(0.056)	(0.024)	(0.031)	(0.042)	(0.027)	(0.027)	(0.040)	(0.016)	(0.018)	(0.025)
Diarrhea episode during last 4 weeks	0.249	0.144	0.043	0.087	0.089	–0.018	0.131	0.138	–0.011	0.145	0.123	–0.002
	(0.027)	(0.027)	(0.042)	(0.014)	(0.018)	(0.024)	(0.018)	(0.019)	(0.028)	(0.011)	(0.012)	(0.017)

Note: All the regressions control for settlement fixed effects. Robust standard errors are reported in parenthesis. *Significant at 10% level. **Significant at 5% level. ***Significant at 1% level.

Table A.8
Differences in Pre–Treatment Means Across Countries. Baseline Survey.

Variables	Mean El Salvador (1)	Mean Uruguay (2)	Mean Mexico (3)	Mean differences (1) - (2)	Mean differences (1) - (3)	Mean differences (2) - (3)
Characteristics of the house						
Number of rooms	2.435	2.883	2.814	−0.448	−0.379	0.069
	(0.087)	(0.079)	(0.065)	(0.116)***	(0.108)***	(0.101)
Share of rooms with good quality floors	0.144	0.372	0.649	−0.228	−0.505	−0.276
	(0.014)	(0.030)	(0.027)	(0.033)***	(0.031)***	(0.040)***
Share of rooms with good quality walls	0.109	0.236	0.248	−0.127	−0.140	−0.012
	(0.013)	(0.033)	(0.031)	(0.035)***	(0.034)***	(0.045)
Share of rooms with good quality roofs	0.120	0.350	0.485	−0.230	−0.365	−0.135
	(0.034)	(0.024)	(0.031)	(0.041)***	(0.046)***	(0.039)***
Share of rooms with windows	0.166	0.571	0.273	−0.405	−0.107	0.298
	(0.017)	(0.016)	(0.025)	(0.023)***	(0.030)***	(0.029)***
On-site water supply	0.215	0.913	0.510	−0.700	−0.295	0.403
	(0.051)	(0.014)	(0.052)	(0.053)***	(0.072)***	(0.054)***
Sink in room where food is prepared	0.012	0.254	0.019	−0.242	−0.008	0.235
	(0.005)	(0.025)	(0.004)	(0.024)***	(0.007)	(0.024)***
Electricity connection inside the house	0.391	0.959	0.838	−0.568	−0.447	0.121
	(0.058)	(0.006)	(0.031)	(0.058)***	(0.065)***	(0.031)***
Use gas or kerosene stove to cook	0.173	0.453	0.278	−0.280	−0.105	0.175
	(0.034)	(0.052)	(0.057)	(0.061)***	(0.066)	(0.076)**
House with own bathroom	0.483	0.634	0.397	−0.151	0.085	0.237
	(0.041)	(0.024)	(0.035)	(0.047)***	(0.054)	(0.042)***

Note: Responses regarding construction materials used in rooms were included only for those households that reported information for all rooms. Standard errors clustered at cluster level shown in parentheses. **Significant at 5% level. ***Significant at 1% level.

Table A.9
Differences in means for non–slum poors and slum dwellers in El Salvador.

Variable	Mean national poor (EHPM 2008) (1)	Mean settlements (UTPMP 2007–08) (2)	Difference (1) - (2)	Difference (1) - (2) (including zone dummy)
Income indicator				
Monthly income per capita (US$)	37.293	30.146	7.147	2.844
	(0.622)	(1.777)	(1.896)***	(2.173)
Employment indicators				
Employment rate: 16–64	0.540	0.510	0.030	0.019
	(0.006)	(0.018)	(0.019)	(0.019)
Employment rate: Males 16–64	0.352	0.368	−0.015	0.000
	(0.006)	(0.014)	(0.016)	(0.018)
Employment rate: Females 16–64	0.188	0.143	0.046	0.018
	(0.006)	(0.014)	(0.016)***	(0.016)
Wage employment rate: 16–64	0.328	0.195	0.134	0.122
	(0.007)	(0.016)	(0.018)***	(0.017)***
Wage employment rate: Males 16–64	0.234	0.172	0.061	0.065
	(0.006)	(0.014)	(0.015)***	(0.015)***
Wage employment rate: Females 16–64	0.095	0.022	0.073	0.058
	(0.003)	(0.005)	(0.007)***	(0.006)***
Self-employment rate: 16–64	0.212	0.313	−0.100	−0.101
	(0.006)	(0.020)	(0.021)***	(0.021)***
Self-employment rate: Males 16–64	0.119	0.192	−0.074	−0.061
	(0.005)	(0.022)	(0.023)***	(0.024)**
Self-employment rate: Females 16–64	0.094	0.121	−0.027	−0.040
	(0.004)	(0.010)	(0.012)**	(0.012)***
Average wage (US$): 16–64 Males	132.607	87.041	45.565	35.581
	(2.206)	(5.850)	(6.167)***	(5.356)***
Average wage (US$): 16–64 Females	111.619	84.060	27.560	18.781
	(2.216)	(5.105)	(5.514)***	(6.059)***

Note: For National Poor, figures computed at household and individual levels using the 2008 multi–purpose household survey for all provinces (known as "departments") in which there are UTPMP households (excludes San Salvador Department). For settlements, figures computed at household and individual levels using UTPMP impact evaluation baseline data sources. Standard errors clustered at the primary sample unit level are shown in parentheses. The term "national poor "refers to households whose members were living on less than USD 89.4 per capita per month in urban zones and less than USD 58.2 per capita per month in rural zones in 2008; these figures are equivalent to two basic baskets for urban and rural areas, which represent the national poverty line and basic needs in El Salvador as of 2008. Since price levels in urban and rural zones in El Salvador differ, the last column tests the hypothesis of equal means by controlling for a dummy variable that is equal to 1 if the household is located in a rural zone. In the case of monetary variables, figures are US dollars of December 2008 and observations over the 99th percentile were excluded. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A.10
Differences in means for non–slum poors and slum dwellers in El Salvador (cont.).

Variable	Mean national poor (EHPM 2008) (1)	Mean settlements (UTPMP 2007–08) (2)	Difference (1) - (2)	Difference (1) - (2) (including zone dummy)
Demographics indicators				
HH size	4.669	4.977	−0.308	−0.181
	(0.052)	(0.129)	(0.132)**	(0.138)
Female head	0.288	0.213	0.075	0.047
	(0.009)	(0.015)	(0.018)***	(0.020)**
Head of HH's age	46.904	44.717	2.187	1.783
	(0.383)	(0.927)	(1.019)**	(0.989)*
Head of HH's years of schooling	3.693	2.438	1.255	0.825
	(0.086)	(0.184)	(0.198)***	(0.161)***
Children 5–12 years old enrolled in school	0.827	0.931	−0.104	−0.120
	(0.009)	(0.013)	(0.016)***	(0.017)***
Children 13–18 years old enrolled in school	0.622	0.578	0.044	0.010
	(0.015)	(0.037)	(0.041)	(0.040)
Housing and assets indicators				
Dorms per capita	0.507	0.126	0.381	0.343
	(0.009)	(0.012)	(0.015)***	(0.019)***
Share of rooms with good quality floors	0.606	0.144	0.462	0.385
	(0.014)	(0.014)	(0.019)***	(0.029)***
On-site water supply	0.553	0.215	0.339	0.249
	(0.017)	(0.051)	(0.051)***	(0.042)***
House with own toilet	0.781	0.483	0.298	0.279
	(0.010)	(0.041)	(0.042)***	(0.040)***
Connected to sewerage service	0.534	0.009	0.525	0.382
	(0.034)	(0.004)	(0.033)***	(0.064)***
Electricity connection inside the house	0.805	0.391	0.414	0.352
	(0.011)	(0.058)	(0.060)***	(0.051)***
Refrigerator	0.331	0.075	0.256	0.199
	(0.012)	(0.019)	(0.023)***	(0.032)***
T.V.	0.666	0.436	0.230	0.168
	(0.014)	(0.037)	(0.039)***	(0.030)***

Note: For National Poor, figures computed at household and individual levels using the 2008 multi–purpose household survey for all provinces (known as "departments") in which there are UTPMP households (excludes San Salvador Department). For settlements, figures computed at household and individual levels using UTPMP impact evaluation baseline data sources. Standard errors clustered at the primary sample unit level are shown in parentheses. The term "national poor "refers to households whose members were living on less than USD 89.4 per capita per month in urban zones and less than USD 58.2 per capita per month in rural zones in 2008; these figures are equivalent to two basic baskets for urban and rural areas, which represent the national poverty line and basic needs in El Salvador as of 2008. Since price levels in urban and rural zones in El Salvador differ, the last column tests the hypothesis of equal means by controlling for a dummy variable that is equal to 1 if the household is located in a rural zone. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A.11
Differences in means for non–slum poors and slum dwellers in Uruguay.

Variable	Mean non-slum poors (ECH 2008 National Survey) (1)	Mean settlements (ECH 2008 National Survey) (2)	Difference (1)-(2)
Income indicators			
Monthly income per capita (US$)	77.561	132.936	−55.376
	(0.627)	(3.475)	(3.364)***
Employment indicators			
Employment rate: 16–64	0.584	0.647	−0.063
	(0.004)	(0.007)	(0.007)***
Employment rate: Males 16–64	0.337	0.388	−0.051
	(0.009)	(0.006)	(0.010)***
Employment rate: Females 16–64	0.247	0.260	−0.012
	(0.011)	(0.006)	(0.011)
Wage employment rate: 16–64	0.404	0.467	−0.063
	(0.005)	(0.008)	(0.009)***
Wage employment rate: Males 16–64	0.225	0.271	−0.046
	(0.008)	(0.007)	(0.009)***
Wage employment rate: Females 16–64	0.178	0.196	−0.017
	(0.010)	(0.006)	(0.012)
Self-employment rate: 16–64	0.181	0.180	0.000
	(0.003)	(0.007)	(0.008)
Self-employment rate: Males 16–64	0.112	0.116	−0.005
	(0.003)	(0.004)	(0.005)
Self-employment rate: Females 16–64	0.069	0.064	0.005
	(0.002)	(0.004)	(0.005)
Average wage (US$): Males 16–64	187.336	260.234	−72.899
	(6.969)	(5.858)	(9.489)***
Average wage (US$): Females 16–64	74.283	108.738	−34.455
	(2.086)	(4.156)	(3.657)***

Note: Figures computed at household and individual levels in Montevideo and Canelones provinces (known as "departments") in Uruguay using the 2008 continuous household survey (ECH). Standard errors clustered at the primary sample unit level are shown in parentheses. The term "national poor" refers to households whose members are below the national poverty line in urban zones in Uruguay. This line is calculated monthly; in 2008, it ranged between USD 213 and USD 234 per capita per month. The poverty line represents a basic basket of "staple food needs" plus a basic basket of "non-food needs". In the case of monetary variables, figures are US dollars of December 2008 and observations over the 99th percentile were excluded. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A.12
Differences in means for non–slum poors and slum dwellers in Uruguay (cont.).

Variable	Mean non-slum poors (ECH 2008 National Survey) (1)	Mean settlements (ECH 2008 National Survey) (2)	Difference (1)-(2)
Demographics			
HH size	4.274	3.691	0.584
	(0.091)	(0.053)	(0.118)***
Female head	0.378	0.372	0.005
	(0.038)	(0.013)	(0.039)
Head of HH's age	45.311	45.423	−0.112
	(0.213)	(0.352)	(0.395)
Head of HH's years of schooling	6.351	6.169	0.182
	(0.190)	(0.099)	(0.140)
Children 5–12 enrolled in school	0.980	0.978	0.002
	(0.002)	(0.003)	(0.004)
Children 13–18 enrolled in school	0.707	0.661	0.046
	(0.011)	(0.019)	(0.024)*
Housing and assets			
Rooms per capita	0.836	0.977	−0.141
	(0.024)	(0.020)	(0.039)***
Share of rooms with good quality floors	0.758	0.596	0.162
	(0.010)	(0.017)	(0.016)***
On-site water supply	0.864	0.989	−0.125
	(0.061)	(0.004)	(0.057)**
House with own toilet	0.922	0.895	0.027
	(0.006)	(0.009)	(0.012)**
Connected to sewerage service	0.543	0.604	−0.061
	(0.033)	(0.023)	(0.025)**
Electricity connection inside the house	0.988	0.996	−0.008
	(0.003)	(0.001)	(0.003)**
Refrigerator	0.886	0.860	0.027
	(0.006)	(0.011)	(0.011)**
T.V.	0.939	0.919	0.020
	(0.007)	(0.008)	(0.009)**

Note: Figures computed at household and individual levels in Montevideo and Canelones provinces (known as "departments") in Uruguay using the 2008 continuous household survey (ECH). Standard errors clustered at the primary sample unit level are shown in parentheses. The term "national poor" refers to households whose members are below the national poverty line in urban zones in Uruguay. This line is calculated monthly; in 2008, it ranged between USD 213 and USD 234 per capita per month. The poverty line represents a basic basket of "staple food needs" plus a basic basket of "non-food needs". *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A.13
Differences in means for non–slum poors and slum dwellers in Mexico.

Variable	Mean national poor (ENIGH 2010) (1)	Mean settlements (UTPMP 2010–11) (2)	Difference (1) - (2)	Difference (1) - (2) (including zone dummy)
Income indicators				
Monthly income per capita (US$)	86.274	107.674	−21.399	−34.770
	(1.629)	(6.073)	(6.218)***	(9.504)***
Employment indicators				
Employment rate: 16–64	0.877	0.563	0.315	0.278
	(0.010)	(0.009)	(0.014)***	(0.017)***
Employment rate: Males 16–64	0.529	0.406	0.124	0.104
	(0.015)	(0.007)	(0.017)***	(0.026)**
Employment rate: Females 16–64	0.348	0.157	0.191	0.174
	(0.013)	(0.008)	(0.016)***	(0.022)***
Wage employment rate: 16–64	0.621	0.509	0.113	0.064
	(0.020)	(0.011)	(0.023)***	(0.037)*
Wage employment rate: Males 16–64	0.387	0.378	0.009	−0.012
	(0.014)	(0.010)	(0.017)	(0.023)
Wage employment rate: Females 16–64	0.234	0.130	0.104	0.075
	(0.013)	(0.007)	(0.015)***	(0.021)***
Self-employment rate: 16–64	0.252	0.049	0.203	0.214
	(0.016)	(0.008)	(0.018)***	(0.028)***
Self-employment rate: Males 16–64	0.140	0.024	0.116	0.116
	(0.010)	(0.005)	(0.011)***	(0.013)***
Self-employment rate: Females 16–64	0.112	0.025	0.087	0.098
	(0.015)	(0.004)	(0.015)***	(0.031)***
Average wage (US$): Males 16–64	237.071	252.964	−15.893	−30.158
	(4.699)	(7.439)	(8.725)*	(8.264)***
Average wage (US$): Females 16–64	152.216	253.512	−101.295	−110.316
	(4.922)	(20.365)	(20.726)***	(36.068)***

Note: For National Poor, figures computed at household and individual levels in Estado de Mexico, Mexico, using the 2010 national household income and expenditure survey (ENIGH). For settlements, figures computed at household and individual levels using UTPMP impact evaluation baseline data sources (including non-eligible UTPMP households). Standard errors clustered at the primary sample unit level are shown in parentheses. The term "national poor" refers to households whose members were living on less than USD 167.67 per capita per month in urban zones and less than USD 107.29 in rural zones between August and November 2010; these figures are equivalent to two basic baskets, which represent the national poverty line and basic needs in Mexico as of 2010. Since price levels in urban and rural zones in Mexico differ, the last column tests the hypothesis of equal means by controlling for a dummy variable that is equal to 1 if the household is located in a rural zone. In the case of monetary variables, observations over the 99th percentile were excluded. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A.14
Differences in means for non–slum poors and slum dwellers in Mexico (cont.) .

Variable	Mean national poor (ENIGH 2010) (1)	Mean settlements (UTPMP 2010–11) (2)	Difference (1) - (2)	Difference (1) - (2) (including zone dummy)
Demographics				
HH size	4.658	4.721	−0.063	0.013
	(0.074)	(0.148)	(0.164)	(0.182)
Female head	0.208	0.201	0.006	0.017
	(0.012)	(0.014)	(0.018)	(0.023)
Head of HH's age	46.130	43.537	2.592	2.580
	(0.512)	(0.711)	(0.870)***	(1.159)**
Head of HH's years of schooling	6.897	5.214	1.682	1.134
	(0.165)	(0.227)	(0.279)***	(0.431)***
Children 5–12 enrolled in school	0.980	0.966	0.015	0.005
	(0.006)	(0.007)	(0.009)	(0.014)
Children 13–18 enrolled in school	0.632	0.430	0.202	0.148
	(0.025)	(0.030)	(0.039)***	(0.061)**
Housing and assets				
Rooms per capita	0.921	0.854	0.067	0.034
	(0.022)	(0.023)	(0.032)**	(0.045)
Share of rooms with good quality floors	0.959	0.738	0.220	0.227
	(0.006)	(0.019)	(0.020)***	(0.034)***
On-site water supply	0.926	0.574	0.353	0.331
	(0.014)	(0.050)	(0.051)***	(0.098)***
House with own toilet	0.835	0.481	0.354	0.310
	(0.012)	(0.032)	(0.034)***	(0.044)***
Connected to sewerage service	0.903	0.311	0.592	0.450
	(0.018)	(0.048)	(0.051)***	(0.057)***
Electricity connection inside the house	0.988	0.885	0.103	0.071
	(0.003)	(0.022)	(0.022)***	(0.023)***
Refrigerator	0.700	0.195	0.504	0.296
	(0.024)	(0.034)	(0.041)***	(0.070)***
T.V.	0.953	0.640	0.313	0.223
	(0.010)	(0.039)	(0.040)***	(0.048)***

Note: For National Poor, figures computed at household and individual levels in Estado de Mexico, Mexico, using the 2010 national household income and expenditure survey (ENIGH). For settlements, figures computed at household and individual levels using UTPMP impact evaluation baseline data sources (including non-eligible UTPMP households). Standard errors clustered at the primary sample unit level are shown in parentheses. The term "national poor" refers to households whose members were living on less than USD 167.67 per capita per month in urban zones and less than USD 107.29 in rural zones between August and November 2010; these figures are equivalent to two basic baskets, which represent the national poverty line and basic needs in Mexico as of 2010. Since price levels in urban and rural zones in Mexico differ, the last column tests the hypothesis of equal means by controlling for a dummy variable that is equal to 1 if the household is located in a rural zone. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

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