

Aspiration Adaptation in Resource-Constrained Environments ¹

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Abstract

We use a multi-country field experiment that combines random variation at the treatment level with exogenous variation in the length of exposure to treatment to test the effect of a slum-housing intervention on the evolution of housing aspirations of untreated co-resident neighbors over time. Initially after treatment, we observe a huge control-treatment housing gap in favor of treated units. As a result, non-treated households' aspirations to upgrade their dwelling are significantly higher compared to the treatment group, suggesting that they aspire to “keep-up” with the treated Joneses', as in standard models of peer effects. However, eight months later, no effects are found on housing investments and the aspirational effect completely disappears. Estimates based on a structural model of aspiration adaptation show that the decay rate is 38% per month. Our evidence suggests that simply fostering higher aspirations may be insufficient to encourage forward-looking behavior among the poor.

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I. Introduction

Internal constraints such as low self-esteem, stress, depression, and hopelessness can frustrate aspirations and thus make the poor less willing to take forward-looking actions to exit poverty (Appadurai (2004), Bertrand et al. (2004)). Material deprivation may itself dampen aspirations and lead to lower levels of effort; which, in turn, may lower material outcomes, creating a vicious downward cycle. Accordingly, policies oriented to improve material deprivation are increasingly asked to include interventions that stimulate aspirations, with the hope that this will enhance their effectiveness. Indeed, some authors posit that policies that simply raise aspirations without relaxing material constraints could be effective to encourage forward-looking behavior that enables the poor to exit poverty (Dalton et al. (2016)).

However, in the presence of resource constraints that thwart people’s attempts to achieve their aspirations, aspirations may have to be adjusted downward to relieve the resulting frustration (Selten (1998, 2001) and Karandikar et al. (1998)). In essence, aspirations adapt to suit the circumstances. They remain constant if they can be fulfilled, but are lowered when they are difficult to realize². The latter begs the question of whether higher aspirations are sufficient to trigger forward-looking actions by the poor; or, aspirations adjust downward over time as resource constraints frustrate aspirational achievement.

We examine this question in the context of a housing quality experiment for households living in extreme deprivation in Latin America. Our study population consists of severely poor households that have been “trapped” in urban and rural slums for many years and thus have unusually low housing aspirations. Following standard models of peer effects, we test how housing aspirations and housing investment of untreated units react over time to positive exogenous shocks to the housing quality of some of their neighbors. We use data generated by a large-scale multi-country randomized field experiment in El Salvador, Mexico and Uruguay. The program is run by TECHO, an NGO that aims to improve housing quality in poor slums in more than 20 countries. For identification, we exploit experimentally generated variation in housing quality at the household level combined with exogenous variation in the length of exposure to the treatment.

The experiment went as follows. A number of eligible slums were randomly sampled within each country. Within each slum, a set of randomly chosen families received new houses. We show that, due to the randomization, there was no gap in material circumstances nor in housing aspirations between the treatment and control groups at baseline. Overall, aspirations were very low with around 10% of household heads aspiring to improve their housing conditions. The randomly introduced housing improvement serves as an exogenous shock to the gap in material circumstances between treated and untreated households.

Our first objective is to determine whether the control-treatment housing quality gap

²Aspiration adaptation is a central idea in Herbert Simon’s early writings on bounded rationality. In his view, an individual’s decision-making process is a sequence characterized by three key features: a search for alternatives, satisficing, and aspiration adaptation (Simon (1957)).

generated by the experiment was followed by a control-treatment housing aspiration gap. Put another way, we hope to determine if being exposed to larger housing gaps encourages the control neighbors to increase their housing aspirations and housing investments relative to their treatment counterparts. The question follows the tradition of [Veblen \(1899\)](#) and [Duesenberry \(1949\)](#), namely whether the economic well-being of one household affects that of another in terms of aspirations and actual spending, and thus contributes to a growing literature studying the effects of relative performance on individual economic behavior, most notably [Azmat and Iriberry \(2010\)](#), [Blanes et al. \(2011\)](#), [Card et al. \(2012\)](#), [Bandiera et al. \(2013\)](#), [Bursztyn et al. \(2014\)](#), [Bursztyn and Jensen \(2015\)](#), [Bursztyn et al. \(2018\)](#), and [Bursztyn et al. \(forthcoming\)](#).

After 16 months of treatment exposure, we find that the control group’s housing quality is significantly lower than that of the treatment group, with no other noticeable material differences between these groups. The program is effective at improving housing conditions, with no effects found on incomes, savings, assets, or labor supply. At the same time, the probability of aspiring to upgrade housing conditions within the slum is 56% higher among control units than in the treatment group³. In other words, the housing gap was internalized by control households, whose members now aspire to “keep-up with their treatment Joneses”.

Note, however, that even though control housing aspirations are higher, we do not find effects on actual investment in housing in the control group. The development practitioner would argue that this is indeed the expected result after just 16 months of treatment exposure (a relatively short period of time to observe significant effects on housing investment), being the higher housing aspirations observed in the control group a prelude of housing investment. Under the assumption that housing aspirations are a good predictor of housing investment, the results meet reasonably well with a standard model of housing externalities where homeowners are likely to invest in housing renovation based upon the decisions taken by their neighbors. But to what extent are housing aspirations a good predictor of housing investment in resource-constrained environments?

Our experimental design includes exogenous variation in the length of exposure to the treatment at the slum level. This allows us to compare the housing investment behavior of those exposed to a longer period of treatment exposure (24 months, on average) with that of those exposed to a shorter period (16 months, on average) and causally identify (i) whether higher housing aspirations among control units actually translated into higher housing investment over time; and (ii) whether the evolution of the control-treatment aspiration gap observed in the short-run remains constant or adapts downward over time.

Indeed, after eight additional months of treatment exposure, on average, we observe no additional housing investment and no adaptation in housing quality in either experimental

³This effect cannot be interpreted as a causal spillover effect on control aspirations. For that we would need to observe what would happen with the control units in the absence of treatment neighbors—something that we lack in our experimental design. However, we do show that the treatment group’s housing aspirations remained invariant between baseline and post-treatment round, suggesting that the control-treatment aspiration gap was in fact mediated by a positive treatment-to-control peer effect.

group. Consequently, the control-treatment housing quality gap in favor of treated units remains constant. However, the initial 56% control-treatment aspiration gap in favor of untreated units totally disappears. This suggests that the experimentally induced higher aspirations among control households went unfulfilled. Higher aspirations did not translate into forward-looking actions of housing investment, against the hypothesis that the underlying mechanism of housing aspiration adaptation was their fulfillment. Importantly, the adaptation effect is explained entirely by adaption in the control group, as the treatment group’s housing aspirations remain invariant over the entire period of analysis (from baseline to 16 to 24 months of treatment exposure). Furthermore, we find that external constraints such as low incomes, savings, assets, and labor supply remained constant over time in both groups, indicating that the adaptation in controls’ housing aspirations cannot be explained through non-housing, alternative material improvements.

Our evidence is consistent across the three country experiments, as well as for different measures of housing aspirations, which lends credibility to the external and construct validity of the results. Extrapolation achieved through estimation of a structural model of aspiration adaptation suggests that the housing aspiration effects declined in proportion to the number of months of indirect exposure to the treatment. In fact, we find that this became indistinguishable from zero after 28 months, for a rate of aspiration adaptation of 38% per month. Our paper differs from previous analyses on aspirations (see, for example, [Beaman et al. \(2012\)](#), [Glewwe et al. \(2018\)](#), [Macours and Vakis \(2014\)](#), [Bernard et al. \(2014\)](#), and [Lybbert and Wydick \(2016\)](#)) in that none of the earlier experiments is based on the kind of data that would make it possible to test whether aspirational effects change as a result of adaptation over time. To the best of our knowledge, this is the first paper to empirically examine aspiration adaptation by the poor; and the first that uses exogenous variation for this purpose.

Overall, our aspiration adaptation result has three important implications. First, the poor may be trapped in an aspiration failure equilibria not because they lack the capacity to aspire to higher living standards ([Appadurai \(2004\)](#)), but because they lack the capacity to sustain higher aspirations, i.e., resource constraints make higher aspirations unattainable causing downward adaptation over time. That is, in excessively resource-constrained environments, encouraging aspirations that are not attainable may result in a full reverse-adaptation of aspirations as opposed to sustained forward-looking behavior. Since aspirations are not necessarily fixed over time, higher aspirations are not a sufficient condition for forward-looking behavior among the poor. Thus, policies designed to stimulate future-oriented actions by raising the aspirations of poor persons without also providing economic means required to satisfy those aspirations are likely doomed to fail.

Second, our evidence points to the importance of long-term follow-up to intervention studies focusing on subjective outcomes such as aspirations or happiness. In particular, the aspiration adaptation result warns against the claim of development responses based on outcomes following a policy change but observed only in the very short-run. As suggested by [Jayaraman et al. \(2016\)](#), “once the euphoria dies down, such effects may vanish” (*p.* 318). Consequently, testing for long- run effects becomes critical, for which it is necessary to implement research designs that track responses over time as in [Gneezy and List \(2006\)](#), [Hossain and List. \(2012\)](#), [Allcott and Rogers \(2014\)](#), [Baird et al. \(2016\)](#), [Galiani et al.](#)

(2018), and [Celhay et al. \(forthcoming\)](#).

Third, our paper contributes to the literature of how aspirations are formed. [Ray \(1998, 2006\)](#) and [Genicot and Ray \(2017\)](#) posit that aspirations are socially dependent, i.e., individuals’ goals are determined by both personal characteristics and the characteristics of their reference groups^{4,5}. In this setting, material aspirations are a non-monotonic function of the social distance between an individual’s characteristics and the characteristics of her reference group, or what [Ray \(1998, 2006\)](#) calls the “aspirations gap”. The relationship is non-monotonic in the sense that the aspirations gap must be large enough to encourage effort, but not so large as to induce frustration. If the gap is too large, the cost of the investment required to satisfy those aspirations may be unrealistic, in which case the individual will adjust her aspirations downward to more reasonable levels⁶. Therefore, a positive shock to the reference group’s material situation will prompt a positive change in an individual’s aspirations only if the resulting material gap between her and her reference group is perceived as “moderate” so that she foresees that she can close it by her own efforts.

Interestingly, we find that the initial control-treatment aspiration gaps are mainly observed among untreated urban slum dwellers. We find no gaps in the housing aspirations of their rural counterparts, despite statistically comparable levels of housing aspirations to urban counterparts at baseline. We hypothesize that this is attributable to the fact that urban households enjoyed higher baseline incomes and better housing conditions than rural slum dwellers. Since urban and rural treatment families received exactly the same type of house, the control-treatment housing gap for the urban controls was significantly smaller than the housing gap faced by their rural counterparts. The seemingly insurmountable control-treatment housing gap confronted by untreated rural units did not allow them to even aspire to emulate the new housing standard of their neighbors, unlike the moderate and apparently actionable housing gap faced by their urban counterparts⁷. This result is consistent with the theoretical work of [Ray \(1998, 2006\)](#) and [Genicot and Ray \(2017\)](#) in that the relationship between aspirations gaps and aspirations formation is non-monotonic and depends on the size of the aspirations gap.

A complementary hypothesis is given by [Glaeser \(2011\)](#) who argues that urban slum dwellers are willing to live in substandard housing and hostile geographical environments if

⁴Other models that are based on the principle that aspirations are socially dependent have been developed by [Bogliacino and Ortoleva \(2013\)](#) and [Besley \(2016\)](#).

⁵The alternative approach is to assume that only personal experiences determine future goals, in which case each individual could be analyzed as a self-contained unit. See, for example, [Carroll and Weil \(1994\)](#), [Gilboa and Schmeidler \(1995\)](#), [Karandikar et al. \(1998\)](#), [Overland and Weil \(2000\)](#), [De la Croix and Michel \(2001\)](#), [Alonso-Carrera et al. \(2007\)](#), and [Dalton et al. \(2016\)](#).

⁶By the same token, a very small gap relative to the characteristics of the reference-group members means that the aspirations are closely aligned with the individual’s current standard of living, which produces little incentive for taking action to raise her standard.

⁷This result is in line with [Undurraga \(2017\)](#) who exploits the random allocation of TECHO houses across the slum’s space to estimate how controls’ housing aspirations react to the average distance to treated neighbors in Mexico, and finds that the proximity to treatment units (i.e., larger housing gap) generates a negative effect on their housing aspirations, on average.

it enables them to escape subsistence-level rural poverty 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 would allow the slum dwellers to gradually improve their living conditions and lead them to aspire to transform the slums into non-slum neighborhoods or move to formal housing within the city. Accordingly, the observed effects among untreated urban slum dwellers may also be due to a positive interaction between better neighborhood housing quality and city level effects, i.e. it is the exposure to cities that makes of the within-neighborhood housing gaps an inspirational device that increases the housing aspirations of poorer urban slum dwellers.

The rest of this paper is organized as follows. Section II describes the intervention and the experimental design. In Sections III and IV, we discuss the construct validity of our aspiration measures and introduce the identification strategy used to estimate causal adaptation effects. In Section V, we present our empirical results, both reduced-form and structural estimates. Section VI concludes.

II. The TECHO Experiment

The experiment was conducted in partnership with TECHO (Spanish for “roof”), a Latin American NGO whose mission is to provide basic, pre-fabricated houses to slum dwellers with the express goal of improving their housing conditions and well-being. As is extensively described in Galiani et al. (2017) and Galiani et al. (2018), TECHO targets the poorest slums and, within these slums, the families who live in the most extremely substandard housing. TECHO offers an 18 square meter ($6m \times 3m$) house made of insulated pinewood panels. The house costs less than US\$1,000 and beneficiary families pay only 10% of that cost under a scheme of flexible installment payments that allows the families to smooth out the expenditure. 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, respectively⁸.

Between 2007 and 2010, TECHO implemented the program in a number of urban and rural slums in El Salvador, Mexico, and Uruguay. Beneficiaries were selected by a lottery system that gave all eligible households within a slum an equal opportunity to receive one of the units, so that treatment and control units are co-residents. By that time, the objective of TECHO was to expand the presence of the program in as many slums as possible, regardless of the proportion of treated residents in each selected slum. Hence, households that agreed to participate in the lottery understood that lottery losers would not receive the program benefits in the future. Hence, the behavior of control units should not have been affected by the expectation of being treated in the next round^{9,10}.

Since TECHO did not have the financial capacity to build the houses in all the targeted slums at the same time, the program was rolled out in two phases at the slum level so

⁸For a full description of the program, see [Appendix B](#).

⁹Indeed, robust evidence supporting this claim is provided in Section V.

¹⁰For a full description of the sampling procedure within each country experiment, see [Appendix B](#).

that, in each country, Phase I slums were treated in the first year and Phase II slums in the second year. Baseline surveys were conducted approximately one month before the start of the construction work in each slum, which gave households time to acquire the funds to make the 10% contribution required by the program. The follow-up surveys were conducted simultaneously for all slums (Phase I and Phase II) in each country, approximately one year after the construction of the last house in the Phase II slums. As a result, Phase I slums had 24 months of exposure, on average, while Phase II slums had an average of 16 months of exposure, for a difference of 8 months. Table A1 and Figure A1 in Appendix A detail the sample and timeline of the study.

While the slums were not randomly allocated to phases, there is exogenous variation in the amount of time that beneficiaries had occupied the TECHO house at the time of the follow-up survey, since no discretionary criteria were used to select which slum was assigned to which phase. Instead, the decision of which slums would be treated first or second was based on the availability of census information about the eligible households in each slum at the time of the assignment, which, as we show, was orthogonal to slums and slum dwellers baseline characteristics. In order to collect census information, TECHO volunteers were organized into subgroups and sent to each selected slum at the same time. Census data collection included social and demographic characteristics of the slum dwellers. Then, with that information in hand, a set of eligible households was to be selected by volunteers. As soon as each volunteer team came back to the central office with the list of eligible households in their assigned slum, TECHO officials immediately asked the research team to implement the household-level randomization in that slum, a process that typically took no more than one day. TECHO organized its internal resources in order to build the houses for the assigned-to-treatment households in that slum as soon as possible. That way, slums were allocated to phases on a “first come, first served” basis. Once the resources required to treat the next slum in line were insufficient, TECHO decided to allocate that slum and the remaining slums in line to the following round (Phase II), which were treated once sufficient resources to build houses for all of them were obtained (about a year after Phase I). TECHO followed the same implementation process in all the selected slums in each country.

Importantly, while the census was conducted in all the selected slums within a country at the same time, it is likely that the data collection process in some slums was more efficient than in others, which would explain why some slums were treated first and the remaining slums second. If differences in efficiency are fully explained by differences in the capacity of the volunteers assigned to each slum, then we cannot rule out that the phase rule is exogenous to slum characteristics. A valid concern, however, is that the timing of the delivery of the list of eligible households from each slum to the TECHO office depended on the distance to the office, the slum’s size or its level of poverty, since it presumably takes longer to conduct a census of eligible households in farther, larger and/or poorer slums than in closer, smaller and/or less poor ones. However, as shown in Galiani et al. (2018) and replicated in the next section, we test whether Phase I and Phase II slums were statistically comparable at the pre-treatment level in terms of distance to TECHO office, slum size, mean income per capita, mean housing quality, and a battery of mean satisfaction measures and find no statistically significant differences across them. These

results suggest that the populations in Phases I and II were statistically similar before phase assignment, lending credibility to our assumption that the assignment of slums to phases followed a process that had nothing to do with slum characteristics¹¹.

Our sample includes a total of 74 slums located in both urban and rural zones; 29 in Phase I and 45 in Phase II. There were a total of 2,373 eligible households in these slums. Our baseline population of slum dwellers is composed of households whose members have been living in the slum for 12 years, on average. Their monthly income per capita is, on average, US\$55, and most live in overcrowded houses made of very poor materials. 80% of the rooms have walls built of low-quality materials such as plastic and cardboard; 67% have low-quality roofs, and only half of the units have a bathroom of their own. The number of rooms per capita is 0.7, compared to an average of 0.77 for non-slum but poor households in each country (see [Galiani et al. \(2017\)](#)). Despite this, only 13% of these households reported aspirations to upgrade the quality of their dwelling’s walls and/or roofs, increase the number of rooms in the house, or improve the quality of indoor materials such as flooring, doors, windows and/or kitchen equipment. Finally, nearly all (97%) of the 13% of households reporting such aspirations said they cannot satisfy their housing aspirations because of financial constraints that make them unable to afford the desired housing improvements, rather than because of any lack of knowledge or time to do so. Overall, our population of study is made up of poor households that have been “trapped” in slums for many years and face severe resource constraints. We hypothesize that these factors have discouraged them from aspiring to upgrade their housing conditions within the slum.

Treatment was offered to 57% of the households, and over 85% of those households actually received a new house. The remaining 15% that were assigned to treatment could not afford the required 10% co-payment under the flexible payment scheme offered by TECHO officials and hence did not receive a house. The compliance rate with the treatment is balanced across phases (see [Table A2 in Appendix A](#)). Attrition rates between baseline and follow-up rounds amounted to 6% of the households in the assigned-to-treatment group and 7% of those in the control group, with most of the attriters being households whose members moved out of the slum and could not be reached in their new location. The difference in attrition rates between groups is not statistically significant at conventional levels, and this is the case for: (i) the whole sample; and (ii) each phase sample (treatment versus control in phase I sample; treatment versus control in phase II sample). Also, the attrition rates are balanced between phase groups (Phase I versus Phase II). Finally, since our sample considers slum dwellers residing in urban and rural zones, we replicated the analysis for each zone and find that there is statistical balance in terms of compliance and attrition rates within each zone as well.

¹¹In [Section IV](#) we also test the statistical balance across phases using household level characteristics and, again, we find no differences at all which reinforces the validity of our claim.

III. Measurement

The possibility of constructing an aspiration metric is supported by research that demonstrates that people have a common understanding of subjective perceptions and that numerical measures of attitudinal indicators are effective in capturing those feelings. However, as is extensively discussed in [Bernard and Taffesse \(2014\)](#), cardinality problems related to anchoring, wording, or scale dependence can all affect inter-person comparability of responses when measuring aspirations. Moreover, respondents may understand aspiration questions differently, e.g., they may interpret the same wording in their own way when their attention is directed toward distinct aspects of their lives. A number of recent studies have attempted to assess the reliability of aspirations-related measures like subjective well-being and expectations (see, for example, [Manski \(2004\)](#), [Krueger and Schkade \(2008\)](#), and [Delavalande and Mckenzie \(2011\)](#)). The general conclusion to be drawn from this body of work is that, provided enough care is taken when designing the instruments, the analysis derived from these data can usefully inform researchers about individuals’ decision-making processes.

Our housing aspirations measure was designed based on multiple qualitative and quantitative pre-tests implemented in out-of-sample slums with similar characteristics to the ones included in the evaluation sample. By contracting a highly respected survey firm in each country, we survey roughly 40 household heads per country before the implementation of baseline surveys. The objective is two-fold. First, to “fine-tune” the questionnaire and thus guarantee that the wording of the questions is properly understood by local population. Second, to design a battery of housing aspiration questions that encompasses the housing “aspiration window” of slum dwellers ([Ray \(2006\)](#)). In order for our aspiration measures to be comparable across individuals (and thus avoid problems of cardinality) we need that individuals with different reference points be able to map their housing aspirations over the same metric scale, for which a closed set of aspiration options/levels is required.

Our qualitative pre-test revealed that the housing aspirations of slum dwellers are generally inseparable from location aspirations, i.e. individuals who aspire to improve their housing conditions typically linked this attitude to either do it within-slum (stay in the same location) or out-of-slum (upgrade housing in other neighborhood). Also, our pre-tested household heads typically referred to housing upgrading as a “yes/no” binary decision. In contrast, location aspirations were associated to geographical categories at slum, municipality or state levels, a desire that was typically related to getting closer to household heads networks and/or get better job opportunities. In particular, individuals who do aspire to improve their housing conditions usually refer to both improved housing and owning land. On the other side, those who do not aspire to improve housing typically did not offer an alternative at all and desired to continue living under the same conditions. Finally, some pre-tested household heads declared that they did not have the aspirations to improve their housing quality, but instead aspired to move to another slum.

Accordingly, we measure housing aspirations by using a closed set of aspirations that combine location and housing prospects. The measures are based on responses to the following question, each part of which highlights the specific aspiration to be evaluated:

“Right now, if you had to choose among the following alternatives of housing and location, what would you choose?” The question offered seven housing and location options, in and out of the slum, which were organized into four mutually exclusive categories of aspirations:

- (i) Continue living in the same slum under the same conditions (i.e., “no-upgrade”);
- (ii) Continue living in the same slum but get improved housing and own land (i.e., “within-slum-upgrade”);
- (iii) Move to another slum; and
- (iv) Move and get improved housing and own land outside of a slum, with four location alternatives: in the same municipality, in another municipality, in another state, or somewhere else (i.e., “out-of-slum-upgrade”).

There are four key features to highlight. First, these categories are all related to future-oriented actions (Bandura et al. (2001)). Second, aspiration options are reasonably achievable given the housing and location conditions of our sampled individuals, such that they are all part of their “aspirations window” (Ray (2006)). The set offers the option of choosing to keep the actual housing and location conditions or choose between a balanced set of feasible housing and location upgrades. From the perspective of a benevolent dictator that is interested in people’s housing conditions, “within-slum-upgrade” and “out-of-slum-upgrade” dominate options “no-upgrade” and “move-to-another-slum” since the first include housing improvements while the latter do not. The rank order between “no-upgrade” and “within-slum-upgrade” (and between “move-to-another-slum” and “out-of-slum-upgrade”) is not clear since location preferences are non-excludable — slum dwellers may aspire to stay in the slum so they can keep their family networks or move to another neighborhood such that they can optimize their geography of labor opportunities. The fact that we cannot rank locational aspirations should not threaten the validity of our aspiration measure as far as the objective is to measure housing aspirations, and the inclusion of locational alternatives plays mostly the role of making the aspiration set more flexible and realistic. For housing aspirations, we therefore concentrate on first testing whether being exposed to treated neighbors generates any increase in the probability of aspiring to “within-slum-upgrade” or “out-of-slum-upgrade”, to the detriment of options “no-upgrade” or “move-to-another-slum”. Then, we use the battery of locational questions to study whether the exogenous shock on the neighbors’ housing quality generate any divergence on the aspiration to upgrade housing conditions within- versus out-of-slum.

Third, in regards to the time horizon, we ask the aspiration question in present tense (“Right now...”). Hence, the question does not try to capture when people expect to satisfy their housing aspirations, but whether or not they have any housing aspirations. Fourth, note that our question takes aspirations as distinct from beliefs about what is achievable, i.e. housing and location preferences are thought of as potentially affordable. Participants are therefore encouraged to think about whether they would spend their own money to change their current housing situation, and about what they would do. That is, it is a hypothetical question but conditional on the individual’s actual situation. In fact, in order to check the construct validity of our aspiration question, we replicated exactly the same question (with the very same housing and locational alternatives) but instead

of pointing the question to “What would you choose?” we asked “What do you expect to happen in the next 5 years?”. That is, we used a measure of “expectations” (beliefs) as a related but nonequivalent variable to test the construct validity of our aspiration measure. As we show in the robustness checks section, our aspiration measure does not capture beliefs but mostly a wish for achievement or aspirations.

Quantitatively, we define each aspiration category ‘i’ as a dummy variable that equals 1 if the respondent reports that she aspires to option ‘i’ (instead of options ‘j’s ≠ ‘i’) and 0 otherwise. One issue that arises with respect to measures based on multiple-choice questions is that respondents may be prone to choose first alternatives instead of evaluating the merit of all the listed options equally. The concern here is that different individuals may have different likelihoods of choosing first alternatives. However, in randomized experiments such as ours, this should not be a concern because if the treatment itself does not affect an individual’s willingness to evaluate the merit of all the alternatives on an equal basis, then the distribution of “first-choice” respondents would be the same across experimental groups by virtue of random assignment^{12,13}.

IV. Empirical Strategy

We report estimates of non-intention-to-treat effects by time of exposure (phase) for the following linear probability model:

$$Y_{ij} = \alpha + \gamma_1 Control_{ij} + \gamma_2 Control_{ij} \times PhaseI_j + \beta X_{ij} + \mu_j + \varepsilon_{ij} \quad (1)$$

where Y_{ij} is a dummy variable equal to 1 if head of household i living in slum j aspires to a given housing upgrading category, and 0 otherwise; $Control_{ij}$ is a dummy variable equal to 1 if household i in slum j was not offered a TECHO house and 0 otherwise; $PhaseI_j$ is a dummy variable equal to 1 if slum j was treated in Phase I and 0 otherwise; X_{ij} is a vector of household characteristics measured at baseline; μ_j is a vector of slum

¹²There is always the risk that treatment units change their interpretation of aspirations because of the treatment. Treatment individuals may now have a different sense of what a housing upgrade really means, and that can affect the comparability of housing aspirations across treated and untreated units. However, this is especially a problem for subjective questions that attempt to differentiate degrees of actions within a unique category (e.g., choosing different degrees of housing upgrading, where the category “not upgrading at all” is not permitted), which is not our case. Indeed, our aspiration measure is designed to capture categorical options in a dichotomic “aspiration window” that do not make differences on the degrees of housing quality upgrades. Individuals can only choose “to upgrade” or “not to upgrade”, and do it “within-slum” or “out-of-slum”. The latter guarantees stability in the interpretation of the housing aspiration question, which minimizes the comparability problems across experimental groups.

¹³Control units could be more prone to declare that they aspire to improve their housing conditions if that opinion increase their chances to obtain the TECHO house in the future. However, as mentioned in the previous section, the households that agreed to participate in the lottery were told that lottery losers would not receive the benefits provided by the program in the future, and they accepted this condition before agreeing to participate in the study. Hence, the housing aspirations of control units should not have been affected by the expectation of being treated in the next round.

fixed effects; and ε_{ij} is the error term^{14,15}.

The slum fixed effects capture the average unobservable differences across slums (and hence countries). This is important, since randomization was conducted within each slum. Another important factor is that slum fixed effects also control for differences in the reference points for housing aspirations, which may vary geographically. Finally, after controlling for slum fixed effects, we assume that the error terms are independent and thus report only robust standard errors¹⁶.

The parameters γ_1 and $\gamma_1 + \gamma_2$ are the non-intention-to-treat effect for Phase II (short-term exposure) and Phase I (long-term exposure) households, respectively. Note that our experimental design involves the randomization of the TECHO houses at the household level within each slum, and the treatment households may therefore have changed their aspirations over time owing to the experience of having a new house or the presence of possible changes on the part of their treated peers. Hence, γ_1 and $\gamma_1 + \gamma_2$ do not estimate the treatment-to-control spillover effects on controls' housing aspirations, but just identify the "control-treatment housing aspiration gap" in the short- and long-run, respectively. Nonetheless, as we show in the next section, the aspirations of people in the treated groups to upgrade their housing conditions within the slum were not only statistically balanced with respect to the controls' aspirations, but also remained invariant between baseline and follow-up round. This is the case for all the housing aspirations measures, which suggests that our control-treatment housing aspiration gap estimates are unlikely to be influenced by either the realization or frustration of treatment households' aspirations but mostly to changes in the aspirations of control units.

Finally, γ_2 , our parameter of interest, is the degree of aspiration adaptation, i.e., the difference in the control-treatment aspiration gap between long- and short-term treatment exposure. Conditional on that treatment's aspirations being invariant between phases I and II, then a negative γ_2 would be consistent with an at least partial aspiration adaptation on the part of control units. If γ_2 fully offsets γ_1 , then we would have complete adaptation, i.e., the probability of control individuals reporting that they aspire to the given option returns to its reference level after an average of 8 additional months of indirect treatment exposure.

Identification. Our identification strategy is two-fold. First, random assignment of treatment status guarantees treatment exogeneity, both overall and within phases, and

¹⁴As we explained in the last section, our aspiration measures take the form of binary outcomes (limited dependent variable (LDV)). 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 baseline covariates are included only in order to improve efficiency, but their omission would not bias the estimates of the parameters of interest.

¹⁵Since the phase design of the intervention is given at the slum level, there is no within-slum variation in phase.

¹⁶The statistical inference of our results is robust to clustering the standard errors at the slum level since rejection decisions of the null hypothesis remain the same at conventional levels of statistical significance. These results are available upon request.

thus provides the identification for both γ_1 and γ_2 . [Galiani et al. \(2017\)](#) demonstrate that the overall sample was balanced over a large number of characteristics. We extend this analysis by testing the balance across experimental groups within each phase and also across phase samples. As [Tables A3, A4, and A5](#) in [Appendix A](#) show, the experimental groups are balanced within phases, and this is the case for the full sample as well as for urban and rural sub-samples.

Second, a negative and significant γ_2 can be interpreted as evidence of aspiration adaptation on the part of control units only if (i) the samples in both phases were balanced in terms of their characteristics (naturally also starting from the same level of housing aspirations), and (ii) treatment aspirations do not vary over time. If the allocation of slums to phases in each country were orthogonal to their baseline characteristics, then condition (i) would be complied. Indeed, we cannot reject the null hypothesis of no differences for a variety of baseline covariates between Phase I and Phase II households, including housing aspirations, economic and demographic indicators, housing quality and satisfaction measures, all of which are likely to be predictors of slum dwellers' aspirations. Moreover, we test whether Phase I and Phase II slums are statistically comparable in terms of the number of eligible households per slum (slum size), Euclidean distance to TECHO office, mean income per capita, mean housing quality, and a set of mean aspirations and satisfaction measures for the residents. We find no statistically significant differences across them at all (see [Table A6](#)). These results show that populations from Phases I and II were statistically comparable before treatment, thereby lending credibility to our interpretation of γ_2 as a measure of aspiration adaptation.

In regards to condition (ii), our experimental design involves the randomization of the TECHO houses at the household level within each slum, and the treatment households may therefore have been subject to both direct and spillover effects. This may have changed their aspirations over time owing to the experience of having a new house or the presence of possible changes on the part of their treated peers. Nonetheless, as we show in the next section, the aspirations of people in the treatment groups to upgrade their housing conditions within the slum did not decrease after treatment, and post-treatment housing aspirations remained invariant across phases. This was the case of all the housing aspirations measures, which suggests that our aspiration adaptation estimates are not influenced by either the realization or frustration of treatment households' aspirations and only obey to changes in the aspirations of control units over time.

Finally, two main econometric concerns may arise in regard to the treatment group as a valid counterfactual of the control groups' behavior over time, and thus the internal validity of our causal estimates. First, the control-treatment housing aspiration gap is hypothetically induced by the observed higher housing quality enjoyed by the treated neighbors. Thus, if the wear and tear on the TECHO houses reduces the level of housing quality over time, then the adaptation effects might not be attributable to the aspiration mechanism, but instead could be transmitted through endogenous changes in the quality of the TECHO house based on the length of time of exposure to the treatment. However, we provide robust evidence that the housing quality did not deteriorate over the period corresponding to the time of exposure, and our results are robust to controlling for housing quality at the post-treatment level (see next section). This concern is also applicable to

any other change in the material circumstances of treated units. However, as shown by [Galiani et al. \(2017\)](#), receipt of the TECHO house only produced effects dealing with the quality of floors, walls, and roofs, and no other noticeable changes were observed in terms of material enhancements such as income, assets, non-durable goods, or housing services (water, electricity, and sanitation). Indeed, we find no differences across experimental groups over time in any of these dimensions, which rules out the presence of alternative mechanisms related to changes in material standards through which control-treatment aspiration gap may have been reduced over time (see Section [V](#)).

V. Results

V.1. *Reduced-Form Estimates*

Control-treatment housing aspiration gap. We report the results of estimating equation (1) for two different specifications —one with and one without a set of control variables. We first estimate the model for urban and rural samples separately and then combined^{17,18}. Table 1 presents estimates of γ_1 and γ_2 on control-treatment aspiration gaps. Our dependent variable corresponds to each of the four dummy indicators for housing aspirations. The specific control variables included in the second specification are listed in the notes to Table 1. In each model, we also report the p -value for an F-test of the null hypothesis of full adaptation ($H_0 : \gamma_1 + \gamma_2 = 0$).

First, in urban slums, the probability of aspiring to upgrade housing conditions within the slum in Phase II (short exposure) is substantially higher among control units than among treatment units, as indicated by the positive and significant estimate of γ_1 . Indeed, this difference amounts to 56% with respect to the treatment mean, and it is robust across the two models. At the same time, on average, the probability that a control-group household aspires to upgrade its housing conditions outside of a slum is 23% lower than in the treatment group¹⁹.

The latter does not identify a causal spillover effect as treatment aspirations might also have changed as a result of the treatment and/or the presence of their treated peers (treatment-to-treatment spillovers). Indeed, one might argue that the control-treatment aspiration gap is not due to an increase in the controls’ housing aspirations (the keeping-up with the Joneses’ story) but to a decrease in the housing aspirations of treated units. Note, however, that baseline aspirations were already very low (13% of treated urban units aspired to upgrade the materials used in their existing houses) and are almost the same than the aspiration to upgrade housing conditions within the slum at follow-up

¹⁷Table A15 in [Appendix A](#) provides a detailed definition and sample size for each variable considered in this study.

¹⁸Our results are robust to using a Probit model as the order of magnitude of the effects remains the same and rejection decisions of the null hypothesis do not change at conventional levels of statistical significance. These results are available upon request.

¹⁹We observe no differences at all across experimental groups in terms of the aspiration to either remain in the same conditions in the slum of residence or move to another slum, however.

(16%). This suggests that, if having access to a better house and being exposed to treated neighbors had any effect on aspirations to upgrade housing conditions within the slum, this was close to zero, indicating that the housing program “neutralized” the “within-slum-upgrade” aspirations of treated units (who actually were quite satisfied with their housing conditions after the program, as is shown by [Galiani et al. \(2018\)](#))²⁰.

Second, while secular trends might play a role on the increased housing aspirations of untreated units, it is unlikely that a 56% increase (a huge effect) is fully explained by them. Indeed, as we show in the following sections, the size of the effects is comparable across the three country experiments. Therefore, if there was any secular trend operating, then that trend would have to operate in a similar way across the three countries, which is not obvious at all. Finally, the positive housing aspiration result is in contrast to the null effects on housing expectations. Consequently, the potential secular trend would have to be extremely selective in order to be valid, one that affect housing aspirations but not housing expectations.

Overall, the latter suggests that the control-treatment aspiration gap is likely explained by a positive treatment-to-control peer effect, i.e. the housing gap was internalized by untreated households, whose members now aspire to “keep-up with their treatment Joneses”.

Testing non-monotonicity of housing aspirations. Interestingly, the aspirational effects are observed only among urban households, with no effects found in the rural sample. We hypothesize that this is due to differences in the treated-untreated post-program housing gaps confronted by control units in each zone. Urban and rural households have statistically similar levels of housing aspirations at baseline (see Table A7 in [Appendix A](#)). However, urban households are richer than their rural counterparts at baseline, and they are better-off in regards to housing conditions –although less satisfied with them. Since the TECHO houses provided to urban and rural program beneficiaries are the same, then the post-program control-treatment housing gap in urban slums is smaller relative to the gap in rural slums. Indeed, as is shown in Table A8 in [Appendix A](#), the order of magnitude of housing treatment effects is generally larger in rural slums, especially in terms of the quality of walls and the percentage of rooms with windows. Moreover, while the effects on housing satisfaction indicators are positive and significant in both urban and rural samples, the effects are systematically larger in rural slums²¹.

[Ray \(1998, 2006\)](#) point out that large living-standard gaps with respect to reference groups can exacerbate frustration among the very poor. Indeed, the discrepant results across urban and rural slums are consistent with [Genicot and Ray \(2017\)](#)’s hypothesis that the relationship between the aspirations gap and aspirations formation is non-monotonic

²⁰While our baseline measure captures the aspiration to upgrade housing-specific materials in slum dwellers’ existing houses (e.g., the quality of walls, roofs, flooring, and indoor equipment), our follow-up measure mainly captures the general aspiration to upgrade housing quality within the slum. Although the two measures are not exactly the same, both are indicators of aspirations to upgrade housing conditions within the slum and we believe they are fairly comparable over this particular dimension.

²¹In order to interpret these results more accurately, it is important to note that, for all the satisfaction and housing quality variables considered in this study, there was no instance in which the average outcome for the control group decreased between the baseline and follow-up measures.

and depends on the size of the aspirations gap. We hypothesize that the “seemingly insurmountable” control-treatment housing gap confronted by untreated rural households frustrated their housing aspirations. In contrast, the “moderate” housing gap faced by their non-beneficiary counterparts in urban slums encouraged them to “keep-up” with the housing conditions of the treatment Joneses’ (which is why they increased their “within-slum-upgrade” aspirations).

A complementary hypothesis is given by Glaeser (2011) who argues that urban slum dwellers are willing to live in substandard housing and hostile geographical environments if it enables them to escape subsistence-level rural poverty 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 would allow the slum dwellers to gradually improve their living conditions and lead them to aspire to transform the slums into non-slum neighborhoods or move to formal housing within the city. Accordingly, the observed effects among untreated urban slum dwellers may also be due to a positive interaction between better neighborhood housing quality and city level effects, i.e. it is the exposure to cities that makes of the within-neighborhood housing gaps an inspirational device that increases the housing aspirations of poorer urban slum dwellers.

Testing Aspiration Adaptation. The control-treatment housing aspiration gap in urban slums does not appear to be fully sustained after eight months of additional treatment exposure, as indicated by the negative estimates of γ_2 . Indeed, this is 69% lower in Phase I than in Phase II and we cannot reject the null hypothesis of full adaptation (see Table 1). Moreover, both the “within-slum-upgrade” and “out-of-slum-upgrade” aspirations of untreated units were not higher than the treatment group’s reference level, indicating that aspirations to upgrade in and out of the slum are, to some degree, partial substitutes and thus react inversely with respect to each other. Figure A2 in Appendix A illustrates these results.

As long as the treatment group aspirations remain constant between month 16 and month 24 (and thus do not change over time as a result of the treatment and/or the presence of changes in the living conditions of their treated peers), then γ_2 can be interpreted as an adaptation of controls’ aspirations. In contrast, if treatment aspiration levels differ across phases, then the observed adaptation effects may not be causally attributable to controls’ downward movements. We test this by evaluating whether the distributions of slum fixed effects significantly differ across phase samples²². In particular, using the Kolmogorov-Smirnov test, we cannot reject the null hypothesis of equality of distributions for all aspiration variables, indicating that the treatment groups for Phases I and II do not differ significantly in their post-treatment housing aspirations^{23,24}. Overall, this suggests

²²In the full regression, we could infer this from the coefficient for a Phase I dummy variable, but since this does not vary within slums, and since slum fixed effects (the strata units within which household-level randomization is due) must be included, this cannot be estimated in the main specification.

²³Note that, as is shown by Figure A2, the treatment means never differ between phases, which is the case for both the “within-slum-upgrade” aspirations and “out-of-slum-upgrade” aspirations, as well as for the full sample and urban and rural sub-samples.

²⁴Note also that, as shown in the last three columns of Table A4, treatment groups are well balanced

that the treatment group’s housing aspirations remain invariant across phases, and our aspiration adaptation estimates are consequently not influenced by the ups or downs in treated neighbors’ aspirations. The latter lends credibility to our claim that γ_2 is a causal estimate of the aspiration adaptation effect on untreated units.

External Constraints and Forward-Looking Behavior. Resource constraints may discourage effort, which in turn can lead to the adaptation of housing aspirations. Hence, a potential condition in order for the untreated slum dwellers to sustain their new housing aspirations is to have access to credit markets, incomes or savings so that they actually have the financial means to invest in housing upgrades. In particular, we hypothesize that untreated individuals adapted their housing aspirations downward because they realized that their baseline material means were insufficient to close the control-treatment housing gap.

We test for this possibility by estimating the equation 1 for various measures of material well-being, including assets, income, savings, and labor supply. The results are reported in Table 2, which shows no differences between treatment and control groups in Phase II (γ_1) and no adaptation across phases (γ_2), with the untreated households being equally poor over time. Second, we asked the heads of household whether they had invested in a series of potential housing upgrades, such as housing materials, and access to water, sanitation, and/or electricity, and, if so, how much they had invested. These indicators work as a proxy for the level of effort exerted to satisfy housing aspirations. As shown in Table 3, we find no effect on whether the investment effort was made or on the level of investment. Furthermore, we also test whether the treatment generated any change in the extent of access to housing services. We again find no effect.

We examine the role of material means on aspiration adaptation in more detail by testing for heterogeneous adaptation effects across subgroups of relatively high- and low-income urban slum dwellers. One would expect that adaptation effects are, if anything, smaller in less poor groups. As Table A9 in Appendix A shows, while the housing conditions and housing satisfaction do not differ much between above- and below-the-median baseline income subgroups, above-the-median individuals earn on average US\$110 per capita per month, which is more than 5 times higher than what is earned by their below-the-median counterparts; and being richer can influence the course and sustainability of housing aspirations. Indeed, Table 4 shows that while above-the-median households display moderate adaptation effects in terms of “within-slum-upgrade” aspirations, the adaptation effect exhibited by their poorer counterparts is 3.4 times greater^{25,26}. In fact, on average, the below-the-median untreated units end up having a significantly

across phases at baseline. Therefore, potential pre-treatment differences across treated individuals are less of a concern here.

²⁵This is calculated as the quotient of the adaptation rates between below-the-median and above-the-median baseline income groups. Taking Model 2 estimates, we have $(-0.15/0.06)/(-0.08/0.11) = -2.5/-0.72 = 3.44$.

²⁶The same exercise was performed for the rural sample and we find no differences in the adaptation effects between below-the-median and above-the-median baseline income subgroups. See Table A10 in Appendix A.

lower level of “within-slum-upgrade” aspirations than their treatment-group counterparts ($H_0 : \gamma_1 + \gamma_2 = 0$ is rejected). Then, when looking at the “out-of-slum-upgrade” aspirations, we observe the opposite trend, i.e., an upward adaptation, which is again larger among poorer households.

The above findings indicate that higher housing aspirations are not a sufficient condition for higher levels of housing investments and that material means play a key role in the aspiration adaptation process of resource-constrained individuals²⁷.

V.2. Robustness Checks

Multiple-Hypothesis Testing. In studies with multiple outcomes, a few statistically significant effects may emerge simply by chance. The larger the number of tests, the greater the likelihood of a type I error. We reduce the risk of false positives deriving from an examination of large numbers of individual outcomes by using Holm (1979) Family-Wise Error Rates (FWER) to adjust the p -values of the individual tests as a function of the number of aspiration variables. We have 4 aspiration indicators and thus 4 associated null hypotheses. The marginal p -values are ordered from smallest to largest: $\hat{p}_{n,(1)} \leq \hat{p}_{n,(2)} \leq \hat{p}_{n,(3)} \leq \hat{p}_{n,(4)}$ with their corresponding null hypotheses labeled accordingly: $H(1), H(2), H(3), H(4)$. Then, $H(s)$ is rejected if and only if $\hat{p}_{n,(j)} \leq \frac{\alpha}{S-j+1}$ for $j = 1, \dots, 4$. In other words, the method starts with testing the most significant hypothesis by comparing its p -value to α/S , just as the Bonferroni method. If the hypothesis is rejected, then the method moves on to the second most significant hypothesis by comparing its p -value to $\alpha/(S-1)$, and so on, until the procedure comes to a stop. We compute Holm FWER corrections at the 10% level of statistical significance. That is, for our most significant hypothesis (“out-of-slum-upgrade” aspirations), the corrected p -value is $0.1/4 = 0.025$; for the second most significant hypothesis (“within-slum-upgrade” aspirations), the corrected p -value is $0.1/3 = 0.0333$; and so on. The statistical inference of our results reported in Table 1 is robust to this stringent test, since rejection decisions of the null hypothesis remain the same for each of the four aspiration indicators.

Country-Specific Estimates and External Validity. Table A11 in Appendix A reports the estimates separately by country. The estimated magnitudes of the short-run effect on aspirations to upgrade either within-slum or out-of-slum, γ_1 , are of about the same magnitudes for all countries, but statistically significant mostly for the case

²⁷We further test whether the TECHO program had any effect on residential mobility and use this as a proxy indicator of forward-looking behavior related to “out-of-slum-upgrade” aspirations. We recorded whether households moved out of the slum between the baseline and the follow-up surveys. Among those that moved out of the slum (6% of the sample), those that could be located and surveyed are referred to here as “movers”, while those that could not be located are referred to as attriters (less than 5% of this latter group corresponds to households that were located but refused to be surveyed). The proportions of attriters and movers are very low in the sample as a whole (roughly 10% combined); the differences are insignificant across experimental groups within each phase. The results remain constant between phases and are robust across urban and rural zones. While we are unable to determine the post-treatment characteristics of attriters (they could have migrated either to a better environment and obtained formal housing or to a poorer place and be worse off), our evidence at least suggests that “out-of-slum-upgrade” aspirations did not translate into higher migration rates. These results are available upon request.

of Uruguay. The aspiration adaptation effect, γ_2 , is consistent across countries as well, but, again, chiefly significant for the case of Uruguay, most likely owing to the fact that the sample size in that country is much larger. The magnitudes of the estimates for the γ_2 parameters relative to the estimated γ_1 parameters are comparable in all three countries, which is consistent with the finding that the degree of aspiration adaptation is similar across countries. In addition, we cannot reject the null hypothesis that the estimated coefficients are jointly equal for all countries (see the p -value for the F-Test for the pooling of countries), and this is robust across models, all of which lends credibility to the external validity of the results.

Housing Quality. One concern regarding our interpretation of the results is that the wear and tear on the house may have resulted in a deterioration in housing quality over time. If this is the case, then γ_2 could represent a decline in housing aspirations due to reduced housing quality rather than aspiration adaptation. We examine this possibility by testing whether the effects on housing quality diminish across phases or not. In general, the results reported in Table A12 point to a large and significant gap in housing quality across beneficiaries and non-beneficiaries of the TECHO program (γ_1), but no statistically significant differences in the housing gap between Phase I and Phase II households (γ_2). Figure A3 in Appendix A illustrates these results.

A second robustness check in this regard consists of testing whether the adaptation effects on housing aspirations reported in Table 1 are robust to controlling for follow-up housing quality measures in our main regression. As is shown in Table A13 in Appendix A, the order of magnitude and significance of γ_2 remain the same for all the aspiration indicators, which confirms that any wear and tear on the house had little or no effect on the treated individuals' levels of aspiration adaptation. Interestingly, we observe that γ_1 is somewhat lower than it is for the same estimates in Table 1 (when not controlling for housing quality measures). This should not be surprising, as the ex-post housing quality measures are positively correlated with both the control dummy and the aspiration measures, and their inclusion will therefore generate a downward bias in the estimation of the non-intention-to-treat effect²⁸.

Aspirations and Expectations. Expectations and aspirations are two distinct concepts. An accurate aspiration measure distinguishes what people desire for the future from their beliefs about what will happen in the future. In order to check the construct validity of our aspiration measures, we replicated the question that we had asked about people's aspirations but in terms of "expectations"; in other words, we used the concept of expectations as a nonequivalent variable. Thus, we asked the heads of household the

²⁸As argued by Sen (2002), self-reported measures of aspirations or subjective well-being may diverge from objective indicators, since individuals may not necessarily care about the objective housing quality when evaluating their housing aspirations, but instead about their perception of housing quality, which may or may not be correlated with actual housing quality. Given that the latter depends on each individual's structure of preferences, we do not believe that this should be a concern here, since, even though part of the effect is explained by adaptations in the perception of housing quality over time, it seems implausible that the large and significant adaptation effects that we have observed could be fully explained by this factor.

following question: “Over the next 5 years, you expect you will...” and then offered the very same set of options used to build the aspiration measures:

- (i) Continue living in the same slum under the same conditions (i.e., “no-upgrade”);
- (ii) Continue living in the same slum but get improved housing and own land (i.e., “within-slum-upgrade”);
- (iii) Move to another slum; and
- (iv) Move and get improved housing and own land outside of a slum, with four location alternatives: in the same municipality, in another municipality, in another state, or somewhere else (i.e., “out-of-slum-upgrade”).

As is shown in Table A14 in Appendix A, we do not observe significant differences across treated and untreated units at any point in time, and this is consistent across the four expectation indicators as well as across models.

This evidence is helpful for three reasons. First, the effects on housing aspirations and housing expectations are asymmetric, which lends credibility to the construct validity of our aspiration measures. Second, it rules out potential validity threats associated with untreated units having expectations of receiving a TECHO house in the near future. If that were the case, then the aspiration adaptation might not be due to aspiration mechanisms but to changes in the expectations of being treated in the following round. Indeed, the null effect on housing expectations suggests that the aspirations of the members of the control group are likely to be unaffected by behavioral biases associated with selective perception (Dearborn and Simon (1958)).

Lastly, the null effect on housing expectations may have influenced the decay of housing aspirations. As rational expectation theory suggests, individuals can anticipate what is achievable and what is not. Thus, if untreated individuals did not believe that their higher housing aspirations were going to be met, this could have led them to adopt a self-fulfilling equilibrium of low expectations and, in turn, low aspirations and low housing quality. This raises the possibility that aspirations and expectations may be complementary internal resources, such that, in order for higher aspirations to be sustainable over time, expectations would need to be aligned with aspirations. We examine this hypothesis by testing whether untreated individuals whose housing aspirations were aligned with their housing expectations also adapted over time. We found that this was precisely the case. This suggests that expectations played no role in the aspiration adaptation process^{29,30}.

Happiness Adaptation and Aspirations. Aspirations may also be determined

²⁹In particular, we built a dummy variable that equals 1 if individual housing aspirations were equal to (aligned with) housing expectations and 0 if not, and we did this for each of our four aspiration indicators. Then, we estimated the equation 1 for each one of these four indicators as dependent variables. Our results are consistent with the results shown in Table 1. These results are available upon request.

³⁰A related hypothesis is that aspiration adaptation occurs because poor people are reluctant to think about the future, adopting an avoidance strategy that will shield them against discouragement and inaction. This hypothesis seems to be consistent with the null effects on housing expectations. However, we do not think that this is a plausible explanation since, if that were the case, then untreated units would not have even raised their aspirations, which are, by definition, future-oriented attitudes.

by subjective well-being (SWB). We will first present a brief summary of the happiness adaptation literature and will then discuss a series of robustness tests that we used to determine to what extent SWB is affecting the evolution of aspirations in our experimental setting.

People’s levels of SWB may adapt to higher levels of consumption over time due to the factors described in any one of the three traditional hypotheses presented in the economic and psychological literature on happiness adaptation. The first of these hypotheses deals with the diminishing marginal utility of consumption. According to this line of reasoning, there is a satiation point before which SWB increases with income and after which additional income buys little, if any, extra happiness (the “basic needs hypothesis”, [Veenhoven \(1991\)](#)). According to a second hypothesis, SWB levels may adapt because of relative status effects ([Clark et al. \(2008\)](#), [Haushofer et al. \(2015\)](#), among others), that is, individuals evaluate their level of life satisfaction by comparing their level of wealth with the wealth level of some reference individual or group. In line with this view, increases in income will produce increases in SWB only if the social distance between the individual and the reference group is shortened. A third hypothesis posits that SWB levels may be adapted by hedonic mechanisms ([Frederick and Loewenstein \(1999\)](#)), i.e., SWB may not improve in step with increases in consumption due to a psychological process that attenuates the long-term emotional impact of a favorable or unfavorable change in circumstances; as a result, people’s degree of SWB eventually returns to a stable reference point. While there is a large body of evidence that suggests that people’s degree of SWB actually adapts over time to increases in income and consumption ([Easterlin \(1974, 2005, 2006\)](#), [Di Tella et al. \(2010\)](#), among many others), there is surprisingly little evidence that can be used to determine which of these three mechanisms is the most influential and whether they are consistent across rich and poor groups.

Interestingly, in a previous study ([Galiani et al. \(2018\)](#)), we found that TECHO beneficiaries’ level of satisfaction with their housing quality and their quality of life had improved substantially after 16 months of treatment exposure but that, after on average 8 additional months, 60% of that gain had dissipated, suggesting at least a partial adaptation in the SWB of TECHO beneficiaries. Since our study population is extremely poor and clearly has not yet satisfied its members’ basic housing needs, there were only two plausible hypotheses that could explain this adaptation in SWB: relative status effects or hedonic adaptation. We ran numerous robustness checks in order to test whether the adaptation effects differed across different income subgroups within the slum population and found no differences at all, which would seem to indicate that, at least for the case of housing improvements for slum dwellers, the mechanism through which the subjective well-being adaptation effects are produced is mostly hedonic rather than being related to relative position effects.

Nevertheless, there could be situations in which life satisfaction levels adapt downward as a result of adjustments in aspiration levels rather than because of factors associated with hedonic adaptation; this has been described by [Kahneman \(1999\)](#) as a “satisfaction treadmill”. The initial rise in SWB derived from increases in wealth (in the form of better housing, in this case) may be offset by a rise in housing aspirations that are not met over time. If such a treadmill exists, [Kahneman \(1999\)](#) suggests that “at any level of objective

happiness, people with a higher aspiration level will report themselves less happy and less satisfied than others whose aspirations are lower. [In contrast], if the results for both groups fall on the same regression line, then there is no satisfaction treadmill” (p.16). In essence, then, the question is whether the observed adaptation in the SWB of TECHO beneficiaries is due to the workings of a satisfaction treadmill (in which case TECHO beneficiaries would have adapted their SWB in response to increases in their material aspirations) or if it is a result of a hedonic process (no correlation between SWB and aspiration paths).

There are two empirical facts that appear to rule out the satisfaction treadmill hypothesis. First, the correlation between satisfaction with quality of life (SQL) and our four aspiration measures is generally low, with the greatest correlation being between SQL and “No-upgrade” aspiration (0.145) and the least correlation being between SQL and “Out-of-slum-upgrade” aspiration (−0.003). More importantly, as we have previously shown the housing aspirations of treated units remain unchanged over time, and this is consistent across the four aspiration measures. This suggests that the adaptation of the level of subjective well-being has nothing to do with mechanisms associated with the hypothesized satisfaction treadmill.

Aspiration Adaptation and Happiness. Even though the satisfaction treadmill hypothesis seems to have been ruled out (aspirations do not influence hedonic adaptation), it might still be the case that the causal chain runs in the opposite direction, i.e., while higher aspirations do not reduce happiness, higher levels of happiness could make aspirations more sustainable over time, such that unhappier people (i.e., controls) would be less able (with fewer internal resources) to sustain their aspirations over time³¹. If that were the case, then the mechanism behind aspiration adaptation would not be a lack of the “external” means to attain higher housing standards (such as higher incomes or better access to financial services), but a lack of “internal” resources (such as life satisfaction).

However, in addition to the low correlation between aspirations and SWB discussed in the previous subsection, there is the fact that the SWB of control units remains invariant over the entire period of analysis, and this is consistent across multiple subjective well-being measures. In particular, using the Kolmogorov-Smirnov test, we cannot reject the null hypothesis of equality of distributions for 4 out of 5 satisfaction variables (life satisfaction, and satisfaction with quality of floors, walls, roofs, and protection against water when it rain); in fact, the null hypothesis can be rejected only in the case of “Satisfaction with protection against water when it rains”, indicating that, in general, the control groups for Phases I and II do not differ significantly in their pre- and post-treatment SWB levels³². This suggests that aspiration adaptation is unrelated to changes in internal resources such as subjective well-being.

³¹Indeed, as shown by [Seligman and Nolen-Hoeksema. \(1987\)](#), depression and unhappiness can affect how individuals approach the future, as it may encourage the development of what the authors call a “pessimistic explanatory style” that leads such persons to make negative predictions about the future, which in turn give rise to resignation and indifference.

³²These results are available upon request.

V.3. Structural Estimation

As in [Kimball et al. \(2015\)](#) and [Galiani et al. \(2018\)](#), in this section we present a parsimonious model of aspiration adaptation that allows life events to have both transitory and permanent effects on aspirations. The model assumes that the impulse response of aspirations to an event is indicative of the importance of that event in terms of lifetime aspirations. In particular, we theorize that the rate of aspiration adaptation of untreated units depends on the particular type of event, which in our case corresponds to their exposure to TECHO-beneficiary neighbors. Thus, we estimate the event-specific rate at which the housing aspirational effects derived from indirect exposure to the TECHO program decay over time. Our analysis is restricted to the urban sample, and we consider just two housing aspiration measures: “Within-slum-upgrade” and “Out-of-slum-upgrade”³³. We then test whether housing aspirations return to their baseline level and, if so, after what length of treatment exposure.

We model aspiration adaptation by exponential decay, where the decay rate is estimated simultaneously with the intensity of the initial response of aspirations to the exogenous shock, thus generating three structural parameters in the model: the permanent effect, the transitory effect, and the rate of decay of the shock. Following that structure, our empirical model is given by:

$$Y_{ij} = \alpha + Control_{ij} \times [\beta_P + \beta_T e^{-\delta(t_i - t_0)}] + \beta X_{ij} + c_j + \epsilon_{ij} \quad (2)$$

where Y_{ij} is the aspiration dummy (a binary outcome), $Control_{ij}$ the control dummy, t_i the individual’s months of exposure to the program, t_0 the minimum treatment exposure observed in the sample (13 months for urban households), X_{ij} a set of baseline covariates, and c_j the country fixed effects³⁴. A positive β_T , the transitory effect, suggests that, at least partially, the non-intention-to-treat effect increased soon after the implementation of the TECHO program. Conditional on a positive and significant β_T , if β_P , the permanent effect, is non-distinguishable from zero, then the transitory effect totally disappeared over time and the treatment therefore did not generate a permanent gain in the individual’s housing aspirations. Conversely, a non-zero β_P would be indicative of a partial adaptation in housing aspirations. Finally, the aspiration adaptation rate, δ , indicates the rate at which the transitory effect weakens over time, expressed as a monthly rate.

Our empirical strategy to estimate the structural parameters of the model is simple.

³³The results derived from reduced-form analysis indicate that being indirectly exposed to the TECHO program had no impact at all on the aspirations to “No-upgrade” and “Move-to-another-slum”, so we discarded these outcomes from the structural analysis.

³⁴Since the number of months of exposure to the treatment, t_i , does not vary within slums, controlling for slum fixed effects would impede the identification of δ . Hence, we control for country fixed effects, which incorporate a sufficient variation in time of treatment exposure and thus allows us to capture the average unobservable differences across countries.

We use a non-linear least squares (NLS) estimator given by:

$$\hat{\theta} = \underset{(\theta)}{\operatorname{argmin}} \sum_{i=1}^N [y_i - f(x_i; \theta)]^2 \quad (3)$$

, where $f(x_i; \theta)$ is the nonlinear model, y_i is the endogenous variable, N is the number of observations, and θ the parameter vector. Columns 3 to 5 in Table 6 report the results of estimating equation 2 through MLE, which presents estimates of β_P , β_T , and δ for the two housing aspirations described above.

First, in the case of “within-slum-upgrade” aspirations, we observe a large positive transitory effect. The likelihood of reporting upgrading housing conditions within the slum increased by 30 percentage points, as is indicated by the positive (although non-significant) β_T . The effect is somewhat greater than the one observed in the reduced-form regressions (Table 1), and this is in part because β_T captures the immediate effect after 13 months of treatment exposure, while γ_1 in Table 1 represents the non-intention-to-treat effect in Phase II, i.e., households that have been untreated for an average of 16 months – a sufficient amount of time for some degree of adaptation in the aspiration gains to appear.

Second, the permanent effect is almost zero, which suggests that the adaptation was total. Indeed, we find a positive rate of aspiration adaptation, δ , of about 38% per month. If we linearly project the survival rate of the transitory effect at this rate of depreciation, we find that, after the 28th month of exposure, the effect should be close to zero. Exposure in the sample ranges from 13 to 30 months. Therefore, at this rate of aspiration adaptation, it is not surprising to observe a null permanent effect for the period under analysis. Note that the analysis follows the inverted trend for the case of “out-of-slum-upgrade” aspirations, which exhibits a 10% reduction after 13 months of treatment exposure, an effect that is transitory and is fully adapted at a 16% monthly rate over the following 17 months.

The adaptation sequence is illustrated in Figure A4, which maps the likelihood of reporting each type of housing aspiration for the months of exposure to the treatment. We do this separately for treatment and control households. Both graphs show a reduction in the distance between the treatment and control groups as treatment exposure increases, with this difference narrowing to almost zero by the 28th month. While the structural estimate of the adaptation effect is not statistically significant, we hypothesize that, if we had had access to a larger window of time of treatment exposure, including months 1 to 12, we would probably have observed a larger decay in housing aspirations, with the greater decline being explained primarily by a greater non-intention-to-treat effect in the period immediately after the treatment. A natural explanation for why the aspiration to upgrade within the slum shows a stronger adaptation than the aspiration to upgrade out of a slum is that “out-of-slum-upgrade” aspirations are not the only substitute of “within-slum-upgrade” aspirations. Indeed, not all individuals that have abandoned “within-slum-upgrade” aspirations are now aspiring to upgrade housing out of a slum. Some of them have felt frustrated and no longer aspire to improve their housing conditions or just aspire to move to another slum.

Finally, Figure A5 replicates the same exercise but divides the corresponding pop-

ulation into income subgroups. Consistent with the reduced-form analysis described in the previous section, we observe that the “within-slum-upgrade” aspirations of above-the-median untreated households (those with a lower level of resource constraints) are adapted much less than those of their poorer counterparts. Interestingly, until the 18th month of exposure, the richer neighbors adapted their aspirations much faster than the poorer ones. However, it seems that from the 18th month onward, the richer untreated units were able to moderate the decay in aspirations, which remained above the treatment mean during the entire period of analysis. In contrast, the “within-slum-upgrade” aspirations of low-income untreated households continued to decrease and, after the 22nd month, their aspirations fell to a lower level than the aspirations of the low-income treated households. All in all, this illustrates the significant role that resource constraints may play in determining the sustainability of aspirations over time.

Cumulative Impact. We consider the cumulative impact of an event (“the area under the curve” associated with the aspiration response to an event) and measure the specific proportions of that area that can be attributed to permanent and transitory effects, respectively. In particular, for an individual with an annual mortality risk d and an interest rate r , the total gains, i.e. the total “area under the curve” can be calculated as:

$$\beta_{cumm.} = \int_{t_0}^t (\beta_P e^{-(d+r)(s-t_0)} + \beta_T e^{-(d+r+\delta)(s-t_0)}) \partial s = \frac{\beta_P}{d+r} + \frac{\beta_T}{d+r+\delta} \quad (4)$$

The advantage of this formulation is that it gives a single statistic that can be used to compare events in terms of their aspirational importance. This statistic also allows these results to be compared with static estimates in the existing literature, given that both are measures of a cumulative aspiration effect.

Table 6 presents these estimates for our experiment. Columns under the heading “Aspiration Gains Area” show the areas corresponding to permanent, transitory, and total gains, respectively. The last column shows the pooled estimate of the non-intention-to-treat effect, i.e., the raw effect using ordinary least squares (OLS) estimates. Our estimation of d is based on the actuarial mortality rates by age, gender, and country published by the World Health Organization (WHO) for the years in which the follow-up survey was conducted, which are 0.04 in El Salvador, 0.01 in Uruguay, and 0.02 in Mexico. For r , we assume a conventional 5% interest rate.

Consistent with our estimates of β_P , β_T , and δ , the permanent gains are not significant for our indicator of “within-slum-upgrade” aspirations, with the positive and significant total gains mostly explained by the transitory effect. Second, the OLS pooled coefficient is positive and highly significant, a result that contradicts the almost null and insignificant permanent effect found in our NLS estimation. Analogously, while “out-of-slum-upgrade” aspirations show insignificant permanent, transitory, and total gains, the pooled OLS coefficient is negative at the 10% level of significance. These results suggest that studying adaptation of aspirations over time is crucial for a rigorous interpretation of life-event effects on aspiration outcomes in the long run.

VI. Conclusion

Hope operates as a capability in Sen’s sense of the term, as it can fuel the aspirations of the poor, which in turn can encourage a future-oriented behavior that fosters development outcomes. However, psychology and economics are still very far from having an evidence base for all the possible implications of hope in terms of economic development and more research should be done to understand this link. In fact, little is known about the extent to which poor populations can sustain higher aspirations over time, or about whether aspirations alone are sufficient to mobilize forward-looking actions that allow the poor to exit poverty.

In this paper, we rely on a large-scale, multi-country field experiment to test the effect that a major in situ housing intervention for slum dwellers in El Salvador, Mexico, and Uruguay has on the housing aspirations and housing investments of non-beneficiary neighbors who have not yet improved their housing conditions. By exploiting plausible exogenous variation in the length of exposure to the treatment, our experimental design allows us to determine if any significant degree of adaptation in non-beneficiaries’ housing aspirations takes place over time. To the best of our knowledge, this is the first paper to examine aspiration adaptation on the part of poor populations and the first to use exogenous sources of variation for this purpose.

After 16 months of indirect treatment exposure, we find that the control group’s housing quality is significantly lower than that of the treatment group, and no other noticeable material gaps across groups are observed. The program is effective at improving housing conditions, with no effects found on incomes, savings, assets, or labor supply. At the same time, aspirations to upgrade housing conditions within the slum are significantly higher among control units than they are in the treatment group, suggesting that non-beneficiary households internalized the control-treatment housing gap and thus now aspire to “keep-up” with the treatment Joneses’. However, after 8 additional months the aspirational difference totally disappeared, and this effect is completely explained by the adaption of the control groups aspirations, as the treatment group’s housing aspirations remain unchanged over the same period of analysis. This evidence is consistent across the three country experiments as well as for different measures of aspirations, which lends credibility to the external and construct validity of the results. The aspiration adaptation result suggests that if the poor are trapped in an aspiration failure equilibrium, this may not be because they lack the capacity to aspire to higher living standards ([Appadurai \(2004\)](#), [Dalton et al. \(2016\)](#)) but because they are unable to sustain higher aspirations, which tend to quickly adapt downward over time.

The results are consistent with an aspiration adaptation model that follows the basic structure proposed by [Kimball et al. \(2015\)](#) for studying the hedonic treadmill. Using an NLS estimator we find that the housing aspirations effects observed for untreated units declined in proportion to the number of months of indirect exposure to the treatment, and became indistinguishable from zero after 28 months, corresponding to a rate of aspiration adaptation of 38% per month. This is large compared to structural estimates of the hedonic adaptation rate experienced by comparable individuals in relation to housing improvements such as those provided by the TECHO program, which has been found to

be roughly 20% per month by [Galiani et al. \(2018\)](#). This suggests that the dynamics of the aspirations of poor individuals who seek to improve their material conditions but are unable to do so may fluctuate more sharply than the fluctuations in subjective well-being experienced by those whose basic needs have been partially satisfied. Importantly, this may be the case even though aspirations and subjective well-being are generated through independent processes. Indeed, we show that the observed adaptation in the aspirations of untreated households had nothing to do with hedonic mechanisms, as the levels of subjective well-being of untreated units remain constant over the period of analysis. Analogously, the hedonic adaptation observed in the treatment group by [Galiani et al. \(2018\)](#) cannot be explained by the satisfaction treadmill mechanisms suggested by [Kahneman \(1999\)](#), since the treatment group’s housing aspirations did not undergo any change at all during the very same period of analysis.

However, our results are valid only for non-treated urban slum dwellers, who are confronted with moderate housing gaps with respect to their treated neighbors. In contrast, the housing aspirations of untreated rural slum dwellers, whose economic status and housing conditions differ more from those of their treated neighbors, did not change at all. We conclude that the moderate control-treatment housing gap confronted by urban controls encouraged them to aspire to replicate the housing conditions of their treatment-group neighbors, while the seemingly insurmountable housing gap faced by rural households thwarted their housing aspirations. This result is consistent with the theoretical work of [Ray \(1998, 2006\)](#) and [Genicot and Ray \(2017\)](#) and reinforces the hypothesis that aspiration formation processes behave non-convexly over aspiration gaps.

Finally, and consistent with the aspiration adaptation result, we find that differences in housing quality across experimental groups remain unchanged over time, and no effects are found either on housing investment efforts or on external constraints such as income, savings, asset values, or labor supply. Overall, we conclude that in excessively resource-constrained environments such as those found in informal slums, significant changes in the material conditions experienced by reference-group neighbors can encourage the poor to aspire to material conditions that they are not capable of attaining and that, in these circumstances, aspiration gains may quickly adapt downward. Since aspirations are not necessarily fixed over time, we argue that higher aspirations are not a sufficient condition for prompting forward-looking behavior among poor populations.

Our evidence may be relevant for policymakers in situations marked by sharp inequalities, where aspiration gaps will naturally be larger and consequently costlier to narrow or close. In such contexts, policies designed to stimulate forward-looking behavior simply by raising the aspirations of poor persons without helping to provide them with the external or internal means required to satisfy those aspirations are likely doomed to fail. As long as material gains do not structurally alter the relative position of poor individuals with respect to their reference groups, aspiration gaps are likely to continue to appear to be insurmountable. As [Genicot and Ray \(2017\)](#) argue, from a general equilibrium perspective, tackling poverty traps will not only require improvements in the internal capacities of poor populations, but must also promote those improvements by generating a proportionally higher growth rate relative to richer groups. Following this logic, household-level social programs that potentially generate large unintended inequalities among neighbors should

attempt to guarantee that non-treated neighbors are not being negatively affected by such gaps in terms of their aspirations and forward-looking behavior. If so, then neighborhood-level interventions seem to be a suitable substitute as benefits would be equally distributed across neighbors and thus potential effects on within-neighborhood inequality are neutralized.

In this respect, what is needed in order to tackle behavioral poverty traps is not to find means of indiscriminately raising the aspirations of poor populations, but rather to find means of fostering the setting of achievable goals for poor populations, thus averting aspiration adaptation and frustration. This is consistent with recent literature, which argues that lowering the aspirations of low-income students to more reachable levels will reduce the likelihood of their dropping out of school in France (see [Goux et al. \(2014\)](#)). Furthermore, reducing the costs of risk-taking promises to be an effective policy for breaking down aspirational poverty traps. A good example is provided by [Bryan et al. \(2014\)](#), who randomly assigned a US\$8.50 incentive to households in rural Bangladesh to prompt them to temporarily out-migrate during the pre-harvest lean season. The authors find that the incentive induced 22% of the households to send out a seasonal migrant; consequently, their consumption level rose significantly, and treated households were around 10 percentage points more likely to re-migrate between 1 and 3 years after the incentive was removed. Their results suggest that very poor individuals require individual-specific learning opportunities in order to take risky, poverty-escaping action. This is an experience that has generally been very rare among poor populations because risk-taking that results in failure can be extremely costly given their situation. Hence, small subsidies that compensate for the potential costs of risk-taking by the poor may encourage them to acquire valuable learning experiences (with the attendant wins and losses) that will reduce those risks in the long run and thus enable them, over time, to aspire to progressively higher living standards.

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Table 1: Adaptation in Housing and Location Aspirations

Dependent Variable	Urban						Rural						All					
	Model 1		Model 2		Model 1		Model 2		Model 1		Model 2		Model 1		Model 2			
	Cont. γ_1	Cont. \times Phase I γ_2	Cont. γ_1	Cont. \times Phase I γ_2	Follow-Up Treat. Mean	Cont. \times Phase I γ_2	Cont. γ_1	Cont. \times Phase I γ_2	Follow-Up Treat. Mean	Cont. \times Phase I γ_2	Cont. γ_1	Cont. \times Phase I γ_2	Follow-Up Treat. Mean	Cont. \times Phase I γ_2	Cont. γ_1	Cont. \times Phase I γ_2		
“No-upgrade”	0.01 (0.04)	-0.08 (0.06)	0.01 (0.04)	-0.07 (0.06)	0.59 (0.49)	0.04 (0.04)	0.00 (0.07)	0.04 (0.04)	-0.01 (0.07)	0.46 (0.50)	0.03 (0.03)	-0.05 (0.04)	0.03 (0.03)	0.03 (0.03)	-0.04 (0.04)			
p-value ($\gamma_1 + \gamma_2 = 0$)	0.16		0.17			0.52		0.55		0.57			0.45					
“Within-slum-upgrade”	0.09*** (0.03)	-0.11** (0.05)	0.09** (0.03)	-0.11** (0.05)	0.28 (0.45)	-0.03 (0.04)	0.01 (0.06)	-0.03 (0.04)	0.01 (0.06)	0.22 (0.41)	0.03 (0.03)	-0.05 (0.04)	0.03 (0.03)	0.03 (0.03)	-0.05 (0.04)			
p-value ($\gamma_1 + \gamma_2 = 0$)	0.65		0.59			0.59		0.63		0.48			0.45					
“Move-to-another-slum”	0.01 (0.01)	0.02 (0.02)	0.01 (0.01)	0.02 (0.02)	0.01 (0.10)	-0.01 (0.01)	0.02 (0.02)	-0.01 (0.01)	0.02 (0.02)	0.01 (0.11)	0.00 (0.01)	0.02 (0.01)	0.00 (0.01)	0.00 (0.01)	0.02 (0.01)			
p-value ($\gamma_1 + \gamma_2 = 0$)	0.14		0.14			0.42		0.43		0.10			0.11					
“Out-of-slum-upgrade”	-0.11*** (0.04)	0.17*** (0.06)	-0.11*** (0.04)	0.17*** (0.06)	0.12 (0.33)	0.00 (0.02)	-0.02 (0.04)	0.00 (0.02)	-0.02 (0.04)	0.31 (0.46)	-0.05** (0.02)	0.07* (0.04)	-0.05** (0.02)	0.07** (0.02)	0.07** (0.04)			
p-value ($\gamma_1 + \gamma_2 = 0$)	0.23		0.23			0.53		0.54		0.50			0.49					

Slum Fixed Effects
Baseline Covariates

Note: Each row represents a separate dependent variable. The analysis is divided into three groups: urban households, rural households, and all households. The first column reports the mean and standard deviation of the dependent variable for the treatment group measured at follow-up. The next two columns, under the heading Model 1, report the results of a regression of the dependent variable on Control Assignment and Control Assignment interacted with Phase I plus slum fixed effects. Reports are the estimated coefficients and robust standard errors. The last two columns, Model 2, additionally control for the household head's Years of Schooling, Gender, Age, Years living in the slum, as well as the value of household assets per capita, monthly income per capita, and whether the household's head aspires to upgrade housing quality materials *in-situ*, 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. Finally, we report the p-values of F-tests of the null hypothesis that $\gamma_1 + \gamma_2 = 0$ for each model. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table 2: Income, Assets, and Labor Supply - Urban Only

Dependent Variable	Follow-Up Treat. Mean	Model 1		Model 2	
		Cont.	Cont. × Phase I	Cont.	Cont. × Phase I
		γ_1	γ_2	γ_1	γ_2
Assets Value Per Capita (USD)	74.89 (163.24)	0.89 (12.48)	-25.46 (21.61)	2.19 (12.30)	-25.84 (20.31)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.16		0.15	
Monthly Income Per Capita (USD)	77.40 (115.15)	-1.31 (9.76)	19.38 (19.41)	-1.99 (9.65)	20.86 (19.51)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.28		0.26	
Hours worked last week by HH	40.78 (19.23)	0.21 (1.96)	-0.05 (3.07)	-0.05 (2.00)	-0.50 (3.04)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.95		0.81	
Hours worked last week by Spouse	36.97 (20.08)	3.49 (2.91)	-5.95 (4.40)	3.26 (2.92)	-5.49 (4.42)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.46		0.50	
If any household's member have savings	0.03 (0.16)	0.02 (0.01)	0.00 (0.02)	0.02 (0.02)	0.00 (0.02)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.35		0.42	
Slum Fixed Effects		Yes		Yes	
Baseline Covariates		No		Yes	

Note: Only urban households are considered. Each row represents a separate dependent variable. Monetary variables in US dollars as of June 2007. In the case of monetary variables, observations over the 99th percentile were excluded. The first column reports the mean and standard deviation of the dependent variable for the treatment group measured at follow-up. The next two columns, under the heading Model 1, report the results of a regression of the dependent variable on Control Assignment and Control Assignment interacted with Phase I plus slum fixed effects. Reports are the estimated coefficients and robust standard errors. The last two columns, Model 2, additionally control for the household head's Years of Schooling, Gender, Age, Years living in the slum, as well as the value of household assets per capita, monthly income per capita, and whether the household's head aspires to upgrade housing quality materials *in-situ*, 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. Finally, we report the p-values of F-tests of the null hypothesis that $\gamma_1 + \gamma_2 = 0$ for each model. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table 3: Housing Investment - Urban Only

Dependent Variable	Follow-Up Treat Mean	Model 1		Model 2	
		Control	Control × Phase I	Control	Control × Phase I
		γ_1	γ_2	γ_1	γ_2
If invested on housing materials during the last 12 months	0.40 (0.49)	0.04 (0.04)	-0.02 (0.08)	0.05 (0.04)	-0.03 (0.08)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.74		0.75	
If invested on access to water during the last 12 months	0.09 (0.28)	-0.02 (0.03)	0.02 (0.05)	-0.02 (0.03)	0.02 (0.05)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.97		0.96	
If have access to water in terrain	0.81 (0.39)	-0.03 (0.03)	0.05 (0.05)	-0.03 (0.03)	0.05 (0.05)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.52		0.49	
If invested on sanitation during the last 12 months	0.08 (0.27)	-0.01 (0.03)	0.03 (0.05)	-0.01 (0.03)	0.03 (0.05)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.66		0.64	
If have access to own bathroom	0.69 (0.46)	-0.02 (0.04)	0.03 (0.06)	-0.02 (0.04)	0.03 (0.06)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.80		0.75	
If invested on electricity during the last 12 months	0.12 (0.32)	-0.01 (0.03)	0.00 (0.05)	-0.01 (0.03)	-0.01 (0.06)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.86		0.68	
If have access to electricity	0.90 (0.31)	0.00 (0.02)	0.04 (0.03)	0.00 (0.02)	0.03 (0.03)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.17		0.17	
Amount invested on housing during the last 12 months	68.29 (226.71)	-10.45 (12.34)	-13.50 (28.46)	-10.30 (12.53)	-15.62 (28.53)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.35		0.31	
Slum Fixed Effects		Yes		Yes	
Baseline Covariates		No		Yes	

Note: Only urban households are considered. Each row represents a separate dependent variable. Monetary variables in US dollars as of June 2007. In the case of monetary variables, observations over the 99th percentile were excluded. The first column reports the mean of the dependent variable for the treatment group measured at follow-up. The next two columns, under the heading Model 1, report the results of a regression of the dependent variable on Control Assignment and Control Assignment interacted with Phase I plus slum fixed effects. Reports are the estimated coefficients and robust standard errors. The last two columns, under the heading Model 2, additionally control for the household head's years living in the slum, years of schooling, gender and age, as well as the value of household assets per capita and monthly income per capita, and whether the household's head aspires to upgrade housing quality materials *in-situ*, all of which were 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. Finally, we report the p-values of the F-tests of the null hypothesis $\gamma_1 + \gamma_2 = 0$. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table 4: Adaptation in Housing and Location Aspirations, by Income Status - Urban Only

Dependent Variable	High Income Status ($> p50$)					Low Income Status ($\leq p50$)				
	Follow-Up Treat. Mean	Model 1		Model 2		Follow-Up Treat. Mean	Model 1		Model 2	
		Cont.	Cont. \times Phase I	Cont.	Cont. \times Phase I		Cont.	Cont. \times Phase I	Cont.	Cont. \times Phase I
		γ_1	γ_2	γ_1	γ_2		γ_1	γ_2	γ_1	γ_2
“No-upgrade”	0.33 (0.47)	-0.01 (0.05)	-0.06 (0.08)	-0.03 (0.05)	-0.05 (0.08)	0.36 (0.48)	0.04 (0.06)	-0.09 (0.09)	0.06 (0.06)	-0.12 (0.09)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.22		0.19			0.53		0.39	
“Within-slum-upgrade”	0.16 (0.37)	0.10** (0.04)	-0.08 (0.07)	0.11** (0.04)	-0.08 (0.07)	0.16 (0.37)	0.08 (0.06)	-0.18** (0.07)	0.06 (0.06)	-0.15** (0.07)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.64		0.59			0.04		0.07	
“Move-to-another-slum”	0.01 (0.11)	0.03 (0.02)	0.00 (0.03)	0.03 (0.02)	0.00 (0.03)	0.02 (0.14)	-0.02 (0.01)	0.04 (0.03)	-0.02 (0.02)	0.04 (0.03)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.12		0.13			0.35		0.36	
“Out-of-slum-upgrade”	0.49 (0.50)	-0.11** (0.05)	0.13* (0.08)	-0.11** (0.05)	0.13 (0.08)	0.46 (0.50)	-0.10* (0.06)	0.22** (0.09)	-0.10* (0.06)	0.23** (0.09)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.76		0.75			0.10		0.09	
Slum Fixed Effects		Yes		Yes			Yes		Yes	
Baseline Covariates		No		Yes			No		Yes	

Note: Only urban sample is considered. Each row represents a separate dependent variable. The analysis is divided into two income sub-groups defined by whether the baseline monthly income per capita is below or above the median in the income distribution of the urban sample (median equal to US\$39). The first column reports the mean and standard deviation of the dependent variable for the treatment group measured at follow-up. The next two columns, under the heading Model 1, report the results of a regression of the dependent variable on Control Assignment and Control Assignment interacted with Phase I plus slum fixed effects. Reports are the estimated coefficients and robust standard errors. The last two columns, Model 2, additionally control for the household head’s Years of Schooling, Gender, Age, Years living in the slum, as well as the value of household assets per capita, monthly income per capita, and whether the household’s head aspires to upgrade housing quality materials *in-situ*, 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. Finally, we report the p-values of F-tests of the null hypothesis that $\gamma_1 + \gamma_2 = 0$ for each model. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table 5: Housing Investment, by Income Status - Urban Only

Dependent Variable	Follow-Up Treat. Mean	High Income Status ($> p50$)				Low Income Status ($\leq p50$)				
		Model 1		Model 2		Model 1		Model 2		
		Cont.	Cont. \times Phase I	Cont.	Cont. \times Phase I	Cont.	Cont. \times Phase I	Cont.	Cont. \times Phase I	
		γ_1	γ_2	γ_1	γ_2	γ_1	γ_2	γ_1	γ_2	
If invested on housing quality during the last 12 months	0.21 (0.41)	0.00 (0.04)	0.06 (0.07)	0.01 (0.04)	0.06 (0.07)	0.26 (0.44)	0.05 (0.05)	-0.11 (0.09)	0.05 (0.06)	-0.11 (0.09)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.28		0.26			0.46		0.44	
If invested on access to water during the last 12 months	0.06 (0.23)	-0.02 (0.02)	-0.01 (0.04)	-0.01 (0.02)	-0.01 (0.04)	0.04 (0.21)	0.00 (0.05)	0.05 (0.07)	-0.01 (0.03)	0.05 (0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.42		0.45			0.29		0.29	
If have access to water in terrain	0.82 (0.39)	-0.02 (0.04)	0.02 (0.06)	-0.02 (0.04)	0.01 (0.06)	0.81 (0.39)	-0.05 (0.05)	0.07 (0.07)	-0.05 (0.04)	0.07 (0.07)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.99		0.84			0.64		0.70	
If invested on sanitation during the last 12 months	0.05 (0.22)	-0.02 (0.03)	0.04 (0.04)	-0.01 (0.03)	0.04 (0.04)	0.04 (0.21)	0.02 (0.03)	-0.01 (0.05)	0.01 (0.03)	0.00 (0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.45		0.41			0.91		0.83	
If have access to own bathroom	0.65 (0.48)	0.04 (0.05)	0.03 (0.08)	0.05 (0.05)	0.03 (0.08)	0.73 (0.45)	-0.11* (0.06)	0.07 (0.09)	-0.10 (0.06)	0.05 (0.09)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.31		0.25			0.52		0.47	
If invested on electricity during the last 12 months	0.05 (0.22)	0.02 (0.03)	0.02 (0.04)	0.02 (0.03)	0.01 (0.04)	0.09 (0.29)	-0.05 (0.03)	0.02 (0.05)	-0.05* (0.03)	-0.02 (0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.24		0.36			0.13		0.13	
If have access to electricity	0.89 (0.31)	0.03 (0.03)	0.04 (0.04)	0.03 (0.03)	0.04 (0.04)	0.90 (0.30)	-0.05 (0.04)	0.08 (0.06)	-0.05 (0.04)	0.08 (0.06)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.04		0.04			0.52		0.54	
Amount invested on housing during the last 12 months	71.80 (271.76)	5.93 (15.17)	-13.79 (54.85)	6.97 (15.63)	-19.60 (54.77)	83.71 (280.13)	-29.05 (18.96)	20.74 (47.54)	-24.36 (20.43)	18.42 (49.27)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.88		0.81			0.85		0.89	
Slum Fixed Effects		Yes		Yes			Yes		Yes	
Baseline Covariates		No		Yes			No		Yes	

Note: Only urban sample is considered. Each row represents a separate dependent variable. The analysis is divided into two income sub-groups defined by whether the baseline monthly income per capita is below or above the median in the income distribution of the urban sample (median equal to US\$39). The first column reports the mean and standard deviation of the dependent variable for the treatment group measures at follow-up. The next two columns, under the heading Model 1, report the results of a regression of the dependent variable on Control Assignment and Control Assignment interacted with Phase I plus slum fixed effects. Reports are the estimated coefficients and robust standard errors. The last two columns, Model 2, additionally control for the household head's Years of Schooling, Gender, Age, Years living in the slum, as well as the value of household assets per capita, monthly income per capita, and whether the household's head aspires to upgrade housing quality materials *in-situ*, 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. Finally, we report the p-values of F-tests of the null hypothesis that $\gamma_1 + \gamma_2 = 0$ for each model. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

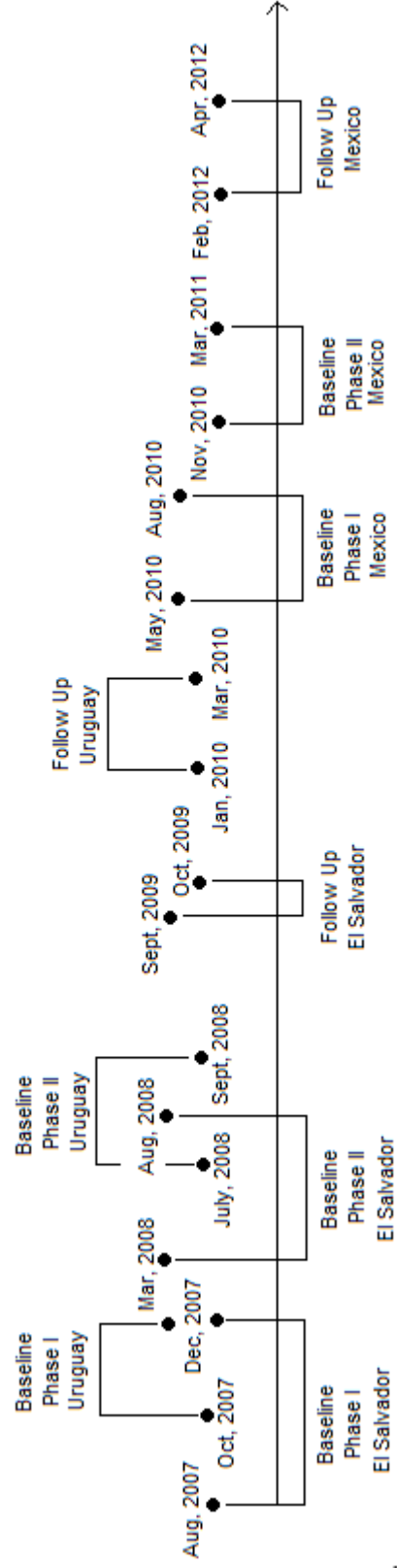
Table 6: Structural Estimation - Urban Only

Dependent Variable	NLS Estimates				Aspiration Gains Area			OLS Estimate
	Follow-Up Treat. Mean	Permanent Effect	Transitory Effect	Aspiration Adaptation Rate	Permanent Gains (PG)	Transitory Gains (TG)	Total Gains	Pooled Coefficient
		β_P	β_T	δ	$\frac{\beta_P}{\eta+r}$	$\frac{\beta_T}{\eta+r+\delta}$	PG+TG	
“Within-slum-upgrade”	0.16 (0.37)	0.01 (0.03)	0.30 (0.27)	0.38 (0.31)	0.15 (0.48)	0.66** (0.33)	0.82* (0.44)	0.06** (0.02)
“Out-of-slum-upgrade”	0.48 (0.50)	-0.02 (0.10)	-0.09 (0.09)	0.16 (0.54)	-0.21 (1.46)	-0.38 (1.05)	-0.59 (0.54)	-0.05* (0.03)

Note: Only urban households are considered. Each row represents a separate dependent variable. The first column reports the control mean at follow-up round and its standard deviation. The next three columns under the heading of NLS Estimates report the structural parameter estimates of the NLS regression $Y_{ij} = \alpha + Control_{ij} \times [\beta_P + \beta_T e^{-\delta(t_i - t_0)}] + c_j + \epsilon_{ij}$, with t_i the months of exposure to the program enjoyed by individual i , t_0 the minimum treatment exposure (13 months), and c_j the country fixed effects. δ is expressed as a monthly rate. Reports are the estimated coefficients and robust standard errors. The next three columns, under the heading Aspiration Gains Area, report the area under the permanent, transitory, and total effects, respectively. Permanent Effect Area is calculated as β_P divided by the sum of the mortality rate, d , which is equal to 0.021, and the interest rate, r , which is assumed to be 0.05. Transitory Effect Area is calculated as β_T divided by the sum of d , r , and the aspiration adaptation rate, δ . Total Area is the sum of the permanent and transitory effects areas. Standard errors of the estimated areas calculated by the Delta Method are reported in parenthesis. Finally, the last column reports the pooled linear regression coefficient of the assigned-to-control effect and its associated robust standard error. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Appendix A. Tables and Figures (For Online Publication)

Figure A1: Timeline of Intervention and Surveys



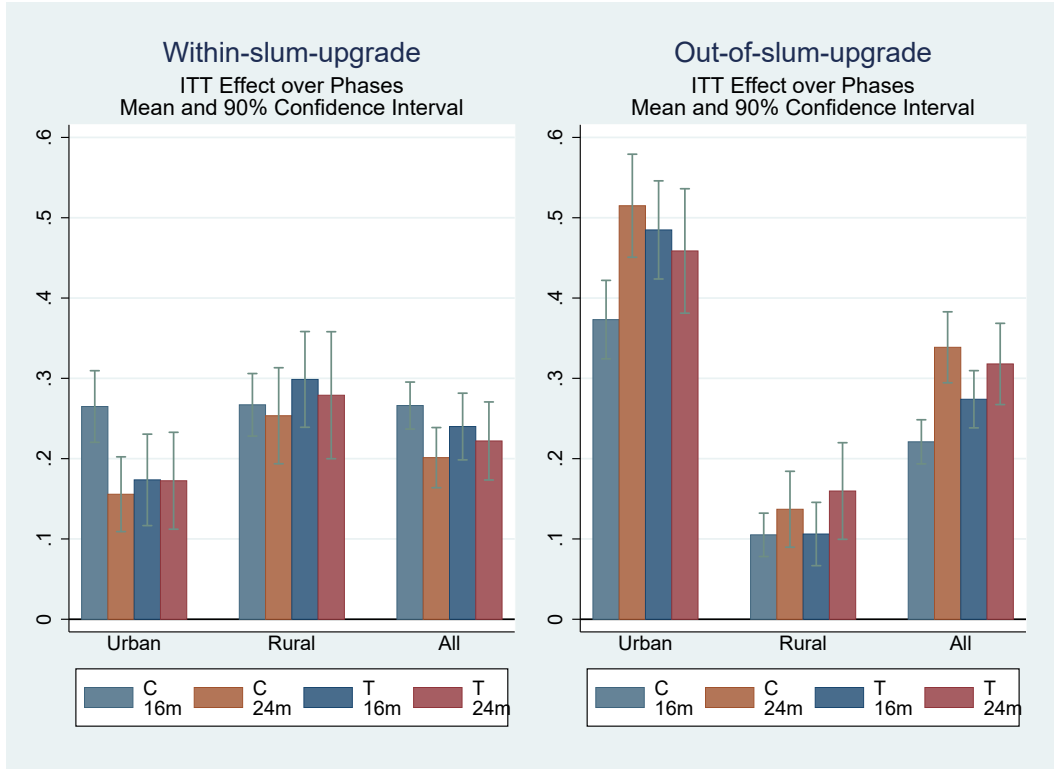


Figure A2: Treatment Effects on Aspirations, by Phase

For each aspiration variable and zone, the first bar is the control mean at Phase II (16 months of exposure, on average) at follow-up, while the second bar represents the control mean at Phase I (24 months of exposure, on average) and is estimated as the mean of the control group in Phase II plus the non-intention-to-treat effect for the Phase I group. Third and fourth bars replicate the same exercise but for treated units. The difference between the first bar and the third bar is the non-intention-to-treat effect on the housing aspiration for the Phase II group. The difference between the second bar and the fourth bar is the non-intention-to-treat effect on the housing aspiration for the Phase I group. Then, the double difference between the first and third bars, on the one side, and the second and fourth bars, on the other side, is therefore the extent of aspirations adaptation.

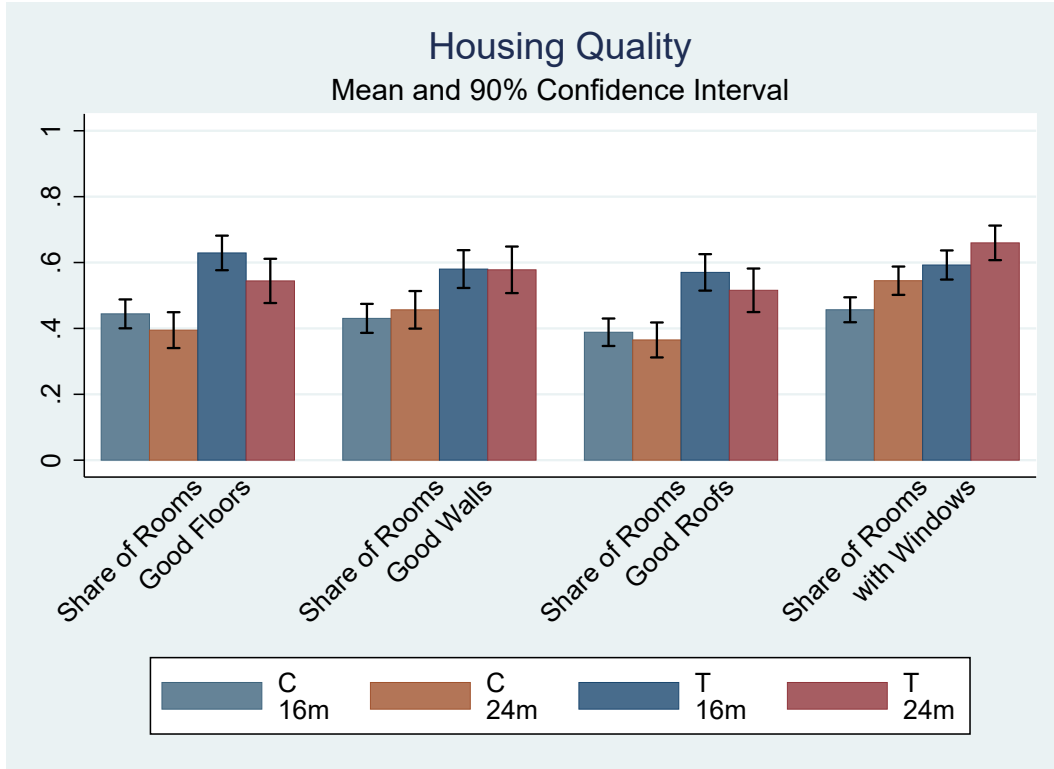


Figure A3: Treatment Effects on Housing Quality, by Phase - Urban Only

For each housing variable, the first bar is the control mean at Phase II (16 months of exposure, on average) at follow-up, while the second bar represents the control mean at Phase I (24 months of exposure, on average) and is estimated as the mean of the control group in Phase II plus the non-intention-to-treat effect for the Phase I group. Third and fourth bars replicate the same exercise but for treated units. The difference between the first bar and the third bar is the non-intention-to-treat effect on the housing quality for the Phase II group. The difference between the second bar and the fourth bar is the non-intention-to-treat effect on the housing quality for the Phase I group. Then, the double difference between the first and third bars, on the one side, and the second and fourth bars, on the other side, is the extent of adaptation in housing quality.

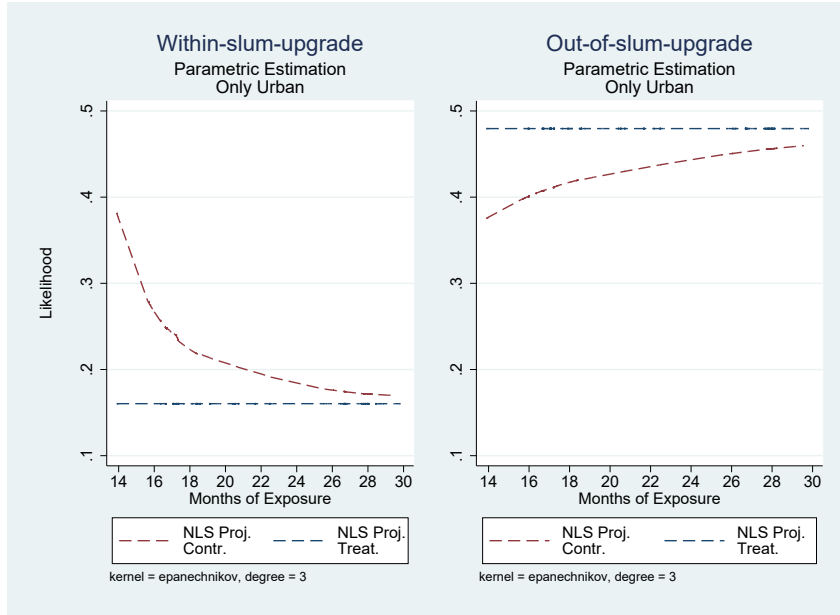


Figure A4: Aspirations Adaptation - NLS Estimation - Urban Only

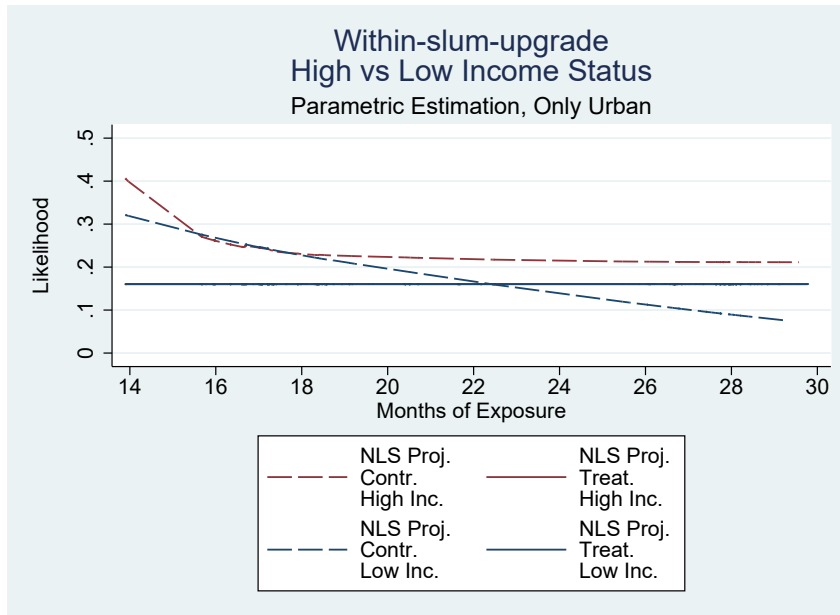


Figure A5: Aspirations Adaptation by Income Status - NLS Estimation - Urban Only

Table A1: Timeline of Intervention and Surveys

	Urban			Rural			All		
	Phase 1 Constr.	Phase 2 Constr.	Follow-Up Survey	Phase 1 Constr.	Phase 2 Constr.	Follow-Up Survey	Phase 1 Constr.	Phase 2 Constr.	Follow-Up Survey
<i>El Salvador</i>									
Average Exposure	25 months	17 months		25 months	17 months		25 months	17 months	
HHs Sample Size	89	52	141	199	316	515	288	368	656
Number of Slums	2	3	5	6	12	18	8	15	23
<i>Uruguay</i>									
Average Exposure	27 months	17 months		-	-		27 months	17 months	
HHs Sample Size	353	375	728	-	-	-	353	375	728
Number of Slums	6	6	12	-	-	-	6	6	12
<i>Mexico</i>									
Average Exposure	19 months	16 months		20 months	14 months		20 months	15 months	
HHs Sample Size	93	155	248	193	385	578	286	540	826
Number of Slums	5	5	10	10	19	29	15	24	39
<i>All countries</i>									
Average Exposure	25 months	17 months		23 months	15 months		24 months	16 months	
HHs Sample Size	535	582	1,117	392	701	1,093	927	1,283	2,210
Number of Slums	13	14	27	16	31	47	29	45	74

Table A2: Sample Size, Attrition and Compliance

	Phase I			Phase II			Phase I vs Phase II All			Phase I vs Phase II Only Treatments		
	Mean Treat.	Mean Control	Diff.	Mean Treat.	Mean Control	Diff.	Mean Phase I	Mean Phase II	Diff.	Mean Phase I	Mean Phase II	Diff.
Panel A. Full Sample												
Baseline Households Sample	653	342		703	675		995	1,378		653	703	
Follow-Up Households Sample	611	316		658	625		927	1,283		611	658	
Attrition Rate	0.06 (0.01)	0.08 (0.01)	-0.01 (0.02)	0.06 (0.01)	0.07 (0.01)	-0.01 (0.01)	0.07 [0.01]	0.07 [0.01]	0.00 [0.01]	0.06 [0.01]	0.06 [0.01]	0.00 [0.02]
Compliance Rate	0.88	0.99		0.86	1.00		0.92	0.93		0.88	0.86	
Panel B. Urban Sample												
Baseline Households Sample	393	189		331	283		582	624		393	331	
Follow-Up Households Sample	365	170		310	272		535	582		365	310	
Attrition Rate	0.07 (0.01)	0.10 (0.02)	-0.03 (0.03)	0.06 (0.01)	0.07 (0.02)	-0.01 (0.02)	0.08 [0.02]	0.07 [0.01]	0.01 [0.02]	0.07 [0.01]	0.06 [0.02]	0.01 [0.02]
Compliance Rate	0.88	0.99		0.83	1.00		0.91	0.91		0.88	0.83	
Panel C. Rural Sample												
Baseline Households Sample	260	153		372	382		413	754		260	372	
Follow-Up Households Sample	246	146		348	353		392	701		246	348	
Attrition Rate	0.05 (0.01)	0.05 (0.02)	0.01 (0.02)	0.07 (0.01)	0.08 (0.01)	-0.01 (0.02)	0.05 [0.01]	0.07 [0.01]	-0.02 [0.02]	0.05 [0.02]	0.06 [0.01]	-0.01 [0.02]
Compliance Rate	0.87	1.00		0.89	1.00		0.92	0.94		0.87	0.89	

Note: This table reports means and differences in means between experimental groups, by phase and zone. For Phase I and Phase II columns, robust standard errors are reported in parenthesis. For Phase I vs Phase II columns, standard errors clustered at slum level are reported in brackets. Compliance rate refers to the share of households assigned to treatment that indeed received TECHO houses and to the share of households in the control group that indeed did not receive TECHO houses. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A3: Baseline Balance Within and Between Phases - Full Sample

Dependent Variable	Phase I			Phase II			Phase I vs Phase II All			Phase I vs Phase II Only Treatments		
	Treat.	Control	Diff.	Treat.	Control	Diff.	Phase I	Phase II	Diff.	Phase I	Phase II	Diff.
Years living in the slum	9.82 (0.66)	11.19 (0.89)	0.26 (0.91)	12.80 (0.54)	13.32 (0.56)	0.84 (0.74)	10.34 [2.47]	13.06 [1.33]	-2.72 [2.78]	9.82 [2.18]	12.80 [1.54]	-2.97 [2.65]
Z-score Housing Quality Summary Index	-0.05 (0.03)	0.00 (0.03)	-0.03 (0.05)	0.01 (0.03)	0.00 (0.03)	0.03 (0.04)	-0.03 [0.04]	0.01 [0.02]	-0.04 [0.05]	-0.05 [0.07]	0.01 [0.05]	-0.07 [0.08]
Aspire to Upgrade Housing Quality Materials <i>In-situ</i>	0.17 (0.02)	0.14 (0.02)	-0.03 (0.03)	0.12 (0.02)	0.12 (0.02)	-0.01 (0.02)	0.16 [0.03]	0.12 [0.02]	0.04 [0.03]	0.17 [0.03]	0.12 [0.02]	0.05 [0.04]
Satisfaction with Floor Quality	0.19 (0.02)	0.21 (0.02)	0.01 (0.03)	0.25 (0.02)	0.27 (0.02)	0.01 (0.02)	0.20 [0.02]	0.26 [0.04]	-0.06 [0.04]	0.19 [0.03]	0.25 [0.04]	-0.06 [0.05]
Satisfaction with Wall Quality	0.15 (0.01)	0.18 (0.02)	-0.02 (0.03)	0.16 (0.01)	0.17 (0.01)	0.03 (0.02)	0.16 [0.02]	0.16 [0.02]	-0.01 [0.03]	0.15 [0.03]	0.16 [0.02]	-0.02 [0.04]
Satisfaction with Roof Quality	0.17 (0.02)	0.20 (0.02)	-0.02 (0.03)	0.16 (0.01)	0.17 (0.01)	0.02 (0.02)	0.18 [0.02]	0.16 [0.02]	0.01 [0.03]	0.17 [0.02]	0.16 [0.02]	0.01 [0.03]
Satisfaction with Rain Protection	0.16 (0.01)	0.19 (0.02)	-0.01 (0.03)	0.15 (0.01)	0.14 (0.01)	0.03 (0.02)	0.17 [0.02]	0.14 [0.02]	0.02 [0.03]	0.16 [0.03]	0.15 [0.02]	0.01 [0.03]
Satisfaction with Quality of Life	0.28 (0.02)	0.25 (0.02)	0.02 (0.03)	0.28 (0.02)	0.27 (0.02)	0.01 (0.02)	0.27 [0.02]	0.27 [0.03]	0.00 [0.04]	0.28 [0.03]	0.28 [0.03]	0.01 [0.04]
Monthly Income Per Capita (USD)	49.45 (2.63)	59.85 (4.29)	-8.61 (5.99)	52.86 (2.54)	58.74 (2.94)	-5.08 (4.32)	53.08 [4.01]	55.77 [4.27]	-2.69 [5.82]	49.45 [4.54]	52.86 [4.34]	-3.40 [6.24]
Head's Years of Schooling	4.09 (0.14)	4.34 (0.20)	-0.01 (0.21)	4.37 (0.12)	3.87 (0.12)	0.26 (0.17)	4.18 [0.52]	4.13 [0.29]	0.05 [0.59]	4.09 [0.45]	4.37 [0.32]	-0.29 [0.55]
Head is Male	0.69 (0.02)	0.69 (0.03)	-0.01 (0.03)	0.69 (0.02)	0.71 (0.02)	0.00 (0.03)	0.69 [0.04]	0.70 [0.03]	-0.01 [0.05]	0.69 [0.04]	0.69 [0.04]	0.00 [0.05]
Head's Age	42.09 (0.63)	41.33 (0.77)	0.52 (1.07)	41.2 (0.59)	40.73 (0.61)	1.01 (0.87)	41.83 [0.96]	40.97 [0.70]	0.86 [1.18]	42.09 [1.09]	41.20 [0.72]	0.89 [1.29]
Slum Fixed Effects	Yes			Yes			No			No		

Note: This table reports baseline means and differences in means of the full sample. For Phase I and Phase II main columns, differences in means are estimated by regressions that include settlement fixed effects, and robust standard errors are reported in parentheses. For the Phase I vs Phase II main columns, standard errors clustered at the slum level are reported in brackets. Monetary variables in US dollars as of June 2007. In the case of monetary variables, observations over the 99th percentile were excluded. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A4: Baseline Balance Within and Between Phases - Urban Only

Dependent Variable	Phase I			Phase II			Phase I vs Phase II All			Phase I vs Phase II Only Treatments		
	Treat.	Control	Diff.	Treat.	Control	Diff.	Phase I	Phase II	Diff.	Phase I	Phase II	Diff.
Years living in the slum	7.94 (0.70)	8.50 (1.02)	0.72 (0.90)	9.27 (0.64)	11.70 (0.85)	-0.37 (0.96)	8.14 [3.00]	10.41 [2.06]	-2.27 [3.56]	7.94 [2.55]	9.27 [2.01]	-1.33 [3.18]
Z-score Housing Quality Summary Index	-0.04 (0.04)	0.00 (0.05)	0.00 (0.07)	0.15 (0.02)	0.19 (0.03)	0.01 (0.04)	-0.02 [0.07]	-0.02 [0.03]	-0.01 [0.07]	-0.04 [0.10]	-0.04 [0.06]	0.00 [0.11]
Aspire to Upgrade Housing Quality Materials <i>In-situ</i>	0.16 (0.02)	0.11 (0.03)	0.00 (0.04)	0.11 (0.02)	0.12 (0.02)	-0.01 (0.03)	0.14 [0.03]	0.11 [0.01]	0.03 [0.04]	0.16 [0.04]	0.11 [0.02]	0.05 [0.04]
Satisfaction with Floor Quality	0.15 (0.02)	0.19 (0.03)	0.01 (0.04)	0.21 (0.02)	0.27 (0.03)	-0.01 (0.03)	0.16 [0.03]	0.23 [0.06]	-0.07 [0.07]	0.15 [0.03]	0.21 [0.05]	-0.05 [0.06]
Satisfaction with Wall Quality	0.11 (0.02)	0.15 (0.03)	-0.03 (0.04)	0.15 (0.02)	0.18 (0.02)	0.00 (0.03)	0.12 [0.02]	0.16 [0.04]	-0.04 [0.04]	0.11 [0.03]	0.15 [0.04]	-0.04 [0.04]
Satisfaction with Roof Quality	0.14 (0.02)	0.20 (0.03)	-0.03 (0.04)	0.16 (0.02)	0.17 (0.02)	0.03 (0.03)	0.16 [0.02]	0.16 [0.03]	0.00 [0.04]	0.14 [0.02]	0.16 [0.04]	-0.02 [0.04]
Satisfaction with Rain Protection	0.13 (0.02)	0.19 (0.03)	-0.01 (0.04)	0.15 (0.02)	0.14 (0.02)	0.01 (0.03)	0.15 [0.03]	0.14 [0.01]	0.00 [0.03]	0.13 [0.03]	0.15 [0.02]	-0.01 [0.04]
Satisfaction with Quality of Life	0.24 (0.02)	0.20 (0.03)	0.01 (0.04)	0.25 (0.02)	0.31 (0.03)	-0.03 (0.04)	0.23 [0.03]	0.28 [0.04]	-0.05 [0.05]	0.24 [0.03]	0.25 [0.04]	-0.01 [0.05]
Monthly Income Per Capita (USD)	56.03 (3.86)	61.17 (5.38)	-3.88 (7.86)	65.25 (4.86)	81.11 (7.51)	-17.32* (10.47)	60.00 [4.55]	69.53 [4.60]	-9.54 [6.34]	57.74 [5.74]	65.25 [5.69]	-7.51 [7.93]
Head's Years of Schooling	4.81 (0.18)	5.53 (0.27)	-0.26 (0.31)	5.55 (0.17)	4.79 (0.18)	0.47* (0.24)	5.04 [0.60]	5.19 [0.33]	-0.15 [0.67]	4.81 [0.52]	5.55 [0.23]	-0.74 [0.56]
Head is Male	0.61 (0.03)	0.63 (0.04)	-0.03 (0.05)	0.57 (0.03)	0.63 (0.03)	-0.01 (0.04)	0.62 [0.04]	0.60 [0.05]	0.02 [0.06]	0.61 [0.05]	0.57 [0.05]	0.04 [0.07]
Head's Age	41.52 (0.78)	39.93 (1.01)	1.61 (1.33)	39.84 (0.78)	39.46 (0.90)	1.17 (1.21)	41.00 [1.43]	39.66 [0.79]	1.34 [1.59]	41.52 [1.61]	39.84 [0.70]	1.68 [1.71]
Slum Fixed Effects	Yes			Yes			No			No		

Note: This table reports baseline means and differences in means of the urban sample. For Phase I and Phase II main columns, differences in means are estimated by regressions that include slum fixed effects, and robust standard errors are reported in parentheses. For the Phase I vs Phase II main columns, standard errors clustered at the slum level are reported in brackets. Monetary variables in US dollars as of June 2007. In the case of monetary variables, observations over the 99th percentile were excluded. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A5: Baseline Balance Within and Between Phases - Rural Only

Dependent Variable	Phase I			Phase II			Phase I vs Phase II All			Phase I vs Phase II Only Treatments		
	Treat.	Control	Diff.	Treat.	Control	Diff.	Phase I	Phase II	Diff.	Phase I	Phase II	Diff.
Years living in the slum	13.63 (1.36)	15.37 (1.57)	-0.52 (1.91)	15.92 (0.82)	14.56 (0.74)	1.88* (1.11)	14.36 [3.16]	15.23 [1.32]	-0.88 [3.36]	13.63 [3.19]	15.92 [1.42]	-2.30 [3.43]
Z-score Housing Quality Summary Index	-0.08 (0.06)	0.00 (0.05)	-0.07 (0.08)	0.06 (0.04)	0.00 (0.03)	0.06 (0.05)	-0.05 [0.06]	0.03 [0.03]	-0.08 [0.06]	-0.08 [0.09]	0.06 [0.06]	-0.14 [0.11]
Aspire to Upgrade Housing Quality Materials <i>In-situ</i>	0.18 (0.03)	0.23 (0.05)	-0.09 (0.07)	0.13 (0.03)	0.12 (0.02)	-0.01 (0.04)	0.20 [0.04]	0.12 [0.03]	0.07 [0.05]	0.18 [0.05]	0.13 [0.05]	0.05 [0.07]
Satisfaction with Floor Quality	0.26 (0.03)	0.24 (0.04)	0.03 (0.05)	0.30 (0.02)	0.28 (0.02)	0.03 (0.03)	0.25 [0.04]	0.29 [0.05]	-0.04 [0.06]	0.26 [0.05]	0.30 [0.05]	-0.04 [0.07]
Satisfaction with Wall Quality	0.21 (0.03)	0.21 (0.03)	0.00 (0.04)	0.18 (0.02)	0.15 (0.02)	0.05* (0.03)	0.21 [0.04]	0.17 [0.03]	0.05 [0.05]	0.21 [0.05]	0.18 [0.03]	0.03 [0.06]
Satisfaction with Roof Quality	0.20 (0.03)	0.19 (0.03)	0.00 (0.04)	0.16 (0.02)	0.16 (0.02)	0.01 (0.03)	0.20 [0.04]	0.16 [0.03]	0.04 [0.05]	0.20 [0.04]	0.16 [0.03]	0.04 [0.05]
Satisfaction with Rain Protection	0.19 (0.03)	0.18 (0.03)	-0.01 (0.04)	0.15 (0.02)	0.13 (0.02)	0.04 (0.03)	0.19 [0.04]	0.14 [0.03]	0.05 [0.05]	0.19 [0.05]	0.15 [0.03]	0.04 [0.06]
Satisfaction with Quality of Life	0.34 (0.03)	0.31 (0.04)	0.03 (0.05)	0.30 (0.02)	0.24 (0.02)	0.05 (0.03)	0.33 [0.03]	0.27 [0.04]	0.06 [0.05]	0.34 [0.04]	0.30 [0.04]	0.05 [0.05]
Monthly Income Per Capita (USD)	38.57 (2.65)	58.12 (7.16)	-12.76* (7.28)	41.48 (2.30)	45.44 (2.34)	-1.55 (3.05)	42.68 [4.41]	44.23 [4.51]	-1.55 [6.22]	38.57 [4.11]	42.21 [3.80]	-3.63 [5.53]
Head's Years of Schooling	3.00 (0.20)	2.88 (0.25)	0.32 (0.28)	3.33 (0.16)	3.17 (0.16)	0.07 (0.23)	2.96 [0.48]	3.25 [0.22]	-0.29 [0.52]	3.00 [0.46]	3.33 [0.30]	-0.33 [0.54]
Head is Male	0.81 (0.02)	0.77 (0.03)	0.03 (0.05)	0.79 (0.02)	0.76 (0.02)	0.01 (0.03)	0.80 [0.02]	0.78 [0.02]	0.02 [0.03]	0.81 [0.02]	0.79 [0.02]	0.02 [0.03]
Head's Age	42.94 (1.06)	43.09 (1.19)	-0.91 (1.76)	42.41 (0.86)	41.70 (0.82)	0.87 (1.23)	43.00 [0.98]	42.05 [1.00]	0.95 [1.38]	42.94 [1.31]	42.41 [1.10]	0.53 [1.68]
Slum Fixed Effects	Yes			Yes			No			No		

Note: This table reports baseline means and differences in means of the rural sample. For Phase I and Phase II main columns, differences in means are estimated by regressions that include settlement fixed effects, and robust standard errors are reported in parentheses. For the Phase I vs Phase II main columns, standard errors clustered at the slum level are reported in brackets. Monetary variables in US dollars as of June 2007. In the case of monetary variables, observations over the 99th percentile were excluded. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A6: Baseline Balance Between Phases at Slum Level

Dependent Variable	Urban			Rural			All		
	Phase I Slums Mean	Phase II Slums Mean	Mean Diff.	Phase I Slums Mean	Phase II Slums Mean	Mean Diff.	Phase I Slums Mean	Phase II Slums Mean	Mean Diff.
Years living in the slum	11.75 (12.42)	12.49 (6.88)	-0.75 [4.02]	13.63 (12.08)	15.83 (6.54)	-2.21 [3.21]	12.82 (12.03)	14.79 (6.75)	-1.98 [2.48]
Z-score Housing Quality Summary index	-0.02 (0.32)	0.01 (0.11)	-0.03 [0.10]	0.00 (0.25)	0.04 (0.16)	-0.04 [0.07]	-0.01 (0.27)	0.03 (0.15)	-0.04 [0.06]
Aspire to Upgrade Housing Quality Materials <i>In-situ</i>	0.16 (0.11)	0.13 (0.06)	0.03 [0.04]	0.17 (0.09)	0.12 (0.12)	0.05 [0.05]	0.17 (0.10)	0.13 (0.10)	0.04 [0.03]
Satisfaction with Quality of Life	0.23 (0.11)	0.26 (0.12)	-0.04 [0.05]	0.37 (0.13)	0.29 (0.17)	0.08* [0.04]	0.31 (0.14)	0.28 (0.15)	0.03 [0.04]
Satisfaction with Floor Quality	0.16 (0.09)	0.21 (0.17)	-0.05 [0.05]	0.27 (0.13)	0.29 (0.28)	-0.02 [0.06]	0.22 (0.13)	0.26 (0.25)	-0.04 [0.04]
Satisfaction with Wall Quality	0.12 (0.09)	0.15 (0.11)	-0.04 [0.04]	0.28 (0.19)	0.18 (0.17)	0.10* [0.06]	0.21 (0.17)	0.17 (0.15)	0.04 [0.04]
Satisfaction with Roof Quality	0.13 (0.09)	0.15 (0.10)	-0.02 [0.04]	0.23 (0.12)	0.16 (0.16)	0.07 [0.04]	0.19 (0.12)	0.16 (0.15)	0.03 [0.03]
Satisfaction with Rain Protection	0.14 (0.09)	0.12 (0.07)	0.02 [0.03]	0.20 (0.13)	0.16 (0.16)	0.04 [0.04]	0.17 (0.11)	0.15 (0.14)	0.03 [0.03]
Monthly Income Per Capita (USD)	56.87 (16.16)	65.02 (20.94)	-8.14 [7.29]	47.11 (19.58)	49.27 (22.11)	-2.16 [6.28]	51.47 (19.02)	54.23 (22.93)	-2.76 [4.96]
Head's Years of Schooling	4.37 (1.64)	4.71 (1.49)	-0.34 [0.62]	3.24 (1.68)	3.34 (1.18)	-0.10 [0.47]	3.73 (1.73)	3.77 (1.42)	-0.43 [0.39]
Head is Male	0.65 (0.15)	0.63 (0.18)	0.02 [0.06]	0.80 (0.11)	0.78 (0.10)	0.02 [0.03]	0.74 (0.15)	0.74 (0.15)	0.00 [0.04]
Head's Age	43.07 (6.62)	41.28 (5.87)	1.79 [2.47]	43.46 (4.19)	43.47 (6.38)	-0.01 [1.55]	43.29 (5.26)	42.79 (6.24)	0.50 [1.36]
Slum Size (Number of Households)	48.50 (31.35)	44.57 (31.80)	3.93 [12.41]	25.81 (18.24)	24.32 (16.12)	1.49 [5.37]	35.54 (26.76)	30.62 (23.79)	4.91 [6.16]
Distance to TECHO Office (<i>kms</i>)	47.77 (57.36)	47.70 (34.52)	0.07 [19.53]	107.76 (48.32)	94.21 (32.45)	13.57 [13.64]	82.38 (59.47)	79.74 (39.29)	2.64 [13.01]
Sample Size (Number of Slums)	12	14	26	16	31	47	28	45	73

Note: This table reports baseline means and differences in means of Phase I and Phase II slums for urban, rural, and full sample. Standard Deviations are reported in parenthesis and robust standard errors are reported in brackets. Monetary variables in US dollars as of June 2007. In the case of monetary variables, observations over the 99th percentile were excluded. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A7: The Rural-Urban Divide

Dependent Variable	Mean Urban Slums	Mean Rural Slums	Diff.
Monthly Income Per Capita (USD)	63.23 (76.62)	44.39 (44.10)	18.84*** (2.79)
Number of Rooms per Capita	0.73 (0.54)	0.69 (0.49)	0.03 (0.04)
Share of Rooms with Good Quality Floors	0.40 (0.43)	0.42 (0.42)	-0.02 (0.02)
Share of Rooms with Good Quality Walls	0.22 (0.36)	0.18 (0.30)	0.04*** (0.01)
Share of Rooms with Good Quality Roofs	0.33 (0.42)	0.33 (0.41)	0.00 (0.02)
Share of Rooms with Windows	0.46 (0.40)	0.22 (0.32)	0.24*** (0.01)
Aspire to Upgrade Housing Quality Materials <i>In-situ</i>	0.13 (0.33)	0.15 (0.36)	-0.02 (0.02)
Satisfaction with Floors Quality	0.20 (0.40)	0.27 (0.45)	-0.07*** (0.02)
Satisfaction with Walls Quality	0.14 (0.35)	0.18 (0.39)	-0.04** (0.02)
Satisfaction with Roofs Quality	0.16 (0.37)	0.18 (0.38)	-0.01 (0.02)
Satisfaction with Protection against Rain	0.15 (0.35)	0.16 (0.37)	-0.01 (0.01)
Satisfaction with Quality of Life	0.25 (0.44)	0.29 (0.45)	-0.04** (0.02)
Z-score Satisfaction Summary Index	0.02 (0.72)	0.07 (0.71)	-0.05* (0.03)

Notes: This table reports baseline means, and differences in means between urban and rural slum dwellers. Robust standard errors are reported in parenthesis. Monetary variables in US dollars as of June 2007. In the case of monetary variables, observations over the 99th percentile were excluded. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A8: Treatment Effect on Housing Quality and Housing Satisfaction, by Zone

Dependent Variable	Urban			Rural		
		Model 1	Model 2		Model 1	Model 2
	Follow-Up Control Mean	Treat	Treat	Follow-Up Control Mean	Treat	Treat
Number of Rooms per Capita	0.79 (0.60)	0.01 (0.05)	0.00 (0.04)	0.69 (0.46)	0.04 (0.04)	0.03 (0.04)
Share Rooms Good Quality Floors	0.44 (0.44)	0.18*** (0.03)	0.19*** (0.03)	0.44 (0.43)	0.18*** (0.02)	0.18*** (0.03)
Share Rooms Good Quality Walls	0.43 (0.44)	0.15*** (0.03)	0.14*** (0.04)	0.23 (0.34)	0.25*** (0.02)	0.25*** (0.02)
Share Rooms Good Quality Roof	0.39 (0.42)	0.18*** (0.03)	0.18*** (0.03)	0.43 (0.41)	0.15*** (0.03)	0.15*** (0.03)
Share Rooms with Windows	0.46 (0.38)	0.14*** (0.03)	0.14*** (0.03)	0.24 (0.30)	0.21*** (0.02)	0.22*** (0.02)
Satisfaction with Floors Quality	0.36 (0.48)	0.13*** (0.04)	0.13*** (0.04)	0.37 (0.48)	0.26*** (0.04)	0.26*** (0.04)
Satisfaction with Walls Quality	0.27 (0.44)	0.21*** (0.04)	0.21*** (0.04)	0.27 (0.46)	0.36*** (0.04)	0.36*** (0.04)
Satisfaction with Roofs Quality	0.28 (0.45)	0.23*** (0.04)	0.23*** (0.04)	0.31 (0.46)	0.33*** (0.04)	0.33*** (0.04)
Satisfaction with Rain's Protection	0.28 (0.45)	0.18*** (0.04)	0.17*** (0.04)	0.25 (0.43)	0.31*** (0.04)	0.31*** (0.04)
Slum Fixed Effects		Yes	Yes		Yes	Yes
Baseline Covariates		No	Yes		No	Yes

Note: We analyze urban and rural samples from Phase II (short treatment exposure), separately. Each row represents a separate dependent variable. The first column reports the mean of the dependent variable for the control group measured at follow-up. The next column, under the heading Model 1, reports the results of a regression of the dependent variable on Treatment Assignment plus slum fixed effects. Reports are the estimated coefficients and robust standard errors. The next column, under the heading Model 2, additionally control for the household head's years living in the slum, 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. 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. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A9: High vs Low Income Divide - Urban Only

Dependent Variable	Mean High Income Status (> p50)	Mean High Income Status (≤ p50)	Diff.
Monthly Income Per Capita (USD)	110.54 (94.39)	19.06 (11.49)	92.32*** (4.29)
Number of Rooms per Capita	0.84 (0.60)	0.56 (0.24)	0.19*** (0.07)
Share of Rooms with Good Quality Floors	0.39 (0.43)	0.38 (0.41)	0.01 (0.03)
Share of Rooms with Good Quality Walls	0.23 (0.37)	0.23 (0.36)	-0.02 (0.02)
Share of Rooms with Good Quality Roofs	0.32 (0.42)	0.31 (0.41)	0.00 (0.02)
Share of Rooms with Windows	0.47 (0.40)	0.43 (0.39)	0.01 (0.02)
Z-score Housing Quality Summary Index	-0.07 (0.79)	0.00 (0.76)	-0.06 (0.04)
Aspire to Upgrade Housing Quality Materials <i>In-situ</i>	0.14 (0.34)	0.13 (0.34)	0.01 (0.02)
Satisfaction with Floors Quality	0.20 (0.40)	0.15 (0.36)	0.04 (0.02)
Satisfaction with Walls Quality	0.12 (0.33)	0.13 (0.33)	0.00 (0.02)
Satisfaction with Roofs Quality	0.17 (0.37)	0.13 (0.34)	0.01 (0.02)
Satisfaction with Protection against Rain	0.16 (0.36)	0.11 (0.32)	0.05** (0.02)
Satisfaction with Quality of Life	0.24 (0.42)	0.26 (0.44)	0.00 (0.03)
Z-score Satisfaction Summary Index	0.06 (0.74)	-0.02 (0.67)	0.05 (0.04)

Note: This table reports baseline means, and differences in means between urban households who are above and below the median monthly income per capita at baseline, which is US\$39. Robust standard errors are reported in parenthesis. Monetary variables in US dollars as of June 2007. In the case of monetary variables, observations over the 99th percentile were excluded. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A10: Adaptation in Housing and Location Aspirations, by Income Status - Rural Only

Dependent Variable	High Income Status ($> p50$)					Low Income Status ($\leq p50$)				
	Follow-Up Treat. Mean	Model 1		Model 2		Follow-Up Treat. Mean	Model 1		Model 2	
		Cont.	Cont. \times Phase I	Cont.	Cont. \times Phase I		Cont.	Cont. \times Phase I	Cont.	Cont. \times Phase I
		γ_1	γ_2	γ_1	γ_2		γ_1	γ_2	γ_1	γ_2
“No-upgrade”	0.57 (0.50)	0.03 (0.05)	0.06 (0.09)	0.05 (0.05)	0.05 (0.09)	0.61 (0.49)	0.03 (0.06)	-0.08 (0.11)	0.03 (0.06)	-0.08 (0.11)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.17		0.17			0.61		0.58	
“Within-slum-upgrade”	0.30 (0.46)	-0.06 (0.05)	-0.03 (0.08)	-0.07 (0.05)	-0.01 (0.08)	0.26 (0.44)	0.03 (0.06)	0.03 (0.10)	0.03 (0.06)	0.04 (0.11)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.15		0.22			0.52		0.45	
“Move-to-another-slum”	0.01 (0.08)	-0.01 (0.01)	0.02 (0.02)	-0.01 (0.01)	0.02 (0.02)	0.02 (0.12)	0.00 (0.01)	0.01 (0.04)	0.00 (0.01)	0.01 (0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.49		0.47			0.76		0.79	
“Out-of-slum-upgrade”	0.13 (0.33)	0.03 (0.03)	-0.05 (0.06)	0.03 (0.03)	-0.06 (0.06)	0.11 (0.32)	-0.06 (0.04)	0.03 (0.06)	-0.06 (0.04)	0.03 (0.07)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.70		0.54			0.67		0.59	
Slum Fixed Effects		Yes		Yes			Yes		Yes	
Baseline Covariates		No		Yes			No		Yes	

Note: Only rural sample is considered. Each row represents a separate dependent variable. The analysis is divided into two income sub-groups defined by whether the baseline monthly income per capita is below or above the median in the income distribution of the urban sample. The first column reports the mean and standard deviation of the dependent variable for the treatment group measures at follow-up. The next two columns, under the heading Model 1, report the results of a regression of the dependent variable on Control Assignment and Control Assignment interacted with Phase I plus slum fixed effects. Reports are the estimated coefficients and robust standard errors. The last two columns, Model 2, additionally control for the household head’s Years of Schooling, Gender, Age, Years living in the slum, as well as the value of household assets per capita, monthly income per capita, and whether the household’s head aspires to upgrade housing quality materials *in-situ*, 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. Finally, we report the p-values of F-tests of the null hypothesis that $\gamma_1 + \gamma_2 = 0$ for each model. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A11: Adaptation in Housing and Location Aspirations, by Country - Urban Only

Country	Sample Size	Follow-Up Treat. Mean	“Within-slum-upgrade”				“Out-of-slum-upgrade”					
			Model 1		Model 2		Model 1		Model 2			
			Cont.	Cont. × Phase I	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I		
		γ_1	γ_2	γ_1	γ_2	γ_1	γ_2	γ_1	γ_2			
El Salvador	140	0.28 (0.45)	0.05 (0.15)	-0.13 (0.15)	0.04 (0.15)	-0.17 (0.17)	140	0.05 (0.22)	-0.06 (0.06)	0.18 (0.15)	-0.04 (0.07)	0.16 (0.16)
Uruguay	708	0.12 (0.33)	0.10*** (0.04)	-0.10* (0.06)	0.10** (0.04)	-0.09 (0.06)	708	0.64 (0.48)	-0.11** (0.05)	0.16** (0.08)	-0.11** (0.05)	0.16** (0.08)
Mexico	248	0.31 (0.47)	0.08 (0.08)	-0.15 (0.13)	0.11 (0.08)	-0.24* (0.13)	248	0.18 (0.39)	-0.12* (0.06)	0.20* (0.10)	-0.13** (0.06)	0.21** (0.10)
All Countries	1,096	0.19 (0.39)	0.09*** (0.03)	-0.11** (0.05)	0.09** (0.03)	-0.11** (0.05)	1,096	0.46 (0.50)	-0.11*** (0.04)	0.17*** (0.06)	-0.11*** (0.04)	0.17*** (0.06)
p-value for F-test of Pooling Countries			0.60		0.65		0.91		0.84			
Slum Fixed Effects			Yes		Yes		Yes		Yes			
Baseline Covariates			No		Yes		No		Yes			

Note: Each row represents a separate country. Only urban households are considered. For each aspiration variable, the first column reports the sample size. The second column reports the mean and standard deviation of the dependent variable for the treatment group measures at follow-up. The next two columns, under the heading Model 1, report the results of a regression of the dependent variable on Control Assignment and Control Assignment interacted with Phase I plus slum fixed effects. Reports are the estimated coefficients and robust standard errors. The last two columns, Model 2, additionally control for the household head’s Years of Schooling, Gender, Age, Years living in the slum, as well as the value of household assets per capita, monthly income per capita, 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. Additionally, we report the p-values of F-tests of the null hypothesis that $\gamma_1 + \gamma_2 = 0$ for each model. Finally we report the p-values of F-tests of the null hypothesis that the estimated coefficients on Control and the estimated coefficient on Control × Phase I are jointly equal to all countries for models 1 and 2. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A12: Adaptation in Housing Quality - Urban Only

Dependent Variable	Follow-Up Control Mean	Model 1		Model 2	
		Treat	Treat × Phase I	Treat	Treat × Phase I
		γ_1	γ_2	γ_1	γ_2
Number of Rooms per Capita	0.80 (0.55)	0.01 (0.05)	0.07 (0.07)	0.00 (0.04)	0.07 (0.07)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.16		0.17	
Share Rooms Good Quality Floors	0.43 (0.43)	0.18*** (0.03)	-0.04 (0.05)	0.19*** (0.03)	-0.04 (0.05)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Share Rooms Good Quality Walls	0.44 (0.44)	0.15*** (0.03)	-0.03 (0.06)	0.14*** (0.04)	-0.02 (0.06)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Share Rooms Good Quality Roof	0.38 (0.42)	0.18*** (0.03)	-0.03 (0.05)	0.18*** (0.03)	-0.03 (0.05)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Share Rooms with Windows	0.49 (0.37)	0.14*** (0.03)	-0.02 (0.04)	0.14*** (0.03)	-0.02 (0.04)
<i>p-value</i> ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Slum Fixed Effects		Yes		Yes	
Baseline Covariates		No		Yes	

Note: Only urban households are considered. Each row represents a separate dependent variable. The first column reports the mean of the dependent variable for the control group measured at follow-up. The next two columns, under the heading Model 1, report the results of a regression of the dependent variable on Treatment Assignment and Treatment Assignment interacted with Phase I plus slum fixed effects. Reports are the estimated coefficients and robust standard errors. The last two columns, under the heading Model 2, additionally control for the household head's years living in the slum, 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. 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. Finally, we report the p-values of the F-tests of the null hypothesis $\gamma_1 + \gamma_2 = 0$. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A13: Adaptation in Housing and Location Aspirations in the presence of potential wear-and-tears of the house - Urban Only

Dependent Variable	Follow-Up Treat. Mean	Model 1		Model 2	
		Cont.	Cont. × Phase I	Cont.	Cont. × Phase I
		γ_1	γ_2	γ_1	γ_2
“No-upgrade”	0.34 (0.48)	0.02 (0.04)	-0.08 (0.06)	0.02 (0.04)	-0.07 (0.06)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.22		0.24	
“Within-slum-upgrade”	0.16 (0.37)	0.07* (0.03)	-0.10** (0.05)	0.07* (0.04)	-0.10** (0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.37		0.31	
“Move-to-another-slum”	0.02 (0.12)	0.01 (0.01)	0.02 (0.02)	0.01 (0.01)	0.02 (0.02)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.13		0.13	
“Out-of-slum-upgrade”	0.48 (0.50)	-0.10** (0.04)	0.16*** (0.06)	-0.09** (0.04)	0.16*** (0.06)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.17		0.16	
Slum Fixed Effects		Yes		Yes	
Baseline Covariates		No		Yes	

Note: Only urban households are considered. Each row represents a separate dependent variable. The first column reports the mean and standard deviation of the dependent variable for the treatment group measured at follow-up. The next two columns, under the heading Model 1, report the results of a regression of the dependent variable on Control Assignment and Control Assignment interacted with Phase I plus slum fixed effects and a set of housing quality measures including number of rooms, share of rooms with good quality floors, share of rooms with good quality walls, share of rooms with good quality roofs, share of rooms with windows, all measured at the follow-up round. Reports are the estimated coefficients and robust standard errors. The last two columns, Model 2, additionally control for the household head’s Years of Schooling, Gender, Age, Years living in the slum, as well as the value of household assets per capita, and monthly income per capita, and whether the household’s head aspires to upgrade housing quality materials *in-situ*, 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. Finally, we report the p-values of F-tests of the null hypothesis that $\gamma_1 + \gamma_2 = 0$ for each model. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A14: Adaptation in Housing and Location Expectations - Urban Only

Dependent Variable	Follow-Up Treat. Mean	Model 1		Model 2	
		Cont.	Cont. × Phase I	Cont.	Cont. × Phase I
		γ_1	γ_2	γ_1	γ_2
“No-upgrade” (Expectation)	0.62 (0.49)	-0.03 (0.04)	0.07 (0.07)	-0.03 (0.04)	0.08 (0.07)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.45		0.38	
“Within-slum-upgrade” (Expectation)	0.09 (0.29)	0.03 (0.03)	-0.01 (0.04)	0.03 (0.03)	-0.02 (0.04)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.58		0.68	
“Move-to-another-slum” (Expectation)	0.02 (0.15)	0.01 (0.01)	-0.02 (0.02)	0.01 (0.01)	-0.02 (0.02)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.47		0.41	
“Out-of-slum-upgrade” (Expectation)	0.27 (0.44)	-0.01 (0.04)	-0.04 (0.06)	0.00 (0.04)	-0.04 (0.06)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.32		0.33	
Slum Fixed Effects		Yes		Yes	
Baseline Covariates		No		Yes	

Note: Only urban households are considered. Each row represents a separate dependent variable. The first column reports the mean and standard deviation of the dependent variable for the treatment group measures at follow-up. The next two columns, under the heading Model 1, report the results of a regression of the dependent variable on Control Assignment and Control Assignment interacted with Phase I plus slum fixed effects. Reports are the estimated coefficients and robust standard errors. The last two columns, Model 2, additionally control for the household head’s Years of Schooling, Gender, Age, Years living in the slum, as well as the value of household assets per capita, monthly income per capita, and whether the household’s head aspires to upgrade housing quality materials *in-situ*, 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. Finally, we report the p-values of F-tests of the null hypothesis that $\gamma_1 + \gamma_2 = 0$ for each model. *Significant at 10%. **Significant at 5%. ***Significant at 1%.

Table A15: Description of Variables and Sample Sizes. Follow-Up Survey

Variable	Description	Phase I		Phase II		All	
		Obs. Control	Obs. Treat.	Obs. Control	Obs. Treat.	Obs. Control	Obs. Treat.
Monthly Income Per Capita (USD)	Monthly Income per capita in US dollars as of July 2007.	265	513	532	557	797	1,070
Assets Value Per Capita (USD)	It is calculated as the sum of the monthly earnings of each household's member divided by the household size. Total Assets Value per capita in US dollars as of July 2007. It is calculated as the sum of the value of each household's asset from a list of 20 items divided by the household size.	281	543	562	595	843	1,138
Head of HH's Age	Age of head of household in years.	312	601	618	651	930	1,252
Head of HH's Gender	Indicator equal to one if the head of household is a man.	316	610	625	658	941	1,268
Head of HH's Years of Schooling	Years of Schooling of head of household equivalent to the higher level of education reached.	313	594	609	649	922	1,243
Hours worked last week by Head	Hours worked last week by Head of Household.	230	469	469	504	699	973
Hours worked last week by Spouse	Hours worked last week by the Spouse of Head of Households.	107	190	143	179	250	369
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: "Unsatisfied", "Neither Satisfied nor Unsatisfied", "Satisfied", and "Very Satisfied".	313	606	623	657	936	1,263
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: "Unsatisfied", "Neither Satisfied nor Unsatisfied", "Satisfied", and "Very Satisfied".	313	607	623	657	936	1,264
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: "Unsatisfied", "Neither Satisfied nor Unsatisfied", "Satisfied", and "Very Satisfied".	313	607	623	657	936	1,264
Satisfaction with Rain Protection	Indicator equal to one if respondent reports being "Satisfied" or "Very satisfied" with the houses' protection against water when it rains, measured by a Likert scale of 4 categories: "Unsatisfied", "Neither Satisfied nor Unsatisfied", "Satisfied", and "Very Satisfied".	313	607	623	657	936	1,264
Satisfaction with Quality of Life	Indicator equal to one if respondent reports being "Satisfied" or "Very satisfied" with the quality of life, measured by a Likert scale of 4 categories: "Unsatisfied", "Neither Satisfied nor Unsatisfied", "Satisfied", and "Very Satisfied".	293	584	622	644	915	1,228
Share Rooms Good Quality Floors	Proportion of rooms with floors made of good quality materials like cement, brick, or wood (observed by the enumerator).	312	608	625	658	937	1,266
Share Rooms Good Quality Walls	Proportion of rooms with walls made of good quality materials like wood, cement, brick or zinc metal (observed by the enumerator).	316	610	621	658	937	1,268
Share Rooms Good Quality Roof	Proportion of rooms with roofs made of good quality materials like cement, brick, tile and zinc metal (observed by the enumerator).	315	609	623	657	938	1,266
Share Rooms with Windows	Proportion of rooms with at least one window (observed by the enumerator).	315	610	625	658	940	1,268
"No-upgrade"	Indicator equal to one if the respondent reports to aspire to stay and keep the same housing conditions within the slum.	313	599	620	653	933	1,252
"Within-slum-upgrade"	Indicator equal to one if the respondent reports to aspire to upgrade housing conditions and get own land within the slum.	313	599	620	653	933	1,252
"Move-to-another-slum"	Indicator equal to one if the respondent reports to aspire to move to another slum.	313	599	620	653	933	1,252
"Out-of-slum-upgrade"	Indicator equal to one if the respondent reports to aspire to upgrade housing conditions and get own land outside of a slum.	313	599	620	653	933	1,252
"No-upgrade" (Expectation)	Indicator equal to one if the respondent reports to expect to stay and keep the same housing conditions within the slum during the next 5 years.	313	599	620	653	933	1,252
"Within-slum-upgrade" (Expectation)	Indicator equal to one if the respondent reports to expect to upgrade housing conditions and get own land within the slum during the next 5 years.	313	599	620	653	933	1,252
"Move-to-another-slum" (Expectation)	Indicator equal to one if the respondent reports to expect to move to another slum during the next 5 years.	313	599	620	653	933	1,252
"Out-of-slum-upgrade" (Expectation)	Indicator equal to one if the respondent reports to expect to upgrade housing conditions and get own land outside of a slum during the next 5 years.	313	599	620	653	933	1,252

Appendix B. The TECHO Program: Description and slums sample design (For Online Publication)

The TECHO Program. The TECHO program provides basic, pre-fabricated, transitional houses to extremely poor families living in informal settlements (or the so-called “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 started 21 years ago in Chile and now works in more than 20 Latin American countries. This NGO has built more than 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.

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 square meters (*6m* by *3m*) 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. Floors are built on top of 15 stacks that raise them up to between 30 and 80 centimeters 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\$1,000 - 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) under a scheme of flexible payments over time that allows the families to smooth consumption. 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.

Figure B1 shows examples of the TECHO houses. Importantly, in addition 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 does not offer these houses on the market but instead makes them available only to selected



Figure B1: TECHO House

slum dwellers living under the poorest conditions. Consequently, even if households did not face credit constraints that hampered their access to housing improvements, they would not have access to houses of the type or at the price offered by the TECHO program.

Finally, the TECHO houses are 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.

Slum Sample Design. The experiment was conducted in three countries: El Salvador, Mexico, and Uruguay. The TECHO program’s budget and personnel constraints limit the number of housing units that can be built at any one time, which in turn constrained the size of the sample used in our study in each country. Under these constraints, TECHO opted to select beneficiaries through a lottery system that gives all eligible households in each selected slum an equal opportunity to receive the housing upgrade in a given year.

TECHO first selected a set of eligible slums and then conducted a census to identify eligible households within each slum (i.e., those poor enough to be given priority). Eligible settlements are slums where: (i) at least 50% of the residents do not have land title, and/or (ii) the majority of slum dwellers lack access to at least one of the following three basic services: electricity, drinking water or sanitation. Settlements where TECHO had intervened in the past were considered ineligible and were not included in our sample of study.

In El Salvador, we first randomly selected 6 departments (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 16 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

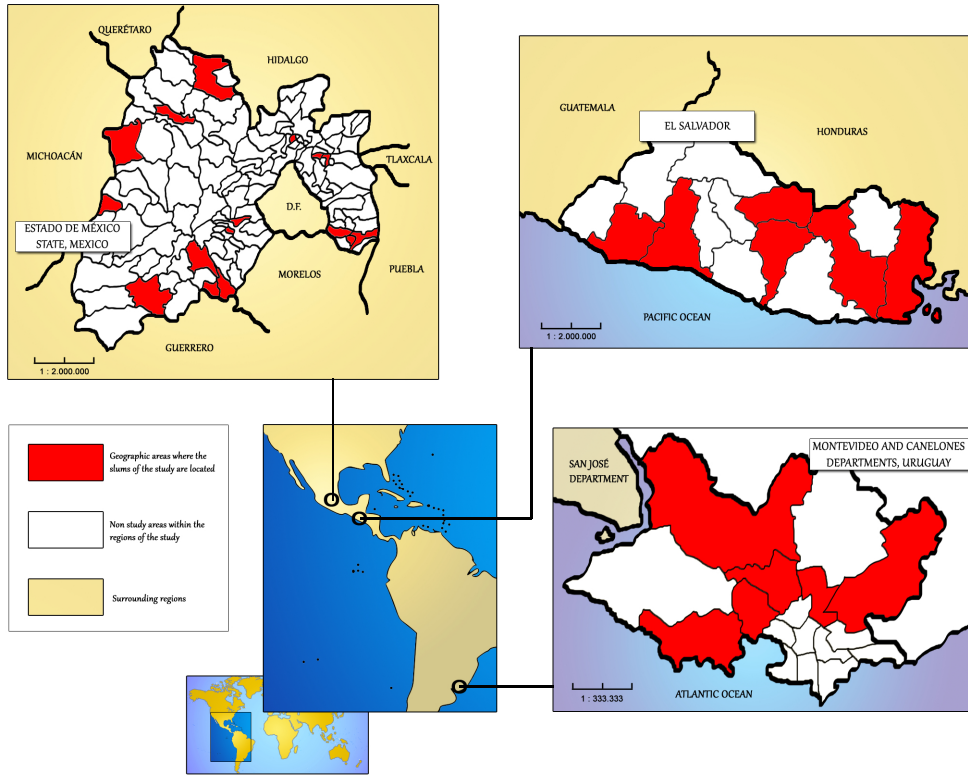


Figure B2: Map of Evaluation Sites

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 Figure B2.

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 urban and rural slums in Estado de Mexico located adjacent to Mexico City and, in Uruguay, only urban slums located in and around Montevideo.