Aspiration Adaptation in Resource-Constrained Environments [1]

Sebastian Galiani

University of Maryland and NBER

Paul J. Gertler

UC Berkeley and NBER

Raimundo Undurraga

Universidad de Chile

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Abstract

We use a multi-country field experiment that combines random variation at treatment level with exogenous variation on the time of exposure to the treatment to test the effect of a slum-housing intervention on the evolution of housing aspirations of non-beneficiary neighbors over time. Initially after treatment, we observe a huge treatment-control 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 their treatment Joneses' as in standard models of peer effects. However, eight months later, no effects are found on housing investments and the aspirational effect fully 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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[1]Corresponding author: Undurraga: Department of Industrial Engineering and Center of Applied Economics (CEA), Universidad de Chile, Beauchef 851, Santiago, Chile (e-mail: raimundo.undurraga@dii.uchile.cl); Galiani: Departament of Economics, University of Maryland, 3105 Tydings Hall; College Park, Maryland 20742 and National Bureau of Economic Research (e-mail: galiani@econ.umd.edu); Gertler: Haas School of Business University of California, MC 1900 Berkeley, California 94720 and National Bureau of Economic Research (e-mail: gertler@haas.berkeley.edu). We thank Martin Rotemberg and Rajeev Dehejia for their valuable comments. The experiment over which this paper is based on is registered at the AEA RCT Registry under the code AEARCTR-0002271.

Internal constraints such as low self-esteem, stress, depression, and hopelessness can frustrate poor persons' aspirations and thus make them less willing to take forward-looking actions to exit poverty (Duflo (2012)). Material deprivation may itself dampen aspirations and lead to even lower levels of effort; this, in turn, may lower material outcomes, thus setting up a vicious cycle, as suggested by Dalton, Ghosal and Mani (2016). These authors hypothesize that policies that stimulate aspiration levels can, at the very minimum, enhance the effectiveness of policies that address material deprivation; they go on to posit that policies that simply raise aspirations could enhance material outcomes even if they do not relax material constraints. Such a policy could be effective, however, only if resource constraints are not binding, so that individuals can sustain their material aspirations. In such a world, an increase in aspirations, in and of itself, could be a successful strategy for encouraging forward-looking behavior.

However, in the presence of resource constraints that thwart people's attempts to achieve their aspirations, those aspirations may have to be adjusted downward in order to relieve the resulting frustration (Selten (1998, 2001) and Karandikar et al. (1998)). Unrealized aspirations may otherwise adversely affect utility. In essence, then, aspirations are adapted to suit the prevailing circumstances, such that they remain constant if they can easily be fulfilled, but are lowered when they are difficult to realize[2]. The latter pose the question of whether higher aspirations are sufficient to trigger forward-looking actions among the poor; or, in contrast, their aspirations may adjust downward over time as resource constraints frustrate aspirational achievement.

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

The experiment worked as follows. Within each slum, a set of randomly chosen families receive new houses. We show that, due to the randomization, there is no gap in the material circumstances between the treatment and non-treatment groups at baseline, and their aspiration to improve within-slum housing conditions are also well balanced and indeed very low. Our first objective is to determine whether the treatment-control housing gap generated by the experiment was followed by a treatment-control housing aspiration gap. Our test is simple. It consists of shocking the housing conditions of randomly

[2] Aspiration 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).

selected neighbors and evaluating whether being exposed to larger housing gaps encourages the untreated neighbors to increase their housing aspirations relative to their treated counterparts.

The randomly introduced housing improvement serves as an exogenous shock to the gap in material circumstances between treated and untreated households. Indeed, after 16 months of treatment exposure, we find that the control group's housing quality is significantly lower than the treatment group's, but no other noticeable material gaps exist between these groups. The program is effective on improving housing conditions but that's all. 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 group. Such an effect cannot be interpreted as a causal spillover effect on control aspirations as for that we would need a counterfactual of what would happen with the control units in the absence of treated neighbors something that we lack in our experimental design. However, we do show that the treatment group's housing aspirations remained invariant in 15% between baseline and post-treatment rounds, suggesting that the treatment-control aspiration gap was in fact mediated by a positive treatment-to-control peer effect. In other words, the housing gap was internalized by untreated households, whose members now aspire to "keep-up with their treatment Joneses"'. Note, however, that we do not find effects on housing investment. The development practitioner would argue that this is pretty much the expected result after just 16 months of treatment exposure, and that the observation of increasing housing investment among control units is a matter of time. Until here, then, we would predict that the results meet reasonably well with a standard model of housing externalities where homeowners will invest on housing renovation based upon the decisions taken by their neighbors (Rossi-Hansberg and Sarte (2012)).

Our experimental design includes, however, exogenous variation in the time of exposure to the treatment at the slum level, so we can compare housing investments and housing aspirations of those exposed to a longer period of treatment exposure with that of those exposed to shorter periods 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 treatment-control aspiration gap observed in the short-run remain constant or adapt downward over time. Indeed, after eight additional months (from month 16th to month 24th), while housing quality and housing investment gaps across experimental groups continue being null, the treatment-control aspiration gap totally disappeared, suggesting not a satisfaction, but a frustration effect among control units. Higher aspirations did not translate into forward-looking actions of housing investment, which discard 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.

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. Finally, we find that external constraints such as income, savings, asset value or labor supply remained constant over time, being treatment and control units equally poor before and after the treatment.

Overall, our aspiration adaptation result suggests two critical aspects for an economic development agenda. First, that if the poor are trapped in an aspiration failure equilibria, this may not be because they lack a certain capacity to aspire to higher living standards (Appadurai (2004)) but because resource constraints make them unable to sustain higher aspirations, which tend to quickly adapt downward 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. Indeed, significant changes in the material conditions experienced by reference-group neighbors can encourage the poor to aspire to unattainably large improvements in their living conditions, in which case their aspiration gains may revert to their baseline levels in relatively short periods of time. We argue that, since aspirations are not necessarily fixed over time, higher aspirations are not a sufficient condition for forward-looking behavior among the poor, and policies designed to stimulate future-oriented actions 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. Our paper differs from previous analyses on aspirations (see, for example, Beaman et al. (2012), Glewwe, Ross and Wydick (2014), 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. Indeed, to the best of our knowledge, this is the first paper to empirically examine aspiration adaptation by the poor; and the first in using experimental variation for this purpose.

Second, our evidence suggests that the observed treatment effects on subjective outcomes may just obey to transitory paths and thus should be followed up for longer periods before claiming robust conclusions about their practical significance. 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, Ray and de Vericourt (2016), "once the euphoria dies down, such effects may vanish". Consequently, testing for short- and long- run effects becomes critical, for which 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) and Galiani, Gertler and Undurraga (2017).

Last but not least, a central element in the literature of aspirations and development is how aspirations are formed, and our paper contributes to this literature as well. 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[3,4]. In this setting, material aspirations are a non-convex 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-convex 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 levels[5]. 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 dint of her own efforts.

Interestingly, we find that the initial treatment-control aspiration gaps are mainly observed among untreated urban slum dwellers. Indeed, we find no gaps at all on the housing aspirations of their rural counterparts, even though they had statistically comparable levels of housing aspirations with respect to urban counterparts at baseline. We hypothesize that this is attributable to the fact that the urban households enjoyed higher incomes and better housing conditions at baseline than the rural slum dwellers did. Since all treatment families receive exactly the same type of house, the size of the experimentally induced gap depends on initial housing conditions. Thus, the treatment-control gap in housing quality for the urban controls was significantly smaller than the housing gap faced by their rural counterparts. In other words, the moderate housing gap confronted by urban controls encouraged them to aspire to achieve the housing conditions of their treatment-group neighbors, while the seemingly insurmountable treatment-control housing gap confronted by untreated rural units thwarted the realization of their housing aspirations. 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-convex and depends on the size of the aspirations gap. However, it is still unclear how such gaps relates to aspiration adaptation processes – something that Ray (1998, 2006) and Genicot and Ray (2017) models do not address.

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

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

[4]The 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, Caball and Raurich (2007), and Dalton, Ghosal and Mani (2016).

[5]By 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.

I. 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 extremely poor populations with the express goal of improving their housing conditions and well-being. As is extensively described in Galiani et al. (2017) and Galiani, Gertler and Undurraga (2017), TECHO targets the poorest informal settlements and, within these settlements, 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, respectively[6].

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 means of a lottery system that gives all eligible households within a settlement an equal opportunity to receive one of the units, such 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 much as slums as they could, regardless of the proportion of treated residents in each selected slum. Hence, 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 behavior of control units should not have been affected by the expectation of being treated in the next round[7,8].

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 that, in each country, Phase I slums were treated in the first year and Phase II slums in the following one. 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 around a year after the construction of the last house in the Phase II slums (see Table A1 in Appendix Appendix A). 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. Figure A1 in Appendix Appendix A details the 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 house at the time of the follow-up survey, since no discretionary criteria were used to select which slum was assigned to

[6] For a full description of the program, see Appendix Appendix B.

[7] Indeed, robust evidence supporting this claim is provided in Section IV.

[8] For a full description of the sampling procedure within each country experiment, see Appendix Appendix B.

which phase. Instead, the decision as to which slums would be treated first and which later on 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 the slum and slum dweller baseline characteristics. Indeed, in order to collect census information, TECHO volunteers were organized in 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 them. As soon as each volunteers 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 such a way as 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. Finally, once the resources required to treat the next slum in line were insufficient, TECHO decided to allocate that slum and the incoming ones 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 rest later on. If differences on 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 it was shown in Galiani, Gertler and Undurraga (2017) and replicated in the next section, we tested 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 found no statistically significant differences across them. These results suggest that the populations in Phases I and II were statistically similar before phase assignment, thereby lending credibility to our assumption that the assignment of slums to phases followed a process that had nothing to do with slum characteristics[9].

Our sample includes a total of 74 slums located in both urban and rural zones, of which 29 were in Phase I and 45 were in Phase II. There were a total of $2,373$ eligible households in these settlements. Our baseline population of slum dwellers is composed of households whose members have been living in the slum for 12 years, on average (see Table A3 in Appendix Appendix A). Their monthly income per capita is, on average, US$55, and most of them live in overcrowded houses made up of very poor materials. 80% of the rooms have

[9]In Section III 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.

walls built of poor-quality materials such as plastic and cardboard; 67% have poor-quality roofs, and only half of the units have a bathroom of their own. The number of rooms per capita is 0.7. Despite this, only 13% of these households reported aspirations to upgrade the quality of their dwelling' 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, 97% of the 13% of households reporting such aspirations said that they cannot satisfy their housing aspirations because of financial constraints which 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 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 (T vs C in phase I sample; T vs C in phase II sample). Also, the attrition rates are balanced between phase groups (Phase I vs Phase II). Finally, since our sample consider 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.

II. 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, scale dependence, respondent role playing and instability over time or over respondents' moods can all affect inter-person comparability of responses. Moreover, respondents may understand questions differently, or can even affect intra-person comparability, as they may interpret the same wording differently when their attention is directed toward different aspects of their lives. A number of recent studies have attempted to assess the reliability of attitudinal data on such aspects as subjective well-being and expectation measures and their possible limitations[10]. The general

[10] See, for example, Manski (2004), Krueger and Schkade (2008), and Delavalande and Mckenzie (2011).

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.

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 aspirations over the same metric scale, for which a closed set of aspiration options/levels is required. 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; (ii) Continue living in the same slum but get improved housing and own land; (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).

There are four key features to highlight. First, note that these categories represent reliable measures of aspirations, since they are all future-oriented (Bandura et al. (2001)). Second, the set of aspiration options are all reasonably achievable given the slum dwellers' baseline housing conditions and thus are all expected to be part of the "aspirations window" (Ray (2006)).

Third, the proffered aspiration set is flexible in the sense that it gives 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 on people's housing conditions, aspirations (ii) and (iv) dominate options (i) and (iii) since the first include housing improvements while the latter do not. The rank order between (i) and (ii) (and between (iii) and (iv)) 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 so they can optimize their geography of labor opportunities. The fact that we cannot rank locational aspirations should not thread the validity of our aspiration measure as far as the objective is to measure housing aspirations, and the inclusion of locational alternatives only plays the role of making the aspiration set more realistic. Indeed, our experiment shocks the housing conditions of reference neighbors, such that the primary expected result is a change on the housing aspirations of neighbors, not their locational ones. Hence, for the sake of housing aspirations, we just concentrate on testing whether being exposed to treated neighbors generate any increase in the probability of aspiring to (ii) or (iv) to the detriment of options (i) or (iii).

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. The question was designed and pre-tested to capture aspirations, not beliefs. In that sense, the question should be read in the spirit of "If you had to spend your own money right now on changing your housing situation, what would you do?". Indeed, in order to check the construct validity of our aspiration question, we replicated exactly the same question (with exactly the same 4 alternatives) but instead of end the question by asking "What would you choose?" we asked 'Wwhat do you expect to happen?". That is, we used a measure of "expectations" (beliefs) as a related but nonequivalent variable to test the construct validity of the aspiration measure. And in fact, 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 '$j's \neq$ '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 since, 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.

III. Empirical Strategy

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

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

where Y_{ij} is a dummy variable equal to 1 if head of household i living in settlement j aspires to a given housing upgrading category, and 0 otherwise; $Control_{ij}$ is a dummy variable equal to 1 if household i in settlement j was not offered a TECHO house and 0 otherwise; $PhaseI_j$ is a dummy variable equal to 1 if settlement 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 settlement fixed effects; and ε_{ij} is the error term[11,12].

The settlement fixed effects capture the average unobservable differences across settlements (and hence countries). This is important, since randomization was conducted within

[11] As 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.

[12] Since the phase design of the intervention is given at the settlement level, there is no within-settlement variation in phase.

each settlement. Another important factor is that settlement fixed effects also control for differences in the reference points for housing aspirations, which may vary geographically. Finally, after controlling for settlement fixed effects, we assume that the error terms are independent and thus report only robust standard errors[13].

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 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 continue 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 full or 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 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 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

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

settlements 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 and 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 treated 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 might 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. Nonetheless, as shown by Galiani et al. (2017), receipt of the TECHO house only produced effects dealing with the quality of floors, walls, and roofs, but 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 those 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 IV).

IV. Results

IV.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 for all of them together[14] ,[15]. 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 of all, in urban slums, the probability of aspiring to upgrade housing conditions within the slum in Phase II (short exposure) is substantially higher among untreated units than among the treatment group, 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 it is in the treatment group[16].

The latter is clearly not 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 (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 aspire to upgrade the materials used in their existing houses) and are somewhat lower than the aspiration to upgrade housing conditions within the slum at follow-up (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 housing aspirations of treated units (who actually were quite satisfied with their housing conditions after the program, as is shown by Galiani, Gertler and Undurraga (2017)). Overall, the latter suggests that the control-treatment aspiration gap is likely to be explained by a positive treatment-to-control peer effect, i.e., the housing gap that was internalized by untreated households, whose members now aspire to "keep-up with their treatment Joneses'". [17].

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

[15]Our results are robust to using a Probit or a Logit 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.

[16]We 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

[17]While 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

Testing nonconvexities. Interestingly, the aspirational effects are observed only among urban households, as no effects are found in the rural sample. We hypothesize that this is due to the differences in the treated-untreated post-program housing gaps confronted by control units in each zone. First, urban and rural households have statistically similar levels of housing aspirations at baseline (see Table A7 in Appendix Appendix A). Second, the TECHO house that is provided to the program beneficiaries is the same irrespective of the type of zone. However, urban households are better-off than their rural counterparts at baseline in regard to both income and housing conditions. Hence, the post-treatment treatment-control housing gap in urban slums is smaller relative to the gap in rural slums. Indeed, as is shown in Table A8 in Appendix 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 slums[18].

Ray (1998, 2006) and Genicot and Ray (2017) 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-convex and depends on the size of the aspirations gap. We hypothesize that the "seemingly insurmountable" treatment-control 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 housing aspirations).

Testing Aspiration Adaptation. The control-treatment housing aspiration gap in urban slums do 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 and out-of-a-slum housing 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 the Appendix illustrates these results. As long as the treatments' aspirations continue remaining constant between month 16th and month 24th (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 two measures are not exactly the same, both of them are indicators of aspirations to upgrade housing conditions within the slum; given that this is the key attribute under study, it is reasonable to conclude that they are fairly comparable over this particular dimension.

[18] In 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.

the observed adaptation effects may not be causally attributable to controls' downward movements. We test this by evaluating whether the distributions of settlement fixed effects significantly differ across phase samples[19]. In particular, using the Kolmogorov-Smirnov test, we cannot reject the null hypothesis of equality of distributions for all aspiration variables, indicating that, in general, the treatment groups for Phases I and II do not differ significantly in their post-treatment housing aspirations[20,21]. Overall, this suggests 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 treatment-control 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 at all 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 quality and access to water, sanitation, and/or electricity, and, if so, how much they had invested. These indicators work as a proxy of the level of effort exerted to satisfy their housing aspirations. As shown in Table 3, we find no effect at all either 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 such housing services and, here again, we find no effect at all. This indicates that resource constraints impede the sustainability of housing aspirations.

One way to examine the role of material means on aspiration adaptation in more detail is to test for heterogeneous adaptation effects across high- and low-income subgroups of urban slum dwellers. One would expect that adaptation effects are, if anything, smaller in less poor groups. As Table A9 in Appendix Appendix A shows, while the

[19] In the full regression, we could infer this from the coefficient for a Phase I dummy variable, but since this does not vary within settlements, and since settlement fixed effects are included, this cannot be estimated in the main specification.

[20] Note that, as is shown by Figure A2, the treatment mean never differ between phases, and this is the case for both the aspiration to upgrade in and out of the slum as well as for the full sample and urban and rural sub-samples.

[21] Note also that, as shown in the last three columns of Table A4, treatment groups are well balanced across phases at baseline. Therefore, potential pre-treatment differences across treated individuals are less of a concern here.

housing conditions and housing satisfaction do not differ that much between above- and below-the-median baseline income subgroups, above-the-median individuals earn on average US$110 per capita per month, which is more than 5 times higher than what is earned by their below-the-median counterparts. Being richer can be a factor that influences the course and sustainability of housing aspirations. In fact, as is shown in Table 4, while above-the-median households display moderate adaptation effects in terms of within-slum upgrading aspirations, the adaptation effect exhibited by their poorer counterparts is 3.4 times greater[22,23]. In fact, on average, the below-the-median untreated units end up having a significantly lower level of within-slum housing aspirations than their treatment-group counterparts ($H_0 : \gamma_1 + \gamma_2 = 0$ is rejected). Then, when looking at the out-of-slum housing aspirations, we observe the opposite trend, i.e., an upward adaptation, which is, again, larger among poorer households.

Overall, the latter indicates 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 individuals[24].

IV.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 $j = 1, .., 4$. In other words, the method starts with testing the most significant hypothesis

[22] This 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 $(-0.15/0.06)/(-0.08/0.11) = -2.5/-0.72 = 3.44$.

[23] The 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 groups. See Table A10 in Appendix Appendix A.

[24] We 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 housing 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, 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). We find that the proportions of attriters and movers are very low in the sample as a whole (less than 10%); the differences are insignificant across experimental groups within each phase, and the results remain constant between phases and are robust across urban and rural zones as well. 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 housing aspirations did not translate into higher migration rates. These results are available upon request.

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 Holmes FWER corrections at the 10% level of statistical significance. That is, for our most significant hypothesis (whether the individual aspires to move and upgrade outside of a slum), the corrected p-value is $0.1/4 = 0.025$; for the second most significant hypothesis (whether the individual aspires to upgrade within the slum), 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 Appendix A reports the estimates separately by country. The estimated magnitudes of the short-run effect on aspirations to upgrade either within or out of a slum, γ_1, are of about the same magnitudes for all countries, but statistically significant mostly for the case 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 in all of them. 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 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 A1 are robust to controlling for follow-up housing quality measures in our main regression. As is shown in Table A13 in Appendix 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 effect[25].

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 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; (ii) Continue living in the same slum but get improved housing and own land; (iii) Move to another slum; and (iv) Move and get improved housing and own land outside a slum (with four location alternatives: in the same municipality, in another municipality, in another state, or somewhere else). As is shown in Table A14 in Appendix 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 in three different ways. First, it lends credibility to the construct validity of our aspiration measures. Second, it rules out potential validity threats associated with an expectation on the part of untreated units 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.

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. And, in fact, we found that this was precisely the case. This suggests that expectations played no role in the aspiration adaptation

[25] As 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.

process[26,27].

Happiness Adaptation and Aspirations. Aspirations may also be determined by subjective well-being. 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 subjective well-being (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 owing to the presence of relative status effects (Clark, Frijters and Shields (2008), 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, which may or may not be the case. 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, Haisken-De New and MacCulloch (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, Gertler and Undurraga (2017)), we found that TECHO beneficiaries' level of satisfaction with their housing quality and 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

[26]In 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.

[27]A 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.

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 consumption (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 is simply 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 the one between SQL and the aspiration to stay in the slum and have no change in living conditions (0.145) and the least correlation being the one between SQL and the aspiration to move and obtain improved housing and land outside of a slum (-0.003). Second, and 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, all of which 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 time[28]. 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).

[28]Indeed, 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.

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 almost all of our 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 levels[29]. This suggests that aspiration adaptation is unrelated to changes in internal resources such as subjective well-being.

IV.3. ***Structural Estimation***

As in Kimball, Nunn and Silverman (2015) and Galiani, Gertler and Undurraga (2017), 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: aspiration to upgrade housing within the slum and aspiration to upgrade housing out of a slum[30]. We then test whether housing aspirations return to their baseline level and, if so, when (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:

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

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,

[29] These results are available upon request.

[30] The results derived from reduced-form analysis indicate that being indirectly exposed to the TECHO program had no impact at all on the aspirations to "keep the same conditions within the slum" and "move to another slum", so we discarded these outcomes from the structural analysis.

and c_j the country fixed effects[31]. 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; this is expressed as a monthly rate.

Our empirical strategy to estimate the structural parameters of the model is simple. We use a non-linear least squares (NLS) estimator given by:

$$\hat{\theta} = argmin_{(\theta)} \sum_{i=1}^{N} [y_i - f(x_i;\theta)]^2 \tag{3}$$

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, which presents estimates of β_P, β_T, and δ for the two housing aspirations described above.

First of all, in the case of within-slum housing 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 28^{th} month of exposure, the effect should be close to zero. Our range of months of exposure goes from 13 to 30. 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 housing 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 both treatment and control households. Both graphs show a

[31]Since the number of months of exposure to the treatment, t_i, does not vary within slums, then 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.

reduction in the distance between the treatment and control groups as treatment exposure increases, with this difference narrowing to almost zero by around 28^{th} 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^{th}, we would probably have observed a larger decay in housing aspirations, with that greater decline being explained primarily by a greater non-intention-to-treat effect in the period immediately after the treatment −something that our data does not allow us to observe. In any case, 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 housing upgrades is not the only substitute of within-slum housing aspirations. Indeed, not all individuals that have abandoned within-slum housing 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 population into income subgroups. Not surprisingly, and consistent with the reduced-form analysis described in the previous section, we observe that the within-slum housing 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 18^{th} month of exposure, the richer neighbors adapted their aspirations much faster than the poorer ones. However, it seems that from the 18^{th} 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 housing aspirations of low-income untreated households continued to decrease and, after the 22^{nd} 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, we have that the total gains, i.e., the total "area under the curve", can be calculated as:

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

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.

First of all, and consistent with our estimates of β_P, β_T, and δ, we observe that the permanent gains are not significant for our indicator of within-slum housing aspirations, with the positive and significant total gains being mostly explained by the transitory effect. Second, the OLS pooled coefficient is shown to be 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 housing aspirations show insignificant permanent, transitory, and total gains, the pooled OLS coefficient is negative at the 10% level of significance. Overall, this suggests that studying adaptation of aspirations over time is crucial for a rigorous interpretation of life-event effects on aspiration outcomes in the long run.

V. Conclusion

In her Tanner lectures, Duflo (2012) asserts that 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 their development outcomes. However, the author also recognizes that 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 states that more 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.

Our results are conclusive. 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 on improving housing conditions but nothing else. 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 treatment-control housing gap and thus now aspire to "keep-up" with the treatment Joneses'. However, after 8 additional months (from months 16 to 24) the aspirational difference totally disappeared, and this effect is fully explained by the adaption on the part of the control group, as the treatment group's housing aspirations remain unchanged over the same period of analysis. The 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 equilibria, this may not be because they lack a certain capacity to aspire to higher living standards (Appadurai (2004), Dalton, Ghosal and Mani (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, Nunn and Silverman (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, with 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, Gertler and Undurraga (2017). 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 ups and downs in the level of 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, Gertler and Undurraga (2017) 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.

Interestingly, however, our results are valid only for urban slum dwellers, who are confronted with moderate housing gaps with respect to their treated neighbors. In contrast, the housing aspirations of their rural counterparts, whose economic status and housing conditions differ more sharply from those of their treatment neighbors, did not change at all. The moderate treatment-control 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 better 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 more costly 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 to be 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 can potentially generate large unintended inequalities among neighbors should at least try 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 here benefits are equally distributed across neighbors and thus potential effects on 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 goals that poor populations will actually be able to achieve, thus averting adaptation and frustration. In the words of Duflo (2012), this means to "create goals that are bite-sized and achievable for poor people to get started". This is consistent with recent literature which advances the argument that lowering the aspirations of low-income students to more reachable levels will reduce the likelihood of their dropping out of school in the US (Kearney and Levine (2014)) and in France (see Goux, Gurgand and Maurin (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, Chowdhury and Mobarak (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 at the origin 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 so 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

	Urban					Rural					All				
		Model 1		Model 2			Model 1		Model 2			Model 1		Model 2	
Dependent Variable	Follow-Up Treat. Mean	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I	Follow-Up Treat. Mean	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I	Follow-Up Treat. Mean	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I
		γ_1	γ_2	γ_1	γ_2		γ_1	γ_2	γ_1	γ_2		γ_1	γ_2	γ_1	γ_2
Aspire to stay in the slum	0.34	0.01	-0.08	0.01	-0.07	0.59	0.04	0.00	0.04	-0.01	0.46	0.03	-0.05	0.03	-0.04
and keep the same conditions	(0.48)	(0.04)	(0.06)	(0.04)	(0.06)	(0.49)	(0.04)	(0.07)	(0.04)	(0.07)	(0.50)	(0.03)	(0.04)	(0.03)	(0.04)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.16		0.17			0.52		0.55			0.57		0.59	
Aspire to stay in the slum and get	0.16	0.09	-0.11	0.09	-0.11	0.28	-0.03	0.01	-0.03	0.01	0.22	0.03	-0.05	0.03	-0.05
improved housing and own land	(0.37)	(0.04)	(0.05)	(0.03)	(0.05)	(0.45)	(0.04)	(0.06)	(0.04)	(0.06)	(0.41)	(0.03)	(0.04)	(0.03)	(0.04)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.65		0.59			0.59		0.63			0.48		0.45	
Aspire to move to another slum	0.02	0.01	0.02	0.01	0.02	0.01	-0.01	0.02	-0.01	0.02	0.01	0.00	0.02	0.00	0.02
	(0.12)	(0.01)	(0.02)	(0.01)	(0.02)	(0.10)	(0.01)	(0.02)	(0.01)	(0.02)	(0.11)	(0.01)	(0.01)	(0.01)	(0.01)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.14		0.14			0.42		0.43			0.10		0.11	
Aspire to move and get improved	0.48	-0.11	0.17	-0.11	0.17	0.12	0.00	-0.02	0.00	-0.02	0.31	-0.05	0.07	-0.05	0.07
housing and own land outside of a slum	(0.50)	(0.04)	(0.06)	(0.04)	(0.06)	(0.33)	(0.02)	(0.04)	(0.02)	(0.04)	(0.46)	(0.02)	(0.04)	(0.02)	(0.04)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.23		0.23			0.53		0.54			0.50		0.49	
Slum Fixed Effects		Yes		Yes			Yes		Yes			Yes		Yes	
Baseline Covariates		No		Yes			No		Yes			No		Yes	

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.

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

Dependent Variable	Follow-Up Treat. Mean	Model 1 Cont. γ_1	Model 1 Cont. × Phase I γ_2	Model 2 Cont. γ_1	Model 2 Cont. × Phase I γ_2
Assets Value Per Capita (USD)	74.89	0.89	-25.46	2.19	-25.84
	(163.24)	(12.48)	(21.61)	(12.30)	(20.31)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.16		0.15	
Monthly Income Per Capita (USD)	77.40	-1.31	19.38	-1.99	20.86
	(115.15)	(9.76)	(19.41)	(9.65)	(19.51)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.28		0.26	
Hours worked last week by HH	40.78	0.21	-0.05	-0.05	-0.50
	(19.23)	(1.96)	(3.07)	(2.00)	(3.04)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.95		0.81	
Hours worked last week by Spouse	36.97	3.49	-5.95	3.26	-5.49
	(20.08)	(2.91)	(4.40)	(2.92)	(4.42)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.46		0.50	
If any household's member have savings	0.03	0.02	0.00	0.02	0.00
	(0.16)	(0.01)	(0.02)	(0.02)	(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.

Table 3: Housing Investment - Urban Only

Dependent Variable	Follow-Up Treat Mean	Model 1 Control γ_1	Model 1 Control $\times$ Phase I γ_2	Model 2 Control γ_1	Model 2 Control $\times$ Phase I γ_2
If invested on Housing Quality during the last 12 months	0.40	0.04	-0.02	0.05	-0.03
	(0.49)	(0.04)	(0.08)	(0.04)	(0.08)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.74		0.75	
If invested on access to water during the last 12 months	0.09	-0.02	0.02	-0.02	0.02
	(0.28)	(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.97		0.96	
If have access to water in terrain	0.81	-0.03	0.05	-0.03	0.05
	(0.39)	(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.52		0.49	
If invested on sanitation during the last 12 months	0.08	-0.01	0.03	-0.01	0.03
	(0.27)	(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.66		0.64	
If have access to own bathroom	0.69	-0.02	0.03	-0.02	0.03
	(0.46)	(0.04)	(0.06)	(0.04)	(0.06)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.80		0.75	
If invested on electricity during the last 12 months	0.12	-0.01	0.00	-0.01	-0.01
	(0.32)	(0.03)	(0.05)	(0.03)	(0.06)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.86		0.68	
If have access to electricity	0.90	0.00	0.04	0.00	0.03
	(0.31)	(0.02)	(0.03)	(0.02)	(0.03)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.17		0.17	
Amount invested on housing during the last 12 months	68.29	-10.45	-13.50	-10.30	-15.62
	(226.71)	(12.34)	(28.46)	(12.53)	(28.53)
p-value ($\gamma_1 + \gamma_2 = 0$)		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 $\gamma_1 + \gamma_2 = 0$.

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

	High Income Status ($> p50$)					Low Income Status ($\leq p50$)				
		Model 1		Model 2			Model 1		Model 2	
Dependent Variable	Follow-Up Treat. Mean	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I	Follow-Up Treat. Mean	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I
		γ_1	γ_2	γ_1	γ_2		γ_1	γ_2	γ_1	γ_2
Aspire to stay in the slum	0.33	-0.01	-0.06	-0.03	-0.05	0.36	0.04	-0.09	0.06	-0.12
and keep the same conditions	(0.47)	(0.05)	(0.08)	(0.05)	(0.08)	(0.48)	(0.06)	(0.09)	(0.06)	(0.09)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.22		0.19			0.53		0.39	
Aspire to stay in the slum and get	0.16	0.10	-0.08	0.11	-0.08	0.16	0.08	-0.18	0.06	-0.15
improved housing and own land	(0.37)	(0.04)	(0.07)	(0.04)	(0.07)	(0.37)	(0.06)	(0.07)	(0.06)	(0.07)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.64		0.59			0.04		0.07	
Aspire to move to another slum	0.01	0.03	0.00	0.03	0.00	0.02	-0.02	0.04	-0.02	0.04
	(0.11)	(0.02)	(0.03)	(0.02)	(0.03)	(0.14)	(0.01)	(0.03)	(0.02)	(0.03)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.12		0.13			0.35		0.36	
Aspire to move and get improved	0.49	-0.11	0.13	-0.11	0.13	0.46	-0.10	0.22	-0.10	0.23
housing and own land outside of a slum	(0.50)	(0.05)	(0.08)	(0.05)	(0.08)	(0.50)	(0.06)	(0.09)	(0.06)	(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 subgroups 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.

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

	High Income Status ($> p50$)					Low Income Status ($\leq p50$)				
		Model 1		Model 2			Model 1		Model 2	
Dependent Variable	Follow-Up Treat. Mean	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I	Follow-Up Treat. Mean	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I
		γ_1	γ_2	γ_1	γ_2		γ_1	γ_2	γ_1	γ_2
If invested on housing quality	0.21	0.00	0.06	0.01	0.06	0.26	0.05	-0.11	0.05	-0.11
during the last 12 months	(0.41)	(0.04)	(0.07)	(0.04)	(0.07)	(0.44)	(0.05)	(0.09)	(0.06)	(0.09)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.28		0.26			0.46		0.44	
If invested on access to water	0.06	-0.02	-0.01	-0.01	-0.01	0.04	0.00	0.05	-0.01	0.05
during the last 12 months	(0.23)	(0.02)	(0.04)	(0.02)	(0.04)	(0.21)	(0.05)	(0.07)	(0.03)	(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	-0.02	0.02	-0.02	0.01	0.81	-0.05	0.07	-0.05	0.07
	(0.39)	(0.04)	(0.06)	(0.04)	(0.06)	(0.39)	(0.05)	(0.07)	(0.04)	(0.07)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.99		0.84			0.64		0.70	
If invested on sanitation	0.05	-0.02	0.04	-0.01	0.04	0.04	0.02	-0.01	0.01	0.00
during the last 12 months	(0.22)	(0.03)	(0.04)	(0.03)	(0.04)	(0.21)	(0.03)	(0.05)	(0.03)	(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	0.04	0.03	0.05	0.03	0.73	-0.11	0.07	-0.10	0.05
	(0.48)	(0.05)	(0.08)	(0.05)	(0.08)	(0.45)	(0.06)	(0.09)	(0.06)	(0.09)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.31		0.25			0.52		0.47	
If invested on electricity	0.05	0.02	0.02	0.02	0.01	0.09	-0.05	0.02	-0.05	-0.02
during the last 12 months	(0.22)	(0.03)	(0.04)	(0.03)	(0.04)	(0.29)	(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.24		0.36			0.13		0.13	
If have access to electricity	0.89	0.03	0.04	0.03	0.04	0.90	-0.05	0.08	-0.05	0.08
	(0.31)	(0.03)	(0.04)	(0.03)	(0.04)	(0.30)	(0.04)	(0.06)	(0.04)	(0.06)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.04		0.04			0.52		0.54	
Amount invested on housing	71.80	5.93	-13.79	6.97	-19.60	83.71	-29.05	20.74	-24.36	18.42
during the last 12 months	(271.76)	(15.17)	(54.85)	(15.63)	(54.77)	(280.13)	(18.96)	(47.54)	(20.43)	(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 subgroups 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.

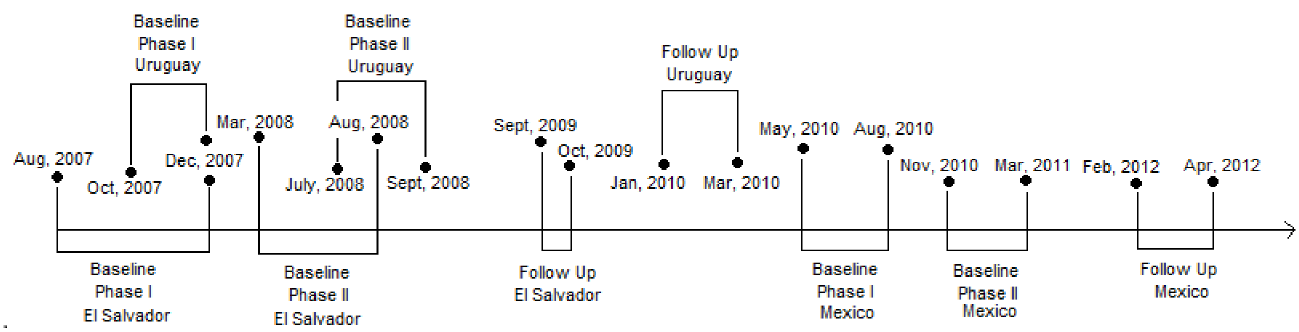
Table 6: Structural Estimation - Urban Only

	NLS Estimates				Aspiration Gains Area			OLS Estimate
Dependent Variable	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	
Aspire to stay in the slum and get improved housing and own land	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)
Aspire to move and get improved housing and own land outside of a slum	0.48 (0.50)	-0.01 (0.10)	-0.10 (0.11)	0.16 (0.54)	-0.21 (1.46)	-0.44 (1.01)	-0.65 (0.55)	-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.

Appendix A. Tables and Figures (For Online Publication)

Figure A1: Timeline of Intervention and Surveys



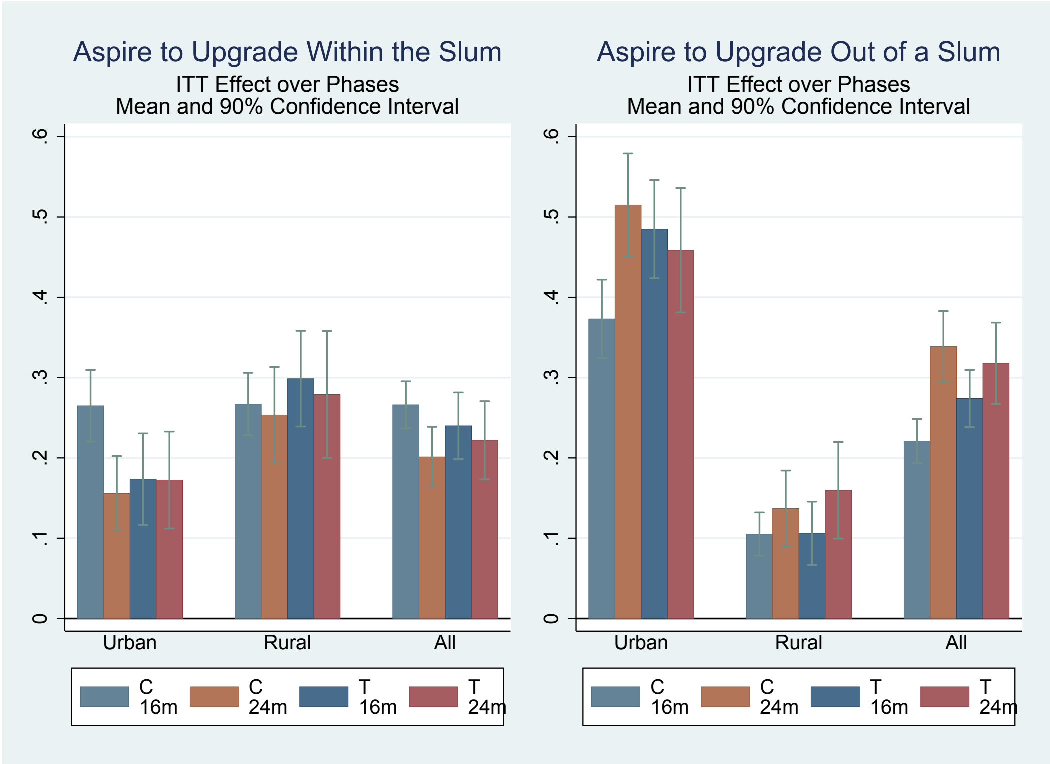


Figure A2: Treatment Effects on Aspirations, by Phase

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.

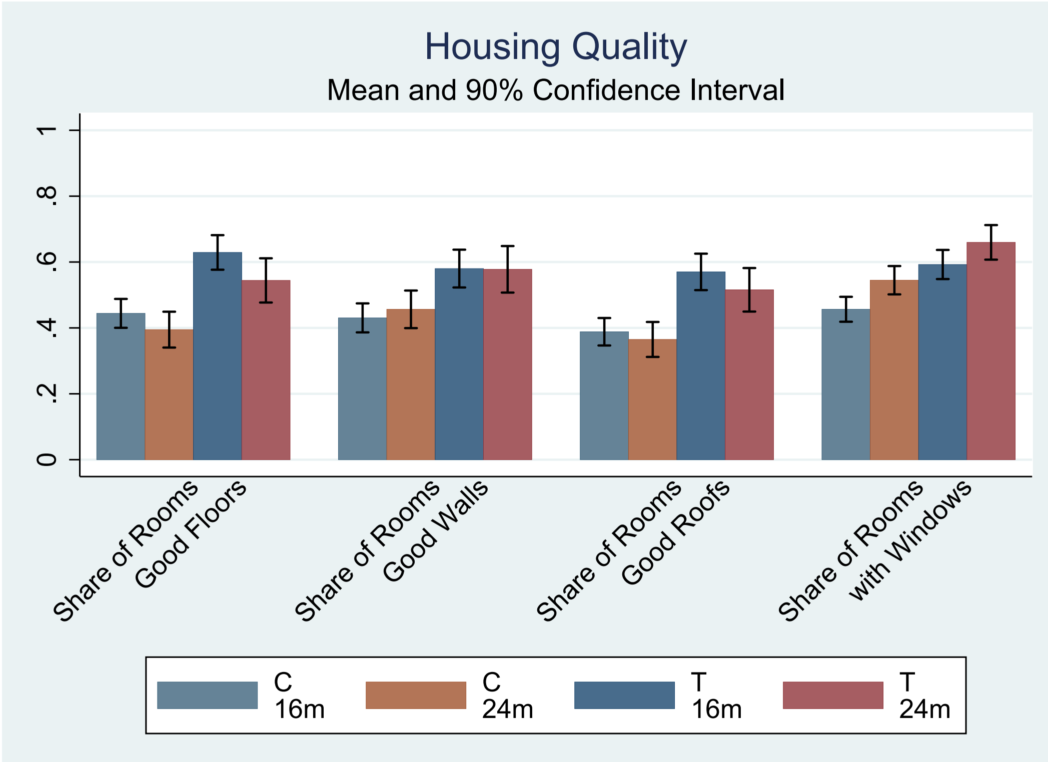


Figure A3: Treatment Effects on Housing Quality, by Phase - Urban Only

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.

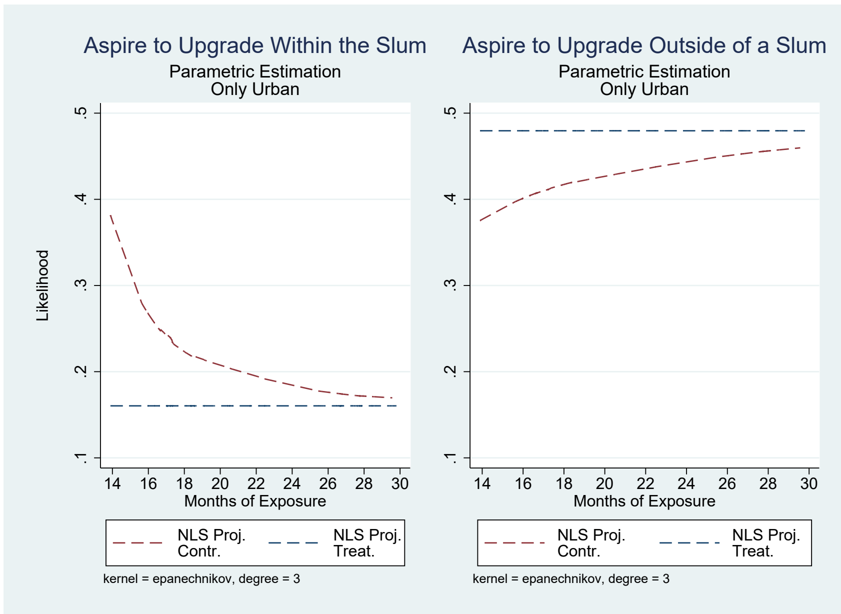


Figure A4: Aspirations Adaptation - NLS Estimation - Urban Only

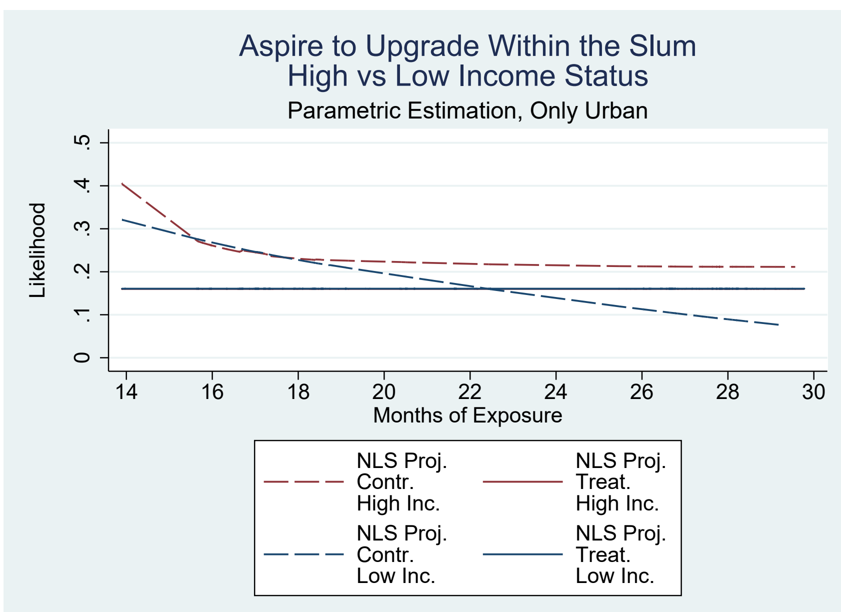


Figure A5: Aspirations Adaptation by Income Status - NLS Estimation - Urban Only

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
El Salvador									
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
Uruguay									
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
Mexico									
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
All countries									
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	0.08	-0.01	0.06	0.07	-0.01	0.07	0.07	0.00	0.06	0.06	0.00
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	[0.01]	[0.01]	[0.01]	[0.01]	[0.01]	[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	0.10	-0.03	0.06	0.07	-0.01	0.08	0.07	0.01	0.07	0.06	0.01
	(0.01)	(0.02)	(0.03)	(0.01)	(0.02)	(0.02)	[0.02]	[0.01]	[0.02]	[0.01]	[0.02]	[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	0.05	0.01	0.07	0.08	-0.01	0.05	0.07	-0.02	0.05	0.06	-0.01
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)	[0.01]	[0.01]	[0.02]	[0.02]	[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.

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

	Phase I			Phase II			Phase I vs Phase II All			Phase I vs Phase II Only Treatments		
Dependent Variable	Treat.	Control	Diff.	Treat.	Control	Diff.	Phase I	Phase II	Diff.	Phase I	Phase II	Diff.
Years living in the slum	9.82	11.19	0.26	12.80	13.32	0.84	10.34	13.06	-2.72	9.82	12.80	-2.97
	(0.66)	(0.89)	(0.91)	(0.54)	(0.56)	(0.74)	[2.47]	[1.33]	[2.78]	[2.18]	[1.54]	[2.65]
Z-score Housing Quality	-0.05	0.00	-0.03	0.01	0.00	0.03	-0.03	0.01	-0.04	-0.05	0.01	-0.07
Summary Index	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.04)	[0.04]	[0.02]	[0.05]	[0.07]	[0.05]	[0.08]
Aspire to Upgrade *in-situ*	0.17	0.14	-0.03	0.12	0.12	-0.01	0.16	0.12	0.04	0.17	0.12	0.05
Housing Quality Materials	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	[0.03]	[0.02]	[0.03]	[0.03]	[0.02]	[0.04]
Satisfaction with Floor Quality	0.19	0.21	0.01	0.25	0.27	0.01	0.20	0.26	-0.06	0.19	0.25	-0.06
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	[0.02]	[0.04]	[0.04]	[0.03]	[0.04]	[0.05]
Satisfaction with Wall Quality	0.15	0.18	-0.02	0.16	0.17	0.03	0.16	0.16	-0.01	0.15	0.16	-0.02
	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	[0.02]	[0.02]	[0.03]	[0.03]	[0.02]	[0.04]
Satisfaction with Roof Quality	0.17	0.20	-0.02	0.16	0.17	0.02	0.18	0.16	0.01	0.17	0.16	0.01
	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	[0.02]	[0.02]	[0.03]	[0.02]	[0.02]	[0.03]
Satisfaction with Rain Protection	0.16	0.19	-0.01	0.15	0.14	0.03	0.17	0.14	0.02	0.16	0.15	0.01
	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	[0.02]	[0.02]	[0.03]	[0.03]	[0.02]	[0.03]
Satisfaction with Quality of Life	0.28	0.25	0.02	0.28	0.27	0.01	0.27	0.27	0.00	0.28	0.28	0.01
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	[0.02]	[0.03]	[0.04]	[0.03]	[0.03]	[0.04]
Monthly Income Per Capita (USD)	49.45	59.85	-8.61	52.86	58.74	-5.08	53.08	55.77	-2.69	49.45	52.86	-3.40
	(2.63)	(4.29)	(5.99)	(2.54)	(2.94)	(4.32)	[4.01]	[4.27]	[5.82]	[4.54]	[4.34]	[6.24]
Head's Years of Schooling	4.09	4.34	-0.01	4.37	3.87	0.26	4.18	4.13	0.05	4.09	4.37	-0.29
	(0.14)	(0.20)	(0.21)	(0.12)	(0.12)	(0.17)	[0.52]	[0.29]	[0.59]	[0.45]	[0.32]	[0.55]
Head is Male	0.69	0.69	-0.01	0.69	0.71	0.00	0.69	0.70	-0.01	0.69	0.69	0.00
	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	[0.04]	[0.03]	[0.05]	[0.04]	[0.04]	[0.05]
Head's Age	42.09	41.33	0.52	41.2	40.73	1.01	41.83	40.97	0.86	42.09	41.20	0.89
	(0.63)	(0.77)	(1.07)	(0.59)	(0.61)	(0.87)	[0.96]	[0.70]	[1.18]	[1.09]	[0.72]	[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 99^{th} percentile were excluded.

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

	Phase I			Phase II			Phase I vs Phase II All			Phase I vs Phase II Only Treatments		
Dependent Variable	Treat.	Control	Diff.	Treat.	Control	Diff.	Phase I	Phase II	Diff.	Phase I	Phase II	Diff.
Years living in the slum	7.94	8.50	0.72	9.27	11.70	-0.37	8.14	10.41	-2.27	7.94	9.27	-1.33
	(0.70)	(1.02)	(0.90)	(0.64)	(0.85)	(0.96)	[3.00]	[2.06]	[3.56]	[2.55]	[2.01]	[3.18]
Z-score Housing Quality	-0.04	0.00	0.00	0.15	0.19	0.01	-0.02	-0.02	-0.01	-0.04	-0.04	0.00
Summary Index	(0.04)	(0.05)	(0.07)	(0.02)	(0.03)	(0.04)	[0.07]	[0.03]	[0.07]	[0.10]	[0.06]	[0.11]
Aspire to Upgrade *in-situ*	0.16	0.11	0.00	0.11	0.12	-0.01	0.14	0.11	0.03	0.16	0.11	0.05
Housing Quality Materials	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)	[0.03]	[0.01]	[0.04]	[0.04]	[0.02]	[0.04]
Satisfaction with Floor Quality	0.15	0.19	0.01	0.21	0.27	-0.01	0.16	0.23	-0.07	0.15	0.21	-0.05
	(0.02)	(0.03)	(0.04)	(0.02)	(0.03)	(0.03)	[0.03]	[0.06]	[0.07]	[0.03]	[0.05]	[0.06]
Satisfaction with Wall Quality	0.11	0.15	-0.03	0.15	0.18	0.00	0.12	0.16	-0.04	0.11	0.15	-0.04
	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)	[0.02]	[0.04]	[0.04]	[0.03]	[0.04]	[0.04]
Satisfaction with Roof Quality	0.14	0.20	-0.03	0.16	0.17	0.03	0.16	0.16	0.00	0.14	0.16	-0.02
	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)	[0.02]	[0.03]	[0.04]	[0.02]	[0.04]	[0.04]
Satisfaction with Rain Protection	0.13	0.19	-0.01	0.15	0.14	0.01	0.15	0.14	0.00	0.13	0.15	-0.01
	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)	[0.03]	[0.01]	[0.03]	[0.03]	[0.02]	[0.04]
Satisfaction with Quality of Life	0.24	0.20	0.01	0.25	0.31	-0.03	0.23	0.28	-0.05	0.24	0.25	-0.01
	(0.02)	(0.03)	(0.04)	(0.02)	(0.03)	(0.04)	[0.03]	[0.04]	[0.05]	[0.03]	[0.04]	[0.05]
Monthly Income Per Capita (USD)	56.03	61.17	-3.88	65.25	81.11	-17.32	60.00	69.53	-9.54	57.74	65.25	-7.51
	(3.86)	(5.38)	(7.86)	(4.86)	(7.51)	(10.47)	[4.55]	[4.60]	[6.34]	[5.74]	[5.69]	[7.93]
Head's Years of Schooling	4.81	5.53	-0.26	5.55	4.79	0.47	5.04	5.19	-0.15	4.81	5.55	-0.74
	(0.18)	(0.27)	(0.31)	(0.17)	(0.18)	(0.24)	[0.60]	[0.33]	[0.67]	[0.52]	[0.23]	[0.56]
Head is Male	0.61	0.63	-0.03	0.57	0.63	-0.01	0.62	0.60	0.02	0.61	0.57	0.04
	(0.03)	(0.04)	(0.05)	(0.03)	(0.03)	(0.04)	[0.04]	[0.05]	[0.06]	[0.05]	[0.05]	[0.07]
Head's Age	41.52	39.93	1.61	39.84	39.46	1.17	41.00	39.66	1.34	41.52	39.84	1.68
	(0.78)	(1.01)	(1.33)	(0.78)	(0.90)	(1.21)	[1.43]	[0.79]	[1.59]	[1.61]	[0.70]	[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 99^{th} percentile were excluded.

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

	Phase I			Phase II			Phase I vs Phase II All			Phase I vs Phase II Only Treatments		
Dependent Variable	Treat.	Control	Diff.	Treat.	Control	Diff.	Phase I	Phase II	Diff.	Phase I	Phase II	Diff.
Years living in the slum	13.63	15.37	-0.52	15.92	14.56	1.88	14.36	15.23	-0.88	13.63	15.92	-2.30
	(1.36)	(1.57)	(1.91)	(0.82)	(0.74)	(1.11)	[3.16]	[1.32]	[3.36]	[3.19]	[1.42]	[3.43]
Z-score Housing Quality	-0.08	0.00	-0.07	0.06	0.00	0.06	-0.05	0.03	-0.08	-0.08	0.06	-0.14
Summary Index	(0.06)	(0.05)	(0.08)	(0.04)	(0.03)	(0.05)	[0.06]	[0.03]	[0.06]	[0.09]	[0.06]	[0.11]
Aspire to Upgrade *in-situ*	0.18	0.23	-0.09	0.13	0.12	-0.01	0.20	0.12	0.07	0.18	0.13	0.05
Housing Quality Materials	(0.03)	(0.05)	(0.07)	(0.03)	(0.02)	(0.04)	[0.04]	[0.03]	[0.05]	[0.05]	[0.05]	[0.07]
Satisfaction with Floor Quality	0.26	0.24	0.03	0.30	0.28	0.03	0.25	0.29	-0.04	0.26	0.30	-0.04
	(0.03)	(0.04)	(0.05)	(0.02)	(0.02)	(0.03)	[0.04]	[0.05]	[0.06]	[0.05]	[0.05]	[0.07]
Satisfaction with Wall Quality	0.21	0.21	0.00	0.18	0.15	0.05	0.21	0.17	0.05	0.21	0.18	0.03
	(0.03)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)	[0.04]	[0.03]	[0.05]	[0.05]	[0.03]	[0.06]
Satisfaction with Roof Quality	0.20	0.19	0.00	0.16	0.16	0.01	0.20	0.16	0.04	0.20	0.16	0.04
	(0.03)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)	[0.04]	[0.03]	[0.05]	[0.04]	[0.03]	[0.05]
Satisfaction with Rain Protection	0.19	0.18	-0.01	0.15	0.13	0.04	0.19	0.14	0.05	0.19	0.15	0.04
	(0.03)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)	[0.04]	[0.03]	[0.05]	[0.05]	[0.03]	[0.06]
Satisfaction with Quality of Life	0.34	0.31	0.03	0.30	0.24	0.05	0.33	0.27	0.06	0.34	0.30	0.05
	(0.03)	(0.04)	(0.05)	(0.02)	(0.02)	(0.03)	[0.03]	[0.04]	[0.05]	[0.04]	[0.04]	[0.05]
Monthly Income Per Capita (USD)	38.57	58.12	-12.76	41.48	45.44	-1.55	42.68	44.23	-1.55	38.57	42.21	-3.63
	(2.65)	(7.16)	(7.28)	(2.30)	(2.34)	(3.05)	[4.41]	[4.51]	[6.22]	[4.11]	[3.80]	[5.53]
Head's Years of Schooling	3.00	2.88	0.32	3.33	3.17	0.07	2.96	3.25	-0.29	3.00	3.33	-0.33
	(0.20)	(0.25)	(0.28)	(0.16)	(0.16)	(0.23)	[0.48]	[0.22]	[0.52]	[0.46]	[0.30]	[0.54]
Head is Male	0.81	0.77	0.03	0.79	0.76	0.01	0.80	0.78	0.02	0.81	0.79	0.02
	(0.02)	(0.03)	(0.05)	(0.02)	(0.02)	(0.03)	[0.02]	[0.02]	[0.03]	[0.02]	[0.02]	[0.03]
Head's Age	42.94	43.09	-0.91	42.41	41.70	0.87	43.00	42.05	0.95	42.94	42.41	0.53
	(1.06)	(1.19)	(1.76)	(0.86)	(0.82)	(1.23)	[0.98]	[1.00]	[1.38]	[1.31]	[1.10]	[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 99^{th} percentile were excluded.

Table A6: Baseline Balance Between Phases at Slum Level

	Urban			Rural			All		
Dependent Variable	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	12.49	-0.75	13.63	15.83	-2.21	12.82	14.79	-1.98
	(12.42)	(6.88)	[4.02]	(12.08)	(6.54)	[3.21]	(12.03)	(6.75)	[2.48]
Z-score Housing Quality	-0.02	0.01	-0.03	0.00	0.04	-0.04	-0.01	0.03	-0.04
Summary index	(0.32)	(0.11)	[0.10]	(0.25)	(0.16)	[0.07]	(0.27)	(0.15)	[0.06]
Aspire to Upgrade *in-situ*	0.16	0.13	0.03	0.17	0.12	0.05	0.17	0.13	0.04
Housing Quality Materials	(0.11)	(0.06)	[0.04]	(0.09)	(0.12)	[0.05]	(0.10)	(0.10)	[0.03]
Satisfaction with Quality of Life	0.23	0.26	-0.04	0.37	0.29	0.08	0.31	0.28	0.03
	(0.11)	(0.12)	[0.05]	(0.13)	(0.17)	[0.04]	(0.14)	(0.15)	[0.04]
Satisfaction with Floor Quality	0.16	0.21	-0.05	0.27	0.29	-0.02	0.22	0.26	-0.04
	(0.09)	(0.17)	[0.05]	(0.13)	(0.28)	[0.06]	(0.13)	(0.25)	[0.04]
Satisfaction with Wall Quality	0.12	0.15	-0.04	0.28	0.18	0.10	0.21	0.17	0.04
	(0.09)	(0.11)	[0.04]	(0.19)	(0.17)	[0.06]	(0.17)	(0.15)	[0.04]
Satisfaction with Roof Quality	0.13	0.15	-0.02	0.23	0.16	0.07	0.19	0.16	0.03
	(0.09)	(0.10)	[0.04]	(0.12)	(0.16)	[0.04]	(0.12)	(0.15)	[0.03]
Satisfaction with Rain Protection	0.14	0.12	0.02	0.20	0.16	0.04	0.17	0.15	0.03
	(0.09)	(0.07)	[0.03]	(0.13)	(0.16)	[0.04]	(0.11)	(0.14)	[0.03]
Monthly Income Per Capita (USD)	56.87	65.02	-8.14	47.11	49.27	-2.16	51.47	54.23	-2.76
	(16.16)	(20.94)	[7.29]	(19.58)	(22.11)	[6.28]	(19.02)	(22.93)	[4.96]
Head's Years of Schooling	4.37	4.71	-0.34	3.24	3.34	-0.10	3.73	3.77	-0.43
	(1.64)	(1.49)	[0.62]	(1.68)	(1.18)	[0.47]	(1.73)	(1.42)	[0.39]
Head is Male	0.65	0.63	0.02	0.80	0.78	0.02	0.74	0.74	0.00
	(0.15)	(0.18)	[0.06]	(0.11)	(0.10)	[0.03]	(0.15)	(0.15)	[0.04]
Head's Age	43.07	41.28	1.79	43.46	43.47	-0.01	43.29	42.79	0.50
	(6.62)	(5.87)	[2.47]	(4.19)	(6.38)	[1.55]	(5.26)	(6.24)	[1.36]
Slum Size (Number of Households)	48.50	44.57	3.93	25.81	24.32	1.49	35.54	30.62	4.91
	(31.35)	(31.80)	[12.41]	(18.24)	(16.12)	[5.37]	(26.76)	(23.79)	[6.16]
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 99^{th} percentile were excluded.

Table A7: The Rural-Urban Divide

Dependent Variable	Mean Urban Slums	Mean Rural Slums	Diff.
Monthly Income Per Capita (USD)	63.23	44.39	18.84
	(76.62)	(44.10)	(2.79)
Number of Rooms per Capita	0.73	0.69	0.03
	(0.54)	(0.49)	(0.04)
Share of Rooms with Good Quality Floors	0.40	0.42	-0.02
	(0.43)	(0.42)	(0.02)
Share of Rooms with Good Quality Walls	0.22	0.18	0.04
	(0.36)	(0.30)	(0.01)
Share of Rooms with Good Quality Roofs	0.33	0.33	0.00
	(0.42)	(0.41)	(0.02)
Share of Rooms with Windows	0.46	0.22	0.24
	(0.40)	(0.32)	(0.01)
Z-score Housing Quality Summary Index	-0.02	0.00	-0.02
	(0.54)	(0.53)	(0.03)
Aspire to Upgrade *in-situ* Housing Quality Materials	0.13	0.15	-0.02
	(0.33)	(0.36)	(0.02)
Satisfaction with Floors Quality	0.20	0.27	-0.07
	(0.40)	(0.45)	(0.02)
Satisfaction with Walls Quality	0.14	0.18	-0.04
	(0.35)	(0.39)	(0.02)
Satisfaction with Roofs Quality	0.16	0.18	-0.01
	(0.37)	(0.38)	(0.02)
Satisfaction with Protection against Rain	0.15	0.16	-0.01
	(0.35)	(0.37)	(0.01)
Satisfaction with Quality of Life	0.25	0.29	-0.04
	(0.44)	(0.45)	(0.02)
Z-score Satisfaction Summary Index	0.02	0.07	-0.05
	(0.72)	(0.71)	(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.

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

	Urban			Rural		
		Model 1	Model 2		Model 1	Model 2
Dependent Variable	Follow-Up Control Mean	Treat	Treat	Follow-Up Control Mean	Treat	Treat
Number of Rooms per Capita	0.79	0.01	0.00	0.69	0.04	0.03
	(0.60)	(0.05)	(0.04)	(0.46)	(0.04)	(0.04)
Share Rooms Good Quality Floors	0.44	0.18	0.19	0.44	0.18	0.18
	(0.44)	(0.03)	(0.03)	(0.43)	(0.02)	(0.03)
Share Rooms Good Quality Walls	0.43	0.15	0.14	0.23	0.25	0.25
	(0.44)	(0.03)	(0.04)	(0.34)	(0.02)	(0.02)
Share Rooms Good Quality Roof	0.39	0.18	0.18	0.43	0.15	0.15
	(0.42)	(0.03)	(0.03)	(0.41)	(0.03)	(0.03)
Share Rooms with Windows	0.46	0.14	0.14	0.24	0.21	0.22
	(0.38)	(0.03)	(0.03)	(0.30)	(0.02)	(0.02)
Satisfaction with Floors Quality	0.36	0.13	0.13	0.37	0.26	0.26
	(0.48)	(0.04)	(0.04)	(0.48)	(0.04)	(0.04)
Satisfaction with Walls Quality	0.27	0.21	0.21	0.27	0.36	0.36
	(0.44)	(0.04)	(0.04)	(0.46)	(0.04)	(0.04)
Satisfaction with Roofs Quality	0.28	0.23	0.23	0.31	0.33	0.33
	(0.45)	(0.04)	(0.04)	(0.46)	(0.04)	(0.04)
Satisfaction with Rain's Protection	0.28	0.18	0.17	0.25	0.31	0.31
	(0.45)	(0.04)	(0.04)	(0.43)	(0.04)	(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.

Table A9: The High vs Low Income divide - Urban Only

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

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

	High Income Status ($> p50$)					Low Income Status ($\leq p50$)				
		Model 1		Model 2			Model 1		Model 2	
Dependent Variable	Follow-Up Treat. Mean	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I	Follow-Up Treat. Mean	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I
		γ_1	γ_2	γ_1	γ_2		γ_1	γ_2	γ_1	γ_2
Aspire to stay in the slum	0.57	0.03	0.06	0.05	0.05	0.61	0.03	-0.08	0.03	-0.08
and keep the same conditions	(0.50)	(0.05)	(0.09)	(0.05)	(0.09)	(0.49)	(0.06)	(0.11)	(0.06)	(0.11)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.17		0.17			0.61		0.58	
Aspire to stay in the slum and get	0.30	-0.06	-0.03	-0.07	-0.01	0.26	0.03	0.03	0.03	0.04
improved housing and own land	(0.46)	(0.05)	(0.08)	(0.05)	(0.08)	(0.44)	(0.06)	(0.10)	(0.06)	(0.11)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.15		0.22			0.52		0.45	
Aspire to move to another slum	0.01	-0.01	0.02	-0.01	0.02	0.02	0.00	0.01	0.00	0.01
	(0.08)	(0.01)	(0.02)	(0.01)	(0.02)	(0.12)	(0.01)	(0.04)	(0.01)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.49		0.47			0.76		0.79	
Aspire to move and get improved	0.13	0.03	-0.05	0.03	-0.06	0.11	-0.06	0.03	-0.06	0.03
housing and own land outside of a slum	(0.33)	(0.03)	(0.06)	(0.03)	(0.06)	(0.32)	(0.04)	(0.06)	(0.04)	(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.

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

	Aspire to stay in the slum and get improved housing and own land						Aspire to move and get improved housing and own land outside of a slum					
			Model 1		Model 2				Model 1		Model 2	
Country	Sample Size	Follow-Up Treat. Mean	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I	Sample Size	Follow-Up Treat. Mean	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	0.05	-0.13	0.04	-0.17	140	0.05	-0.06	0.18	-0.04	0.16
		(0.45)	(0.15)	(0.15)	(0.15)	(0.17)		(0.22)	(0.06)	(0.15)	(0.07)	(0.16)
Uruguay	708	0.12	0.10	-0.10	0.10	-0.09	708	0.64	-0.11	0.16	-0.11	0.16
		(0.33)	(0.04)	(0.06)	(0.04)	(0.06)		(0.48)	(0.05)	(0.08)	(0.05)	(0.08)
Mexico	248	0.31	0.08	-0.15	0.11	-0.24	248	0.18	-0.12	0.20	-0.13	0.21
		(0.47)	(0.08)	(0.13)	(0.08)	(0.13)		(0.39)	(0.06)	(0.10)	(0.06)	(0.10)
All Countries	1,096	0.19	0.09	-0.11	0.09	-0.11	1,096	0.46	-0.11	0.17	-0.11	0.17
		(0.39)	(0.03)	(0.05)	(0.03)	(0.05)		(0.50)	(0.04)	(0.06)	(0.04)	(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. We analyze two aspiration variables: Aspire to stay in the slum and get improved housing and own land; and Aspire to move and get improved housing and own land outside a slum. In each case, 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.

Table A12: Adaptation in Housing Quality - Urban Only

		Model 1		Model 2	
Dependent Variable	Follow-Up Control Mean	Treat	Treat × Phase I	Treat	Treat × Phase I
		γ_1	γ_2	γ_1	γ_2
Number of Rooms per Capita	0.80	0.01	0.07	0.00	0.07
	(0.55)	(0.05)	(0.07)	(0.04)	(0.07)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.16		0.17	
Share Rooms Good Quality Floors	0.43	0.18	-0.04	0.19	-0.04
	(0.43)	(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Share Rooms Good Quality Walls	0.44	0.15	-0.03	0.14	-0.02
	(0.44)	(0.03)	(0.06)	(0.04)	(0.06)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Share Rooms Good Quality Roof	0.38	0.18	-0.03	0.18	-0.03
	(0.42)	(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Share Rooms with Windows	0.49	0.14	-0.02	0.14	-0.02
	(0.37)	(0.03)	(0.04)	(0.03)	(0.04)
p-value ($\gamma_1 + \gamma_2 = 0$)		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 $\gamma_1 + \gamma_2 = 0$.

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

		Model 1		Model 2	
Dependent Variable	Follow-Up Treat. Mean	Cont.	Cont. × Phase I	Cont.	Cont. × Phase I
		γ_1	γ_2	γ_1	γ_2
Aspire to stay in the slum and keep the same conditions	0.34	0.02	-0.08	0.02	-0.07
	(0.48)	(0.04)	(0.06)	(0.04)	(0.06)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.22		0.24	
Aspire to stay in the slum and get improved housing and own land	0.16	0.07	-0.10	0.07	-0.10
	(0.37)	(0.03)	(0.05)	(0.04)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.37		0.31	
Aspire to move to another slum	0.02	0.01	0.02	0.01	0.02
	(0.12)	(0.01)	(0.02)	(0.01)	(0.02)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.13		0.13	
Aspire to move and get improved housing and own land outside of a slum	0.48	-0.10	0.16	-0.09	0.16
	(0.50)	(0.04)	(0.06)	(0.04)	(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.

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

Dependent Variable	Follow-Up Treat. Mean	Model 1 Cont. γ_1	Model 1 Cont. × Phase I γ_2	Model 2 Cont. γ_1	Model 2 Cont. × Phase I γ_2
Expect to stay in the slum and keep the same conditions	0.62	-0.03	0.07	-0.03	0.08
	(0.49)	(0.04)	(0.07)	(0.04)	(0.07)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.45		0.38	
Expect to stay in the slum and get improved housing and own land	0.09	0.03	-0.01	0.03	-0.02
	(0.29)	(0.03)	(0.04)	(0.03)	(0.04)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.58		0.68	
Expect to move to another slum	0.02	0.01	-0.02	0.01	-0.02
	(0.15)	(0.01)	(0.02)	(0.01)	(0.02)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.47		0.41	
Expect to move and get improved housing and own land outside of a slum	0.27	-0.01	-0.04	0.00	-0.04
	(0.44)	(0.04)	(0.06)	(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.

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. It is calculated as the sum of the monthly earnings of each household's member divided by the household size.	265	513	532	557	797	1,070
Assets Value Per Capita (USD)	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
Aspire to stay in the slum and keep the same conditions	Indicator equal to one if the respondent reports to aspire to keep the same housing conditions within the slum.	313	599	620	653	933	1,252
Aspire to stay in the slum and get improved housing and own land	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
Aspire to 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
Aspire to move and get improved housing and own land outside of a slum	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

Appendix B. 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 19 years ago in Chile and now works in 19 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 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

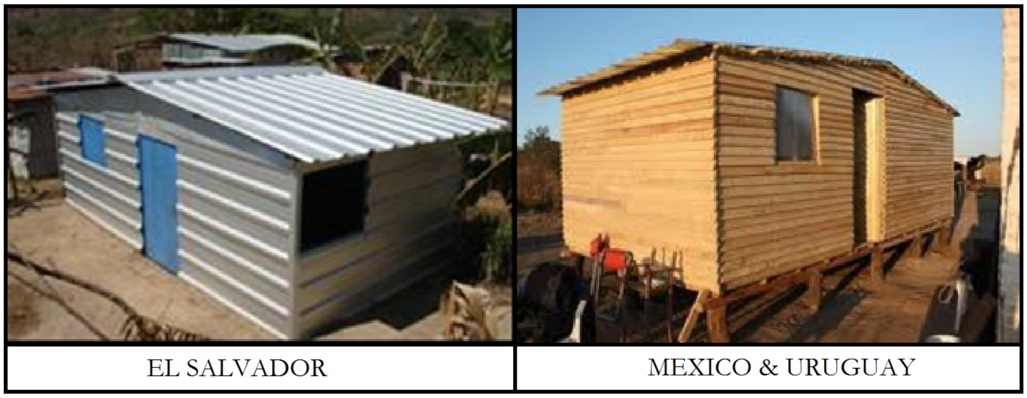


Figure B1: TECHO House

not have access to houses of the type or at the price offered by the TECHO program.

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

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 a pre-determined geographical area an equal opportunity to receive the housing upgrade in a given year.

TECHO first selected a set of eligible settlements and then conducted a census to identify eligible households within each settlement (i.e., those poor enough to be given priority). 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 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 municipalities within Estado de Mexico, and then TECHO did a census of eligible slums within each selected municipality, all of which were considered in the sample. Finally, in the case of Uruguay, since most of the municipalities in Montevideo Department included settlements in which TECHO had already worked, the sampling was non-random and based on a census of settlements where TECHO had not implemented

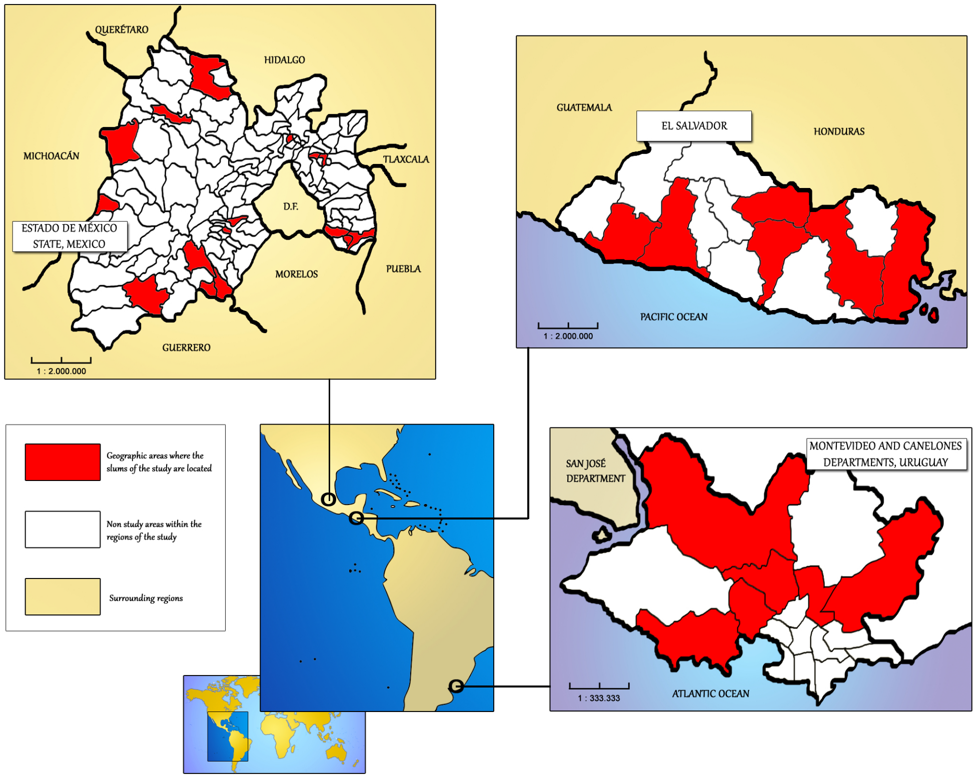


Figure B2: Map of Evaluation Sites

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.