THE HALF-LIFE OF HAPPINESS: HEDONIC ADAPTATION IN THE SUBJECTIVE WELL-BEING OF POOR SLUM DWELLERS TO THE SATISFACTION OF BASIC HOUSING NEEDS

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Abstract
Subjective well-being may not improve in step with increases in material well-being due to hedonic adaptation, 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 happiness eventually returns to a stable reference level. We use a multicountry field experiment to examine the impact on subjective measures of well-being of the provision of improved housing to extremely poor populations in order to test whether they exhibit hedonic adaptation when their basic housing needs are met. After 16 months, we find that subjective perceptions of well-being improve substantially for recipients of improved housing but that, after, on average, eight additional months, 60% of that gain has dissipated. Extrapolation achieved through estimation of a structural model of hedonic adaptation suggests that the decay rate of the treatment effect is 20% per month. As a result, after 28 months of treatment exposure, we forecast that the entire treatment effect will have disappeared. (JEL: D0, I31)

1. Introduction

One of the possible reasons why subjective well-being may not improve in step with increases in material well-being is suggested by the hedonic adaptation hypothesis, which states that there is 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 happiness eventually returns to a stable reference level (Frederick and Loewenstein 1999). According to this hypothesis, variations in happiness and unhappiness are no more than short-lived reactions to changes in people's

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circumstances. In other words, although people initially have strong reactions to events that change their material level of well-being, they eventually return to a baseline level of life satisfaction that is determined by their inborn temperament (Diener, Lucas, and Scollon 2006). In psychology, this idea is known as the "set-point theory" and was labeled the "hedonic treadmill" in the seminal work of Brickman and Campbell (1971).

Veenhoven (1991) and Frederick and Loewenstein (1999) further hypothesize that people do not fully adapt to shocks to the level of satisfaction of basic survival and reproduction-related necessities (the so-called basic needs hypothesis). This suggests that hedonic adaptation is manifested the most strongly in people who have achieved a certain level of basic material well-being rather than being a persistent phenomenon that is evenly distributed across all socioeconomic groups. In essence, then, the idea is that the poor do not display hedonic adaptation or, at least, do not adapt completely to shocks to the satisfaction of their basic needs. Along these same lines, Di Tella and MacCulloch (2010) argue that there is a need to test whether income buys long-term happiness in a low-income context in which the majority of the population has not yet attained a minimum standard of living—something that, to our knowledge, has not yet been done.

In this paper, we use data from field experiments in three developing countries to test the hypothesis that material well-being buys long-term happiness among extremely poor slum dwellers. Specifically, we present the first piece of evidence on hedonic adaptation among the poor to an improvement in the satisfaction of one of their basic needs: shelter. We use data on subjective perceptions of well-being generated by a large-scale multicountry randomized field experiment that was conducted in partnership with TECHO, a nongovernmental organization that provides basic housing units to extremely poor populations living in informal settlements (or the so-called slums) in three Latin American countries: El Salvador, Mexico, and Uruguay. We test the hedonic adaptation hypothesis using experimentally generated variations in the housing supply combined with quasiexperimental variations in the length of exposure to the treatment.

From 2007 to 2010, TECHO built a number of houses in a set of selected slums in each country. Within each slum, TECHO randomly allocated the houses at the household level such that treatment and control units are coresidents. Because TECHO did not have the capacity to work in all the targeted settlements at the same time, the program was rolled out in each country in two phases at the settlement level. The settlements were not randomly allocated to either of the two phases, however. The decision as to which slum would be treated first and which slum would be treated later was based on the availability of census information about the eligible households in each settlement. TECHO volunteers working in each settlement were in charge of collecting census data on the social and demographic characteristics of all slum dwellers living in the slum, from which they selected a set of eligible households. As the information for each set of eligible slum households came into the central office, TECHO organized its internal resources in such a way as to treat the assigned-to-treatment households in that slum as soon as possible. Therefore, the assignment of these slum areas to one or the other phase depended more on the capacity of the

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volunteers working in each slum to conduct the census of eligible households than on the characteristics of the slum dwellers themselves. This suggests that the variation in the amount of time that beneficiaries had occupied the house at the time that the follow-up survey was conducted can be taken as plausibly exogenous.

We find that subjective perceptions of well-being were substantially greater 16 months after the receipt of improved housing but, eight months later, about 60% of that gain had dissipated. Our results are consistent with the hedonic adaptation model of Kimball, Nunn, and Silverman (2015). We estimate a version of this model and, using our estimates, find that the treatment effect on life satisfaction declined in proportion to the number of months of exposure to the treatment, becoming indistinguishable from zero after 28 months, with a rate of hedonic adaptation of 20% per month.

In terms of the level of satisfaction with quality-of-housing characteristics, our ordinary least squares (OLS) estimates initially suggest that there is a partial adaptation effect when comparing households that were treated 16 months ago versus those that were treated 24 months ago, on average . However, these results are likely to be influenced by the very short-run effects on households that were observed after 10–12 months of exposure, which tended to display a much greater effect than those observed at between 13 and 30 months of exposure. Indeed, looking at the structural parameters of our hedonic adaptation model, we find that slum dwellers obtained permanent gains of around 50% in their housing satisfaction levels after receiving TECHO houses; in addition, this effect diminished very little over time, suggesting that the adaptation effect on housing satisfaction is much weaker. Our results are not surprising, as housing is only part of what determines the quality of life, which is a broader attribute that can be affected by many other types of changes in slum dwellers' lives besides housing quality improvements. Overall, our results suggest that an at least partial degree of hedonic adaptation is a common human behavior that is present even among extremely poor populations that experience a major improvement in the level of satisfaction of their basic necessities.

Three main econometric concerns may arise in regard to the internal validity of our causal estimates. First, 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 hedonic mechanisms 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 posttreatment level.

Second, the uncontrolled randomized allocation of slums to phases allows for the possibility of endogenous factors accounting for the exposure to the treatment. However, we provide strong evidence that both treatment and control individuals were well balanced within and across phases for a large number of observable covariates, including all the satisfaction outcomes examined in this study. This indicates that the pretreatment characteristics of slum dwellers were plausibly exogenous to the treatment exposure, thus suggesting that our estimates of adaptation effects are causal.

Third, our experimental design involves the randomization of the TECHO houses at the household level within each settlement, and the control households may therefore

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have been subject to spillover effects and may have changed their behavior owing to the presence of treated neighbors. Nonetheless, we show that, for all the satisfaction 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, which suggests that, if there was any frustration effect among lottery losers, it was not reflected in their reports of subjective well-being over time. In addition, we determined whether or not the satisfaction level of untreated respondents differed across Phase I and Phase II settlements by testing whether the distributions of settlement fixed effects significantly differed across phase samples; the results show that this is not the case. Finally, our control groups were well balanced across phases at baseline, and this is consistent for all the satisfaction variables included in this study. Therefore, potential pretreatment differences across untreated individuals is not an issue here.

A number of previous studies have used observational data to test whether happiness levels in nonpoor settings vary with changes in living standards. Many of these papers examine adaptation to negative shocks such as unemployment (Clark and Oswald 1994; Winkelmann and Winkelmann 1998), disability (Oswald and Powdthavee 2008), hemodialysis (Riis et al. 2005), major illness (Groot and Plug 2004; Ferrer-i Carbonell and Van Praag 2006), divorce (Clark et al. 2008a) and falling below the poverty line (Clark, Dambrosio, and Ghislandi 2016).[1] Others have studied adaptation to positive shocks, including Anusic, Yap, and Lucas (2014a,b) and Grover and Helliwell (2014), who show that marriage has large and long-lasting effects on life satisfaction in the United Kingdom, Switzerland and other developed countries. Nakazato, Schimmack, and Oishi (2011) find that moving to a better home in Germany had lasting effects on housing satisfaction that were measurable five years later, but had no effect at all on life satisfaction. Gardner and Oswald (2007) and Apouey and Clark (2015) document continuing mental health and life satisfaction gains for lottery winners in the United Kingdom. Di Tella, Haisken-De New, and MacCulloch (2010) use observational data to study happiness adaptation to both positive and negative changes in income and status in Germany and find that, within the space of four years, individuals completely adapt to changes in income. With a few exceptions, this research generally shows that people at least partially revert to their reference level of happiness over time. Nonetheless, our study differs from them in that all of these studies are based on samples that either do not include people living in extreme poverty or do not separate out the effects on poor households from the effects on nonpoor households.

In low-income contexts, there have been a number of well-known studies on the effect of major improvements in housing and of cash transfer programs on

1. These authors use observational panel data for the period from 1992 to 2011 in Germany to estimate the effect of falling below the poverty line on the level of satisfaction with quality of life. They find that life satisfaction falls with poverty and that there is little evidence of adaptation within a poverty spell. Their results may, however, suffer from potential selection problems associated with time-varying nonobservable variables, such as productivity or effort, that may well be correlated with both the probability of entering poverty and achieving perceived life satisfaction over time (for causal evidence concerning the positive effects of happiness on productivity, see Oswald, Proto, and Sgroi 2015).

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happiness, but none of these studies has investigated the phenomenon of adaptation. Cattaneo et al. (2009) find that replacing dirt floors with cement floors both increases happiness and reduces symptoms of depression in Mexican slums. Ozer et al. (2011) find that beneficiaries of Mexico's Oportunidades program have significantly lower rates of depression. Ludwig et al. (2012) find that a one standard deviation decline in neighborhood poverty substantially increased the subjective well-being of the beneficiaries of the Moving to Opportunity (MTO) experiment. Devoto et al. (2012) find that greater access to clean municipal water supplies significantly increases happiness. Galiani, Gertler, and Bando (2016) find that giving poor Mexicans noncontributory pensions reduces their symptoms of depression. Taken together, this body of work suggests that reducing poverty is associated with positive effects in terms of happiness. Our paper differs from them in that none of those experiments are based on the kind of data that would make it possible to test whether these effects change as a result of hedonic adaptation over time.

In summary, this paper makes three significant contributions to the existing body of work: (i) the use of plausibly exogenous variation in the time of exposure to the treatment for the identification of hedonic adaptation effects; (ii) the provision of both reduced-form and structural estimates for use in assessing the validity of the results; and (iii) our population of study includes poor individuals who have not yet attained minimum living standards, thus allowing us to test whether the basic-needs hypothesis applies or not. Moreover, to the best of our knowledge, this is the first paper to examine hedonic adaptation by the poor to an improvement in the satisfaction of basic needs and the first to use plausibly exogenous variation for this purpose.

The rest of this paper is organized as follows. In Section 2, we describe the intervention and the experimental design. In Sections 3 and 4, we discuss the strategy used to measure subjective well-being and introduce the identification strategy and econometric methods used in this study. In Section 5, we present our empirical results, including both the reduced-form and the structural estimates. Finally, Section 6 concludes.

2. The Experiment

The 1948 United Nations Universal Declaration of Human Rights identified housing, along with food and clothing, as a basic requirement for achieving an adequate standard of living.[2] Despite this, almost one billion people, primarily in the developing world, live in urban slums and lack proper housing (UN-Habitat 2003).[3] Most slum dwellers live in houses that do not provide proper protection from inclement weather, are not secure, and are not pleasant to live in. Many have insufficient access to services such as clean water, sanitation, and electricity (UN-Habitat 2003; Marx, Stoker, and Suri 2013).

2. United Nations, Universal Declaration of Human Rights, Article 25 (1948).

3. In line with previous work, we define a slum as an overcrowded settlement which affords poor-quality housing and inadequate access to safe water and sanitation and in which there is insecurity of tenure (United-Nations 2003).

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We conducted our experiment in partnership with TECHO, a Latin American NGO whose mission is to provide basic, prefabricated houses to extremely poor populations with the express goal of improving their well-being. TECHO targets the poorest informal settlements and, within these settlements, the families who live in the most extremely substandard housing.[4] The targeted settlements are communities comprised of families that, for the most part, inhabit plots of land that they do not own and that 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. Typical houses are rudimentary units constructed from discarded materials, such as cardboard, tin and plastic, and have dirt floors.

TECHO houses are 18 square meters (6m $\times$ 3m) in size. The walls are made of prefabricated, insulated pinewood panels or aluminum, and the roofs are made of tin and are designed to keep occupants warm and to protect them from humidity, insects and rain. The floors are raised between 30 and 80 cm off the ground to reduce dampness and to protect the occupants from floods and infestations. Although these houses are a major improvement over the recipients' previous dwellings (Galiani et al. 2017), the facilities are limited in that they do not include bathrooms or kitchens or amenities such as plumbing, drinking water hook-ups, or gas connections. The cost of a TECHO house is less than US$1,000—with the bulk of the cost corresponding to the acquisition, storage, and transportation of the materials, because there are essentially no labor costs; the beneficiary family pays 10% of the cost. In El Salvador, US$100 is approximately equivalent to 3.3 months' per capita baseline earnings, whereas in Mexico and Uruguay, it is roughly equivalent to 1.6 and 1.4 months of baseline earnings, respectively. Figure 1 shows examples of TECHO houses.

Between 2007 and 2010, TECHO implemented the program in a number of selected slums in each country. TECHO budget constraints limit the number of housing units that can be built at any one time.[5] Under these constraints, TECHO opted to select beneficiaries by means of a lottery system that gives all eligible households within a settlement an equal opportunity to receive one of the units. TECHO first selected a set of eligible settlements and asked their volunteers to conduct a census to identify eligible households within each settlement, those with extremely poor housing conditions.[6] The eligible households were then surveyed (baseline survey) and randomly assigned

4. Although the work primarily involves building homes, over 3,500 regular volunteers also commit at least one day a week to community organization and participation in social inclusion programs. The primary aim of this second phase of the intervention is to build skills. Our study focuses on evaluating the impact of the first phase of the program: the construction of TECHO houses. We limit the evaluation sample frame to settlements that did not receive the services provided during the second phase of the intervention; accordingly, no intervention other than the construction of housing took place in the settlements studied during the period of analysis.

5. This also constrained the size of the sample used in our study in each country.

6. Eligible settlements were those where: (i) at least 50% of the residents did not have title to the land that they occupied and/or (ii) the slum lacked access to at least one of the following three basic services: electricity, drinking water, and sanitation. Settlements where TECHO had worked in the past were considered ineligible and were not part of the evaluation. In El Salvador, we first randomly selected states, then randomly selected municipalities within each selected state, and then TECHO did a census of

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FIGURE 1. TECHO houses. (See online version of article for color figures.)

to treatment and control groups within each settlement.[7,8] In general, the number of treatments represents a small portion of all the households in the settlement. For example, in around 40% of the settlements, less than 10% of the households were treated, and the proportion of treated households exceeded 50% of the slum population in only 8% of the settlements.

In order to obtain accurate information from the households and to avoid creating any desirability bias in the treatment group, the data collection work was separated from the implementation of the intervention by contracting a highly respected survey firm in each country. The enumerators told the interviewees that they were collecting data for a study on living conditions and did not make any reference to TECHO verbally or in writing. After randomization, treatment households were told about the program and its requirements by TECHO officials. Some of them agreed to participate in the program and some did not. Control households were explicitly not told that they would receive the benefits provided by the program in the future, so their behavior should not have been affected by the expectation of being treated in the next round, although they may have felt frustrated when they realized that they had lost the lottery (we will come back to this subject later in Section 5).

Because TECHO did not have the capacity to work in all settlements at the same time, the program was rolled out in each country in two phases at the settlement level.

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 slums within each selected municipality. Finally, in the case of Uruguay, because TECHO had already worked in most of the settlements in Montevideo and Canelones departments, the sampling was nonrandom and based on a census of settlements where the program had not been implemented in the past. See Figure A.1, for a map of regions where the settlements included in the study are located in each country.

7. In El Salvador and Uruguay, some settlements were randomly assigned a higher intensity-of-treatment level. However, because the number of clusters (settlements) was small, we do not exploit this feature to any significant extent in our analysis.

8. Within each settlement, every household had the same probability of being chosen for inclusion in the intention-to-treat group, but this was not necessarily the case across settlements.

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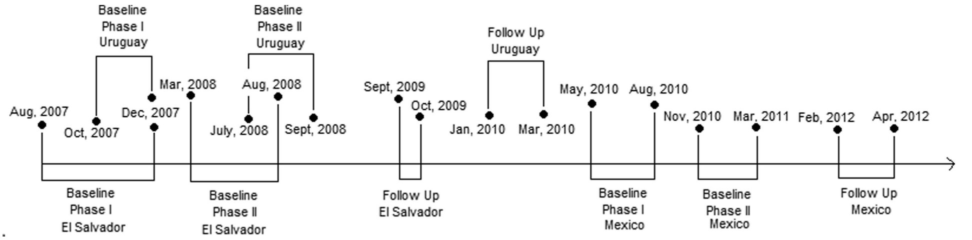

FIGURE 2. Timeline of intervention and surveys.

Baseline surveys were conducted approximately one month before the start of the construction work in each settlement, which gave households time to acquire the funds to make the 10% contribution required by the program, whereas the follow-up surveys were conducted simultaneously for all settlements for both phases in each country (see Table A.1).

Althoughthe settlements were not randomly allocated to phases, there is plausibly exogenous variation in the amount of time that beneficiaries had occupied the house at the time of the follow-up survey, because no discretionary criteria were used to select which slum went to which phase. Instead, the decision as to which slum would be treated first and which would be treated later on depended on the inflow of census information about eligible households for each slum. When the information on the set of eligible households in each slum was delivered to the central office, TECHO organized its internal resources in such a way as to treat the assigned-to-treatment households in that slum as soon as possible. This suggests that the allocation of slums to phases depended more on the capacity of the volunteers assigned to each slum to conduct the census of eligible households than on the characteristics of the slum dwellers in that location.[9] Phase I settlements had 24 months of exposure, on average, whereas Phase II settlements had an average of 16 months of exposure, for a difference of 8 months, on average. Figure 2 shows the timeline of the surveys in each country.

Our sample includes a total of 74 settlements, of which 29 were in Phase I and 45 were in Phase II. The total number of eligible households in these settlements was 2,373. 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% copayment and hence did not receive a house. The compliance rate with the treatment is balanced across phases (see Table A.2).

9. A natural concern regarding the randomness of this process has to do with the slum's size and level of poverty. It may take longer to conduct a census of eligible households in larger and/or poorer slums than in smaller and/or less poor ones, because the number of potential eligible households is naturally higher in the former. We will come back to this later in the section on the identification strategy, where we provide robustness checks that test whether differences in slum size and mean income are statistically significant across Phase I and Phase II slums.

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Attrition rates between baseline and follow-up 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. This difference is not statistically significant at conventional levels. The difference between the attrition rates of the assigned-to-treatment and control groups within each phase was not statistically significant either. Finally, the attrition rates are also balanced across phases.

Under randomization, the outcomes of the assigned-to-treatment and control groups should be equal, on average, prior to treatment. Galiani et al. (2017) tested for the null hypothesis of no difference between the groups for a large set of variables measured at baseline that included socioeconomic characteristics, housing characteristics, assets, satisfaction with the quality of housing and life, perception of security, education, and health. The analysis indicates that there was a statistically significant difference between groups for only 4 out of 44 variables at conventional levels, which is about what would be expected to occur by chance. The test results also show that the samples were balanced in each of the countries, as was the sample when pooled across all the countries.

3. Measurement

We measure subjective well-being (SWB) with respect to housing quality and overall quality of life using self-reported Likert-scale measures. The measures are based on responses to the following question, each part of which highlights the specific attribute to be evaluated: "How satisfied are you with... (i) the quality of your floor; (ii) the quality of your walls; (iii) the quality of your roof; (iv) the protection from water provided by your house when it rains; and (v) the quality of your life. Would you say you are "Unsatisfied", "Neither satisfied nor unsatisfied", "Satisfied", or "Very satisfied"? These measures have been used extensively to gauge general life satisfaction, to arrive at assessments of how people believe their lives are going and, increasingly, to assess the impact of social programs and public policy (Dolan, Layard, and Metcalfe 2011).

The possibility of constructing a SWB metric is based on research that demonstrates that people have a common understanding of subjective well-being and that numerical measures are effective in capturing those feelings. For example, Van Praag (1991) reports that people are able to translate numerical SWB indicators into verbal labels, and Diener and Lucas (1999) show that people are able to predict the SWB levels of others.

Our metric provides four reference points for subjective well-being. Jacoby and Matell (1971) and Lehmann and Hulbert (1972) report that three-point scales do well when the focus is on group averages. Andrews and Withey (1976) find that three-point response scales capture 80%–90% of the variation captured by seven-point scales in the U.S. Finally, Alwin (1992) shows that there are diminishing returns after three-point scales to additional response options on SWB scales.

One issue that arises with respect to Likert-scale measures is how to best summarize the responses into a single indicator. A simple sum requires us to assume cardinality,

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that is, that responses to the SWB question fall on a linear scale. The concern here is that different individuals may have different utility reference points for each of the thresholds, that is, respondents may use different thresholds to map the response categories for SWB into utility. As Ludwig et al. (2012) note, however, in randomized experiments such as ours this should not be a concern because if the treatment itself does not affect SWB thresholds, then the distribution of SWB thresholds would be the same across experimental groups by virtue of random assignment. Thus, our primary measure of analysis is a simple dummy variable that equals 1 if the respondent reports that she is "Satisfied" or "Very satisfied" and 0 otherwise. This measure minimizes the potential problem with cardinality (although it does not overcome it entirely) but discards variation. Hence, in B, we implement a set of robustness checks using standardized measures of SWB that take into account all the variation embedded in the Likert Scale and use ordered probit models to estimate the probability of being in the highest/lowest category of satisfaction, which rules out potential problems associated with cardinality.

Finally, we use the four indicators of housing satisfaction (satisfaction with quality of floors, walls, roofs, and protection against rain) to build a summary index of the levels of satisfaction with housing quality, which is calculated as the sum of the four dummy variables multiplied by 25; accordingly, the index ranges from 0 to 100.

4. Identification Strategy

We report estimates of intention-to-treat effects by time of exposure (phase) for

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

where Y_{ij} is subjective well-being for household i living in settlement j; $Treat_{ij}$ is a dummy variable equal to 1 if household i in settlement j was 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 settlement fixed effect; and ε_{ij} is the error term.[10]

The settlement fixed effects capture the average unobservable differences across settlements (and hence countries). This is important, because randomization was conducted within each settlement. Another important factor is that settlement fixed effects also control for differences in the reference points for subjective well-being, which may vary geographically. Finally, by controlling for settlement fixed effects,

10. As we explained in the last section, our subjective well-being measures can take the form of a binary outcome (limited dependent variable (LDV)) or a continuous outcome (z-scores). The problem posed by causal inference with LDVs is not fundamentally different from the problem of causal inference with continuous outcomes. If there are no covariates or the covariates are sparse and discrete, linear models are no less appropriate for LDVs than for other types of dependent variables. This is certainly the case in a randomized control trial where controls are included only in order to improve efficiency, but their omission would not bias the estimates of the parameters of interest.

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we assume that the error terms are independent and thus report only robust standard errors.[11]

The parameters of interest are γ_1, the treatment effect for Phase II (short-term exposure) households; $\gamma_1 + \gamma_2$, the treatment effect for Phase I (long-term exposure) households; and γ_2, the degree of hedonic adaptation—that is, the difference in the treatment effect between long- and short-term treatment exposure. A negative γ_2 is consistent with an at least partial hedonic adaptation. If γ_2 fully offsets γ_1, then we have full or complete hedonic adaptation, that is, subjective well-being returns to its reference level.

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, and in the Table A.3, we also show that the experimental groups are balanced within phases.

Second, a negative and significant γ_2 can be interpreted as evidence of hedonic adaptation only if the samples in both phases started from the same level of subjective well-being. This would be the case if the allocation of settlements to phases in each country was orthogonal to their characteristics. Indeed, even though the time of exposure to the treatment was not randomly assigned, we cannot reject the null hypothesis of no differences in baseline subjective well-being outcomes and covariates between Phase I and Phase II settlements (see Table A.3). 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), as well as in terms of the mean income per capita, the mean housing quality, and a set of satisfaction measures. We find no statistically significant differences across them (see Table A.4). 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 hedonic adaptation. Note that pretreatment measures are also statistically balanced across intention-to-treat groups within each phase. Hence, potential time effects are controlled for by our experimental design.

5. Results

5.1. Reduced-Form Estimates

We report the results of estimating equation (1) for two different specifications—one with and one without a set of control variables.[12,13] The dependent variable in all

11. As long as the phase design of the intervention is given at a settlement level, there is no within-settlement variation in phase. Thus, controlling for phase effects makes no sense, because phase and settlement fixed effects span the same subspace.

12. Table A.11 provides a detailed definition and sample size for each variable considered in this study.

13. The statistical inference of our results is robust to clustering the standard errors at the settlement level because rejection decisions of the null hypothesis remain the same at conventional levels of statistical

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models is the indicator of subjective well-being. We first estimate specifications 1 and 2 for limited dependent variables and then, in B, we replicate the exercise using z-score measures of satisfaction as well as an ordered probit model so that we can test whether our results are robust to alternative strategies dealing with problems associated with the cardinality of our SWB measures. The specific control variables included in the second specification are listed in the notes to Table 1. This table presents estimates of γ_1 and γ_2 on satisfaction with the housing unit (satisfaction with floor quality, satisfaction with wall quality, satisfaction with roof quality and satisfaction with the protection afforded by the house when it rains), as well as with an overall self-reported measure of quality of life. In each model, we also report the *p*-value for an *F*-test of the null hypothesis of full hedonic adaptation to the TECHO house ($\gamma_1 + \gamma_2 = 0$).

First of all, treatment substantially increased the subjective well-being of beneficiaries in Phase II (short exposure) as indicated by the estimated γ_1, as they are happier with their housing situation and with their lives once they have received their TECHO houses. This holds true for all self-reported measures of satisfaction and is robust across models. Using the indicator-dependent variable, satisfaction with housing quality increased by between 54% and 97% (with the effect shown on the housing satisfaction summary index being around 67%), and gains in the households' overall subjective well-being amounted to increases of about 40%. This smaller effect on satisfaction with quality of life compared to the larger effects on satisfaction with housing quality is not surprising, as housing is only part of what determines quality of life.

Adaptation. The initial gains in subjective well-being afforded by the treatment do not appear to be fully sustained over time, as indicated by the negative estimates of γ_2. The treatment effect on satisfaction with quality of life is 60% lower in households treated in Phase I than it is in those treated in Phase II.[14] However, we reject the null hypothesis of full adaptation in satisfaction with quality of life. After eight months of additional exposure to the treatment, on average, TECHO beneficiaries had partially adapted but were still happier compared to the reference level for no treatment. With respect to satisfaction with housing quality (floor, roof, walls and protection from rain), we find overall positive effects that decrease from short to long exposure by between 41% and 55%—with the average adaptation effect being around 53% on the housing

significance. This result lends credibility to our assumption that the settlement fixed effect captures the systematic unobserved differences across slums. These results are available upon request.

14. Due to a problem with data collection in the follow-up survey in El Salvador, the nonresponse to this question was differentially greater for the control group. Thus, to be on the safe side, we randomly impute a value equal to 3 (Satisfied with quality of life) to 84 missing values in the control group observations, which reduces the nonresponse rate for this variable from 43% to 7% (the same level as recorded for the intention-to-treat group). Without performing this imputation, γ_1 and γ_2 are 0.261 and −0.165, respectively, for Model 1 and are 0.262 and −0.165, respectively, for Model 2.

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TABLE 1. Hedonic adaptation in satisfaction with quality of life and housing.

Dependent variable	Mean control group	Model 1 Treatment γ_1	Model 1 Treatment × Phase I γ_2	Model 2 Treatment γ_1	Model 2 Treatment × Phase I γ_2
Satisfaction with quality of life	0.53	0.20***	−0.12***	0.20***	−0.12***
		(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.04		0.04	
Satisfaction with floor quality	0.37	0.20***	−0.05	0.20***	−0.05
		(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Satisfaction with wall quality	0.30	0.29***	−0.16***	0.29***	−0.16***
		(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Satisfaction with roof quality	0.32	0.29***	−0.12***	0.29***	−0.12***
		(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Satisfaction with rain protection	0.29	0.25***	−0.12***	0.25***	−0.13***
		(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Summary housing	31.96	25.46***	−11.39***	25.59***	−11.48***
Satisfaction index		(2.20)	(3.66)	(2.21)	(3.66)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Control Variables:					
Baseline covariates		No	No	Yes	Yes
Settlement F.E		Yes	Yes	Yes	Yes

Notes: Each row represents a separate dependent variable. For the first five variables, the dependent variable is an indicator equal to 1 if the respondent reports being "Satisfied" or "Very satisfied" and zero otherwise. The summary housing satisfaction index is calculated as the sum of the four housing satisfaction dummy variables (satisfaction with quality of floors, walls, roofs, and rain protection), multiplied by 25, and so the index ranges from 0 to 100. 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 from the regression model $Y_{ij} = \alpha + \gamma_1 Treat_{ij} + \gamma_2 Treat_{ij} \times PhaseI_j + \mu_j + \varepsilon_{ij}$. 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 of schooling, gender, and age, as well as the value of household assets per capita and monthly income per capita, all of which were measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. Finally, we report the p-values of the F-tests of the null hypothesis $\gamma_1 + \gamma_2 = 0$. ***Significant at 1%. For a significance level of 0.1, the Bonferroni p-value is 0.02 (corrected for multiple outcomes).

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satisfaction summary index. Again, the results are consistent with partial but not full hedonic adaptation.[15,16]

In order to interpret these results more accurately, it is important to note that, for all the satisfaction 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. This suggests that, if there was any frustration effect among lottery losers, it was not reflected in their reports of subjective well-being. However, because the randomization was conducted within settlements, control individuals might have been affected by spillover effects derived from the presence of treatment neighbors. If potential spillover effects on control households differ across phases, then the observed adaptation effects may not be causally attributable to hedonic factors. Hence, we determine if the levels of satisfaction of untreated respondents are different across Phase I and Phase II settlements by testing whether the distributions of settlement fixed effects significantly differ across phase samples.[17] In particular, using the Kolmogorov–Smirnov test, we cannot reject the null hypothesis of equality of distributions for almost all satisfaction variables; 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 posttreatment subjective well-being levels.[18]

An alternative robustness test consists in looking at the confidence intervals of the control mean for Phase I and Phase II and checking whether their ranges overlap or not. Consistent with the statistical balance for Phase I versus Phase II control units shown in Table A.3, Figure A.2 shows that the 90% confidence regions for Phase I and Phase II control means overlap for all five satisfaction indicators, suggesting that the control groups did not change their natural satisfaction trend over phases. Overall, this lends credibility to our claim that γ_2 is a causal estimate of the hedonic adaptation effect and is not affected by spillover confounders.

Multiple Outcomes: Statistical Inference. 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

15. Though one could always worry about a "priming" effect on subjective well-being questions that is less of a concern here, because we find large differences in all measures of satisfaction with housing rather than only in satisfaction with quality of life, which could have been subject to priming. More importantly, this should not be an issue in testing the hedonic adaptation hypothesis, because both experimental groups were asked the same questions in the same order.

16. The results for satisfaction with quality of life and with various aspects of the quality of housing are displayed in Figure A.2.

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

18. Note that, as shown in the last three columns of Table A.3, control groups are well-balanced across phases at baseline, and this is consistent for the five satisfaction variables. Therefore, potential pretreatment differences across untreated individuals are not an issue here.

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Table 2. Hedonic adaption in satisfaction with quality of life, by country.

Country	Sample size	Mean control group	Model 1 Treatment γ_1	Model 1 Treatment × Phase I γ_2	Model 2 Treatment γ_1	Model 2 Treatment × Phase I γ_2
El Salvador	606	0.51	0.25***	−0.13	0.25***	−0.13
			(0.05)	(0.10)	(0.06)	(0.10)
Uruguay	715	0.45	0.13**	−0.07	0.13**	−0.07
			(0.05)	(0.08)	(0.05)	(0.08)
Mexico	822	0.59	0.22***	−0.14**	0.22***	−0.14**
			(0.04)	(0.07)	(0.04)	(0.07)
All countries	2,143	0.53	0.20***	−0.12***	0.20***	−0.12***
			(0.03)	(0.05)	(0.03)	(0.05)
p-value for F-test of Pooling countries			0.54		0.50	
Control variables:						
Baseline covariates			No	No	Yes	Yes
Settlement F.E			Yes	Yes	Yes	Yes

Notes: Each row represents a different country. The dependent variable is an indicator equal to 1 if the respondent reports being "Satisfied" or "Very satisfied" and zero otherwise. The second and third columns report the sample size and the mean of the dependent variable for the control group at follow-up. The next two columns, under the heading of Model 1, report the results of the regression model $Y_{ij} = \alpha + \gamma_1 Treat_{ij} + \gamma_2 Treat_{ij} \times PhaseI_j + \mu_j + \varepsilon_{ij}$. Reports are the estimated coefficients and robust standard errors. The last two columns, under the heading of Model 2, additionally control for the household head's years of schooling, gender, and age, as well as the value of household assets per capita and monthly income per capita, all of which were measured during the baseline round. 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 γ_1 and γ_2 are jointly equal to all countries for each model. **Significant at 5%; ***significant at 1%.

deriving from an examination of large numbers of individual outcomes by using Bonferroni Family-Wise Error Rates (FWER) to adjust the p-values of the individual tests as a function of the number of outcome variables. We compute Bonferroni FWER corrections at the 10% level of statistical significance by dividing the desired size of the test by the number of satisfaction variables (five in total). Hence, the Bonferroni-corrected multiple-outcomes p-value is 0.02, for a significance level of 0.10. As can be observed in Table 1, all the results remain significant under this stringent test except the adaptation effect on "Satisfaction with Floor Quality".

Country-Specific Estimates. In Table 2, we report the estimates separately by country. The estimated magnitudes of the short-run effect on subjective well-being, γ_1, are statistically significant for all three countries and are the same magnitudes for El Salvador and Mexico. The effect size for Uruguay is half of what it is for the other two countries. The hedonic adaptation effect, γ_2, is consistent across countries but is only statistically significant for Mexico, most likely owing to the larger size of the sample in that country. The magnitudes of the estimates for the γ_2 parameters are about the

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same relative to the estimated γ_1 parameters in all three countries, which is consistent with the finding that the degree of hedonic adaptation is the same 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.

Effect Size in Context. The estimated effect on SWB is large even after accounting for hedonic adaptation. The long-run effect size (8 percentage points) is equivalent to the difference in SWB between nonbeneficiary slum dwellers whose monthly per capita income differs by around US$150—a huge effect given that the average monthly per capita income in the control group at baseline is US$55. In other words, the effect on SWB is roughly equivalent to have three times the average monthly per capita income. Considering that beneficiary households invested US$100 as a copayment to obtain the TECHO house (worth about US$1,000), and then, on average, their return on the housing investment was around 66% in terms of subjective well-being—that is, it was around two times as much as their baseline average income and yielded a level of SWB equivalent to the level of SWB that they would obtain if they earned three times as much as their baseline income.

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 satisfaction due to reduced housing quality rather than hedonic adaptation. We test for this possibility by estimating equation (1) for various measures of housing quality. In general, the results reported in Table A.5 show a large and significant increase in the housing quality of the TECHO houses (γ_1), but no statistically significant differences in the treatment effect on housing quality between Phase I and Phase II households (γ_2). With the exception of the treatment effect on “Share of rooms with walls of good quality”, which differs only at the 10% level across phases, the effects on the other four indicators of housing quality show no statistically significant difference across phases. But even if this were the case, the relatively large effects discussed in the previous section make it unlikely that the potential wear and tear on the house could account for the entire adaptation effect.

A second robustness check in this regard consists of testing whether the adaptation effects are robust to controlling for follow-up housing quality measures in our main regression. As shown in Table A.6, the order of magnitude and significance of γ_2 remain constant for all the satisfaction indicators, which confirms that any wear and tear on the house had little or no effect on the treated individuals’ level of hedonic adaptation. Interestingly, we observe that γ_1 is systematically lower than for the same estimates in Table 1 (when not controlling for housing quality measures). In fact, it is so much lower that the adaptation effect on satisfaction with quality of life is no longer partial, but total. This should not be surprising, as the ex-post housing quality measures are positively correlated with both the treatment dummy and the satisfaction

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measures, and its inclusion will therefore generate a downward bias in the estimation of the treatment effect.[19]

Material Well-Being. A second concern regarding our interpretation of the results is that income and wealth may have deteriorated over time. In this case, γ_2 could represent a decline in satisfaction due to reduced material well-being rather than to hedonic adaptation. We test for this possibility by estimating equation (1) for various measures of material well-being, including assets, income, and labor-force participation. In general, the results reported in Table A.7 show no difference between treatment and control groups in Phase II (γ_1) and no difference in the treatment effect across phases (γ_2).

5.2. Structural Estimation

A parsimonious model of hedonic adaptation that allows life events to have both transitory and permanent effects on subjective well-being is presented in Kimball and Willis (2006). The model assumes that the impulse response of SWB to an event indicates the importance of that event in terms of lifetime utility. In particular, the theory suggests that the rate of hedonic adaptation should depend on the particular type of event. Indeed, using panel data from the Health and Retirement Study (HRS) in the United States, Kimball et al. (2015) estimate an event-specific rate at which the transitory effect decays for multiple negative life events affecting subjective well-being, some of which show full or partial adaptation and others which do not.

In this section, we slightly adapt the model used in Kimball et al. (2015) and estimate the event-specific rate at which the treatment effect of TECHO housing decays over time, and we do this for both housing and life satisfaction measures. We then test whether the subjective well-being of TECHO beneficiaries returns to its baseline level and, if so, when (after how much time of treatment exposure). Kimball et al. (2015) model hedonic adaptation by exponential decay, where the decay rate is estimated simultaneously with the intensity of the initial response of satisfaction 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

$$Y_{ij} = \alpha + Treat_{ij} \times \left[\beta_P + \beta_T e^{-\delta(t_i - t_0)}\right] + \beta X_{ij} + c_j + \varepsilon_{ij}, \tag{2}$$

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

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with Y_{ij} being our usual satisfaction outcome, $Treat_{ij}$ the treatment dummy, t_i the individual's months of exposure to the program, t_0 the minimum treatment exposure observed in the sample (10.3 months), X_{ij} a set of baseline covariates, and c_j the country fixed effects.[20] A nonzero β_P, the permanent effect, indicates that the treatment effect did not disappear over time and generates a permanent gain in the subjective well-being of the individual. A negative and significant β_T, the transitory effect, suggests that, at least partially, the treatment effect diminishes over time. Finally, the hedonic adaptation rate, δ, indicates the rate at which the transitory effect weakens over time; this is expressed as a monthly rate.

We estimate the structural parameters using a Nonlinear Least Square (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 is the parameter vector.

Following the same logic as was used in Table 1 (reduced-form estimation), in Table 3 we report the results of estimating equation (2) for two different specifications—one with and one without a set of control variables plus country fixed effects. We present estimates of β_P, β_T, and δ on "Satisfaction with quality of life" and on our housing satisfaction summary index.

First of all, in the case of the life satisfaction indicator, we observe a large transitory effect of between 40 and 31 percentage points, as is indicated by the positive and significant β_T in models 1 and 2, respectively. The effect is somewhat greater than the one observed in the reduced-form regressions, and this is in part because β_T captures the immediate effect after 10 months of treatment exposure, whereas γ_1 in Table 1 represents the treatment effect in Phase II, that is, households that have been treated for an average of 16 months—a sufficient amount of time for some degree of adaptation in the SWB gains to appear.

Second, we cannot reject the null hypothesis of zero permanent effects, which suggests that the adaptation was total. Indeed, we find a positive and statistically significant rate of hedonic adaptation, δ, of about 20% per month. If we linearly project the survival rate of the transitory effect at this rate of depreciation, we find that, after the 28th month of exposure, the effect should be close to zero. In fact, our range of months of exposure goes from 10 (the minimum) to 30 (maximum). Therefore, at this rate of hedonic adaptation, it is not surprising to observe a null permanent effect for the period under analysis. This is illustrated in Figure A.3, which maps the proportion of treatment and control households that reported that they were "Satisfied" or "Very

20. Because 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 treatment exposure and which allow us to capture the average unobservable differences across countries.

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TABLE 3. Hedonic adaptation in satisfaction with quality of life and housing (NLS).

		Model 1			Model 2		
Dependent variable	Mean control group	Permanent effect β_P	Transitory effect β_T	Hedonic adaptation Rate δ	Permanent effect β_P	Transitory effect β_T	Hedonic adaptation rate δ
Satisfaction with quality of life	0.53 (0.50)	0.04 (0.05)	0.40*** (0.08)	0.18** (0.08)	0.07 (0.05)	0.31*** (0.09)	0.20* (0.12)
Housing satisfaction Summary index	31.96 (37.75)	14.78*** (4.30)	16.18** (6.79)	0.17 (0.16)	17.97*** (3.92)	7.03 (8.66)	0.20 (0.46)
Control variables:							
Baseline covariates		No	No	No	Yes	Yes	Yes
Country F.E		No	No	No	Yes	Yes	Yes

Notes: Each row represents a separate dependent variable. Satisfaction withquality of lifeis an indicator equal to 1 if the respondent reports being "Satisfied" or "Very satisfied" and zero otherwise. The summary housing satisfaction index is calculated as the sum of the four housing satisfaction dummy variables (satisfaction with quality of floors, walls, roofs, and rain protection), multiplied by 25, and so the index ranges from 0 to 100. The first column reports the mean of the dependent variable for the control group measured at follow-up. The next three columns, under the heading Model 1, report the structural parameters from the NLS regression $Y_{ij} = \alpha + Treat_{ij} \times [\beta_P + \beta_T e^{-\delta(t_i - t_0)}] + \varepsilon_{ij}$, with t_i the months of exposure to the program enjoyed by individual i, and t_0 the minimum treatment exposure (10.3333 months). δ is expressed as a monthly rate. Reports are the estimated coefficients and robust standard errors. The last two columns, under the heading Model 2, additionally control for the number of years living in the slum, household head's years of schooling, gender, and age, as well as the monthly income per capita, number of rooms per capita, the share of rooms with good quality of walls, and the share of rooms with good quality of roofs, all of which were measured during the baseline round, as well as for country fixed effects. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. *Significant at 10%; **significant at 5%; ***significant at 1%.

satisfied" with their quality of life on the months of exposure to the treatment. Our nonparametric estimation of the curve seems to fit relatively well with the parametric one, and both of them show a reduction in the distance between treatment and control groups as treatment exposure increases, with this difference narrowing to almost zero by around the month 28.

This is not the case, however, of satisfaction with housing quality. Here, the hedonic adaptation rate is indistinguishable from zero, and the transitory effect is not robust to controlling for baseline covariates and the country fixed effects. This does not mean that adaptation did not occur at any time along the exposure line, but rather that the transitory effect over the entire range of exposure was, on average, close to zero. Accordingly, we observe a robust and positive permanent effect that ranges between 15 and 18 percentage points. Indeed, the permanent effect resembles the total effect of $\gamma_1 + \gamma_2$ found in Table 1 fairly closely. Figure A.4 illustrates this result. Although the treatment effect on housing satisfaction shows some decay from 10 to 15 months of exposure, this stabilizes from month 16 on, with the treatment and control trends becoming parallel and having a slope close to zero afterwards.

The previous result contrasts with the significant difference between Phase I and Phase II treatment effects on housing satisfaction shown in Table 1 (negative and significant γ_2). However, this should not be surprising at all, because, in the first

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exercise, we were comparing two "phase" means, whereas here we are estimating the adaptation effect considering the entire distribution of times of exposure to the treatment. Indeed, the treatment effect on housing satisfaction for Phase II households (short exposure) is quite strongly influenced by the effect of receiving the treatment in the very first two months of our treatment exposure window (months 10–12). We hypothesize that, if we had had access to a larger window of treatment exposure, including months 1–9, we would probably have observed a larger decay in housing satisfaction, with that greater decline being explained primarily by a greater treatment effect in the period immediately after the treatment—something that our data do not allow us to observe. In any case, a natural explanation of why the overall SWB shows a stronger adaptation than housing satisfaction does is that housing is only part of what determines quality of life, which is a broader attribute that can be affected by multiple other changes in slum dwellers' lives, apart from housing quality improvements.

Cumulative Impact. Interestingly, Kimball et al. (2015) consider the cumulative impact of an event ("the area under the curve" associated with the hedonic response to an event) and measure the specific proportions of that area that can be attributed to the permanent and transitory effects, respectively. In particular, for an individual with d annual mortality risk and an interest rate r, the authors show that the total gains, that is, the total "area under the curve", can be calculated by

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

As highlighted by Kimball et al. (2015), the advantage of this formulation is that it gives a single statistic that can be used to compare events in terms of their hedonic 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 hedonic effect.

Table 4 presents these estimates for our experiment. The first three columns replicate the results from Model 2 in Table 3. Columns 4–6 show the areas corresponding to permanent, transitory, and total gains, respectively. Finally, the last column shows the pooled estimate of the treatment effect, that is, the OLS estimate of the treatment effect for the entire sample. 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 life satisfaction, with the positive and significant total gains mostly explained by the transitory effects. Interestingly, the OLS pooled coefficient is shown to be positive and highly significant, a result that is

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TABLE 4. Hedonic adaptation in satisfaction with quality of life and housing (NLS).

	NLS estimates			Satisfaction gains area			OLS estimate
	Permanent effect β_P	Transitory effect β_T	Hedonic adaptation rate δ	Permanent gains (PG) $\frac{\beta_P}{\eta+r}$	Transitory gains (TG) $\frac{\beta_T}{\eta+r+\delta}$	Total gains PG+TG	Pooled coefficient
Satisfaction with quality of life	0.07 (0.05)	0.31*** (0.09)	0.20* (0.12)	1.04 (0.68)	1.15*** (0.44)	2.19*** (0.37)	0.14*** (0.02)
Housing satisfaction Summary index (*Z*-Score)	17.97*** (3.92)	7.03 (8.66)	0.20 (0.46)	253.08*** (55.18)	25.98 (37.14)	279.06*** (30.32)	19.54*** (1.67)
Control variables:							
Baseline covariates	√	√	√	√	√	√	√
Country F.E	√	√	√	√	√	√	√

Notes: Each row represents a separate dependent variable. Satisfaction with quality of life is an indicator equal to 1 if the respondent reports being "Satisfied" or "Very satisfied" and zero otherwise. The summary housing satisfaction index is calculated as the sum of the four housing satisfaction dummy variables (satisfaction with quality of floors, walls, roofs, and rain protection), multiplied by 25, and so the index ranges from 0 to 100. The first three columns under the heading of NLS estimates report the structural parameter estimates of the NLS regression $Y_{ij} = \alpha + Treat_{ij} \times [\beta_P + \beta_T e^{-\delta(t_i - t_0)}] + \beta X_{ic} + \mu_c + \varepsilon_{ij}$, with t_i the months of exposure to the program enjoyed by individual i, t_0 the minimum treatment exposure (10.3333 months), X_{ic} a vector of baseline covariates including the number of years living in the slum, household head's years of schooling, gender, and age, as well as the monthly income per capita, number of rooms per capita, the share of rooms with good quality of walls, and the share of rooms with good quality of roofs, all of which were measured during the baseline round, and μ_c 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 satisfaction gains area, report the area under the permanent, transitory, and total effects. Permanenteffect area is calculated as β_P divided by the sum of the mortality rate, d, which is assumed to be 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 hedonic 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 treatment effect and its associated robust standard error. *Significant at 10%; ***significant at 1%.

contradictory to the almost null and insignificant permanent effect found in our NLS estimation. This suggests that studying the potential adaptation of subjective well-being over time is crucial for a rigorous interpretation of treatment effects on SWB outcomes in the long run.

In contrast, in the case of housing satisfaction, the permanent gains are positive and statistically significant, whereas the transitory gains are not. Indeed, even though the pooled coefficient is somewhat higher than the permanent effect, the difference in magnitude is only about 10%, which is consistent with a weaker adaptation effect. Although most of the events studied by Kimball et al. (2015) are negative life events, such as widowing, having cancer, heart attacks, or being unemployed, a few of the negative shocks in their study ended up generating positive permanent gains, as in the case of divorce. From a comparative perspective, and by using the same indicator of satisfaction as the one used in Kimball et al. (2015), we find that the total gains in satisfaction with housing quality among slum dwellers receiving a TECHO house are about 20 times greater than the gains obtained by people in the United States as the result of a divorce. However, this comparison should be taken with some cautious

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because neither the populations nor the types of shocks or times of exposure are comparable across these studies.

5.3. Hedonic Adaptation Versus Relative Status Effects

There is a large body of literature on the general relationship between income and subjective well-being that reports conflicting results based on the longitudinal and cross-sectional evidence that has been gathered. Easterlin (1974, 2005, 2006) provides evidence that increased income has not been associated with improvements in subjective well-being over time in the United States, and his analyses have been replicated in various other countries and for different periods of time. However, there is substantial evidence of a positive and statistically significant association between income and happiness in the cross-sectional data on countries and on income groups within countries.[21]

A common explanation of the Easterlin Paradox is based on the diminishing marginal utility of income (Frey and Stutzer 2002; Layard 2005; Di Tella and MacCulloch 2010). According to this line of reasoning, there is a satiation point before which happiness increases with income and after which additional income buys little, if any, extra happiness. This view is consistent with the hedonic adaptation models of Veenhoven (1991) and Frederick and Loewenstein (1999), who hypothesize that poor people do not adapt to shocks to the satisfaction of basic necessities that are related to survival and reproduction. This model is also consistent with both a positive longitudinal association between income and happiness in low-income countries and a weak or null association in high-income countries. If this is the case, then, in cross-country analyses, developed countries that have already reached a certain minimum level of GDP would mainly be plotted along the "flat of the curve", whereas poorer countries would be positioned on the incline. In contrast, Clark, Frijters, and Shields (2008b) and Di Tella et al. (2010) put forward an alternative explanation according to which individuals evaluate their level of life satisfaction by comparing their level of wealth with the wealth level of some reference individual or group in society. Under this hypothesis, increases in income will produce increases in happiness only if the social distance between the individual and the reference group is shortened. In other words, if people only care about their relative social position or "status", then the dissipation of income effects on happiness would be explained by relative position effects to which individuals do not adapt. In this case, increasing levels of income would not buy happiness unless higher incomes generated positive changes in a person's relative position.

Our data reflect some heterogeneity in the socioeconomic positions of slum dwellers within each slum, so we examine whether the dissipation of satisfaction gains can be explained through relative status effects by testing for heterogeneous

21. See Clark (2016) for a thorough survey of this literature.

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effects of housing improvements on hedonic adaptation across high versus low socioeconomic status (SES) groups. Because all beneficiaries received the same kind of house, the lower-income beneficiaries within a slum will have increased their wealth proportionally more than their richer counterparts. Hence, we hypothesize that differences in the adaptation effects across SES groups may be indicative of the presence of relative status effects. In order to look more closely at this possibility, we extend Model 1 and estimate the following linear probability model:

$$
\begin{aligned}
Y_{ij} &= \alpha + \gamma_1 Treat_{ij} + \gamma_2 Treat_{ij} \times PhaseI_j + \gamma_3 Treat_{ij} \times HighStatus_{ij} \\
&\quad + \gamma_4 Treat_{ij} \times HighStatus_{ij} \times PhaseI_j + \gamma_5 HighStatus_{ij} + \beta X_{ij} + \mu_j + \varepsilon_{ij}.
\end{aligned}
$$

The parameters of interest are γ_3, the relative treatment effect for high-status Phase II (short exposure) households with respect to their low-status counterparts; $\gamma_3 + \gamma_4$, the relative treatment effect for high-status Phase I (long exposure) households with respect to their low-status counterparts; and γ_4, the relative degree of hedonic adaptation for high-status households with respect to low-status households within the settlement—that is, the relative difference in the treatment effect between Phase I (long exposure) and Phase II (short exposure) across high- and low-status groups. A negative γ_4 is consistent with a relative differential in hedonic adaptation in favor of the low-status group. In other words, low-status households (those that increased their wealth proportionally more and thus are more likely to improve their relative position within the slum) adapt less than high-status counterparts do, with the difference in the patterns of adaptation across SES groups explained by the larger relative positional changes experienced by low-status households as compared to their high-status counterparts.

We use two different measures of SES. The first is a summary index of housing quality based on a number of housing quality measures, whereas the second is a dummy for high-income versus low-income households based on whether their monthly per capita income is above or below the within-settlement median. As is shown in Tables A.8 and A.9, there is no evidence that higher status, measured either by housing quality or by income, plays any role in the hedonic adaptation of beneficiary households. Overall, these results show that both high- and low-status households hedonically adapt at the same pace to the housing improvements over time, because we cannot reject the null hypothesis of no differences in hedonic adaptation across them. Moreover, the results are robust to the nonlinear model detailed in Subsection 5.2, as the transitory effect and hedonic adaptation rate are not significantly different across status subgroups (see Table A.10, *F*-Tests). Figures A.5 and A.6 illustrate these results. Both of them show that the satisfaction gains derived from treatment across high- and low-status subgroups follow the same trend along the treatment exposure axis. This suggests that, at least for the case of housing improvements for slum dwellers, the mechanism through which the adaptation effect is produced is mostly a hedonic mechanism rather than being related to relative position effects.

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6. Conclusion

A fundamental question in economics is whether happiness increases *pari passu* with material conditions or whether people grow accustomed to better conditions over time. Previous investigations have tested these ideas in high-income countries where most residents have already met their basic needs and hedonic adaptation is likely to be the strongest. In contrast, we test these ideas in the slums of three low-income countries where adaptation is less likely to occur. Specifically, we use data from a large-scale, multicountry field experiment to examine what kind of impact the provision of housing to extremely poor slum dwellers in Latin America has on subjective well-being and to test whether poor population groups display hedonic adaptation in relation to the gains in happiness derived from a reduction in the shortfall in the satisfaction of their basic need for housing. To the best of our knowledge, this is the first paper on test the hypothesis of hedonic adaptation within the context of a change in the level of satisfaction of basic necessities among poor populations.

Our results are conclusive. We find that subjective perceptions of well-being are substantially higher among recipients of improved TECHO housing 16 months after having received their new house but that after, on average, another 8 months, 60% of that gain has dissipated. Our results are consistent with the hedonic adaptation model of Kimball et al. (2015). Using an NLS estimator, we find that the treatment effect on life satisfaction becomes proportionally lower as the number of months of exposure to the treatment increases, becoming indistinguishable from zero after month 28, with a rate of hedonic adaptation of 20% per month. In terms of satisfaction with quality-of-housing characteristics, our OLS estimates initially suggest that there is a partial adaptation effect when comparing households with an exposure to treatment of 16 versus 24 months, on average. However, these results are likely to be influenced by the very short-run effects found in households that were observed after 10–12 months of exposure, which tended to display a much greater effect than those observed at between 13 and 30 months of exposure. Indeed, looking at the structural parameters of our hedonic adaptation model, we find that slum dwellers obtained permanent gains of around 50% in terms of their housing satisfaction as a result of their receipt of a TECHO house and that this effect diminished only slightly over time, suggesting that, if there was any adaptation over time at all, it was probably quite small. Our results are not surprising, as housing is only part of what determines quality of life, which is a broader attribute that can be affected by multiple other changes in slum dwellers' lives apart from housing quality improvements.

Overall, we conclude that hedonic adaptation is a common human trait that, at least to a partial degree, is present even among poor populations that experience a major improvement in the level of satisfaction of their basic necessities. Our results are also consistent with the theoretical work of Pollak (1970), Wathieu (2004), Rayo and Becker (2007), and Graham and Oswald (2010).

Interestingly, Ludwig et al. (2012) measure the long-run effects of the randomized MTO experiment on subjective well-being and find that, after the program had been in effect for between 10 and 15 years, a one standard deviation decline in

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neighborhood poverty (13 percentage points) increased the subjective well-being of MTO beneficiaries by an amount equal to two-thirds of the gap in subjective well-being between US Blacks and Whites. The latter is equivalent to the gap between people whose per capita incomes differ by around US$250 per month—a large effect given that the per capita income of the control group in that study is around US$400 per month. Note, however, that the effect of the MTO program on subjective well-being is roughly equivalent to a 60% increase in the per capita monthly income of an average beneficiary household, which is a proportionally smaller effect than that of the TECHO intervention, even after considering an adaptation effect of 50%. Although the populations and treatment exposures across studies are not entirely comparable (subjective well-being measures are relatively similar across samples, but TECHO beneficiaries are much poorer than their MTO counterparts), this evidence suggests that, even though poor populations in developing countries exhibit hedonic adaptation in terms of the satisfaction of their basic housing needs, improvements in the housing and neighborhood conditions of poor households seem to have a greater effect in developing countries than in developed ones.

Appendix A: Additional Figures and Tables

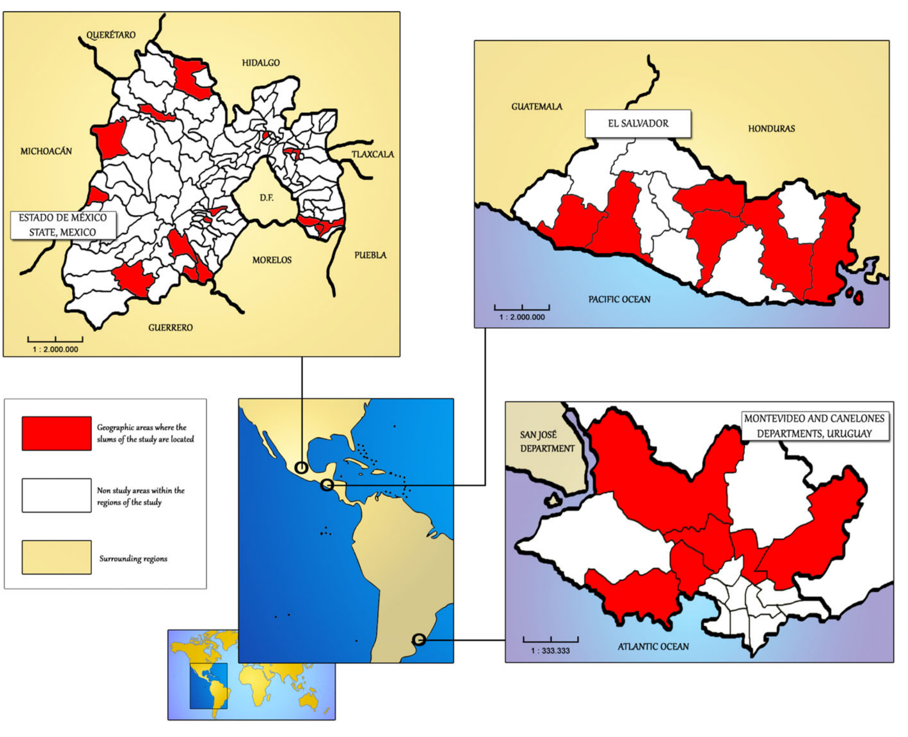

FIGURE A.1. Maps of evaluation sites. (See online version of article for color figures.)

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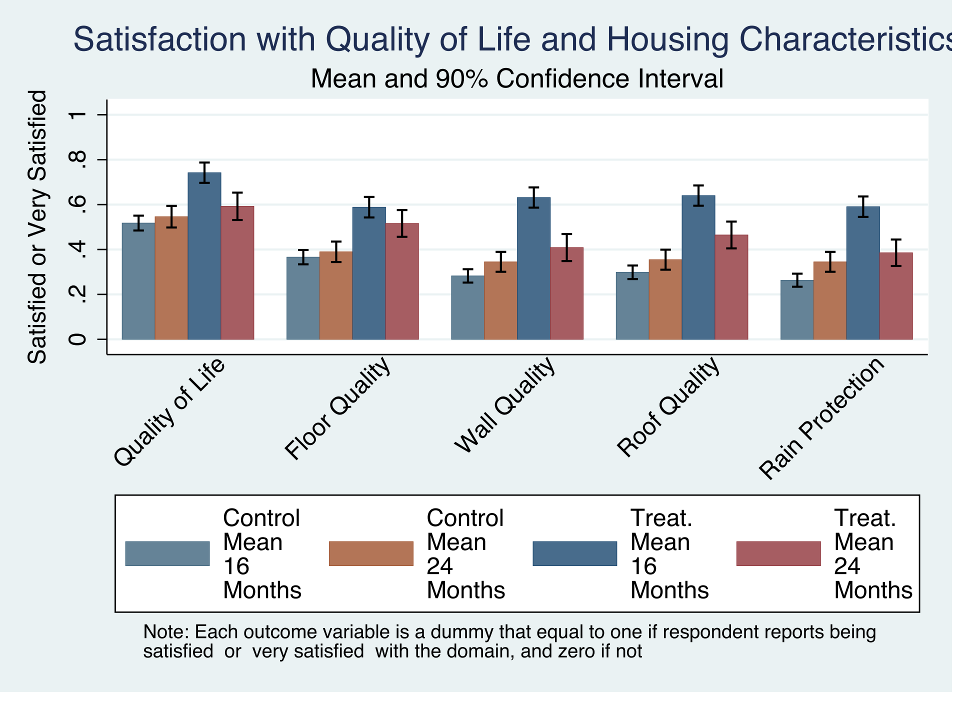

FIGURE A.2. Hedonic adaptation. (See online version of article for color figures.)

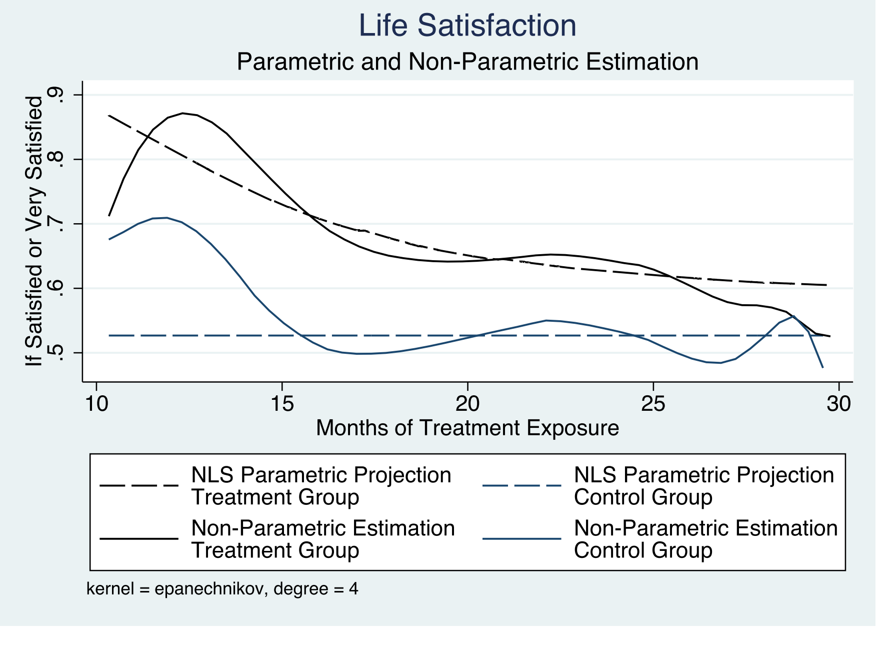

FIGURE A.3. Hedonic adaptation on life satisfaction. (See online version of article for color figures.)

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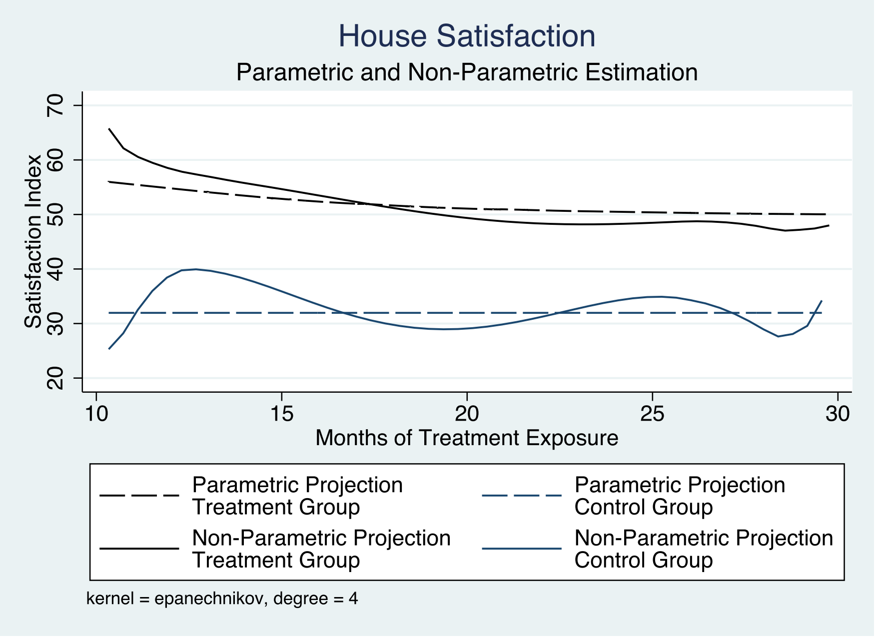

FIGURE A.4. Hedonic adaptation on house satisfaction. (See online version of article for color figures.)

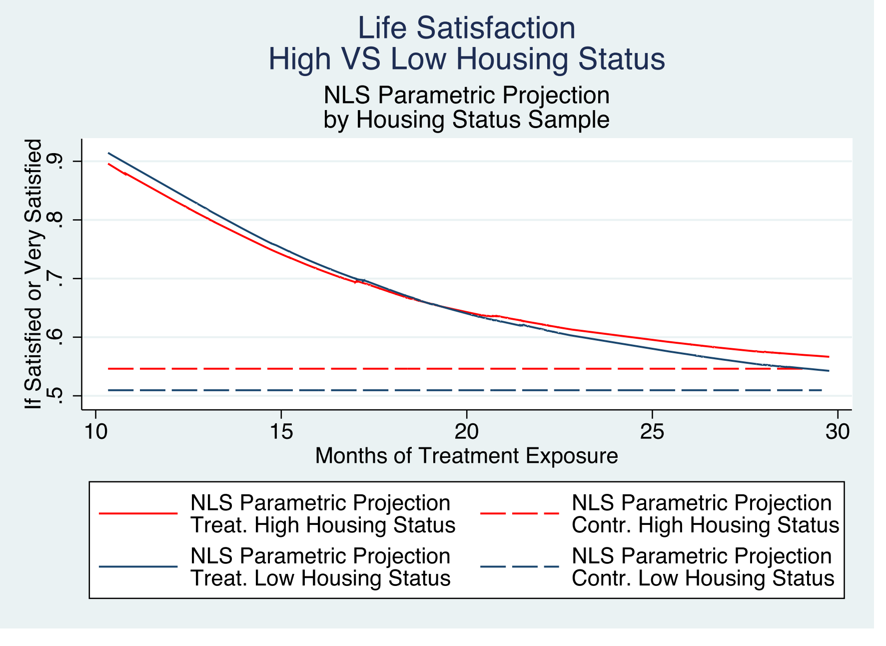

FIGURE A.5. Hedonic adaptation on life satisfaction across housing status groups. (See online version of article for color figures.)

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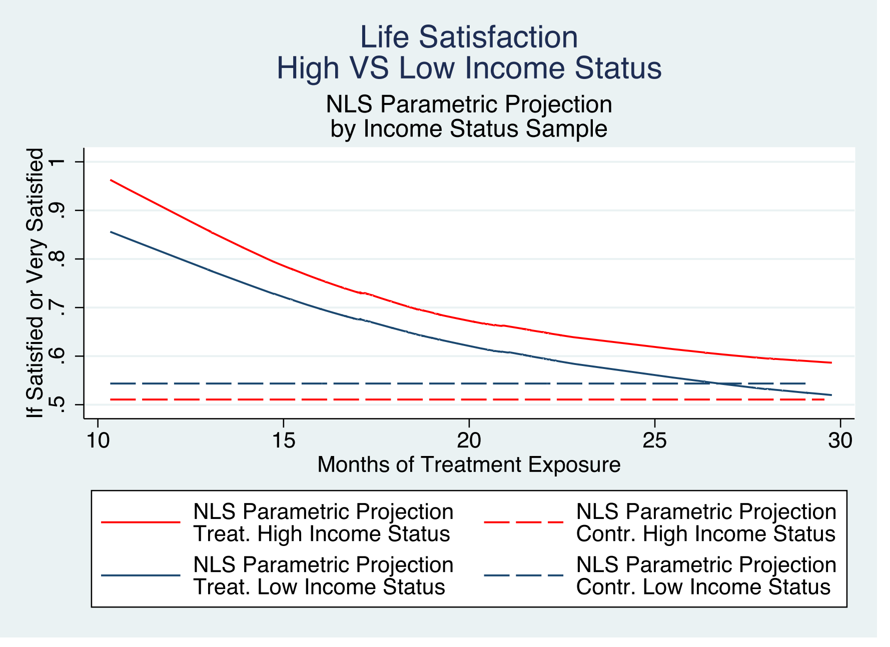

FIGURE A.6. Hedonic adaptation on life satisfaction across income status groups. (See online version of article for color figures.)

TABLE A.1. Length of exposure and sample sizes.

	Phase I construction	Phase II construction	Combined
El Salvador			
Average exposure	25 months	17 months	
Household sample size	288	368	656
Number of settlements	8	15	23
Uruguay			
Average exposure	27 months	17 months	
Household sample size	353	375	728
Number of settlements	6	6	12
Mexico			
Average exposure	20 months	15 months	
Household sample size	286	540	826
Number of settlements	15	24	39
All countries			
Average exposure	24 months	16 months	
Household sample size	927	1,283	2,210
Number of settlements	29	45	74

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TABLE A.2. Sample size, attrition, and compliance.

	Phase I			Phase II			Combined Phases I and II			Phase I versus Phase II		
	Treat.	Control	Diff.	Treat.	Control	Diff.	Treat.	Control	Diff.	Phase I	Phase II	Diff.
Number of households												
Baseline	653	342		703	675		1,356	1,017		995	1,378	
Follow-up	611	316		658	625		1,269	941		927	1,283	
Attrition rate	0.06	0.08	–0.01	0.06	0.07	–0.01	0.06	0.07	–0.01	0.07	0.07	0.00
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Compliance rate	0.88	0.99		0.86	1.00		0.87	1.00		0.92	0.93	

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TABLE A.3. Baseline balance within and between phases.

	Phase I			Phase II			Phase I versus Phase II All			Phase I versus Phase II Only Controls		
Dependent variable	Treat.	Control	Diff.	Treat.	Control	Diff.	Phase I	Phase II	Diff.	Phase I	Phase II	Diff.
Satisfaction with floor quality	0.19	0.21	0.01	0.25	0.27	0.01	0.20	0.26	−0.06	0.21	0.27	−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	0.18	0.16	0.01
	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	[0.02]	[0.02]	[0.03]	[0.03]	[0.03]	[0.04]
Satisfaction with roof quality	0.17	0.20	−0.02	0.16	0.17	0.02	0.18	0.16	0.01	0.20	0.17	0.03
	(0.02)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	[0.02]	[0.02]	[0.03]	[0.02]	[0.03]	[0.03]
Satisfaction with rain protection	0.16	0.19	−0.01	0.15	0.14	0.03	0.17	0.14	0.02	0.19	0.14	0.05
	(0.01)	(0.02)	(0.03)	(0.01)	(0.01)	(0.02)	[0.02]	[0.02]	[0.03]	[0.02]	[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	0.25	0.27	−0.02
	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)	(0.02)	[0.02]	[0.03]	[0.04]	[0.03]	[0.03]	[0.05]
Assets value per capita (USD)	61.85	71.12	3.85	47.86	50.30	−0.38	62.58	50.26	12.32*	67.65	50.30	17.34**
	(5.48)	(7.70)	(11.16)	(2.98)	(3.18)	(4.54)	[5.75]	[3.03]	[6.43]	[6.43]	[3.76]	[7.38]
Monthly income per capita (USD)	49.45	59.85	−8.61	52.86	58.74	−5.08	53.08	55.77	−2.69	59.85	58.74	1.11
	(2.63)	(4.29)	(5.99)	(2.54)	(2.94)	(4.32)	[4.01]	[4.27]	[5.82]	[4.66]	[4.93]	[6.73]
Head's years of schooling	4.09	4.34	−0.01	4.37	3.87	0.26	4.18	4.13	0.05	4.34	3.87	0.47
	(0.14)	(0.20)	(0.21)	(0.12)	(0.12)	(0.17)	[0.52]	[0.29]	[0.59]	[0.70]	[0.28]	[0.75]
Head is male	0.69	0.69	−0.01	0.69	0.71	0.00	0.69	0.70	−0.01	0.69	0.71	−0.02
	(0.02)	(0.03)	(0.03)	(0.02)	(0.02)	(0.03)	[0.04]	[0.03]	[0.05]	[0.05]	[0.03]	[0.05]
Head's age	42.09	41.33	0.52	41.2	40.73	1.01	41.83	40.97	0.86	41.34	40.73	0.60
	(0.63)	(0.77)	(1.07)	(0.59)	(0.61)	(0.87)	[0.96]	[0.70]	[1.18]	[0.93]	[0.96]	[1.33]
Control variables:												
Settlement F.E			Yes			Yes			No			No

Notes: This table reports baseline means and differences in means of the analysis 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 versus Phase II main columns, standard errors clustered at the settlement level are reported in brackets. In the case of monetary variables, observations over the 99th percentile were excluded. *Significant at 10%; **significant at 5%.

TABLE A.4. Baseline balance between phases at slum level.

Dependent variable	Phase I slums mean	Phase II slums mean	Mean diff.
Monthly income per capita (USD)	51.47	54.23	−2.76
	(19.02)	(22.93)	[4.96]
Z-score housing quality	−0.03	0.04	−0.06
	(0.23)	(0.14)	[0.05]
Slum size (number of households)	35.54	30.62	4.91
	(26.76)	(23.79)	[6.16]
Satisfaction with quality of life	0.31	0.28	0.03
	(0.14)	(0.15)	[0.04]
Satisfaction with floor quality	0.22	0.26	−0.04
	(0.13)	(0.25)	[0.04]
Satisfaction with wall quality	0.21	0.17	0.04
	(0.17)	(0.15)	[0.04]
Satisfaction with roof quality	0.19	0.16	0.03
	(0.12)	(0.15)	[0.03]
Satisfaction with rain protection	0.17	0.15	0.03
	(0.11)	(0.14)	[0.03]

Notes: This table reports baseline means and differences in means of Phase I (45 obs.) and Phase II (28 obs.) slums. Standard deviations are reported in parenthesis and robust standard errors are reported in brackets. In the case of monetary variables, observations over the 99th percentile were excluded.

TABLE A.5. Housing quality.

		Model 1			Model 2	
Dependent variable	Mean control group	Treatment γ_1		Treatment × Phase I γ_2	Treatment γ_1		Treatment × Phase I γ_2

		Model 1			Model 2		
Dependent variable	Mean control group	Treatment γ_1		Treatment × Phase I γ_2	Treatment γ_1		Treatment × Phase I γ_2
Sharerooms good quality floors	0.44	0.18***		0.00	0.18***		0.00
		(0.02)		(0.03)	(0.02)		(0.03)
p-value ($\gamma_1 + \gamma_2 = 0$)			0.00			0.00	
Share rooms good quality walls	0.35	0.20***		−0.06*	0.20***		−0.06*
		(0.02)		(0.04)	(0.02)		(0.04)
p-value ($\gamma_1 + \gamma_2 = 0$)			0.00			0.00	
Share rooms good quality roof	0.43	0.17***		−0.02	0.17***		−0.01
		(0.02)		(0.03)	(0.02)		(0.03)
p-value ($\gamma_1 + \gamma_2 = 0$)			0.00			0.00	
Share rooms with windows	0.36	0.18***		−0.02	0.18***		−0.01
		(0.02)		(0.03)	(0.02)		(0.03)
p-value ($\gamma_1 + \gamma_2 = 0$)			0.00			0.00	
Control variables:							
Baseline covariates		No		No	Yes		Yes
Settlement F.E		Yes		Yes	Yes		Yes

Notes: 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 from the regression model $Y_{ij} = \alpha + \gamma_1 Treat_{ij} + \gamma_2 Treat_{ij} \times PhaseI_j + \mu_j + \varepsilon_{ij}$. 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 of schooling, gender, and age, as well as the value of household assets per capita and monthly income per capita, all of which were measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. Finally, we report the p-values of the F-tests of the null hypothesis $\gamma_1 + \gamma_2 = 0$. *Significant at 10%; ***significant at 1%.

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TABLE A.6. Hedonic adaptation in satisfaction with quality of life and housing in the presence of potential wear-and-tears of the house.

		Model 1		Model 2	
Dependent variable	Mean control group	Treatment γ_1	Treatment × Phase I γ_2	Treatment γ_1	Treatment × Phase I γ_2
Satisfaction with quality of life	0.53	0.14***	–0.11**	0.14***	–0.11**
		(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.38		0.38	
Satisfaction with floor quality	0.37	0.14***	–0.04	0.13***	–0.04
		(0.03)	(0.05)	(0.03)	(0.04)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.01		0.01	
Satisfaction with wall quality	0.30	0.23***	–0.15***	0.23***	–0.15***
		(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.02		0.03	
Satisfaction with roof quality	0.32	0.23***	–0.11**	0.23***	–0.11**
		(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Satisfaction with rain protection	0.29	0.20***	–0.11**	0.20***	–0.12**
		(0.03)	(0.05)	(0.03)	(0.05)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.02		0.02	
Summary housing	31.96	19.63***	–10.36***	19.59***	–10.46***
Satisfaction index		(2.31)	(3.62)	(2.31)	(3.61)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Control variables:					
Baseline covariates		No	No	Yes	Yes
Settlement F.E		Yes	Yes	Yes	Yes

Notes: Each row represents a separate dependent variable. For the first five variables, the dependent variable is an indicator equal to 1 if the respondent reports being "Satisfied" or "Very satisfied" and zero otherwise. The Summary Housing Satisfaction Index is calculated as the sum of the four housing satisfaction dummy variables (satisfaction with quality of floors, walls, roofs, and rain protection), multiplied by 25, and so the index ranges from 0 to 100. 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 from the regression model $Y_{ij} = \alpha + \gamma_1 Treat_{ij} + \gamma_2 Treat_{ij} \times PhaseI_j + \mu_j + \sum \gamma_{hqm} HQ_{ij} + \varepsilon_{ij}$, with $\sum \gamma_{hqm} HQ_{ij}$ 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, and share of rooms with window, all measured at follow up round. 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 of schooling, gender and age, as well as the value of household assets per capita and monthly income per capita, all of which were measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. Finally, we report the p-values of the F-tests of the null hypothesis $\gamma_1 + \gamma_2 = 0$.
**Significant at 5%; ***significant at 1%.

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TABLE A.7. Income, assets, and labor outcomes.

		Model 1		Model 2	
Dependent variable	Mean control group	Treatment γ_1	Treatment × Phase I γ_2	Treatment γ_1	Treatment × Phase I γ_2
Assets value per capita (USD)	67	−0.33	3.37	−0.19	2.36
		(8.75)	(13.39)	(8.56)	(13.09)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.76		0.83	
Monthly income per capita (USD)	59	−1.83	1.73	−2.31	1.78
		(4.91)	(7.92)	(4.90)	(7.84)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.99		0.93	
Hours worked last week by head	40	0.02	1.86	0.29	1.53
		(1.33)	(2.18)	(1.34)	(2.13)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.28		0.27	
Hours worked last week by spouse	34	−2.27	3.46	−2.04	3.60
		(2.48)	(3.80)	(2.51)	(3.83)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.68		0.59	
Control variables:					
Baseline covariates		No	No	Yes	Yes
Settlement F.E		Yes	Yes	Yes	Yes

Notes: Each row represents a separate dependent variable. 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 control group measured at follow-up. The next two columns, under the heading Model 1, report the results from the regression model $Y_{ij} = \alpha + \gamma_1 Treat_{ij} + \gamma_2 Treat_{ij} \times PhaseI_j + \mu_j + \varepsilon_{ij}$. 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 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 $\gamma_1 + \gamma_2 = 0$.

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TABLE A.8. Hedonic adaptation across housing quality status.

		Model 1		Model 2	
Dependent variable	Mean control group high-status	Treatment × High-Status γ_3	Treatment × High-Status × Phase I γ_4	Treatment × High-Status γ_3	Treatment × High-Status × Phase I γ_4
Satisfaction with quality of life	0.55	−0.02	−0.06	−0.02	−0.07
		(0.05)	(0.09)	(0.05)	(0.09)
p-value ($\gamma_3 + \gamma_4 = 0$)		0.27		0.24	
Satisfaction with floor quality	0.39	−0.08	0.06	−0.08	0.05
		(0.05)	(0.09)	(0.05)	(0.09)
p-value ($\gamma_3 + \gamma_4 = 0$)		0.78		0.64	
Satisfaction with wall quality	0.30	−0.04	0.03	−0.04	0.01
		(0.05)	(0.09)	(0.05)	(0.09)
p-value ($\gamma_3 + \gamma_4 = 0$)		0.88		0.64	
Satisfaction with roof quality	0.32	−0.05	0.01	−0.05	0.01
		(0.05)	(0.09)	(0.05)	(0.09)
p-value ($\gamma_3 + \gamma_4 = 0$)		0.60		0.54	
Satisfaction with rain protection	0.27	0.01	−0.01	0.01	−0.02
		(0.05)	(0.09)	(0.05)	(0.09)
p-value ($\gamma_3 + \gamma_4 = 0$)		0.97		0.95	
Housing satisfaction	31.91	−4.24	3.19	−4.40	2.23
Summary index		(4.24)	(6.96)	(4.25)	(6.98)
p-value ($\gamma_3 + \gamma_4 = 0$)		0.85		0.69	
Control variables:					
Baseline covariates		No	No	Yes	Yes
Settlement F.E		Yes	Yes	Yes	Yes

Notes: Each row represents a separate dependent variable. For the first five variables, the dependent variable is an indicator equal to 1 if the respondent reports being "Satisfied" or "Very Satisfied" and zero otherwise. The summary housing satisfaction index is calculated as the sum of the four housing satisfaction dummy variables (satisfaction with quality of floors, walls, roofs, and rain protection), multiplied by 25, and so the index ranges from 0 to 100. High-status is a dummy variable equal to 1 if the within-settlement normalized z-score on housing quality is positive, and zero otherwise. The first column reports the mean of the dependent variable for control households with high-status on housing quality measured at follow-up. The next two columns, under the heading Model 1, report the results of the regression model $Y_{ij} = \alpha + \gamma_1 Treat_{ij} + \gamma_2 Treat_{ij} \times PhaseI_j + \gamma_3 Treat_{ij} \times HighStatus_{ij} + \gamma_4 Treat_{ij} \times HighStatus_{ij} \times PhaseI_j + \gamma_5 HighStatus_{ij} + \mu_j + \varepsilon_{ij}$. Reports are the estimated coefficients and robust standard errors of γ_3 and γ_4. The last two columns, under the heading Model 2, additionally control for the household head's years of schooling, gender and age, as well as the value of household assets per capita and monthly income per capita, all of which were measured during the baseline round. 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 $\gamma_3 + \gamma_4 = 0$ for each model.

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TABLE A.9. Hedonic adaptation across income status.

		Model 1		Model 2	
Dependent variable	Mean control group high status	Treatment × High Status γ_3	Treatment × High Status × Phase I γ_4	Treatment × High Status γ_3	Treatment × High Status × Phase I γ_4
Satisfaction with quality of life	0.45	0.04	0.05	0.03	0.06
		(0.06)	(0.10)	(0.06)	(0.10)
p-value ($\gamma_3 + \gamma_4 = 0$)		0.24		0.23	
Satisfaction with floor quality	0.30	0.05	0.06	0.05	0.04
		(0.06)	(0.09)	(0.06)	(0.09)
p-value ($\gamma_3 + \gamma_4 = 0$)		0.17		0.20	
Satisfaction with wall quality	0.24	0.02	0.03	0.02	0.03
		(0.06)	(0.09)	(0.06)	(0.09)
p-value ($\gamma_3 + \gamma_4 = 0$)		0.55		0.57	
Satisfaction with roof quality	0.24	0.08	−0.05	0.07	−0.04
		(0.06)	(0.09)	(0.06)	(0.09)
p-value ($\gamma_3 + \gamma_4 = 0$)		0.68		0.64	
Satisfaction with rain protection	0.21	0.10*	−0.11	0.10*	−0.10
		(0.06)	(0.09)	(0.06)	(0.09)
p-value ($\gamma_3 + \gamma_4 = 0$)		0.93		0.99	
Housing satisfaction	30.25	5.99	−1.78	6.00	−1.79
Summary index		(4.53)	(7.46)	(4.55)	(7.48)
p-value ($\gamma_3 + \gamma_4 = 0$)		0.48		0.48	
Control variables:					
Baseline covariates		No	No	Yes	Yes
Settlement F.E		Yes	Yes	Yes	Yes

Notes: Each row represents a separate dependent variable. For the first five variables, the dependent variable is an indicator equal to 1 if the respondent reports being "Satisfied" or "Very Satisfied" and zero otherwise. Thesummary housing satisfaction index is calculated as the sum of the four housing satisfaction dummy variables (satisfaction with quality of floors, walls, roofs, and rain protection), multiplied by 25, and so the index ranges from 0 to 100. High status is a dummy variable equal to 1 if the household income is above the median income within the settlement, and zero otherwise. The first column reports the mean of the dependent variable for control households with high-status on income measured at follow-up. The next two columns, under the heading Model 1, report the results of the regression model $Y_{ij} = \alpha + \gamma_1 Treat_{ij} + \gamma_2 Treat_{ij} \times PhaseI_j + \gamma_3 Treat_{ij} \times HighStatus_{ij} + \gamma_4 Treat_{ij} \times HighStatus_{ij} \times PhaseI_j + \gamma_5 HighStatus_{ij} + \mu_j + \varepsilon_{ij}$. Reports are the estimated coefficients and robust standard errors of γ_3 and γ_4. The last two columns, under the heading Model 2, additionally control for the household head's years of schooling, gender and age, as well as the value of household assets per capita and monthly income per capita, all of which were measured during the baseline round. 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 $\gamma_3 + \gamma_4 = 0$ for each model. *Significant at 10%.

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Table A.10. Hedonic adaptation in satisfaction with quality of life across housing and income status (NLS).

	NLS estimates		
Status group	Permanent effect β_P	Transitory effect β_T	Hedonic adaptation rate δ
		Panel A. Housing Quality Status	
High housing status	0.00	0.38***	0.15
	(0.10)	(0.12)	(0.14)
Low housing status	0.00	0.43***	0.13
	(0.10)	(0.09)	(0.09)
p-value for F-test of Pooling housing status groups	0.25	0.92	0.34
		Panel B. Income Status	
High income status	0.06	0.43***	0.16
	(0.08)	(0.10)	(0.10)
Low income status	−0.07	0.40***	0.11
	(0.16)	(0.11)	(0.11)
p-value for F-test of Pooling income status groups	0.10	0.97	0.51
Control variables:			
Baseline covariates	Yes	Yes	Yes
Country F.E	Yes	Yes	Yes

Notes: Each row represents a separate status group. Individuals with a baseline Z-Score housing quality above the within-settlement median possess a high housing status, whereas those below that median possess alow housing quality status. Likewise, individuals with a baseline per capita income above the within-settlement median possess a high income status, whereas those below that median possess a low income status. The three columns report the structural parameter estimates of the NLS regression $Y_{ij} = \alpha + Treat_{ij} \times [\beta_P + \beta_T e^{-\delta(t_i - t_0)}] + \varepsilon_{ij}$, with the dependent variable being satisfaction with quality of life, defined as an indicator equal to 1 if the respondent reports being "Satisfied" or "Very satisfied" and zero otherwise. t_i is the months of exposure to the program enjoyed by individual i, t_0 the minimum treatment exposure (10.3333 months), X_{ic} a vector of baseline covariates including household head's years of schooling, gender and age, as well as the monthly income per capita, number of rooms per capita, the share of rooms with good quality of walls, and the share of rooms with good quality of roofs, all of which were measured during the baseline round, and μ_c the country fixed effects. δ is expressed as a monthly rate. Reports are the estimated coefficients and robust standard errors. 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. Lastly, we also report the p-value of F-tests if the null hypothesis that each structural estimate is equal across status group samples. ***Significant at 1%.

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TABLE A.11. Description of variables and sample sizes. Follow-up survey.

		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 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)	Totalasset value per capita reported by the household.	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 byhead 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 four 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

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Table A.11 Continued.

		Phase I		Phase II		All	
		Obs. Control	Obs. Treat.	Obs. Control	Obs. Treat.	Obs. Control	Obs. Treat.
Z-score Satisfaction with floor quality	Ordinal satisfaction with floor quality standardized by the control group mean and standard deviation within each settlement.	316	611	625	658	941	1,269
Z-score Satisfaction with wall quality	Ordinal satisfaction with walls quality standardized by the control group mean and standard deviation within each settlement.	316	611	625	658	941	1,269
Z-score Satisfaction with roof quality	Ordinal satisfaction with roof qualitystandardized by the control group mean and standard deviation within each settlement.	316	611	625	658	941	1,269
Z-score Satisfaction with rain protection	Ordinal satisfaction with the protection against water when it rains standardized by the control group mean and standard deviation within each settlement.	316	611	625	658	941	1,269
Z-score Satisfaction with quality of life	Ordinal satisfaction with quality of lifestandardized by the control group mean and standard deviation within each settlement.	316	611	625	658	941	1,269
Satisfaction summary index	Equally-weighted average of the Z-scores of satisfaction with quality of floors, walls, roofs, protection against water, and life	316	611	625	658	941	1,269
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 theenumerator).	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
High Housing Status	Indicator equal to one if the equally weighted average of z-scores of number of rooms, share of rooms with good quality floors, share of rooms with good quality walls, share of rooms with good quality roofs, and share of rooms with window is positive at baseline, and zero otherwise.	315	606	625	658	940	1,264
High income status	Indicator equal to one if the household's monthly income per capita is above the median income within the settlement at baseline, and zero otherwise.	316	611	625	658	941	1,269

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Appendix B: Alternative Ways of Dealing With Cardinality.

Ordered Probit Model. As detailed in Section 3, our metric provides four response points for relative subjective well-being. We note that Likert-scale measures assume cardinality, that is, that responses to the SWB question fall on a linear scale; thus, the differences in perceived well-being between one response point and the following one are assumed to be the same for all respondents. Nonetheless, people may have different utility reference points for each of the thresholds. In other words, what “Satisfied” or “Very satisfied” means for one respondent is not necessarily what it means for another one. As a result, a binary definition of satisfaction may not reflect each individual’s actual perception of satisfaction.

One strategy for ruling out cardinality problems is to estimate an Ordered Probit model. Assuming that individuals understand that the highest category represents the maximum level of satisfaction, and then no scale of valuation interferes with the interpretation of the probability that an individual will state that he or she feels “Very satisfied”, and the same applies to the lowest category of satisfaction. And in fact, when we re-estimated equation (1) using an Ordered Probit model, the results were qualitatively the same as when we used a binary measure for level of satisfaction. The probability of a household being in the highest satisfaction category always increases with treatment in the second phase (γ_1 positive), and the difference across phases is positive for Phase II households (γ_2 negative); likewise, the probability of a household being in the lowest satisfaction category always decreases with treatment in the second phase (γ_1 negative), and the difference across phases is negative for Phase II households (γ_2 positive). Tables B.1 and B.2 show these results.

Standardized Measure of Subjective Well-Being. A second concern is that when a binary state of satisfaction is considered, any substantial variance in the outcomes is not captured. Hence, we repeat the exercise using a standardized variable as in Kling, Liebman, and Katz (2007) and Ludwig et al. (2012). In particular, we construct a standardized measure using all the values from the satisfaction scale, and we do this for each measure of satisfaction. Specifically, we use the full four-point indicator as a continuous measure and standardize it for each settlement using the mean and standard deviations for the control group of that settlement. The standardization procedure is based on the assumption that respondents have common views of how the thresholds map into utility within settlements but allows them to vary by settlement. Furthermore, we construct a summary index of subjective well-being. The summary index is computed as the sum of standardized satisfaction variables (with the sign of each measure oriented so that more beneficial outcomes have higher scores), divided by the number of satisfaction variables. This summary index aggregates information across related outcomes and is useful both as a summary statistic and possibly as a means of augmenting the statistical power to detect effects of the intervention that are consistent across groups of outcomes.

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TABLE B.1. Hedonic adaptation in satisfaction with quality of life and housing (Ord. Probit).

		Model 1		Model 2	
Dependent variable	"Very satisfied" control group	Treatment γ_1	Treatment × Phase I γ_2	Treatment γ_1	Treatment × Phase I γ_2
Satisfaction with quality of life	0.03	0.07*** (0.01)	–0.03** (0.01)	0.07*** (0.01)	–0.03** (0.01)
Satisfaction with floor quality	0.02	0.05*** (0.01)	–0.01 (0.01)	0.05*** (0.01)	–0.01 (0.01)
Satisfaction with wall quality	0.02	0.06*** (0.01)	–0.02** (0.01)	0.06*** (0.01)	–0.02*** (0.01)
Satisfaction with roof quality	0.02	0.07*** (0.01)	–0.02* (0.01)	0.07*** (0.01)	–0.02* (0.01)
Satisfaction with rain protection	0.02	0.06*** (0.01)	–0.02* (0.01)	0.06*** (0.01)	–0.02* (0.01)
Control variables:					
Baseline Covariates		No	No	Yes	Yes
Settlement F.E		Yes	Yes	Yes	Yes

Notes: Each row represents a separate categorical dependent variable that equals 1 if the individual is "Very satisfied"; 2 if "Satisfied"; 3 if "Neither satisfied nor unsatisfied"; and 4 if "Unsatisfied". The first column reports the proportion of individuals in the control group that report being "Very satisfied" at follow-up. The next two columns, under the heading Model 1, report the results from the Ordered Probit regression $Y_{ij} = \alpha + \gamma_1 Treat_{ij} + \gamma_2 Treat_{ij} \times PhaseI_j + \mu_j + \varepsilon_{ij}$. Reports are the estimated marginal effects for the first category and robust standard errors. The last two columns, under the heading Model 2, additionally control for the household head's years of schooling, gender and age, as well as the value of household assets per capita and monthly income per capita, all of which were measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. *Significant at 10%; **significant at 5%; ***significant at 1%.

First, as shown in Table B.3, treatment substantially increases the subjective well-being of the short-exposure beneficiaries (γ_1), which is consistent with the results presented for binary outcomes. Although the beneficiaries' satisfaction with housing quality increases by between 0.56 and 0.81 standard deviations, gains in their general subjective well-being are about 0.48 standard deviations relative to the control group. In terms of adaptation (γ_2), we find that the treatment effect on satisfaction with quality of life is around 50% lower in households treated in Phase I than in those treated in Phase II; this is about the same result for the adaptation effect as was found using a binary life satisfaction measure. With respect to satisfaction with housing quality (floor, roof, walls and protection from rain), we find that positive effects from treatment decrease from short to long exposure, and this is by a proportion similar to that detected for the adaptation effects found using binary satisfaction measures.

Finally, all the estimated effects except those for satisfaction with the quality of floors remain significant after adjusting the p-values for multiple outcomes, which rules

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TABLE B.2. Hedonic adaptation in satisfaction with quality of life and housing (Ord. Probit).

Dependent variable	"Unsatisfied" control group	Model 1 Treatment γ_1	Model 1 Treatment × Phase I γ_2	Model 2 Treatment γ_1	Model 2 Treatment × Phase I γ_2
Satisfaction with quality of life	0.21	−0.12*** (0.01)	0.05** (0.02)	−0.12*** (0.01)	0.05** (0.02)
Satisfaction with floor quality	0.35	−0.17*** (0.02)	0.03 (0.03)	−0.17*** (0.02)	0.03 (0.03)
Satisfaction with wall quality	0.37	−0.21*** (0.02)	0.09*** (0.03)	−0.21*** (0.02)	0.09*** (0.03)
Satisfaction with roof quality	0.38	−0.22*** (0.02)	0.06* (0.03)	−0.22*** (0.02)	0.06* (0.03)
Satisfaction with rain protection	0.40	−0.20*** (0.02)	0.06* (0.04)	−0.20*** (0.02)	0.06* (0.04)
Control variables:					
Baseline covariates		No	No	Yes	Yes
Settlement F.E		Yes	Yes	Yes	Yes

Notes: Each row represents a separate categorical dependent variable that equals 1 if the individual is "Very satisfied"; 2 if "Satisfied"; 3 if "Neither satisfied nor unsatisfied"; and 4 if "Unsatisfied". The first column reports the proportion of individuals in the control group that report being "Unsatisfied" at follow-up. The next two columns, under the heading Model 1, report the results from the Ordered Probit regression $Y_{ij} = \alpha + \gamma_1 Treat_{ij} + \gamma_2 Treat_{ij} \times PhaseI_j + \mu_j + \varepsilon_{ij}$. Reports are the estimated marginal effects for the first category and robust standard errors. The last two columns, under the heading Model 2, additionally control for the household head's years of schooling, gender and age, as well as the value of household assets per capita and monthly income per capita, all of which were measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. *Significant at 10%; **significant at 5%; ***significant at 1%.

out the presence of false positives in our analysis. Indeed, based on our summary index of satisfaction, we find that, after 16 months of exposure to the TECHO program, beneficiaries' satisfaction with their housing and life increased by 0.66 standard deviations but that 47% of that gain had disappeared 8 months later. An advantage of the standardized measure is that it also allows us to compare the order of magnitude of the effects with those found in other studies. Using standardized data on general subjective well-being from the World Values Surveys (WVSs) in El Salvador (1999), Uruguay (2011) and Mexico (2012), our estimated effect is somewhat higher than the difference in average subjective well-being of those who completed the tertiary level of education and those who completed no more than an elementary level of education—a huge effect considering that the control group in our study had 3.9 years of schooling, on average, at baseline. The effect size is also equivalent to the difference in subjective

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TABLE B.3. Hedonic adaptation in satisfaction with quality of life and housing (Z-score).

Dependent variable	Mean control group	Model 1 Treatment γ_1	Model 1 Treatment × Phase I γ_2	Model 2 Treatment γ_1	Model 2 Treatment × Phase I γ_2
Satisfaction with quality of life	0.00	0.48***	−0.23**	0.48***	−0.23**
		(0.06)	(0.09)	(0.06)	(0.09)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Satisfaction with floor quality	0.00	0.56***	−0.17*	0.56***	−0.17*
		(0.06)	(0.10)	(0.06)	(0.10)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Satisfaction with wall quality	0.00	0.81***	−0.47***	0.81***	−0.48***
		(0.07)	(0.11)	(0.07)	(0.11)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Satisfaction with roof quality	0.00	0.74***	−0.30***	0.75***	−0.31***
		(0.06)	(0.10)	(0.06)	(0.10)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Satisfaction with rain protection	0.00	0.69***	−0.35***	0.70***	−0.36***
		(0.06)	(0.10)	(0.07)	(0.10)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Satisfaction summary index	0.00	0.66***	−0.31***	0.66***	−0.31***
		(0.05)	(0.08)	(0.05)	(0.08)
p-value ($\gamma_1 + \gamma_2 = 0$)		0.00		0.00	
Control variables:					
Baseline covariates		No	No	Yes	Yes
Settlement F.E		Yes	Yes	Yes	Yes

Notes: Each row represents a separate dependent variable. Satisfaction measures are z-scores, standardized by the control group mean and standard deviation within each settlement. The satisfaction summary index is defined as the average of the z-scores of all the variables in the table, with the sign of each measure oriented so that the more beneficial outcomes have higher scores. 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 from the regression model $Y_{ij} = \alpha + \gamma_1 Treat_{ij} + \gamma_2 Treat_{ij} \times PhaseI_j + \mu_j + \varepsilon_{ij}$. 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 of schooling, gender, and age, as well as the value of household assets per capita and monthly income per capita, all of which were measured during the baseline round. Following the standard procedure, when a control variable has a missing value, we impute a value equal to 0 and add a dummy variable equal to 1 for that observation, which indicates that the control variable was missed. Finally, we report the *p*-values of the *F*-tests of the null hypothesis $\gamma_1 + \gamma_2 = 0$. *Significant at 10%; **significant at 5%; ***significant at 1%. The Bonferroni corrected for multiple outcomes *p*-value is 0.02, for a significance level of 0.1.

well-being between individuals who report being in the 8th decile (third richest) and those who report being in the 3rd decile (third poorest) of the income distribution.[22]

22. The subjective well-being measure taken from the WVS consists of a four-point response scale to the question: "Taking all things together, would you say you are very happy, happy, not very happy, not

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happy at all?". Although TECHO's subjective well-being question also includes a four-point response scale, TECHO's phrasing and scale responses are not exactly the same as those of the WVS survey, which implies that there may be differences in cardinality across the surveys' SWB questions. Therefore, these results should be interpreted cautiously.

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Supplementary Data

Supplementary data are available at *JEEA* online.

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