Replication Plan

Study: "Housing, Health and Happiness"

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I. Study Relevance

Inadequate housing is a multidimensional problem that affects a great number of people all over the developing world. A house is considered a slum if it lacks of either access to improved water, access to improved sanitation facilities, sufficient living area, durable housing or secure tenure (UN-Habitat 2010). In 2003, 43% of the urban population in developing regions suffered from at least one feature of inadequate housing and thus was classified as living in a 'slum' (UN-Habitat 2003), by 2010 this fraction decreased to 33% (UN-Habitat 2010). In Latin America and the Caribbean, roughly 24% of its urban population lived in slums during 2010.

Lacking access to adequate housing conditions means that households live in a precarious physical environment, with insufficient living spaces due to small and overcrowded houses, unsafe constructions and limited protection against natural elements. In addition, inadequate housing is also associated with limited access to water and electricity, unhealthy sanitation facilities, limited tenure security, etc. All of these features of inadequate housing have negative impacts on household member's wellbeing.

The literature that studies housing and slum upgrading and its effect on health and socioeconomic outcomes have been mainly developed by the medical literature using cross sectional relationships (see Turley et al. 2013 for a literature review). Despite the increasing use of experiments in the empirical economic literature, only a few papers use experimental designs to study the casual effects from housing upgrade. Among slum interventions that have been studied experimentally we can find: access to water supply, street pavement and, improvements in housing materials. The most studied case is access to water connection. It has been found that connection to water supply reduces child mortality (Galiani et al. 2005), reduces the presence of diarrhea (Galiani et al. 2009) and increases self-reported happiness (Devoto et al. 2010). Street pavement has been found to increase property values and as result increase collateralized credit (Gonzales-Navarro.and Quintana-Domeneque 2012). The closest article to the proposed replication paper is Galiani et al. (2013), where it is shown that better housing material increases

a measure of household subjective well-being. Thus, the analyzed paper stands out for two aspects: first, it adds to the body of literature that studies slum improvements by using a quasi experimental design and, secondly, it contributes to the literature that relates slums conditions to subjective well-being (see Gonzales-Navarro and Quintana-Domeque, 2012 and Galiani et al., 2013).

II. Replication Plan: Measurement and Estimation Analysis

We propose to conduct a Measurement and Estimation Analysis which includes a pure replication component. The pure replication will consist on reproducing all results from the paper by using the author's raw datasets and questionnaires.

We consider the author's identification strategy to be fairly convincing, hence, the additional analysis and robustness checks proposed in these pages aim to strengthen the validity of the results presented in the paper. We propose to expand the analysis in 6 ways: (i) exploring potential heterogeneity in treatment effects, (ii) using 'state of the art' methodologies to deal with missing values, (iii) disaggregating the satisfaction variables and running a multinomial regression, (iv) estimating a treatment effect with program eligibility as an instrument for each room in the house, (v) expanding the control vs. treatment trend analysis by using additional data from ENIGH and, (vi) by including geographic controls. We explain each of these proposed routes in the next few pages.

1. Explore heterogeneity in treatment effects.

The study focuses on the intention to treat estimates, and presents treatment effects as constant across the whole sample. However, it is feasible that improving the floor may have different effects on physical and mental health depending on other materials in the house. In the absence of the ideal experimental set-up to study this heterogeneity, a stratified randomization, we will follow Duflo et. al (2006) in making clear in the replication document that the subgroup classification was chosen ex-post the intervention. In addition,

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to avoid concerns about data mining we have selected only one variable - share of cement floor at baseline - to conduct the subgroup analysis. We will classify households in two groups: high vs. low share of cement floor at baseline. The heterogeneous effect found will be the result of fully interacting the intent to treat regression with the dummy variable high share of rooms with cement floor at baseline.

We find this heterogeneity analysis relevant because the effect from improving floors in dwellings might be different depending on their baseline level of adequate materials. Furthermore, we argue that, depending on their starting situation, households received different levels of the treatment. Indeed, Table 4 of the study shows that Piso Firme had lower effects on bathroom flooring (13.1% more households had cement floors in the bathroom in the treatment group than in the control group) than in dining room flooring (29.6%), bedrooms (35.6%) or kitchens (37.9%). Then, since eligibility was determined by the presence of some dirt floor within the housing unit, while some households might have gone from 0% to 100% share of cement-floored rooms[1], others might have gotten less, presumably those that were better off at the starting date. The authors collected retrospective baseline data on the predominant material of each room in the household which will allow us to construct a baseline measure of share of rooms with cement floor. There are some potential endogeneity issues, since households that received the largest increase in the share of cement floor are those who had the least cement floor at baseline. As we discussed earlier, impacts among the poorest groups are expected to be higher.

2. Use Multiple Imputation and Maximum Likelihood methods to deal with missing values

Throughout the paper, the authors use the "Missing Indicator Method" to deal with missing values. This methodology consists on replacing all missing values in any covariate with a zero and to include in the regression a dummy variable to indicate that such replacement has been made. The main criticism made to this methodology is that it provides biased estimators (Jones 1996 and Enders 2010). To handle the missing value problem we propose two tasks:

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[1] Since we don't have information on the exact area of the house that was covered by cement floor we won't be able to calculate the exact level of treatment received by each household.

a. Describe in detail the structure of missing data: we will include descriptive figures and tables in order to show missing data patterns and mechanisms[2]. Specifically, we are interested in knowing if the likelihood of missingness is related to any variable in the dataset.

b. Implement imputation models: With the purpose of checking the robustness of the findings shown in the published paper, we will implement Multiple Imputation and Maximum Likelihood methodologies, which are considered the actual "state of the art" (Shafer and Graham, 2002; Enders, 2010)[3].

In addition, we will replicate all the results in the paper restricting the sample to only those observations with non-missing values. The lower number of observations lowers the power and thus the ability of finding statistically significant results but the sign of the coefficients will be informative.

3. Split the satisfaction measures into its four categories.

The satisfaction measures (Table 6 in the paper) are summarized in dichotomous outcome variables: satisfied vs. dissatisfied. We will use the original categories: very satisfied, satisfied, fair, and, unsatisfied. This extension to the analysis would consist on estimating the same regressions as in Table 6 with an ordered multinomial choice model, such as ordered probit. This may shed further light on the trade-offs between the different degrees of satisfaction.

4. Estimated the treatment effects using program eligibility as an instrument.

The paper studies the intention to treat, i.e., the effects of living in program areas on the outcome variables. After checking for differential pre-trends, like in figures 1, 2A, 2B, and 3, we find no evidence

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[2] There are three mechanisms for missing data: (i) Missing completely at random (MCAR), (ii) Missing at random (MAR) and (iii) Missing not at random (MNAR). In a broad sense, MCAR means that the likelihood of missingness of a given variable is unrelated to any other variable in the dataset or to unobservable values of the variable itself. Data is said to be MAR when the likelihood of missingness depends on other variables in the dataset. Finally, MNAR means that for a given variable, the probability of missingness is related to the missing values. Given that we do not observe missing data, we can only test if missing data is related to other observable variables.

[3] STATA 12 packages, such as "mi" and "ml" will be employed for this purpose.

to reject the assumptions behind the identification strategy. The results may be pushed a little further to get the average treatment effects. One could use program eligibility and location (treatment vs. control areas) as instruments for a "cement floors" dummy. In footnote 21, the authors present IV results for the share of rooms with cement floors, what we propose is to study separately the effect of having cement floor on different rooms in the house.

In particular, we can estimate the following first stage regression:

$$cement_i = \alpha + \delta\ Treated_i + \beta X_i + \varepsilon_i \dots (1)$$

, where "cement" is a dummy for cement floor in the kitchen, dining room, bathroom and bedroom with cement floor, and "X" are other household-level covariates used in the paper.

Then one can obtain the average treatment effect of this program by plugging the predicted values of (1) in the following second stage regression:

$$outcome_i = \lambda + \gamma \widehat{cement}_i + \mu X_i + \epsilon_i \dots (2)$$

, where γ is the parameter of interest. By estimating different versions of (2), i.e. by instrumenting for the presence of cement-floor in different rooms in the house, we can study a much more precise causation mechanism through which floor quality affects both child health and maternal mental health. One might argue that the positive health effects of cement-flooring is entirely driven by the improvement in the kitchen, where a cement floor can prevent food from being contaminated by parasites living in dirt, or that it is driven by improvement in the dining room where food is actually eaten, or in the bedroom, where children are likely to spend most of their time at home. This hypothesis can be tested by testing the corresponding treatment effect from each different version of (2).

5. Gather data from ENIGH and replicate Figure 3 with health-related outcome variables.

Figure 3 in the paper uses the National Income and Expenditure Household Survey (in Spanish, Encuesta Nacional de Ingresos y Gastos de Hogares or "ENIGH") to show that treatment and control areas had similar shares of households with cement floor before the program started. In 2002, just after the program started, treatment and control areas diverged in levels. To provide further evidence that the only reason for this diversion is the implementation of Piso Firme we will construct similar graphs to Figure 3 for other household characteristics that are reported in ENIGH such as (i) the proportion of households with cement walls and (ii) proportion of households with concrete roof. If we observe that in both districts, Coahuila and Durango, other housing characteristics (except cement floor) show similar trends it can strengthen the argument that Piso Firme was the only factor behind housing improvement. It could be the case that treated households were improving other housing materials after receiving the program as it frees up household resources to spend on house upgrading. If that were the case, then the results would still be causal but the interpretation would be the program effect and not only the result from having cement floors.

In addition, we will use information from ENIGH to include trends on health outcomes for the same time period. We will include the following health measures[4]: (i) number of medical consultations, (ii) household expenditure in medical consultation, (iii) number of clinical analysis, (iv) household expenditure in clinical analysis, (v) household expenditure in medicines and, (vi) total household health care expenditure. These variables would be reported only as an illustrative exercise as the reason for medical consultation is not specified in the data.

6. Take advantage of the GPS information and check differences in the sampling

The authors provided the location of the 136 census-blocks used in their sample. After conducting a preliminary analysis of the location of treated and control census blocks in the sample we have noticed

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[4] The reference window for all the health variables listed corresponds to the last three months.

two things: (i) land elevation is very similar among treatment and control census blocks (see Figure 1) and (ii) one control census block is noticeably separate from all the rest of the sample. To address this second finding we propose to carefully describe characteristics of all household that reside in that census-block and compare it with the rest of the sample. Even though these comparisons will lack of statistical power due to the small number of households living in that particular block we believe it will be informative.

In addition, we will incorporate housing density per block in the analysis. Using the author's publicly available data at the AEJ website, we found that the average number of interviewed households per block was statistically different across treatment and control groups (3.13 and 7.6 for treated and control group, respectively). We propose to expand the analysis made by the authors, by including this density variable in the regressions and check the robustness of the results to this modification.

With this very preliminary look at the data we cannot explain yet why housing density is different across treatment and control groups. One possible explanation is that this difference is related to cities having different block sizes or it can also be due to oversampling of control households within blocks. In order to analyze this possible explanation, we will also make use of the original database, the 2000 Mexican census, which was used by the authors to select the sample. By studying the differences between blocks (size and number) we will try to explain the difference in the density explained above.

IV. REFERENCES

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Figure 1: Map of treated and control census blocks