Improving Housing Among Marginalized Communities: Effects of Social Capital in the Covid-19 Crisis

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Abstract

Social capital may be central to increasing compliance with government directives or mitigating the fallout during pandemics or natural disasters. Through a randomized controlled trial of an NGO-led program in Brazilian slums, I test whether individuals who worked with other community members towards improving housing conditions are more likely to comply with public health measures during the Covid-19 crisis and to help and collaborate with others. My findings show that community members who were part of the program have higher levels of trust and reciprocity with their peers and community leaders than their counterparts who did not take part in the program. These ties translate into better evaluations of the work of community leaders, NGOs, and governments during the crisis. Yet, I find no substantial change in different measures of physical distancing and basic hygiene, nor on measures of solidarity and claim-making activities. This work contributes to a growing literature on how social capital shapes collective action in contexts of extreme vulnerability and humanitarian crises. It also raises questions about the notion that social capital will necessarily promote collective action to fight Covid-19.

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Introduction

The first year of the Covid-19 pandemic raised attention to the different challenges governments around the globe have faced in imposing preventive behavior measures. Since mass-scale vaccination is still not available, authorities frequently rely on policies that promote physical distancing and basic hygiene to control the spread of the disease. However, citizen compliance is difficult to achieve because preventive behavior presupposes incurring high individual costs. In essence, it is a collective action problem, as members of a group (community or society) must coordinate to individually bear the costs of self-isolating (or taking other measures) in exchange for a future outcome with fewer infections within the group. The extent to which coordination will be successful may depend on different levels of social capital, which may substantially vary depending on social, economic, and institutional contexts (Putnam et al., 1994; Bourdieu and Richardson, 1986; Coleman, 1988; Fukuyama, 1995).

Although a growing literature addresses how variations in social capital may affect citizen's adherence to health measures (Bai et al., 2020; Barrios et al., 2020; Bartscher et al., 2020; Borgonovi and Andrieu, 2020; Ding et al., 2020; Fraser and Aldrich, 2020; Kokubun, 2020; Kuchler et al., 2020; Miao et al., 2020; Varshney and Socher, 2020; Wu et al., 2020; Wu, 2020), most of these studies focus on developed countries and employ a macro-level analysis, using data from counties, states, and sometimes whole countries. I evaluate how an NGO-led housing program in Brazilian slums increases beneficiaries' ties to other members of the community, and what the possible implications are for individual behavior and attitudes, as well as for local cooperation in confronting the crisis. While Brazil figures in the top three list of countries with the highest numbers of cases and deaths [1], wretched settlements are the places where the situation becomes even more dramatic (Wasdani and Prasad, 2020). The poorest Brazilian *favelas* are highly precarious, without access to water, sanitation, proper quality housing, adequate heating, or security. Moreover, because slums such as these are highly crowded, they may create a perfect environment for the spread of the disease (Corburn et al., 2020). By looking to a family level intervention in slums of a developing country, the present work contributes to the debate in at least two under-explored aspects of how social capital impacts the pandemic: the role of the context, as most studies focus on developed western democracies (Wu et al. 2020); and the role of individual-level social capital mechanisms (Coleman, 1988; Wu, 2020).

In a partnership with TETO [2], a well-known non-governmental organization (NGO), I randomly selected families to participate in a program that, after a series of meetings, provides them higher quality prefabricated houses. The program is a bundle: though TETO heavily subsidizes the housing unit and provides a voluntary labor force to build them, residents must cooperate with other members of the community to make the construction feasible. They must engage previous networks and form new ones, as well as rely on norms of trust and reciprocity, which in combination allow community members to plan, organize, and execute the

[1] https://www.worldometers.info/coronavirus/countries-where-coronavirus-has-spread/

[2] The word "teto" means "roof" in Portuguese

program. I argue that the TETO's intervention increases beneficiaries' social skills and directly tests potential impacts on different outcomes related to collective action to fight Covid-19. As some authors suggest, different types of social capital may lead to different outcomes (Bai et al., 2020; Ding et al., 2020). Thus, it is essential to conceptualize each component of social capital separately if we want to disentangle the nuances and the role each form plays (Wu, 2020). As far as my research goes, the most commonly used measures of social capital in the literature are civic norms, networks, political trust, and social trust. The majority of these studies use indexes that aggregate these measures into a single indicator. Thus, a possible caveat is that we learn little about the mechanisms driving the results.

After combining a rigorous randomized trial with a series of qualitative interviews, my results indicate that the housing program does increase social capital, but has only marginal effects on preventive behavior outcomes. Though I also cannot neatly parse out the impacts of each component of TETO's program, I find that it increases participants' networks, as well as norms of trust and reciprocity between members of the community. However, it does changes neither civic norms nor public trust, which may be the missing components of the program that can partly explain the lack of substantive behavioral changes in prevention and other measures – as Bai et al. 2020 argue, civic norms are the main drivers of behavioral change towards self-isolation. To complement my theory, I test whether the program affects solidarity among beneficiaries, but I find only marginal increments in a few measures of expected solidarity. Finally, I test whether social capital has any influence on claim-making activities, as dwellers have strong incentives to claim humanitarian aid from governments and civil society institutions. Still, results are null for this hypothesis too. On the other hand, I find that those who participate in TETO's program have a better evaluation of how community leaders, government, and NGOs have dealt with the crisis. Hence, I suggest that social trust, reciprocity, and networks – the types of social capital included in the program – may be channels to improve government accountability or other forms of institutional governance led by NGOs or community leadership (formal or informal). However, I question the notion that social capital necessarily promotes "good outcomes" in terms of collective action to fight Covid-19. For proper collective action, context and certain types of social capital may well prove more salient.

Background and Hypothesis

Covid-19 crisis has hit Brazilian slums in an unprecedented way. Not only families are facing the direct effects of the epidemic, through higher fatality rates than non-slum neighborhoods [3], but also through indirect effects from the social-economic consequences of the crisis. While national and subnational government authorities could not agree on which policies to adopt (Ortega and Orsini, 2020), unorganized physical distancing and self-quarantine measures disrupted production chains, especially in the service sector, where most of the jobs

[3] FioCruz epidemiological bulletin of Slums: https://portal.fiocruz.br/sites/portal.fiocruz.br/files/documentos/boletim_socio epidemiologicos_covid_nas_favelas_1.pdf

are. Informal workers were the first to feel the effects of unemployment, and because *favelas* concentrate low-skilled informal workers, the poorest among them were rapidly facing the threat of hunger [4]. In our sample, 46.5% of the families mentioned having difficulties in buying food, while 58% started to buy cheaper supplies since the beginning of the pandemic. About 45% of the work force was unemployed at the time I made the interviews (unemployment rate in Brazil was around 12.5% at the same period [5]).

Moreover, slum dwellers also have to deal with serious infrastructure deprivations that are clear bottlenecks to the adequate compliance with Covid-19 protocols, such as social distancing and basic hygiene (Wasdani and Prasad, 2020). Not only housing, but also public urban infrastructure are inadequate and unsafe. The poorest urban slums are places that lack pavement, proper sewage systems, clean water, waste collection, and sometimes electricity. Adding to the fact that most of them are overcrowded, it is set a terrible environment to the spread of diseases (Corburn et al., 2020). A survey from TETO with community leaders shows that 50% of the slums in our sample are having troubles with water supply, and 23% lack basic hygiene items (soap, cleaning material, etc.). From the interviewed families, almost 12% do not have either toilet or shower in the house, while 22% of them do not have one of the two facilities. Without water and basic sanitary infrastructure, it is difficult for the families to keep up with measures to fight Covid-19.

If we are to view the pandemic as a collective action problem, we need to consider both material conditions and social relations that underlie life within slums. Take for example the material conditions: the higher levels of deprivation that slum dwellers are facing work as a gridlock to group coordination. Bearing the cost of engaging in collective action can be harder for those who lack minimum material assets (Cleaver, 2005). How can we expect strict physical isolation from a unemployed single mother with four children unable to go to school, while living with contingent water access? From this perspective, deviant behavior is somewhat expected. On the other hand, even within slums, particular forms of social relations can be sources of cooperation. The literature on social capital highlights how norms of trust and reciprocity, as well as network ties, can be crucial either in terms of sanctions or in terms of seeking mutual benefits (Coleman, 1988; Woolcock, 1998). Thus, social capital has been conceptualized as forms of social relations, or individual attributes, that facilitate collective action between members of the same community (Putnam et al., 2000; Ostrom and Ahn, 2009). Considering the coronavirus pandemic, it remains an open question to what extent may social capital counterbalance material hardships that are most common in developing countries' slums.

Despite social capital has become a popular concept in the social sciences (Wu, 2020), some critics argue that it has become a one size fits all construct, which lacks depth and precise definition (Cleaver, 2005). To argue against critics, some scholars have focused on analyzing particular aspects of social capital (Carpiano and Moore, 2020; Wu, 2020; Yip et al., 2007). In the Covid-19 studies, for instance, Bai et al. (2020) compare the impacts of civic norms and social networks. They find that while civic norms facilitate cooperation and

[4]Locomotiva Institute bulletin: https://www.slideshare.net/ILocomotiva/pandemia-na-favela?from_action=save

[5]see PNAD-COVID19 (IBGE-BRASIL, 2020): https://covid19.ibge.gov.br/pnad-covid/

self-sacrifice, social networks, on the other hand, increase inertia in maintaining social interactions, thus inducing less physical distancing. Comparison between U.S. counties provides evidence that places with high-density social networks perform worse in terms of compliance with distancing rules, whereas places with stronger individual commitment to civic norms perform better. Similarly, Ding et al. (2020) find that social distancing is larger in counties with historically less community engagement and in counties where people are more willing to incur individual costs to contribute to social objectives.

These two works that focus on the U.S., as well as many other studies on western democracies (Bai et al., 2020; Barrios et al., 2020; Bartscher et al., 2020; Borgonovi and Andrieu, 2020; Ding et al., 2020; Fraser and Aldrich, 2020; Kokubun, 2020; Kuchler et al., 2020; Miao et al., 2020; Varshney and Socher, 2020), find mixed relations between different types of social capital and compliance with preventive measures. However, even in authoritative regimes, nuanced patterns appear. Wu (2020) investigates the channels through which different features of social capital have influenced the outbreak of the Covid-19 epidemic in the Hubei province of China. He employs a multi-level approach and finds that social capital facilitates collective action and promotes public acceptance of control measures in the form of trust and norms at the individual level. More than that, and different from other studies, social capital can also help mobilize resources in the form of networks at the community level. Furthermore, social capital in authoritarian regimes works more through people's trust in their political institutions than on trust in each other. Within democracies, not only trust in authorities may be at stake (Blair et al., 2017; Tsai et al., 2020), but also the extent to which citizens comply with preventive measures may depend on partisan beliefs (Allcott et al., 2020). Thus, it is critical that new studies consider not only potential mechanisms of social capital but also how they play in different contexts (Carpiano and Moore, 2020; Wu, 2020; Yip et al., 2007).

TETO's social intervention in Brazilian slums contributes to these discussions because it combines a set of activities that promote particular types of social capital but leaves away others. Precisely, by engaging beneficiaries in a collective project, TETO promotes the use of well-established networks and the creation of new ones. It also enhances norms of trust and reciprocity between different benefited families and between them and community leaders. Nonetheless, TETO does not encourage civic norms neither trust in public authorities. The weekly meetings that beneficiaries must attend are designed to help them plan the activities, connect to each other, and to develop a sense of mutual trust and community engagement. Yet, there are no incentives to cultivate any sense of civic duty or responsibility towards society, at least as far as my research goes. Although social trust and community embeddedness may lead to a shared system of civic norms, they do so over a long period of time (Portes, 1998). Thus, in the short period between TETO's interventions and the outbreak of Covid-19, it is less likely that changes in civic norms appear. As Bai et al. (2020) point out, given the unprecedented nature of the crisis, normative expectations towards social distancing may not be well established yet.

Considering the possible types of social capital that TETO's program enhances between treated families, I developed a set of hypothesis to test the relationship between the intervention and how dwellers are collectively dealing with the pandemic. My first hypothesis relates to preventive behavior measures to slow the spread of the infections. It goes as follows:

H1: Families that were part of TETO's program are more willing to assume preventive behavior measures, such as adhering to social distancing and caring for basic hygiene practices.

In addition to coping with preventive measures, dwellers may find alternative ways of cooperating to fight the crisis. Solidarity between neighbors and members of the same community may be a path to alleviate the economic distress that has become more salient during the pandemic. In contexts of extreme vulnerability, solidarity probably plays an important role for individuals to be able to comply with social distancing practices while being able to provide for themselves. TETO's mission is only possible because the NGO relies on the sense of solidarity of its volunteers. It is possible that beneficiary dwellers reciprocate the solidarity received from volunteers by being more supportive to community members. I then formulate the following hypothesis:

H2: Dwellers who mobilized their networks in the process of building their own home (with TETO's support) will have higher levels of solidarity towards neighbors and members of the community.

On the other hand, the threat of economic recession and sanitary crisis increases the need for broader social security measures. Community members may not be able to provide for themselves, given their high vulnerability. Yet, governments in a context of low state capacity infrequently provide public goods to slum dwellers, let alone in a context of a pandemic that requires vast amounts of public funds to be channeled to health policies. The extent to which slum dwellers will be able to successfully claim aid and resources from governments, NGOs, and other non state providers probably lies in their ability to mobilize for preparedness as a community. In the house building process, TETO provides incentives for beneficiaries to rely on other slum dwellers in the hope of developing reciprocity, and connections among squatters. The social abilities learned from the intervention may help beneficiaries engage in claim-making activities. The third hypothesis goes as follows:

H3: Beneficiaries may have more skills in claim-making in periods of crisis.

Although I directly test the three hypothesis using the experimental trial from our partnership with TETO, ideally I would like to also test the mechanisms through which social capital operates. However, I can not neatly parse out the effects of each component of the intervention. For instance, what would be the effects of the quality improvement of the house and what would be the contribution of the social interactions taken along the meetings? Additionally, how could I know the role of networks vis-a-vis social trust? I rely on theory and on indirect evidence to address the following hypotheses regarding mechanisms.

The slums where TETO operates are places ripe for political agents who take advantage of the informality, weak enforcement of property rights, insecurity, and lack of government presence, to buy votes and dampen community organization (Cox, 1982; Gay, 2012). Mistrust in government is high, which compromises good citizen-state relations, leading to low public policy provision. Moreover, these are relatively new urban settlements where social ties have not been built yet. Because dwellers live in informality, they spend much of their time seeking better housing conditions according to job opportunities (Glaeser, 2011). It is not uncommon that settlers keep moving from one settlement to another. Furthermore, there is always a high risk of eviction. I identified that around 75% of sampled families have been living in the same slum for less than six years, while 50% of them do not even reach three years of living in the same slum. Hence, geographic mobility is probably high, which hinders bonds between neighbors and community members. TETO's program, unlike other housing policies, do not reallocate dwellers. Instead, it only upgrades homes for residents in their original land plot. Previous evidence suggests that reallocation contributes to breaking social ties and dismantling communal networks. For example, Gay (2012) finds that the relocation of households in the Moving to Opportunities Program reduced voter turnout among adults by breaking their ties to local communities. Barnhardt et al. (2017) reached similar conclusions from a housing lottery that reallocated slum dwellers to distant neighborhoods in the periphery of Ahmedabad, India. In this case, beneficiaries reported increased isolation from family and caste networks and reduced informal insurance.

Although there is evidence of homeownership increasing social capital and investment in local amenities (DiPasquale and Glaeser,1999) because homeowners have a financial stake in their neighborhoods, fewer studies focus on housing quality improvements. TETO does not provide ownership titles, so beneficiaries are not homeowners. However, I hypothesize that a shock to home quality also produces a higher commitment to communities through the mechanism of geographic mobility. Less mobility produces incentives for residents to invest in the community and improve social ties because they expect to stay longer in the community. Squatters who move frequently may show difficulty in connecting to other people and caring for their environment, thereby reducing their ability to mobilize collectively, make demands on behalf of their community and engage in political activity. Hence, reduced mobility may be a channel to increase beneficiaries' community embeddedness, thus social capital:

M1: higher quality homes reduce geographic mobility, which in turn increases social capital.

The other feature of TETO's intervention is that beneficiaries must participate in a month-long program in which they must collaborate with other residents and TETO's volunteers. In the process of building the house, squatters must engage neighbors to help volunteers prepare the logistics of the construction day and sometimes help to build the houses as well. Hence, I also hypothesize that being part of TETO's construction program could change residents' behavior towards reciprocity, trust, and social norms. Such social skills may arise because residents must trust that volunteers will deliver the new house on time, and they must additionally engage family members, friends, and neighbors to help with the construction. Furthermore, the program may induce reciprocity from beneficiaries towards their networks, volunteers, and ultimately towards TETO. All the collective effort made by beneficiaries and their networks during construction may function as a social learning. As some evidence suggest building social capital is an act of learning from experiences (Ostrom, 2000). Even though construction is a short-term program (one-month planning and two days building the houses), it requires a lot of effort to reach the desired outcome, and beneficiaries must use their social skills and networks in this collective action process. Therefore, the second mechanism would be as follows:

M2: the intervention induces social connections between beneficiaries and their communities through a hands-on program in which they need to rely on norms of trust, reciprocity, as well as engaging their networks.

Both mechanisms are difficult to test: the first one because TETO just recently delivered the housing units (less then one year) and not sufficient time has past to correctly state that non-beneficiaries indeed left the community; the second mechanism is difficult because the different types of social capital are acting at the same time and they may contribute to each other. To circumvent these challenges I rely on observational data and qualitative findings.

Intervention and experimental design

TETO provides a pre-fabricated wooden made units that can be build by volunteers in two days. More than 95% of the house cost is subsidised, and beneficiaries must pay around R$200,00 (US$40,00 in current exchange rates), which correspond to 20% of a minimum wage in Brazil. TETO's houses provide isolation against changing temperature and windows to provide ventilation. Better structured foundations prevent the buildings from flooding or falling apart when tropical storms hit *favelas*. These basic characteristics represent an improvement as substandard housing units in the comparison group are mostly made out of paper/plastic and some do not have any window. However, the housing units are small-sized (18 square meters), have just one room (families improvise a wall to create a living room and a bedroom), are not connected to basic sanitation infrastructure, and overcrowding is still verified. Therefore, housing quality is limited to an improvement in the structure of walls, roof, floor and foundation. In summary, it is a safer house when it comes to protecting families to common tropical natural disasters (stroms and floods), but it does not provide the most basic sanitary infrastructure. It is also more comfortable. Figures 1 and 2 show typical housing units in the comparison and treatment groups, respectively.

Figure 1: Comparison Unit

Figure 2: Treatment unit

At least two months before construction day, volunteers visit communities to pre-select eligible families to take part in the program. They look for households living in extreme poverty and precarious housing conditions. After conducting a baseline survey, volunteers jointly decide with community leaders which are the families that will be able to participate in the program. The program starts one month before construction day. Every weekend volunteers visit the community and gather elected families to discuss the construction plans. But meetings are also aimed at developing beneficiaries' social skills. They must work with each other in the workshops and jointly decide the logistics of the event. They also need to engage their networks in all those efforts, which sometimes means self-sacrificing for the group. For instance, someone needs to give up space in his or her terrain to store the houses' wooden boards, while others need to cook for the volunteers during construction day. The night before the massive event, families need to destroy their previous homes, and if they need help they must ask their networks because volunteers only come on the next day. This also implies that they need to find a trustworthy place to sleep along the weekend. In sum, the program encourages a series of skills, such as trusting each other, accessing networks, bearing costs, reciprocating, and coordinating.

To test the effects of the intervention, I implemented a trial with 611 families (from March 2019 until February 2020) in 25 slums in Brazil — 220 families have been initially randomly assigned to the treatment group.[6] In each community, TETO volunteers selected the group of families eligible to participate in the lotteries according to a vulnerability index. Some families lived in such extreme conditions that TETO decided to provide them with the house before I conduct the lottery. The remaining families entered the lottery, and my research team had full control over the randomization process. I conducted a block randomization within each community, but I was unable to perform any further stratification.[7] To be precise, for each block

[6]IRB exemptions and approvals were acquired at Emory University and Fundação Getulio Vargas.

[7]Part of the assigned to treatment subjects never gets to receive the house, either because they refuse to agree to TETO's conditions to build a new home or the land plot is not suitable for construction.

(community), I created randomized waitlists. Implementers go down the waitlist to offer the remaining houses to the next applicants until all units are allocated.

Baseline interviews were conducted by volunteers when they visited communities in search of eligible households that met minimum requirements of house infrastructure and poverty. I merged our baseline questionnaire with TETO's previous eligibility questionnaire, which was used to decide the priority families. To guarantee the quality of data collection I personally engaged volunteers to special training sections, where I introduced interview techniques and ethical protocols. Volunteers are usually well educated undergrad students that quickly assimilate knowledge. For follow-up interviews my team hired a specialized company that performed all interviews by phone calls, coping with security measures and ethical standards. The follow-up phase was conducted from May 22th to July 14th. From April to August Brazil faced the peak of the crisis in terms of total deaths per day, cases, and self-isolation measures, all of which influence directly our interpretations of the data.

Model specifications

I conducted new lotteries for each wave of construction within communities. An important concern is that the number of eligible families, as well as the number of houses to be delivered, varies for each construction, which causes differential probabilities of assignment. Our model specifications rely on varying solutions to account for these different probabilities, and the most straight forward is to control for fixed effects. I simply add to our models community-lottery dummies and let the regressions weight the differential probabilities. However, if treatment effects are not the same across blocks, simply controlling for fixed-effects may bias our estimates because the OLS regression will end up using the wrong weights (it will put more weight on the blocks with greater variance in the treatment). I call this first model as the pre-specified model. I correct the weights following three well known approaches from the literature: I re-weight our regressions using Inverse Probability Weights; I interact fixed effect dummies with treatment assignment as proposed by Lin (2013); and I use Gibbons et al. (2018) "Interaction-weighted estimator" (IWE) and "Regression-weighted estimator" (RWE) estimators. I present results for the pre-specified model, as well as Lin (2013) and Gibbons et al. (2018) estimators - all including pre-treatment control variables. The other specifications I leave to the appendix. I next present the three models' specification and how they deal with block randomization problems.

The first model is the pre-specified fixed-effects:

$$y_{ij} = \alpha + \rho Treat_{ij} + \beta X_{ij} + \gamma_j + \varepsilon_{ij} \quad (1) \quad OLS$$

where y_{ij} is the outcome for individual or household i participating in lottery-settlement j. $Treat_{ij}$ is the randomization assignment dummy, and the parameter of interest is ρ, the impact of the assignment to the intervention. I also include pre-treatment characteristics (X) measured at baseline, γ_j is a community (slum) fixed effect and ε is the idiosyncratic error term. Although the pre-specified fixed-effect model can produce biased estimates, I included it for comparison purposes.

Another way to produce unbiased estimates is to run OLS fixed-effects interacting them with the treatment assignment variable (Lin 2013). We then have our third specification:

$$y_{ij} = \alpha + \rho Treat_{ij} + \phi Treat_{ij}\, \gamma_j + \beta X_{ij} + \gamma_j + \varepsilon_{ij} \qquad (3)$$

The intuition of the interaction approach is that it allows for different treatment effects across blocks, since the interaction produce slopes for each group. The global average treatment effect (ATE) across all groups will be an implicit weighted average of every ATE within each block.

Gibbons et al. (2018) have also proposed an estimator using interactions, the IWE estimator. Estimates in this third specification will be the same as Lin's, but standard errors are slightly different. When controlling for pre-treatment covariates, estimates may be slightly different.

Sample adjustments

I build the sample by aggregating waitlists from lotteries of different construction moments and places. Most communities received only one construction task force along the research period (from March 2019 to February 2020), that is, only one lottery. However, there were some communities where TETO decided to build houses twice, leaving room for a second lottery after a couple of months. As we should not prevent losers from the first lottery to take part in the second, we pooled them in the new lottery with dwellers that had recently moved to the settlement. Consequently, some households had a greater chance of being treated within the same community. If we pool all households of two different lotteries within a given community, we would wrongly attribute them equal weights. I circumvent this issue by using a community-lottery unit of analysis. In other words, I duplicate households (or individuals) that appear in two different lotteries and use fixed effects not at the community level, but in the community-lottery level. Communities in which TETO performed two lotteries appear twice in our data set as if they were distinct communities.

Although most lotteries went on well and TETO's volunteers succeeded with the task of delivering houses according to the waitlist, the take-up rate of the intervention was surprisingly high. In many cases the land plot would not fit TETO's house or the terrain was very steep [8]. Volunteers almost always guaranteed that the waitlist was respected, but there were minor cases in which we had always takers brought to the top of the list.

[8] Other examples of non-compliance include individuals in discordance with the rules of the program, families moving out of the community or giving up from receiving the intervention, people going to jail and death.

I used an audited national brazillian lottery to randomize our waitlists. In our protocols, first I would receive the list of eligible families from TETO and then I would assign random ticket numbers to each family. I then sort the tickets and use the audited national lottery number as a cutoff point from where we started delivering the houses. All but two lotteries happened according to plan. I include them in the analysis by using inverse probability reweighing, but I also run the same regressions excluding these lotteries [9]. All results remain similar.

Choosing between estimators

Historically, in the econometric methods literature, there are two mostly used estimators for designs that rely on waitlists: the Initial Offer (IO) estimator and the Ever Over estimator (EO). Both are extensively used in different contexts of field experiments and other experimental studies [10]. Some studies choose one or another (Abdulkadiroğlu et al. 2013, West et al. 2016), and some even combine both estimators as instruments for LATE or CATE estimates (Angrist et al. 2013).The IO estimator takes into consideration only the first round of offers to treatment in a particular list of eligible subjects. In this sense, for Intention-to-treat or Complier-Average-Treatment-Effects estimations it ignores subsequent offerings, once the program has one or more subjects that did not comply with the initial offer. Using TETO's experiment as an example: when an individual or family refuses to be treated, then the housing unit should go to the next household on the waitlist. In this case, the IO estimator will not consider the new offer as an assignment to treatment, but rather a control unit. The EO estimator, on the contrary, will take into consideration all households (or individuals) that have received offerings, regardless of which round the offer was made. I opted to use only the IO estimator because, as De Chaisemartin and Behaghel (2020) shows, the EO estimator is inconsistent. I discuss this decision better in the appendix.

Experimental group balance

To attest the validity of the lottery I check whether pre-treatment variables are well balanced between our treatment assignment status (using IO estimator). If the randomization works, there should be no correlation between pre-treatment variables and our dummy for treatment. I run the fixed-effect model with interactions using the following specification:

$$X_{ij} = \alpha + \rho Treat_{ij} + \gamma_j + \varepsilon_{ij}$$

Where X_{ij} are pre-treatment variables at the household or individual level, and $Treat_{ij}$ is treatment assignment status.

[9] More on that in the appendix

[10] see de Chaisemartin and Behaghel (2019) for an extensive list

As can be seen in Table 1, most variables are well balanced at the household level. That is to say that in most cases I fail to reject the null hypothesis that treatment status and pre-treatment variables are in fact correlated. Unbalance was only noticed in one variable at the 5% level and another two at the 10% level. At the individual level, Table 2 shows only one variable unbalanced at the 10% level of significance. Taken together, I have 4 variables out of 38 that are unbalanced, slightly above the 10% threshold in which it would be safe to say that unbalance was due to chance. However, when I perform a conjoint F-test of a regression where I run treatment assignment status over all pre-treatment variables, coefficients are not jointly different from zero (p-values are 0.32 and 0.4 for household and individual variables, respectively). When I do the same analyses to the EO estimator, in many cases it fails, giving one extra reason not to use it [11].

Results

I test each hypothesis by creating an index that aggregates different questions into one summary outcome. Thus, I have an index that synthesizes the family of outcomes for each hypothesis. As Kling et al. (2007) proposes, I create the indexes by first subtracting the mean value of the control group from each variable in my family of outcomes, then dividing by the control group standard deviation. Finally, I sum up the standardized outcomes into one summary index, being careful to orient the sign of each outcome so that it reflects higher scores whenever the variable measures positive benefits of the intervention.

An advantage of using a summary index, as well as not so many outcomes, is that it minimize multiple-hypothesis testing penalties. For the index, I do not need to add any penalties on p-values because it represents only one given hypothesis. However, when looking separately to each outcome that composes the index, one should account for the probability of rejecting any null hypothesis just by chance. I follow Benjamini and Hochberg (1995) for multiple-hypothesis corrections, but I do not report adjusted p-values in my tables because in almost all cases the penalty is too high when I consider the sample size, making most estimates imprecise [12]. Unfortunately, the partner organization was unable to provide as many houses as we first planned, which raises power concerns not so easy to do deal with. The risk of being exceedingly rigorous with multiple-hypothesis corrections is to incur in type II error. On the other hand, I rigorously cope with the registered Pre-Analysis Plan, which makes me confident in not reporting adjusted p-values, as hypotheses were well defined and justified [13].

The following set of tables report estimates for the three specifications presented in the methods section for the summary index and its underlying outcomes. Given that all variables were standardized, coefficients must be interpreted as percentages of standard deviations from the control group mean (Kling et al. 2007). In all tables Model 1 holds for the pre-specified fixed-effects specification, Model 2 is Lin's specification, and Model

[11] more information in the Appendix section

[12] I report the adjusted p-values in the appendix tables

[13] The Pre-Analysis Plan can be found in the Center for Open Science's OSF Registries. https://osf.io/registries

3 follows Gibbons and coauthors.

I begin by showing results for the preventive behavior hypothesis (H1). The first row of Table 3 reports the estimated coefficients for the three intention-to-treat specifications of the Preventive Behavior Index. Bellow the coefficients, I report estimated standard errors and p-values, respectively. The subsequent rows report results for the outcome variables that compose the index. In Model 1, we should interpret the impact of TETO's program on the Preventive Behavior Index as being 0.129 standard deviations above the control group average. Model 1 and 2 have only slightly bigger coefficients. Although all the index coefficient are positive, they are not statistically significant, meaning that our main test fails to confirm any impacts of the program on preventive behavior measures. Looking at the individual variables that make up the index, it is only possible to verify positive impacts in two of them. The outcome "soft social distance measures" is a dummy variable that assumes value one if the interviewed family member mentions that he or she avoids being close to other people or agglomerations. Treated beneficiaries comply with such measures from 0.235 to 0.238 standard deviations more than the average of the control group. When it comes to washing hands, beneficiaries also seem to comply more (from 0.145 to 0.171 standard deviations). However, TETO's program fails to change behavior towards self-quarantine measures, as either control and treated households stay the same amount of time at home and leave it when needed at the same proportion. They are also not different in terms of cleaning measures or usage of masks to avoid contamination. Therefore, if the program has any effects at all on dwellers preventive behavior, they are only marginal. I discuss theoretical interpretations of the results in the next sections.

The Solidarity index not only combines two measures of how dwellers are dealing with the economic distress of the pandemic, but also six measures of who they would count on if they get ill. We ask respondents: "to what extent they can count on family, friends, community members, church members, community leaders, and alderman if they suffer health issues from Covid-19". We also ask if they have provided any form of aid or help to other needed families, and if they have received any type of aid. The overall Solidarity index of Table 4 does not confirm the hypothesis that the program would increase solidarity from benefited families. Although coefficient are positive, p-values are too high, leading me to conclude that the effects are null. Most individual outcomes are also null, except for two measures. Dwellers who received the program seem to rely more on friends and members of the church in the eventuality of being sick with Covid-19. In both outcomes, coefficients are higher than 0.2 standard deviations and significant. I interpret these results the following way: although TETO does not increase solidarity among beneficiaries, it increases their bonds with the groups of people that are already close to them (excluding family members). This makes sense since the networks that they try to engage during the construction process are usually made of close friends or members of institutionalized groups, such as churches. Because family members are the most obvious answer when it comes to informal social security measures, I find no difference between treated and untreated households.

Of all the stated hypotheses, the Claim-making is the one that presents the most unquestionable results.

Either the aggregated index and the individual measures have null effects. We asked households whether they claimed aid from government, community leaders, and NGOs. None of these measures are different between beneficiaries and control units. For these variables, Table 5 in fact reports coefficients that are very close to zero standard deviation in all three models. In terms of actually receiving aid, coefficients are positive for help that comes from NGOs and for the federal coronavirus voucher, but effects are statistically null. Coefficients are negative but irrelevant for aid that comes from other tiers of government. Thus, the only possible conclusion is that TETO does not induce any skills related to claim-making activities. However, a possible caveat from our formulated hypothesis is that claim-making may be an activity that only community leaders or local brokers play. Individual claims may not be taken seriously by authorities, who would only listen to well organized leadership. Yet, more studies are needed to confirm this assumption.

Overall, I find marginal to null effects of the program on the main hypotheses related to how dwellers behave regarding the pandemic. Preventive behavior and solidarity are mostly null results, while claim-making is definitely null. Nevertheless, I further tested how beneficiaries evaluate key actors of the Covid-19 crisis within *favelas*. TETO and other NGOs have promoted humanitarian campaigns to raise funds for the most vulnerable. Community leaders have also attempted to help in many ways, assuming more responsibilities and working closely to governments and NGOs. Many tiers of government have also tried different initiatives, that were more or less successful. Although these three types of key actors have different roles within communities, together they have the capacity of establishing formal and informal social security measures that could save lives. Thus, I also tested how the intervention influences the evaluation of these actors by community members. We asked how dwellers evaluate them with respect to their actions towards the pandemic. Table 6 reports strong positive effects for all variables as well as the aggregated index. All coefficients are significant at the conventional levels. It is interesting that beneficiaries not only evaluate TETO better but also leaders and governments. I did expect a large positive evaluation of TETO, since it is natural that beneficiaries reciprocate to the NGO that helped them in their most basic needs. It is also somewhat expected a good evaluation of community leaders because they are the ones who directly deal with TETO, and they are key for the success of the program. However, it surprises how beneficiaries evaluate the government better than non-beneficiaries, even in the absence of any extra favors or policies, as the previous hypotheses show. This raises a discussion on how TETO may affect government accountability. I further discuss these results in light of the analysis of mechanisms on the next section.

Mechanisms

According to my theorizing, TETO could only change settlers' behaviors towards the pandemic because both aspects of the program would foment higher levels of social capital within participants. By receiving a higher quality home, beneficiaries would settle down and start creating community bonds. In this rationale, they would have incentives to establish roots within the community since they would no longer envision themselves living in other places. Additionally, selected dwellers would learn and practice social skills while collectively working with TETO to achieve the goal of building the houses. Through these two channels we should see an increase in trust relationships, reciprocity and access to networks. In this section, I directly test the mobility mechanism and a measure of trust. I further use observational data to show that beneficiaries are increasing their networks within and beyond communities. I then discuss implications to the social capital theory.

Regarding the mobility mechanism, the follow-up questionnaire asks if dwellers are still living in the same community as they did when we first interviewed them at the baseline. The follow-up interviews were made six to twelve months after the baseline, which is a short period of time for people to find new homes. Nevertheless, around 10% of the sampled families had already moved out. Table 7 reports results of the ITT estimates for the geographic mobility outcome. This is a dummy variable that assumes number one if the family still lives in the same place (and zero otherwise). Positive coefficients indicate that treated families are more likely to stay in the same community than control families. Models one and two of Table 7 have statistically positive coefficients at the 10% level, which confirm the mobility hypothesis. The impact is also relevant in terms of standard deviations from control group average (0.16 to 0.15 sds.). From my qualitative fieldwork after visiting many precarious slums and talking to volunteers and families, I expect that in a longer period, this difference between those who received the home and those who did not will be even larger.

To further measure how TETO increases social capital, I ask to what extent dwellers trust the information that they receive about the Covid-19 crisis from community leaders and government agencies. After analysing a series of interviews that TETO has done with leaders, I was convinced that they played a key role in orienting how families should behave during the pandemic. They directly assumed responsibility in communicating preventive procedures, although they were not always aware of the best practices. Dwellers would also receive government information through television, radio and internet. On Table 8 trust in community leader's information seems to be higher within treated families, while trust in government is no different between treatment and control. The statistical relevance is so strong in leaders' variable that the trust index is also relevant at the 10% level of significance. Probably, the norms of trust that TETO enhances between dwellers get reflected in their trust in community leaders. That is, families who took part in TETO's program recognize the importance of leaders to make the construction viable, and they keep trusting leaders even in different contexts and six to twelve months later. Observational data confirms that the program indeed creates bonds and networks. In a parallel survey, we ask dwellers who received the house a series of questions regarding their experiences in the construction process. For instance, we ask if they got to know more people within the community and if they still keep in touch with these people. More than 94% responded that they did meet new people. Around 36% mentioned that they met new neighbors and 34.7% that they further connected to community members that they knew but that they were not close. 69% of the surveyed dwellers still keeps frequently in touch with these new or closer relationships, while 24% have moderate connections. When it comes to connecting to community leaders, the answers are similar. 36% mentioned the they only got to know leaders during construction, and 41% answered that construction led them to know leaders better, even though they already met leaders. 47% mentioned that still keeps in touch with leaders frequently, 30% talk to leaders moderately, and only 22% never communicate with leaders. Hence, TETO was successful in its effort to increase connections and networks.

We also asked what beneficiaries could remember from their meetings with TETO's volunteers. What were the main topics of the conversations [14]? Around 35% of the sampled dwellers mentioned topics such as how to organize the intervention and how to actually build the pre-fabricated houses. It is no surprise that after six to twelve months these were the most well remembered issues, since they were indeed the main topics. However, about 20% of the interviewees remembered topics such as how to work with neighbors and how to ask help and engage others. Topics such as community life and engagement as well as family history were also mentioned by 20% of the sample. These observational data contribute to confirm the assumption that TETO increases cooperation between participants at least to what concerns the intervention.

Operating mechanisms are crucial for how social capital impacts the outcomes related to preventive behavior and compliance with government policies. If the intervention fails to increase social capital among participants, then there is no reason to believe in any changes in behavior, attitudes, or beliefs. Yet, results suggest that TETO successfully increases at least two types of social capital: networks and social trust. The mobility reduction promotes incentives for families to invest in social relations within the community, while the intervention itself is a good opportunity to strengthen networks. Treated families end up more embedded in community life through both theorized mechanisms. On the other hand, I find no evidence that TETO could influence civic norms towards the pandemic, neither public trust in authorities. This could partly explain why the main hypotheses fail.

Discussion and Conclusion

Social capital may be central to increase compliance with government directives or mitigating the fallout during pandemics or natural disasters. In partnership with the NGO TETO, I test to what extent this view stands. We jointly formulated an experimental research design, in which we randomly selected poor families to participate in a month-long program that culminates in the building of wooden-made higher-quality houses. During the program families would engage their networks and work closely with other community members and volunteers to plan and execute the construction. The intervention supposedly increased participants' social skills, particularly social trust, bonds, and reciprocity, as well as helped to foment new and old networks. Results confirm that TETO indeed creates these types of social capital through the mechanisms of reduced geographic mobility and the hands-on experience of collective action that the program offers. However, the increase in social capital does not translate into higher preventive behavior, solidarity, or claim-making among benefited families. The intention-to-treat estimates are only statistically positive for a few measures

[14] This was a multiple choice question.

of preventive behavior and solidarity, but null in all other measures (including claim-making).

Although TETO does contribute to form bonds and trust between neighbors, the types of norms that were created do not necessarily produce any sense of civic duty. To understand why the intervention fails to change dwellers' behavior and attitudes, one should recognize that civic norms are conceptually different from the social trust and networks that TETO foments. A group's civic norms include a set of beliefs, informal rules, and sanctions that moderate individual sacrifice for the common good (Coleman, 1988). By one mean or another, the individual feels pressured to comply with the group's expected behavior. Although social trust and broadened networks may also facilitate cooperation, they do so by different channels. At the individual level, social trust (or trust in pairs) facilitates compromise between two or more agents into achieving better outcomes (think of a prisoners' dilemma game). Better connected and developed networks act similarly, as they allow repeated interaction that reduces opportunistic behavior (Dasgupta, 2000). Given these different mechanisms, norms of social trust, as well as improved networks, may facilitate cooperation only in certain contexts, while civic norms would work in others. In Brazilian slums, it may be the case that dwellers have not yet created particular civic norms on how to deal with the pandemic outbreak and how to sanction deviant behaviors (Bai et al., 2020). In this sense, TETO's intervention provides little contribution, as it never intended to focus on pandemic collective action problems, and it does not address how to create the civic norms that some scholars believe to be fundamental (Bai et al., 2020; Ding et al., 2020).

Another possible reason why TETO's intervention fails to increase preventive behavior and claim-making is that public trust has not changed. Although trust in community leaders has increased, the same did not happen with trust in authorities. In times of crisis, trust in government has been viewed as a key factor for citizen's adherence to public health policies (Blair et al., 2017; Tsai et al., 2020). Because leaders lack sanctioning powers and can only suggest that community members take precautions, actual effects are basically null. From our interviews with community leaders, it is clear that most of them were poorly informed on how to deal with the pandemic. Thus, even being more trustworthy, they could not convince close neighbors of the importance of keeping distance and being at home. To make things worse, public information about the pandemic has been chaotic in Brazil since the outbreak of the crisis, and authorities never agree on which measures to recommend (Barberia and Gómez, 2020) - Brazil is also among the worst countries in the world in terms of spreading fake news related to Covid-19 (Biancovilli and Jurberg, 2020). The combination of low public trust, fake news, and conflicting guidelines between different tiers of government hinders community level coordination, even among those most likely to reciprocate for the common good. Trust in community leaders would not be sufficient to counterbalance general mistrust in government. On the other hand, families that received the house have a better evaluation on how leaders and governments are dealing with the crisis. Hence, the partnership that TETO maintains with community leaders could be a channel to improve government accountability or other forms of institutional governance.

Finally, there is still an alternative explanation for the null results. While some works support that social capital is crucial for understanding social behavior, others are less optimistic (Portes and Landolt, 1996; Cleaver, 2005). In fact, social capital could even have negative consequences (Portes and Landolt, 1996; Woolcock, 1998). In the context of the Covid-19 pandemic, Brazilian slums are facing extreme poverty conditions, and it may be the case that dwellers lack sufficient resources to cope with the required health measures. From this perspective, all the previously social capital that TETO's beneficiaries accumulated would not be enough to substantially changing their behavior. It is such an emergency situation that slightly higher levels of social capital would not make any difference. In other contexts, as in well-developed countries, social capital would indeed be relevant.

The present work contributes to a growing literature that attempts to measure how social capital may affect citizen's adherence to health and social measures to fight the outbreak of Covid-19. Although I find evidence of increased social capital among participants of TETO's program, results indicate only marginal to null impacts on prevention, solidarity and claim-making. Thus, I question the notion that social capital will necessarily promote collective action to fight Covid-19. I advocate a nuanced view, which considers that impacts may depend on the context and on the types of social capital that are more salient at the moment.

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Tables

Table 1 - T-test for each pre-treatment variable - Household level

Reg Lin - Only Comunity FE	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	mean
Household size	0.06	0.16	0.34	0.73	510	22	2.61
Average age of family	0.49	1.44	0.34	0.73	510	22	32.00
Dummy social security pension	-0.03	0.05	-0.74	0.46	510	22	0.63
Dummy Employment	-0.02	0.05	-0.37	0.71	510	22	0.59
Index Health Problem	0.04	0.06	0.70	0.49	510	22	0.48
Long lasting diseases index	0.01	0.03	0.35	0.73	510	22	0.31
Sanitary diseases index	0.00	0.04	0.05	0.96	510	22	0.28
Respiratory diseases index	0.00	0.03	-0.08	0.93	510	22	0.25
Disabilities index	-0.01	0.02	-0.55	0.58	510	22	0.10
Hospital visits index	-0.05	0.04	-1.38	0.17	510	22	0.56
Leader visit	0.08	0.04	1.96	0.05	510	22	0.35
Education level index	0.02	0.09	0.19	0.85	510	22	2.16
Year arrived community	0.28	0.34	0.83	0.41	510	22	2014.79
Intention to leave community	-0.18	0.15	-1.18	0.24	510	22	4.28
Location previous home	0.13	0.06	2.13	0.03	510	22	3.07
Ever evicted	0.01	0.03	0.30	0.77	510	22	0.10
Dummy Infrastructure problems	-0.02	0.05	-0.40	0.69	510	22	0.41
House quality index (roof)	-0.14	0.07	-1.96	0.05	510	22	2.86
House quality index (wall)	-0.11	0.07	-1.54	0.12	510	22	2.76
House quality index (floor)	0.03	0.07	0.39	0.69	510	22	3.01
TETO Vulnerability Index (Pontos)	0.02	0.21	0.11	0.91	510	22	6.70

Table 2 - T-test for each pre-treatment variable - Individual level

Reg Lin - Only Comunity FE	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	mean
Gender 1st respondent	0.08	0.05	1.75	0.08	510	22	1.38
Age 1st respondent	0.60	1.38	0.44	0.66	510	22	38.64
Race 1st respondent	0.13	0.09	1.51	0.13	510	22	3.97
Dummy Employment 1st respondent	0.04	0.05	0.79	0.43	510	22	0.42
Dummy Health Problem 1st	0.10	0.08	1.22	0.22	510	22	0.54
Dummy long lasting diseases 1st	0.04	0.05	0.94	0.35	510	22	0.38
Dummy sanitary disiases 1st	0.01	0.04	0.25	0.81	510	22	0.30
Dummy respiratory diseases 1st	0.01	0.04	0.28	0.78	510	22	0.28
Dummy disability 1st	0.00	0.03	-0.02	0.98	510	22	0.90
Hospital visits 1st	-0.05	0.05	-1.02	0.31	510	22	0.62
Financial Expexctation	0.06	0.04	1.28	0.20	510	22	0.75
Dream housing	-0.01	0.04	-0.20	0.85	510	22	1.80
Institutional afiliation	-0.01	0.03	-0.31	0.76	510	22	0.75
Authority claim-making	0.00	0.04	0.11	0.91	510	22	0.20
Leader claim-making	0.06	0.04	1.57	0.12	510	22	0.22
Elections 2018	0.00	0.03	-0.06	0.95	510	22	0.13
Elections 2016	0.02	0.03	0.83	0.41	510	22	0.12

Table 3: Intention to Treat estimates - Preventive Behavior

Dependent Variable	Follow up Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Preventive Behavior Index	-0.253	0.129	0.154	0.143
	[0.292]	(0.122)	(0.124)	(0.122)
		(0.288)	(0.218)	(0.242)
Number of times leaving home (scale)	-1.409	0.031	-0.001	0.005
	[1.086]	(0.120)	(0.118)	(0.116)
		(0.798)	(0.993)	(0.966)
Number of reasons to leave home (scale)	-1.497	0.106	0.135	0.124
	[0.529]	(0.114)	(0.116)	(0.117)
		(0.353)	(0.247)	(0.287)
Hours out of home (scale)	-2.49	0.116	0.102	0.097
	[1.595]	(0.119)	(0.116)	(0.114)
		(0.330)	(0.377)	(0.397)
Soft social distance measures (dummy)	0.119	0.235	0.238	0.235
	[0.325]	(0.129)*	(0.134)*	(0.132)*
		(0.069)	(0.075)	(0.074)
Sanitary/cleaning measures (scale)	2.084	-0.066	-0.037	-0.040
	[1.049]	(0.108)	(0.097)	(0.097)
		(0.541)	(0.701)	(0.684)
Washing hands (dummy)	0.843	0.145	0.159	0.171
	[0.365]	(0.098)	(0.093)*	(0.093)*
		(0.140)	(0.087)	(0.066)
Using mask (scale)	-1.089	-0.090	-0.042	-0.049
	[0.3]	(0.104)	(0.099)	(0.101)
		(0.385)	(0.670)	(0.631)
Plan on leaving home next week (dummy)	0.267	-0.092	-0.080	-0.104
	[0.412]	(0.115)	(0.110)	(0.108)
		(0.428)	(0.465)	(0.334)
Stay more ate home (dummy)	0.894	0.024	0.013	0.013
	[0.309]	(0.121)	(0.118)	(0.117)
		(0.840)	(0.912)	(0.912)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coeffcient, robust standard error, and p-value, in that order. Robust standard errors, clustered at community level: ***p<0.01, **p<0.05, *p<0.1. Note: Specification included pre-treatment control variables for community leader visit, quality of the house roof, and household index for respiratory diseases. Model 1: Pre-specified Fixed Effects model; Model 2: Fixed Effects interacted with treatment assignment Lin (2013); Model 3: Interaction-weighted estimator Gibbons et al. (2018). Outcome variables have been standardized according to Kling et al. (2007). Fixed-Effects are lottery dummies.

Table 4: Intention to Treat estimates - Solidarity

Dependent Variable	Follow up Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Solidarity index	-1.131	0.170	0.155	0.159
	[0.407]	(0.110)	(0.119)	(0.118)
		(0.122)	(0.193)	(0.177)
If ill, can count on family	-1.714	0.032	0.017	0.047
	[0.788]	(0.115)	(0.111)	(0.110)
		(0.777)	(0.879)	(0.667)
If ill, can count on friends	-2.261	0.259	0.204	0.217
	[0.729]	(0.106)**	(0.111)*	(0.112)*
		(0.015)	(0.065)	(0.053)
If ill, can count on community members	-2.009	0.105	0.097	0.095
	[0.698]	(0.111)	(0.122)	(0.121)
		(0.347)	(0.427)	(0.432)
If ill, can count on church members	-1.887	0.237	0.270	0.267
	[0.685]	(0.106)**	(0.103)***	(0.103)***
		(0.027)	(0.010)	(0.010)
If ill, can count on community leaders	-1.941	0.137	0.102	0.101
	[0.752]	(0.117)	(0.116)	(0.116)
		(0.242)	(0.379)	(0.386)
If ill, can count on alderman	-2.697	-0.110	-0.073	-0.072
	[0.567]	(0.116)	(0.116)	(0.114)
		(0.345)	(0.529)	(0.530)
Provided aid to other families	1.791	0.086	0.108	0.092
	[1.242]	(0.116)	(0.123)	(0.127)
		(0.456)	(0.384)	(0.469)
Received aid from other families	1.668	-0.084	-0.120	-0.126
	[1.22]	(0.096)	(0.091)	(0.093)
		(0.380)	(0.190)	(0.175)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coeffifcient, robust standard error, and p-value, in that order. Robust standard errors, clustered at community level: ***p<0.01, **p<0.05, *p<0.1. Note: Specification included pre-treatment control variables for community leader visit, quality of the house roof, and household index for respiratory diseases. Model 1: Pre-specified Fixed Effects model; Model 2: Fixed Effects interacted with treatment assignment Lin (2013); Model 3: Interaction-weighted estimator Gibbons et al. (2018). Outcome variables have been standardized according to Kling et al. (2007). Fixed-Effects are lottery dummies.

Table 5: Intention to Treat estimates - Claim making

Dependent Variable	Follow up Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Claim-making index	-1.126	-0.014	-0.015	-0.016
	[0.18]	(0.104)	(0.105)	(0.104)
		(0.895)	(0.882)	(0.877)
Claimed aid from government	-1.936	-0.002	-0.011	-0.031
	[0.245]	(0.117)	(0.108)	(0.100)
		(0.986)	(0.921)	(0.759)
Claimed aid from community leader	-1.798	-0.037	-0.067	-0.071
	[0.4]	(0.107)	(0.105)	(0.107)
		(0.728)	(0.522)	(0.505)
Claimed aid from NGO	-1.781	-0.015	-0.054	-0.048
	[0.413]	(0.103)	(0.097)	(0.096)
		(0.888)	(0.581)	(0.619)
Help to receive corona voucher	-1.615	0.101	0.160	0.155
	[0.434]	(0.115)	(0.116)	(0.116)
		(0.380)	(0.169)	(0.179)
Received aid from NGO	-1.443	0.058	0.019	0.036
	[0.493]	(0.108)	(0.109)	(0.109)
		(0.591)	(0.859)	(0.741)
Received other government aid	1.816	-0.142	-0.089	-0.085
	[0.387]	(0.115)	(0.111)	(0.110)
		(0.216)	(0.426)	(0.439)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefifcient, robust standard error, and p-value, in that order. Robust standard errors, clustered at community level: ***p<0.01, **p<0.05, *p<0.1. Note: Specification included pre-treatment control variables for community leader visit, quality of the house roof, and household index for respiratory diseases. Model 1: Pre-specified Fixed Effects model; Model 2: Fixed Effects interacted with treatment assignment Lin (2013); Model 3: Interaction-weighted estimator Gibbons et al. (2018). Outcome variables have been standardized according to Kling et al. (2007). Fixed-Effects are lottery dummies.

Table 6: Intention to Treat estimates - Evaluation of key actor

Dependent Variable	Follow up Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Evaluation index	-1.747	0.444	0.455	0.448
	[0.694]	(0.098)***	(0.101)***	(0.099)***
		(0)	(0)	(0)
How evaluate leaders	-2.758	0.217	0.213	0.192
	[1.686]	(0.100)**	(0.108)**	(0.107)*
		(0.030)	(0.049)	(0.073)
Knowledge about TETO	0.902	0.169	0.181	0.177
	[0.298]	(0.092)*	(0.100)*	(0.102)*
		(0.069)	(0.071)	(0.082)
How evaluate TETO	-1.569	0.288	0.313	0.313
	[1.036]	(0.093)***	(0.088)***	(0.086)***
		(0.002)	(0.000)	(0.000)
How evaluate Government	-3.564	0.221	0.208	0.219
	[1.65]	(0.112)**	(0.110)*	(0.111)**
		(0.049)	(0.060)	(0.047)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefifcient, robust standard error, and p-value, in that order. Robust standard errors, clustered at community level: ***p<0.01, **p<0.05, *p<0.1. Note: Specification included pre-treatment control variables for community leader visit, quality of the house roof, and household index for respiratory diseases. Model 1: Pre-specified Fixed Effects model; Model 2: Fixed Effects interacted with treatment assignment Lin (2013); Model 3: Interaction-weighted estimator Gibbons et al. (2018). Outcome variables have been standardized according to Kling et al. (2007). Fixed-Effects are lottery dummies.

Table 7: Intention to Treat estimates - Mobility mechanism

Dependent Variable	Follow up Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Geographic Mobility	0.906	0.110	0.160	0.151
	[0.292]	(0.100)	(0.087)*	(0.090)*
		(0.272)	(0.068)	(0.094)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefifcient, robust standard error, and p-value, in that order. Robust standard errors, clustered at community level: ***p<0.01, **p<0.05, *p<0.1. Note: Specification included pre-treatment control variables for community leader visit, quality of the house roof, and household index for respiratory diseases. Model 1: Pre-specified Fixed Effects model; Model 2: Fixed Effects interacted with treatment assignment Lin (2013); Model 3: Interaction-weighted estimator Gibbons et al. (2018). Outcome variables have been standardized according to Kling et al. (2007). Fixed-Effects are lottery dummies.

Table 8: Intention to Treat estimates - Trust mechanism

Dependent Variable	Follow up Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Trust index	0.681	0.206	0.180	0.186
	[0.886]	(0.114)*	(0.106)*	(0.105)*
		(0.070)	(0.090)	(0.075)
Trust government information	0.523	0.092	0.069	0.086
	[1.115]	(0.112)	(0.111)	(0.113)
		(0.416)	(0.533)	(0.445)
Trust community leaders' information	0.84	0.238	0.218	0.211
	[1.117]	(0.109)**	(0.096)**	(0.094)**
		(0.029)	(0.023)	(0.025)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefifcient, robust standard error, and p-value, in that order. Robust standard errors, clustered at community level: ***p<0.01, **p<0.05, *p<0.1. Note: Specification included pre-treatment control variables for community leader visit, quality of the house roof, and household index for respiratory diseases. Model 1: Pre-specified Fixed Effects model; Model 2: Fixed Effects interacted with treatment assignment Lin (2013); Model 3: Interaction-weighted estimator Gibbons et al. (2018). Outcome variables have been standardized according to Kling et al. (2007). Fixed-Effects are lottery dummies.

Appendix

Complementing models

We can correct the biases of the pre-especified model using Inverse Probability Weights (IPW). Instead of using a fixed-effect approach we can run a weighted least squares model (WLS) using the IPWs as weights. IPWs are as follows:

$$W_{ij} = \frac{1}{P_{ij}^{t}}$$

Where P_{ij}^{t} is the probability of assignment to treatment of individual i within lottery j. Our second specification then is the following:

$$y_{ij} = \alpha + \rho Treat_{ij} + \beta X_{ij} + \varepsilon_{ij} \quad (2) \;\; WLS$$

An alternative specification is the one that relies on the RWE estimator also from Gibbons et al. (2018). Instead of estimating each group's treatment effect as the IWE, the RWE re-weights each observation according to the following weights:

$$W_{ij}^{RWE} = [\hat{Var}(Treat_j|g(j) = g(i))]^{-\frac{1}{2}}$$

Where $\hat{Var}(Treat_j)$ is the standard deviation of the conditional treatment values within the groups, and $g = 1, ..., G$ groups.

Results from these alternative specifications will be available in an online appendix. All results are very similar to what has been shown in the article.

Sample adjustments

In "Terra Nossa" and "Piquete", we unfortunately had repeated members of the same household running for the lottery, which accidentaly gave them a greater chance of being assigned to treatment. Our full sample analysis includes these two lotteries, but we attribute different weights to households that had a higher chance of being chosen. For the IPW regression the weights are:

$$W_{ij} = \frac{1}{P_{ij}^t} \quad , for\ unique\ households$$

$$W_{ij} = \frac{1}{P_{ij}^t + P_{ij}^t P_{ij}^c} \quad , for\ doubled\ households$$

Where P_{ij}^t is the probability of assignment to treatment of individual i within lottery j and P_{ij}^c is the probability of assingment to control.

In the cases of Lin (2013) and Gibbons et al. (2018) we consider repeated households as different strata within our fixed-effect approach, implicitly re-weighting them. Even though we corrected the weights of the doubled listed families in the full sample analysis, we do drop the two problematic communities in our robustness checks samples.

A choice between estimators

Recently, De Chaisemartin and Behaghel (2020) proposed a new estimator called Doubly-reweighted-ever-offer (DREO). They argue that the Ever Offer estimator is in fact inconsistent, while Initial Offer is less precise than the DREO. They recommend reporting EO and DREO estimates because IO, albeit consistent, is dominated by the more precise DREO. However, a few conditions must hold if I am to use the DREO estimator. First, the take up rates between the initially offered households and the subsequent offers (replacement group) must be equal. If they are significantly different, then estimator may be biased (De Chaisemartin and Behaghel 2018). Second, attrition rates must also be balanced between treatment and non-treatment assignments.

I opted to report only the Initial Offer estimator for two reasons: EO and DREO fail to accomplish the second condition above mentioned and just barely pass the first one; if I am to use EO and DREO I would have even more serious issues of statistical power, as I would have to drop many communities where the waitlist was exhausted. The difference in the take up rate between initial offers and replacement group is about 10 percentage points. Although I cannot reject the null hypothesis that the two rates are equal, p-values are very close to the 10% level of significance. As for the unbalance of attrition, the IO estimator is well balanced, while the EO does not pass t-test for some specifications [15]. When take-up rates are not equal, De Chaisemartin and Behaghel (2018) recommend using the initial-versus-no-offer estimators (INO) estimator. It consists of dropping the entire replacement group, and comparing initial offers to never offered households. I unfortunately had to discard this option because our replacement groups are large we would end up with a very small sample. Although we recognize that it would be useful to use DREO or INO estimators, we are confidant in using the IO estimator because it is consistent and unbiased.

Compliance Analysis

I build analyses of differential compliance rates between initial offer, replacement group and never offer subjects. The take-up rate of those assigned to treatment in the initial offer is 0.66, while the take-up rate of thouse assigned to control is 0.24. The difference is 0.42, without accounting fixed-effects weights.

I first compare the take-up rate between individuals that were initially offered treatment versus individuals that were initially not offered. Using our full sample we run the following regression: $Treatment_{ij} = \beta_0 + \beta_1 TreatAssignment_{ij}^{IO} + \beta_2 CommunityLotery_j + \eta_{ij}$, where i is a subscript for individuals and j is a subscript for community lotteries. This regression estimates the difference between the take-up rates of those assigned and not assigned to treatment according to the Initial Offer

[15] See the Appendix section for more information on attrition and compliance analyses

estimator. This is the same as making the difference between take-up rate of those assigned to initial treatment and those assigned to control. Controlling for fixed effects the difference is of 0.38 percentage points and statistically significant. This is somewhat expected since we want a high compliance rate in our initial offer. We have only 7 always takers from a sample size of 386.

Table 9: Compliance IO Analysis

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	0.258	0.140	1.841	0.066
treat_sort	0.388	0.053	7.335	0.000

I then estimate the same regression, but using the ever offer treatment assignment variable. That is, I consider not the initial offer, but all the subsequent offers and compare to those who never received any offer: $Treatment_{ij} = \beta_0 + \beta_1 TreatAssignment^{EO}_{ij} + \beta_2 CommunityLotery_j + \eta_{ij}$. This regression estimates the difference between the take-up rates of those assigned and not assigned to treatment according to the Ever Offer estimator. Again I expect this difference to be large. Take up within assigned to treatment is 0.56 and within not assigned is 0.05. When controling for fixed-effects I have a difference of 0.58.

Table 10: Compliance EO Analysis

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	-0.081	0.189	-0.432	0.666
treat_Ever	0.581	0.041	14.202	0.000

Finally, I exclude from our sample all individuals who were never offered treatment. By doing so, I compare the take-up rate of individuals who received the initial offer to individuals who received subsequent offers (the replacement group). The take-up rate of those that received the initial offer is again 0.66, while the take-up rate of the replacement group is 0.45, giving a difference of 0.21 percentage points when I do not control for fixed-effects. When do control for fixed-effects the difference drops to 0.1 and I fail to reject the null hypothesis that both rates are equal. However, our p-value is very close to 10% raising doubts towards the validity of the assumption that I have equal compliance rates between the two groups. If this assumption does not hold DREO and Initial Offer estimators can be biased and Chaismartin and Behaghel (2020) recommend using the INO estimator. Since the INO estimator drops all the replacement group units I would end up with too few observations and serious power problems.

Table 11: Compliance Replacement Group Analysis

	Estimate	Std. Error	t value	Pr(>|t|)
(Intercept)	0.435	0.178	2.440	0.015
treat_sort	0.105	0.067	1.567	0.118

Attrition Analysis

Attrition can bias our estimates if I have differential rates between treatment and control units. Our first test to check if our attrition rates are balance is to run the following regression using the same estimators that I use for our outcome variables:

$$InterviewDummy_{ij} = \alpha + \rho TreatAssingment_{ij} + \beta X_{ij} + \gamma_j + \varepsilon_{ij}$$

Where $InterviewDummy_{ij}$ is a dummy that assumes value 1 when a household was interviewed and 0 otherwise. $TreatAssingment_{ij}$ is our assignment to treatment variable and I also include pre-treatment characteristics (X) measured at baseline, γ_j is a community (slum) fixed effect and ε is the idiosyncratic error term. To atest balance in attrition I want our parameter of interest ρ to be equal to zero.

Attrition Initial Offer I start using the Initial Offer estimator. All specifications appoint to a fine balance between treatment and control assignment as I fail to reject the null hypothesis of the t-test.

Table 12: Attrition Analyses Initial Offer

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks
Standard Reg. with Cov. FE	0.006	0.044	0.138	0.890	530	22
Reg Lin - Only Comunity FE	0.002	0.044	0.036	0.971	530	22
Reg Lin - All Cov.	0.002	0.044	0.043	0.966	530	22
IWE - Without Covariate	0.002	0.043	0.036	0.971	530	22
IWE - With Covariate	0.005	0.044	0.114	0.910	530	22
RWE - Without Covariate	0.001	0.044	0.016	0.988	530	22
RWE - With Covariate	0.005	0.045	0.121	0.904	530	22

I now further investigate if I have patterns of attrition across our treatment arms. Like before, I regress our attrition indicator on treatment assignment, but I also interact pre-treatment covariates with our treatment assignment variable. I then perform a F-test of the hypothesis that all the pre-treatment interaction coefficients are zero. The regression is:

$$y_{ij} = \alpha + \rho Treat_{ij} + \phi Treat_{ij} X_{ij} + \theta Treat_{ij}\ \gamma_j + \beta X_{ij} + \gamma_j + \varepsilon_{ij}$$

The analysis show that I reject the null when I use the three chosen pre-treatment covariates from our outcome regressions, raising issues towards a pattern of attrition between these covariates. I further estimate alternative covariates to check if the problems persists and it does not. The problem seems to be with the variable CV_3.5_INDEX (Respiratory diseases household index)

Balance Pre-treatment Analysis

Finally, an important test is to check if our randomization process is good. I check if our pre-treatment variables are not correlated to treatment assignment. I run fixed-effect alternatives to the following specification:

$$X_{ij} = \alpha + \rho Treat_{ij} + \gamma_j + \varepsilon_{ij}$$

T-test for each variable - Household level

Reg Lin - Only Comunity FE	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	mean
Household size	0.06	0.16	0.34	0.73	510	22	2.61

Reg Lin - Only Comunity FE	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	mean
Average age of family	0.49	1.44	0.34	0.73	510	22	32.00
Dummy social security pension	-0.03	0.05	-0.74	0.46	510	22	0.63
Dummy Employment	-0.02	0.05	-0.37	0.71	510	22	0.59
Index Health Problem	0.04	0.06	0.70	0.49	510	22	0.48
Long lasting diseases index	0.01	0.03	0.35	0.73	510	22	0.31
Sanitary diseases index	0.00	0.04	0.05	0.96	510	22	0.28
Respiratory diseases index	0.00	0.03	-0.08	0.93	510	22	0.25
Disabilities index	-0.01	0.02	-0.55	0.58	510	22	0.10
Hospital visits index	-0.05	0.04	-1.38	0.17	510	22	0.56
Leader visit	0.08	0.04	1.96	0.05	510	22	0.35
Education level index	0.02	0.09	0.19	0.85	510	22	2.16
Year arrived community	0.28	0.34	0.83	0.41	510	22	2014.79
Intention to leave community	-0.18	0.15	-1.18	0.24	510	22	4.28
Location previous home	0.13	0.06	2.13	0.03	510	22	3.07
Ever evicted	0.01	0.03	0.30	0.77	510	22	0.10
Dummy Infrastructure problems	-0.02	0.05	-0.40	0.69	510	22	0.41
House quality index (roof)	-0.14	0.07	-1.96	0.05	510	22	2.86
House quality index (wall)	-0.11	0.07	-1.54	0.12	510	22	2.76
House quality index (floor)	0.03	0.07	0.39	0.69	510	22	3.01
TETO Vulnerability Index (Pontos)	0.02	0.21	0.11	0.91	510	22	6.70

T-test for each variable - Individual level

Reg Lin - Only Comunity FE	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	mean
Gender 1st respondent	0.08	0.05	1.75	0.08	510	22	1.38
Age 1st respondent	0.60	1.38	0.44	0.66	510	22	38.64
Race 1st respondent	0.13	0.09	1.51	0.13	510	22	3.97
Dummy Employment 1st respondent	0.04	0.05	0.79	0.43	510	22	0.42
Dummy Health Problem 1st	0.10	0.08	1.22	0.22	510	22	0.54
Dummy long lasting diseases 1st	0.04	0.05	0.94	0.35	510	22	0.38
Dummy sanitary disiases 1st	0.01	0.04	0.25	0.81	510	22	0.30
Dummy respiratory diseases 1st	0.01	0.04	0.28	0.78	510	22	0.28
Dummy disability 1st	0.00	0.03	-0.02	0.98	510	22	0.90
Hospital visits 1st	-0.05	0.05	-1.02	0.31	510	22	0.62
Financial Expexctation	0.06	0.04	1.28	0.20	510	22	0.75
Dream housing	-0.01	0.04	-0.20	0.85	510	22	1.80
Institutional afiliation	-0.01	0.03	-0.31	0.76	510	22	0.75

Reg Lin - Only Comunity FE	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	mean
Authority claim-making	0.00	0.04	0.11	0.91	510	22	0.20
Leader claim-making	0.06	0.04	1.57	0.12	510	22	0.22
Elections 2018	0.00	0.03	-0.06	0.95	510	22	0.13
Elections 2016	0.02	0.03	0.83	0.41	510	22	0.12