FUNDAÇÃO GETULIO VARGAS
ESCOLA DE ADMINISTRAÇÃO DE EMPRESAS DE SÃO PAULO

LEONARDO DA ROCHA LOURES BUENO

A HOUSING DEVELOPMENT PROGRAM IN BRAZILIAN SLUMS: THE SOCIAL IMPACT OF TETO

SÃO PAULO

2021

LEONARDO DA ROCHA LOURES BUENO

A HOUSING DEVELOPMENT PROGRAM IN BRAZILIAN SLUMS: THE SOCIAL IMPACT OF TETO

Tese apresentada à Escola de Administração de Empresas de São Paulo da Fundação Getulio Vargas, como requisito para a obtenção do título de Doutor em Administração Pública e Governo.

Linha de pesquisa: Política e Economia do Setor Público.

Orientador: Prof. Dr. Ciro Biderman

SÃO PAULO

2021

Bueno, Leonardo da Rocha Loures.
A housing development program in Brazilian slums : the social impact of TETO / Leonardo da Rocha Loures Bueno. - 2021.
159 f.

Orientador: Ciro Biderman

Tese (Doutorado CDAPG) – Fundação Getulio Vargas, Escola de Administração de Empresas de São Paulo.

1. Política Habitacional - Brasil 2. Habitação - Aspectos sociais. 3. Favelas. 4. Pobreza. 5.Comunidade - Desenvolvimento. I. Biderman, Ciro. II. Tese (doutorado CDAPG) - Escola de Aministração de Empresas de São Paulo. III. Fundação Getulio Vargas. IV. Título.

CDU 332.8(81)

Ficha Catalográfica elaborada por: Isabele Oliveira dos Santos Garcia CRB SP-010191/O Biblioteca Karl A. Boedecker da Fundação Getulio Vargas - SP

LEONARDO DA ROCHA LOURES BUENO

A HOUSING DEVELOPMENT PROGRAM IN BRAZILIAN SLUMS: THE SOCIAL IMPACT OF TETO

Tese apresentada à Escola de Administração de Empresas de São Paulo da Fundação Getulio Vargas, como requisito para a obtenção do título de Doutor em Administração Pública e Governo.

Linha de pesquisa: Política e Economia do Setor Público.

Orientador: Prof. Dr. Ciro Biderman

Data da aprovação:

______/______/______

Banca Examinadora:

Prof. Dr. Ciro Biderman (Orientador)
FGV-EAESP

Profa. Dra. Natália Salgado Bueno
Emory University

Prof. Dr. Daniel da Mata
FGV-EESP

Prof. Dr. George Avelino Filho
FGV-EAESP

SÃO PAULO

2021

Este trabalho é dedicado aos meus pais,
pois com muito suor dedicaram suas vidas pela educação dos seus filhos.
Sem eles eu nunca teria sequer sonhado em fazer doutorado.

Acknowledgements

Encaro minhas pesquisas como empreendimentos. Em primeiro lugar é preciso ter uma boa ideia. Em seguida, traçar um plano de ação e ir atrás das oportunidades. Costuma ser uma atividade arriscada, pois nunca se sabe onde se vai chegar. Como qualquer empreendedor, enfrentei diversas dificuldades no caminho. A primeira delas, foi acreditar no meu projeto. Foi preciso uma certa dose de confiança e de ousadia. Por outro lado, eu teria facilmente desistido logo no começo e em diversas ocasiões caso eu não tivesse pessoas íncríveis ao meu lado em todo o tempo. Por isso, digo que esse trabalho não é apenas meu, é um esforço conjunto de grandes companheiros que tenho na minha vida.

Em primeiro lugar, agradeço aos meus pais Leusi e Renato por sempre acreditarem em mim e no meu potencial. Mesmo nos momentos mais difíceis em que me faltou confiança e esperança vocês sempre estiveram lá. Seu amor incondicional e exemplo de vida são tudo para mim. Agradeço também ao meu querido irmão Thiago por ser meu melhor amigo, um exemplo de pessoa do bem e batalhadora, você me inspira, me diverte e me motiva a dar meu máximo sempre. Agradecimento também aos meus avós, que são uma grande fonte de inspiração. Muito obrigado família.

Devo também agradecer imensamente ao meu orientador Ciro Biderman. Agradeço por toda a ajuda durante o trabalho e por ser uma grande inspiração profissional para mim. É com certeza mais do que um orientador para mim, foi e sempre será um grande tutor. Me ajudou não só com a tese, mas em muitos outros conselhos para a vida. Me espelho muito nele e quero sempre manter essa ótima relação que temos. Extendo meus cumprimentos ao professor George Avelino, que acompanhou minha evolução e foi sem dúvida uma peça fundamental para o meu amadurecimento intelectual. Ciro e Avelino são a dupla de orientadores que todo mundo sonha em ter. Agradeço muito por tê-los em minha vida.

Tenho muito a agradecer a outros dois professores muito especiais. O professor Daniel da Mata foi a primeira pessoa a acreditar no meu projeto e a me incentivar a torná-lo realidade. Lembro claramente quando fui convensar com ele ao final da aula de economia urbana. Esse foi um daquele momentos em que a vida dá uma guinada. Sem o incentivo do Daniel eu provavelmente teria desistido do projeto. Por isso e por tantas horas de trocas de ideias e ensinamentos, meu muito obrigado.

A professora Natália é uma daquelas pessoas que todo mundo quer ser quando crescer. Inteligente, dedicada, criativa e sempre muito profissional. A presença dela nesse projeto de fato nos levou a um patamar que eu jamais poderia imaginar. Vislumbro um dia chegar ao nível profissional e intelectual dela. Além disso, ela transparece amor pelo que faz e nos contagia a todos.

Quero também agradecer aos meus amigos de FGV, tanto pelas risadas nas horas de descontração como pelas trocas de ideias sobre nossas pesquisas. Não poderia deixar de citar a Juliana Camargo, Guilherme Russo, Lara Mesquita, Eliane Teixeira, Tainá Pacheco, Bruno Panteão, Vitor Estrada, Patricia Alencar, Camila Soares e Sarah Marinho. Também devo agradecer aos sempre parceiros de pesquisa Murilo Sepulvida e Letícia de Souza, que foram fundamentais para o andamento do projeto.

Em tempo, gostaria de fazer um agradecimento mais que especial para a TETO como organização. Sempre acreditei no potencial transformador da educação e da pesquisa acadêmica. Mas a entrada da TETO na minha vida ressignificou completamente meus entendimentos sobre propósito e missão profissional. A TETO me tornou mais humano e deu sentido ao meu trabalho. Eu teria tantas pessoas para agradecer que contribuíram para parceira que não caberiam nessas poucas páginas. Por isso, agradeço nominalmente, como representates da TETO, a Juliana Simionato, Camila Jordan, Nina Scheliga e Ygor Melo. Sem ter vocês comprando a idea da pesquisa nada teria acontecido.

O presente trabalho também contou com o apoio de algumas agências de fomento. Agradeço à Fundação Getúlio Vargas e ao CEPESP/FGV por terem fornecido minhas bolsas de pesquisa, à FAPESP pelo financiamento SPRINT e ao MIT GOV/LAB e Halle Institute Emory pelos demais financiamentos.

Por fim, se a jornada vale a pena é porque temos pessoas que amamos e que nos amam ao nosso lado. Tamara, minha linda, você é muito mais que minha esposa. É a pessoa com quem decidi pegar na mão e sair passeando por essa linda trilha que é a vida.

*"The differences in income between
the poor world and the rich world are
so great that people have to be interested."*
(Esther Duflo)

*"Poverty is not just a lack of money;
it is not having the capability to realize
one's full potential as human being."*
(Amartya Sen)

Abstract

Lack of access to proper housing infrastructure is a problem affecting thousands of households in Brazil. Poor housing quality can be a source of unwanted consequences that may prevent the overcoming of poverty. The present dissertation explores a few outcomes that can originate from inadequate housing, but that are not commonly tackled in the design of policies. In partnership with the NGO TETO Brazil, I conducted a randomized controlled trial to evaluate the extent to which the organization's housing program alleviates the subjective mental well-being and preventive behavior measures against COVID-19 of benefited families. The results support the claim that TETO's intervention increases mental well-being in the short run but has null effects on behavioral measures in the fight against COVID-19. Additional findings highlight the importance of the social components of TETO's housing program to explain these results. TETO improves pro-social behavior among participants of the program. For example, TETO has increased norms of trust and reciprocity, as well as networks and social ties.

Palavras-chave: Housing program. Development. Poverty in slums.

Resumo

A falta de acesso a uma infraestrutura habitacional adequada é um problema que afeta milhares de famílias no Brasil. A má qualidade da habitação pode ser uma fonte de consequências indesejáveis que pode impedir a superação da pobreza. A presente dissertação explora alguns resultados que podem ter origem em moradias inadequadas, mas que não são comumente abordados na formulação de políticas. Em parceria com a ONG TETO Brasil, conduzi um ensaio clínico randomizado para avaliar em que medida o programa habitacional da organização alivia o bem-estar mental subjetivo e medidas preventivas de comportamento contra o COVID-19 das famílias beneficiadas. Os resultados apoiam a afirmação de que a intervenção do TETO aumenta o bem-estar mental no curto prazo, mas tem efeitos nulos nas medidas comportamentais na luta contra o COVID-19. Descobertas adicionais destacam a importância dos componentes sociais do programa habitacional do TETO para explicar esses resultados. A TETO melhora o comportamento pró-social entre os participantes do programa. Por exemplo, a TETO aumentou as normas de confiança e reciprocidade, bem como as redes e os laços sociais dos beneficiários.

Keywords: Housing program. Development. Poverty in slums.

List of Figures

List of Tables

List of abbreviations and acronyms

ATE	Average Treatment Effects
CATE	Conditional Average Treatment Effects
DREO	Doubly-reweighted Ever-offer Estimator
EO	Ever Offer Estimator
IO	Initial Offer Estimator
ITT	Intention to Treat
RCT	Randomized Controlled Trial
SWE	Subjective Well-being
TETO	NGO TETO Para o Meu País

Contents

Introduction

Lack of access to proper housing is a problem afflicting millions of people in developing countries with far-reaching consequences on families' well-being, health, and ties to communities. Improper housing can also correlate to what economists call poverty traps. A growing number of studies emphasize that life under extreme deprivation represents a vicious cycle. From insufficient access to material conditions, such as food or credit, to social prejudices and psychological difficulties, poor people face barriers that prevent them from escaping poverty (SACHS, 2006; BANERJEE; BANERJEE; DUFLO, 2011). The present dissertation examines the social, economic, and psychological impacts of a slum housing improvement program led by an international non-governmental organization. The NGO TETO provides higher quality pre-fabricated houses to slum residents living in precarious housing conditions. Through a series of quantitative and qualitative impact evaluation methods, the present dissertation tries to address to what extent TETO's housing program may reduce traps that prevent slum residents from overcoming poverty.

The dissertation inserts itself in a broader discussion on local communities development. TETO's mission is to alleviate poverty by promoting community engagement, increased social security networks, and other types of social capital (PUTNAM et al., 2000). The housing program is just one among many other initiatives sponsored by the NGO, perhaps the most well-known. However, in all of its projects, TETO's work model resembles the so-called community-driven development programs (CDDs). This novel approach that has been gaining relevance among international organizations like the World Bank, focuses on bottom-up projects that try to involve members of local communities in decision making and hands-on practices (DONGIER et al., 2003). TETO believes that slum infrastructure projects represent opportunities for increasing community engagement among beneficiaries and participants. I test to what extent the housing program indeed generates development in different dimensions of residents' behavior, beliefs, and material conditions.

Throughout the dissertation, I focus on discussing how TETO transforms a simple housing improvement intervention into a successful social program. TETO's intervention is much more than a housing program because social interaction is a fundamental component. To explain the impacts of the program I go beyond the improvements on material conditions and try to explore changes in human behavior, beliefs, and socialization of beneficiaries in the communities where they spend most of their time. My dissertation suggests that TETO may increase beneficiaries' social capital, and thus collective action within poor communities. Furthermore, I test whether housing units may be key to improve health conditions, especially during the Covid-19 pandemic. Results show that TETO does not contribute to increasing preventive behavior towards protection from the virus, but it does increase psychological well-being during the crisis.

I divide this dissertation into two substantive chapters that together give the picture of how TETO's social programs impact Brazilian slums in the context of the Covid-19 pandemic. The first substantive chapter deals with findings relating to risk behavior towards the pandemic, government and local leaders' accountability, claim-making for aid, and housing improvement. The second substantive chapter is about how housing programs like TETO's improve different measures of psychological and subjective well-being of recipients. I also include one chapter describing TETO's housing program in light of the literature on community-driven development projects.

Additional contributions of my dissertation are the two chapters where I discuss my methodological approach. I have adopted a mixed-methods methodology in many stages of the research. Although I rely primarily on a randomized control trial (RCT) to evaluate the causal impacts of TETO, I extensively make use of qualitative data to better understand processes, mechanisms, and even to make strategic research decisions along the way [1].

Substantively, my work contributes to a growing literature on the impacts of housing policies in promoting local community development. I constantly compare TETO's program to other housing initiatives. Unlike many other social housing programs, TETO does not relocate residents but rather upgrades homes for residents in their original land plot. Furthermore, it is not uncommon that conventional policies neglect the social nature of housing interventions and their potential unintended consequences. Through the dissertation, I call attention to this and other issues that must be addressed by stakeholders and policy-makers.

[1] Hopefully, the experiences reported in the present dissertation will help future doctoral candidates in their researches.

Part I

The NGO TETO

1 What is TETO about?

"Un Techo para mi País" (A roof for my country) [1] is the official name of the NGO TETO. The organization was founded in Chile in 1997 and has already spread to 19 countries in Latin America. TETO is a civil society organization aimed at reducing poverty by promoting community engagement and youth civic participation in infrastructure projects in poor regions of Latin America. The first and most widely known initiative is to build emergency houses for slum residents living at risk of housing collapse. However, TETO also promotes different types of infrastructure projects in vulnerable communities.

Through housing and infrastructure projects, TETO tries to promote social engagement within slum residents. The long-run objective is to increase what TETO calls "community capacities", a concept that resembles what scholars call social capital. For example, TETO designed the housing intervention to reduce risks related to poverty at an initial stage. However, in subsequent moments, the expectation is that beneficiaries get involved in other community projects, further develop local networks, and increase trust and reciprocity with neighbors. Initiatives like these, whose objectives are to strengthen local communities from bottom-up approaches, are usually called community-driven development programs (CDDs).

TETO developed a particular type of CDD that relies on young volunteers, who are usually well-educated and members of universities. In the next section, I further describe the work of volunteers and how they promote social interactions within communities. The focus of the dissertation is on the impacts of TETO's housing program. Yet, TETO's methods of change are basically the same for other types of infrastructure projects. Volunteers have a role in organizing the collective efforts of slum residents, but along the process, it is expected a growing protagonism from the community members. Ideally, by the end of the work, communities should be able to deal with poverty traps by themselves.

[1] In Brazilian Portuguese the translation is: "Um TETO para o meu País".

2 TETO's housing program: intervention details

Top-down development programs have many difficulties in reaching the poorest among the poor either because of corruption, elite capture, lack of good governance, or lack of accountability mechanisms (DASGUPTA; BEARD, 2007; AVDEENKO; GILLIGAN, 2015). An alternative approach that has been calling the attention of the international community is to sponsor bottom-up programs that rely on community civic engagement and inclusive governing institutions that allow citizens to participate in the co-production of public goods [1]. Such interventions are known in the literature as community-driven development programs (CDD). TETO's housing program resembles in part other CDD programs because it presupposes community engagement by those who are to be benefited from the program. In this chapter, I describe the nuances of TETO's intervention. One thing to keep in mind is that it is a bundled intervention: on the one hand, it represents an infrastructure housing project and on the other hand, it promotes social interaction and community engagement.

One of TETO's goals is to make communities more independent from external help. The organization believes that by developing CDD programs within communities, squatters will improve their social skills and learn how to act towards voicing their needs or even organizing themselves to solve collective action problems. The NGO has its own methodology for developing CDD programs, where young high and middle-class volunteers are fully responsible for engaging squatters and organizing activities. Ideally, by the end of the program, communities must be able to care for themselves and develop their own public goods provision projects. The housing program is one of many other infrastructure-related programs that TETO sponsors. Yet, it is probably the most well-known and scaled-up program from the NGO.

On the quality improvement aspect, TETO provides pre-fabricated wooden made housing units that can be built by volunteers in two days. More than 95% of the house cost is subsidized, and beneficiaries must pay around R$200,00 (US$40,00 in current exchange rates), which corresponds to 20% of a monthly 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

[1] The co-production literature explores the delivery of public services where citizens are involved in the creation of public policies and services (TORFING; TRIANTAFILLOU, 2016; BRANDSEN; VERSCHUERE; STEEN, 2018).

not have any windows. 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 from common tropical natural disasters (storms and floods), but it does not provide the most basic sanitary infrastructure. It is also more comfortable and better looking, features that were very salient in my qualitative interviews and may explain some positive results of the intervention. Figures 1 and 2 show typical housing units in the comparison and treatment groups, respectively. Figure 3, gives a sense of how the house is from the inside. See figure 4 to notice that TETO's house is precast.

Figure 1 – Typical Control Housing Unit

Source: TETO disclosure

In terms of facilities, TETO's units have no access to clean water nor sewage. After volunteers finish the construction, dwellers usually build improvised bathrooms by themselves or hire someone to make this type of service. However, they cannot connect the unit to official sewage infrastructure because most residents are not landowners. Although TETO does help dwellers with legal tenure consultancy, the organization never provides land titles. Hence, most families dump their waste into rivers or ditches dug on their land

Figure 2 – Typical Treatment Housing Unit

Source: TETO disclosure

plot [2]. Notice in Figure 5 how informal pipes go directly to the river. At the bottom of the picture, it is possible to see a white pipe, while far in the back of the scene there are more pipes. Concerning access to electricity, the great majority of residents have informal wires connected to the official networks. Similar to the sanitary infrastructure, residents rely on local service providers to afford them with this informal electricity connection. In Figure 6, a volunteer is carrying construction materials, but notice that, in the back of the picture, electric wires connect the shacks to the electrical network. Those are typical informal connections.

TETO has a step-by-step process to allocate houses to families in need. At least two months before construction day, volunteers visit communities to pre-select eligible families to take part in the program. They enter communities and look for households living in extreme poverty and precarious housing conditions. With the help of community leaders, they can identify most of the families that require better housing units. After that, they conduct a survey with the aim of registering families in TETO's databases and to have a sense of the different levels of social and housing vulnerabilities. By the end of the process, volunteers jointly decide with community leaders which are the families that will be eligible to participate in the program. Some families initially surveyed, may not match the initial eligibility criteria, which are the following: (1) live for at least one year in the community, (2) have a shack made out of wood, plastic, or other inadequate materials, (3)

[2] 90% of surveyed households have informal access to water

Figure 3 – Inside of TETO's Housing unit

Source: TETO disclosure

do not show interest in the program. The families that do not match any of these criteria are disconnected from the program.

Once volunteers gather a pool of eligible households, they decide which families should receive priority to have a housing unit. Criteria for priority are usually linked to extreme vulnerability, such as the imminent risk of the housing collapse, family members facing serious health problems, or families with any member with disabilities (physical or mental). The number of families that receive priority can usually vary from one to five, depending on the number of houses being delivered, but there is no cutoff for this decision. Families that receive no priority are eligible to take part in lotteries for housing delivery. My research team and I were always responsible for making the lotteries. I provide more details in the quantitative methodology chapter.

The program starts with selected families one month before construction day. Every weekend volunteers visit the community and gather selected families to discuss the construction plans. But here is when the social aspect of the program comes up. Meetings are not only aimed at teaching family members how to build the housing units. Like in other CDD programs, meetings were also thought to develop beneficiaries' social skills and promote community engagement, with the hope of enhancing social capital. In terms of collective actions, residents must work with each other in the workshops and jointly take decisions about the logistics of the event. For instance, some people need to give space in their land plots to store the houses' wooden boards, while others need to cook

Figure 4 – Precast Housing unit

Source: TETO disclosure

for the volunteers during construction day. These decisions involve negotiations between community residents and substantial planning. Although residents rely on the help of volunteers, they are fully responsible for promoting the engagement of family members, friends, and possibly neighbors willing to help. Hence, it is paramount that they try to access their previous networks in all the construction efforts.

Another example of a task that presupposes social interaction and reliance on previous networks is the destruction of resident's shacks. The night before the construction event, families need to destroy their previous homes. It is not uncommon that they need help in this task, and they cannot count on volunteers because they only arrive on the following day. Therefore, residents must ask friends, relatives, and sometimes neighbors to help them put the houses to the ground. This also implies that they need to find a trustworthy place to sleep along the weekend.

In the social capital literature, TETO would promote norms of trust and reciprocity among beneficiary families. In the pre-construction meetings, beneficiaries learn from volunteers not only how to divide the logistic tasks, but they are also encouraged to interact with previous networks and the other families that will receive the houses. The collective action necessary for the success of the program probably develops social skills like trust and reciprocity. Trust is fundamental in the sense that families completely count on volunteers showing up on the construction weekend. Furthermore, they must also trust that all families will help each other and will fulfill the established task agreements. Norms of

Figure 5 – Typical sewage disposal

Source: TETO disclosure

reciprocity are salient in the sense that benefited families exchange favors and comply with agreements during the entire program. They reciprocate with each other and sometimes with neighbors willing to help or with friends and family available to join efforts.

On the weekend of construction, TETO has serious educational work with volunteers. Apart from making the efforts of building houses, they form groups of discussion in which they debate about poverty, inequality, social exclusion, and other social-economic subjects that are salient in slums. While volunteers help to build the houses, it is not uncommon that they chat about these issues with benefitted family members. This type of interaction may lead to further reflection of family members about their lives and the value of joining collective efforts. There are many moments when residents are encouraged to talk about their lives to volunteers and what they think of the experience of working together to build their new homes. In Figures 7 and 8 , volunteers take a moment to chat with the young couple that is going to receive the housing unit. At this moment, beneficiaries and volunteers were discussing the social aspects of the intervention and how the new home could mean a restart for the life of the family [3].

In sum, the program encourages a series of skills, such as trusting each other, accessing networks, bearing costs, reciprocating, and coordinating. After one month of weekly meetings with volunteers, the program culminates with the construction weekend, when the housing units are finally delivered. In two days, volunteers, family members, and

[3] Those chats are part of a small ceremony that volunteers make to mark the end of the first stage of construction

Figure 6 – Typical electric connection

Source: TETO disclosure

whoever else engages in the efforts must build the wooden-made houses. Most of the time, they succeed, and by Sunday afternoon the house is finished.

Figure 7 – Volunteers and benefitted family chatting

Source: TETO disclosure

Figure 8 – First stage ceremony

Source: TETO disclosure

Part II

Research Methods

3 Mixed Methodology and Epistemological approach

> "A need exists to enhance an experimental study with a qualitative method" (CRESWELL; CLARK, 2017)

In this chapter, I justify my methodological and epistemological decisions while briefly explaining both of them. In the subsequent chapters, I further describe the methods being used (quantitative and qualitative). Because the dissertation' principal objective is to evaluate the impacts of TETO's housing program on a broad range of outcomes, I adopted a pragmatist epistemology with a mixed-methods approach.

3.1 A Choice for Pragmatism

Mixed methods have been called the "third methodological movement" (TASHAKKORI; TEDDLIE, 2003) and the "third research paradigm" (JOHNSON; ONWUEGBUZIE, 2004). There is a debate on whether mixed methods should embrace pragmatism as a philosophical foundation (TASHAKKORI; TEDDLIE; TEDDLIE, 1998) or simply use different paradigms as long as honoring and being explicit when each is used (GREENE; CARACELLI, 1997). Either way, pragmatism has great resonance with mixed methods due to common respect for pluralism.

First and foremost, how can one characterize pragmatism? As Simpson (2017) points out it is difficult to have a precise definition because there are many different views as to what represents pragmatism. As she states, "it is perhaps better understood as a celebration of pluralism that offers a multiplicity of enticing options for researchers who are seeking more dynamic and more processual ways of engaging with their research contexts and questions". To try to pin down pragmatism I focus on its differences to two common philosophical positions (or paradigms) in social sciences: positivism and constructivism.

Simpson (2017) believes that pragmatism is radically committed "to a non-reductive naturalism, which is anti-foundationalist, anti-dualistic and emergent". Positivism and post-positivism are ontologically committed to discovering laws of nature. In this sense, there is an objective reality to be achieved. The "truth" is there to be discovered. Constructivism on the other hand supports that there is not an objective reality but rather subjective realities, that are socially constructed and not context-free. While positivism seeks to find objective truth, constructivism seeks to find a relative truth of multiple realities. However, both seek to find the "truth" and produce knowledge that best corresponds to, or represents, reality (Rorty et al. (1999), APUT Feilzer (2010)). Pragmatism differentiates itself from this "truth" perspective because its main concern is not to find any truth but to be useful to some purpose. Pragmatism is more worried about practical consequences than the nature of reality. As Feilzer (2010) states:

> "pragmatists also hold an "antirepresentational view of knowledge" arguing that research should no longer aim to most accurately represent reality, to provide an "accurate account of how things are in themselves" but to be useful, to "aim at utility for us" (Rorty et al. (1999),pg. xxvi)".

Therefore, "pragmatism denies foundationalism, the view that grounded meaning and truth can be determined once and for all" (CHERRYHOLMES, 1992). Besides, pragmatists argue that values, aesthetics, politics, social and normative preferences should come before descriptions, theories, and explanations. The focus is on the consequences of our research. We should engage in research investigation that is useful for our society in a broad sense (CHERRYHOLMES, 1992). If a theory does not have an objective value, we should abandon it. In this sense, pragmatism differentiates epistemologically from constructivism. While pragmatism is more concerned with building knowledge objectively, constructivism is more into a subjectivist approach. Thus, it could be argued that pragmatism resembles positivism as both adopt an objective perspective. However, positivism's reasoning privileges deductions, while pragmatism could sometimes use deduction and induction, but mostly abduction when doing inference.

Another feature of pragmatism is that it is anti-dualistic. As Simpson (2017) points out "dualism distinguishes between two epistemological categories of nature that are seen as mutually excluding opposites". Descartes' distinction between mind and body is an example of dualism. When dualism such as this ceases to be a linguistic clarification and becomes a habit of thinking there is a risk of "thingification" when we greatly simplify our meaning process (SIMPSON, 2017). Pragmatist authors criticize this "thingification" of the lived experience because they believe the world is a process. One cannot slice reality into boxes and ignore it is a continuum (not a sum of discrete entities). Moreover, because practice is a never-ending process of transformation, creativity and novelty have a major role in pragmatism. If pragmatism is committed to consequences, then the creative process is fundamental to achieve the research purposes. Abduction is then elected as the logical inference method per excellence. As Pierce described it:

Abduction is "the process of forming a hypothesis to explain a given situation; abduction is a creative leap, 'an act of insight' that 'comes to us like a flash' " (Peirce (1974); APUD Simpson (2017))

In sum, pragmatism allows for a flexible but deep understanding of the economic, social, and psychological impacts of the program under study. The type of housing program that TETO promotes is deeply embedded in the communities where it acts. As such, paying attention to context is as important as focusing research on objective causal relationships. The choice for using mixed methods resembles that of using a pragmatistic epistemology, as both fit adequately for the purpose of policy (or program) evaluation.

3.2 Mixed Methods for assessing policy impacts

Before describing in detail what type of mixed methods approach I opted to use, I try to establish an analytical framework. I rely on the typology developed by Creswell & Clark (2017), although I acknowledge that other typologies are equally valid. The authors divide mixed methods into three core designs: sequential explanatory, sequential exploratory, and convergent designs.

Both sequential designs collect quantitative and qualitative data in different moments. The explanatory sequential design starts collecting and analyzing quantitative data. In subsequent periods, the researcher uses qualitative data to explain or expand the first-phase quantitative results. This approach usually follows a positivist epistemology because researchers use deduction to analyze quantitative data that eventually confirm or reject pre-established hypotheses. Qualitative data is marginal in the sense that it is only used to explain the quantitative core results.

The exploratory sequential design works in the opposite direction. First, researchers collect and analyze qualitative data, and only then do they conduct quantitative measures. In this design, the researcher interprets how the quantitative results build on the initial qualitative results. This approach is usually present in studies that adopt a constructivist epistemology. Induction is the core method and theory builds from exploratory qualitative data.

The convergent design is different in the sense that it relies on quantitative and qualitative data at the same time, but separately. The idea is to compare and (or) combine both sources of data into one final analysis. My option was to mostly rely on this last core design in addition to what Creswell & Clark (2017) call a complex design. The complex design that I use is called by the authors the mixed methods experimental design, which I chose to be embedded in the core convergent design. However, I did gather qualitative data before and after implementing the Randomized Controlled Trial of TETO's intervention. Hence, to a minor extent, I do rely on sequential core designs for specific purposes better described in the qualitative methods chapter. I next describe both the core convergent design and the complex experimental design separately, even though they form a single approach in the present dissertation.

The convergent design has four steps. First, quantitative and qualitative data are collected separately and independently. Second, the two datasets are analyzed separately, using proper analytical procedures for each type of data. The third step is when results are finally merged. The final step is when the researcher analyses to what extend the results converge or diverge and how they relate to each other or combine to create a better understanding of the research purposes. If the case is of diverging results, then further steps must be taken to explain the differences. The comings and goings with the data and reinterpretation of the results could be done abductively, with at some time new data being collected. The pragmatist approach plays a major role in guiding how the theory must be oriented and operated, providing a conceptual model on how to merge and analyze both sets of data.

As Creswell & Clark (2017) argue a convergent design fits better with a pragmatist paradigm. In sequential designs, it would be better to adopt a positivist approach on the quantitative phase and a constructivist approach on the qualitative phase. But on a convergent design - in which collecting, analyzing, and merging qualitative and quantitative data happens at the same time - pragmatism provides an "umbrella world-view for the research study" (CRESWELL; CLARK, 2017).

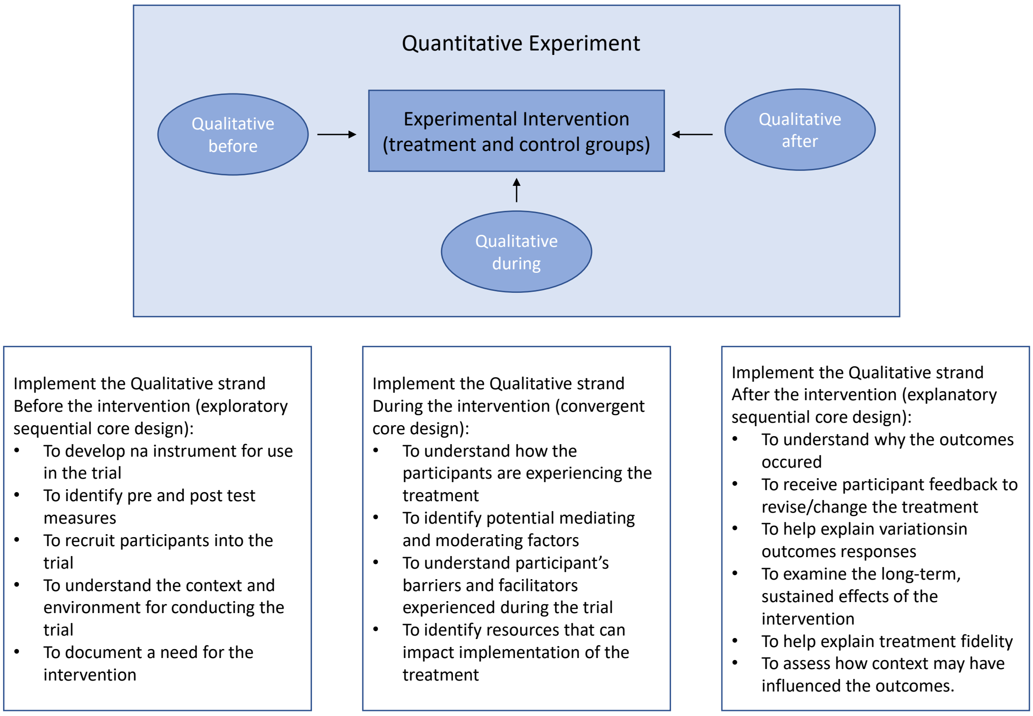
In Creswell & Clark (2017) typology, the experimental design can be combined with either one of the core designs (sometimes even all of them in different moments of the research project). The bottom line of the experimental design is the focus on performing a randomized controlled trial (RCT), or, in other terms, a random experiment. The quantitative data assumes protagonism, while qualitative data provides context and valuable insights. Nonetheless, the higher focus on quantitative data does not mean that qualitative data is less relevant to the analysis. In the experimental design, the collection, analysis, and integration of both quantitative and qualitative data are embedded in the RCT. As Creswell & Clark (2017) state:

> "The choice of this type of mixed methods application is based on the need to add personal experiences and a cultural understanding into an experimental trial aimed at testing the effectiveness of a treatment". (Creswell & Clark (2017), pg. 108)

I find this mixed-methods design the most adequate to address the complexity of TETO's housing intervention. The methodology allows us to objectively evaluate the program by testing causal relationships without denying the subjective nature of this social intervention. The objective is to evaluate the program at the same time as being able to appropriate from contextual and personal knowledge.

As shown in Figure 9 (extracted from Creswell & Clark (2017)), there are many reasons why researchers can use the experimental design. I used it for at least four reasons: (1) to plan the trial implementation and refine research questions; (2) to understand how participants experienced the treatment during implementation; (3) to identify mediating and moderating factors; (4) to better explain causal mechanisms. In the qualitative methods chapter, I detail how my research team and I used qualitative data to achieve these goals in the experimental design.

Figure 9 – Reasons to utilize the Experimental Design



Source: (CRESWELL; CLARK, 2017)

4 Quantitative Methods

To test the effects of the intervention, I implemented a Randomized Control Trial (RCT) 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.[1] [2] 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.[3] To be precise, for each block (community), I created randomized waitlists, which means that lottery participants were ordered from first to the last position in the list. Implementers go down the waitlist to offer the remaining houses to the next applicants until all units were allocated. If any of the assigned families decline the initial offer then the next family will be assigned to treatment. For each wave of construction within a given community we perform a new lottery.

My research team and I performed the lotteries the following way: first, we receive from TETO volunteers the list of eligible families and attribute them a random lottery number from 0 to 99. Then we sort the tickets from smallest to largest. Finally, we use the two last digits of the weekly federal lottery from the public bank Caixa Econômica Federal to define a cutoff in our list. Tickets that are located above the Caixa Econômica number are the first ones on our waitlist to receive the houses. The list continues scaling up until ticket 99 (or as close to it as possible), and after reaching this point it starts again from the bottom number 00. By using this methodology, we guarantee that the list is always random since the Caixa Econômica lottery is completely reliable [4].

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

1 IRB exemptions and approvals were acquired at Emory University (ID: IRB00110928) and Fundação Getulio Vargas (n. 011/2019).
2 Our pre-analysis plan was registered at EGAP prior to the collection of outcome data.
3 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.
4 For those interested in more details about this lotery, please check the following web address: <http://loterias.caixa.gov.br/wps/portal/loterias/landing/federal/>

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 first 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.

4.1 Model specifications

I conducted new lotteries for each wave of construction within communities. This is a valid approach as long as lotteries are correctly managed, leading to random probability of assignment within each block. However, a fundamental 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. If differential probability of assignment is not correctly accounted for, estimates risk to be inconsistent. My econometric 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. I call this first model as the pre-specified model. The econometric intuition behind this and other solutions is to produce the correct weights that will act upon the average treatment effect (ATE) to unbias the estimates, considering that ATE is a weighted average between blocks.

One potential problem with the pre-specified model is the following. 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 (GIBBONS; SERRATO; URBANCIC, 2018). I circumvent this potential problem by following three well known approaches from the literature: (1) re-weight regressions using Inverse Probability Weights; (2) interact fixed effect dummies with treatment assignment as proposed by (LIN, 2013); and (3) use Gibbons, Serrato & Urbancic (2018) "Interaction-weighted estimator" (IWE) and "Regression-weighted estimator" (RWE) estimators. Along the remainder of the dissertation, I only present results for the pre-specified model, Lin's interaction estimators and Gibbons ans co-authors' IWE estimators - all including pre-treatment control variables. I leave other specifications results to the appendix. I next present the all models' specification and how they deal with block randomization problems.

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

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

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 in certain circumstances produce biased estimates, I included it for comparison purposes.

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

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

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 across all groups will be an implicit weighted average of every ATE within each block.

Gibbons, Serrato & Urbancic (2018) have also proposed an estimator using interactions, the Interaction-weighted estimator (IWE). Point estimates in this third specification will be the same as Lin's, but standard errors are slightly different. Specially when controlling for pre-treatment covariates, estimates may be slightly different. I do not describe this third specification because it reassembles exactly the one from Lin's. Yet, Gibbons, Serrato & Urbancic (2018) proposes a fourth solution that I leave results to the appendix. The alternative models is called Regression-weighted estimator (RWE). Instead of estimating each group's treatment effect as the IWE, the RWE re-weights each observation according to the following weights:

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

Where $V\hat{a}r(Treat_j)$ is the standard deviation of the conditional treatment values within the groups, and $g = 1, ..., G$ groups. In the appendix I show that all results are very similar to what has been shown for IWE and Lin's estimator.

Finally, it is possible to correct the potential biases of the pre-especified model by 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. I also leave the following specification to the appendix, but results are usually similar:

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

4.2 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 low. In many cases the land plot would not fit TETO's house or the terrain was very steep [5]. Additionally, 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. [6]

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 [7]. All results remain similar.

The problems were with "Terra Nossa" and "Piquete", where we unfortunately had repeated members of the same household running for the lottery, which accidentally gave

[5] 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.

[6] In technical terms, "always takers" are subjects that receive treatment irrespective of the status of random assignment.

[7] See results in the appendix

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, Serrato & Urbancic (2018) I consider repeated households as different strata within our fixed-effect approach, implicitly re-weighting them. Even though I corrected the weights of the doubled listed families in the full sample analysis, I do drop the two problematic communities in the robustness checks samples.

4.3 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 [8]. Some studies choose one or another (ABDULKADIROĞLU; HU; PATHAK, 2013; WEST et al., 2016), and some even combine both estimators to 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 (CHAISEMARTIN; BEHAGHEL, 2020) shows, the EO estimator is inconsistent. The problem with the EO estimator is that the "expected proportion of takers (those who accept the housing offer) is strictly greater among applicants ever getting an offer than among applicants never getting one"

[8] see (CHAISEMARTIN; BEHAGHEL, 2019) for an extensive list

(CHAISEMARTIN; BEHAGHEL, 2020). This happens because the number of new offers only stop when sufficient houses are delivered, creating an endogenous definition of the size of the two groups receiving or not receiving a house offer. Chaisemartin & Behaghel (2020) prove that the problem is even worse in regressions that rely on fixed-effects, as the endogenous reweighting of waitlists further increases the imbalance between group offers across block lotteries (communities). Using asymptotic theory, they show that EO estimator is inconsistent as the number of lotteries go to infinity and the shorter size of waitlists. This is precisely the challenge with TETO, because what we have is a large number of communities (lotteries) with small waitlists.

To solve the problem, Chaisemartin & Behaghel (2020) proposed a new estimator called Doubly-reweighted-ever-offer (DREO). Because endogeneity in the proportion of take-up probabilities arises due to the last possible offer, the DREO drops this last observation from the sample, thus leading to a restoration of the comparability between both groups. The authors argue that the Ever Offer estimator is in fact inconsistent, while Initial Offer is less precise than the DREO. So 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 (CHAISEMARTIN; 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: (1) EO and DREO fail to accomplish the second condition above mentioned; (2) 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 8 percentage points. However, I cannot reject the null hypothesis that the two rates are equal (p-values are equal to 21% , as I show in the next subsection). As for the unbalance of attrition, the IO estimator is well balanced, while the EO does not pass t-test for some specifications [9]. When take-up rates are not equal, (CHAISEMARTIN; BEHAGHEL, 2018) recommend using the initial-versus-no-offer estimators (INO). It consists of dropping the entire replacement group, and comparing initial offers to never offered households. I unfortunately had to discard this option because replacement groups are large and we would end up with a very small sample. Although I recognize that it would be useful to use DREO or INO estimators, I am confidant in using the IO estimator because it is consistent and unbiased.

[9] See next section for more information on attrition and compliance analyses

4.4 Compliance Analysis

I build analyses of differential compliance rates between initial offer, replacement group and never offer subjects. The initial offer group are the families that receive the first offer of treatment, irrespective of any renunciation in the waitlist. Replacement group are the ones who substitute those who gave up treatment when the waitlist moves on. The never offer group gathers all families that have never received any treatment offer. The take-up rate of those assigned to treatment in the initial offer is 0.65, while the take-up rate of those assigned to control is 0.25. The difference is 0.4, without accounting fixed-effects weights. In other words, here I simply compare the proportion of people accepting treatment in both groups of the first offer.

Alternatively, it is possible to compare the take-up rate between individuals that were initially offered treatment versus individuals that were initially not offered using a regression analysis. Using our full sample we run the following model that account for fixed-effects:

$$Treatment_{ij} = \beta_0 + \beta_1 TreatAssignment_{ij}^{IO} + \beta_2 CommunityLotery_j + \eta_{ij}$$

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 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 36 percentage points and statistically significant. See results in Table 1 bellow:

Table 1 – Compliance Initial Offer Analysis

	Estimate	Std. Error	t value	Pr(>|t|)
Intercept	0.271	0.142	1.907	0.057
Treatment Assignment IO	0.366	0.054	6.813	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:

$$Treatment_{ij} = \beta_0 + \beta_1 TreatAssignment_{ij}^{EO} + \beta_2 CommunityLotery_j + \eta_{ij}.$$

This regression estimates the difference between the take-up rates of those assigned and not assigned to treatment according to the Ever Offer estimator. Take up within assigned to treatment is 0.56 and within not assigned is 0.068. When controlling for fixed-effects I have a difference of 0.56 (just like the IO, the diffence is statistically significant).

See results in Table 2 bellow:

Table 2 – Compliance Ever Offer Analysis

	Estimate	Std. Error	t value	Pr(>|t|)
Intercept	-0.066	0.189	-0.347	0.728
Treatment Assignment EO	0.566	0.042	13.588	0.000

Although in both analysis the take-up is unbalanced, this is not necessarily a problem, since the difference in accepting the offer may be a consequence of the way the lists are constructed. The problem of inconsistency in estimators arises when take-up rates are different between the first offer group and the replacement group. To check this, I exclude from the 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 0.65, while the take-up rate of the replacement group is 0.46, giving a difference of 19 percentage points when I do not control for fixed-effects. When I do control for fixed-effects the difference drops to 0.084 and I fail to reject the null hypothesis that both rates are equal (See Table 3). Thus, the necessary assumption for consistent estimates hold. If this assumption did not hold, DREO and Initial Offer estimators could be biased and Chaisemartin & 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 3 – Compliance Replacement Group Analysis

	Estimate	Std. Error	t value	Pr(>|t|)
Intercept	0.448	0.181	2.473	0.014
Treatment Assignment	0.084	0.067	1.254	0.211

4.5 Attrition Analysis

One serious methodological concern in Randomized Trials is that attrition can bias the estimates if we have differential rates between treatment and control units. Hence, the first test to check if attrition rates are balance is to run the following regression using the same estimators that I use for the 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 the 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 attest balance in attrition the parameter of interest ρ needs to be statistically equal to zero.

Next, I show the balance of attrition rates for Initial Offer and Ever Offer estimators. The DREO estimator reassembles the rates from the Ever Offer, because it is a particular case of the later.

4.5.1 Attrition Initial Offer

I start showing the Initial Offer estimator results. All model specifications appoint to a fine balance between treatment and control assignment as I fail to reject the null hypothesis of the t-test. See Table 4

Table 4 – Attrition Analyses Initial Offer

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks
Standard Reg. with Cov. FE	0.002	0.044	0.047	0.962	530	22
Reg Lin - Only Comunity FE	-0.002	0.044	-0.048	0.961	530	22
Reg Lin - All Cov.	-0.002	0.044	-0.046	0.963	530	22
IWE - Without Covariate	-0.002	0.043	-0.049	0.961	530	22
IWE - With Covariate	0.001	0.044	0.027	0.978	530	22
RWE - Without Covariate	-0.003	0.044	-0.070	0.944	530	22
RWE - With Covariate	0.001	0.045	0.032	0.974	530	22

Attrition rates seem to pass the first test, but there could still be patterns of attrition across treatment arms. Like before, the test is to regress the attrition indicator on treatment assignment, but this time also interact pre-treatment covariates with the treatment assignment variable. 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 we reject the null at the 10% level when including the three chosen pre-treatment covariates from our outcome regressions, raising issues towards a pattern of attrition between these covariates (F-test statistic is 2.6). The covariates are community leader visit, quality of the house roof, and household index for respiratory diseases. We further estimate alternative covariates to check if the problems persists and it does not. The problem seems to be with the variable “Respiratory diseases household index”, so I keep it in all my regressions along the thesis.

4.5.2 Attrition Ever Offer

Now I replicate all the above analysis using the Ever Offer estimator. For the first test of attrition, it seem to be a higher concern. In Table 5, estimates range from 6.7 to 9.1 percentage points difference in group’s attrition. Five out seven models register statistically significant differences. Therefore, the Ever Offer estimator is not only biased *per se*, but it also leads to biased results due to attrition [10].

Table 5 – Attrition Analyses Ever Offer

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks
Standard Reg. with Cov. FE	0.091	0.047	1.935	0.054	530	22
Reg Lin - Only Comunity FE	0.069	0.039	1.757	0.080	530	22
Reg Lin - All Cov.	0.080	0.042	1.916	0.056	530	22
IWE - Without Covariate	0.069	0.040	1.737	0.082	530	22
IWE - With Covariate	0.070	0.041	1.738	0.082	530	22
RWE - Without Covariate	0.067	0.052	1.293	0.196	530	22
RWE - With Covariate	0.070	0.052	1.333	0.182	530	22

4.6 Balance Pre-treatment Analysis

One final test to check if the randomized experiment is adequate is to show balance in pre-treatment covariates. If the lotteries are not biased, the randomization process should make treatment and control groups statistically similar for all the variables collected before the housing assignment. I use the model specification suggested by Lin (2013) to check balance in baseline covariates:

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

[10] I find no structural attrition across treatment arms in the EO estimator, but failing the first attrition test is sufficient to abandon this option

Notice in Table 6 that only three out of twenty-one baseline variables measured at the household level can be considered to be unbalanced at the 10% level of significance, only one at the 5% level, and none at the 1% level. However, by performing a conjoint F-test with all baseline covariates it is possible to reject the null hypothesis that all variables are equal to zero at the 10% level (p-value equals to 0.09). In Table 7, I do the same analysis, but use individual level variables. Only one out of 17 variables is significant at the 10% level and the conjoint F-test fails to reject the null hypothesis. Hence, it is possible to conclude that for the Initial Offer estimator pre-treatment balance is satisfactory, although a few variables were unbalanced.

Table 6 – Pre-treatment Covariate Balance - Household level - IO

Reg Lin - Only Comunity FE	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	mean
Household size	0.04	0.16	0.26	0.79	510	22	2.61
Average age of family	0.60	1.44	0.42	0.68	510	22	32.00
Dummy social security pension	-0.04	0.05	-0.80	0.42	510	22	0.63
Dummy Employment	-0.02	0.05	-0.45	0.66	510	22	0.59
Index Health Problem	0.05	0.06	0.75	0.45	510	22	0.48
Long lasting diseases index	0.01	0.03	0.41	0.68	510	22	0.31
Sanitary diseases index	0.00	0.04	0.08	0.94	510	22	0.28
Respiratory diseases index	0.00	0.03	-0.03	0.98	510	22	0.25
Disabilities index	-0.01	0.02	-0.53	0.59	510	22	0.10
Hospital visits index	-0.06	0.04	-1.44	0.15	510	22	0.56
Leader visit	0.08	0.04	1.97	0.05	510	22	0.35
Education level index	0.02	0.09	0.22	0.82	510	22	2.16
Year arrived community	0.28	0.34	0.81	0.42	510	22	2014.79
Intention to leave community	-0.06	0.04	-1.65	0.10	510	22	0.77
Location previous home	-0.10	0.04	-2.57	0.01	510	22	1.73
Ever evicted	0.00	0.03	0.10	0.92	510	22	0.10
Dummy Infrastructure problems	-0.01	0.05	-0.32	0.75	510	22	0.41
House quality index (roof)	-0.14	0.07	-2.00	0.05	510	22	2.86
House quality index (wall)	-0.11	0.07	-1.59	0.11	510	22	2.76
House quality index (floor)	0.03	0.07	0.38	0.71	510	22	3.01
TETO Vulnerability Index (Pontos)	0.01	0.21	0.06	0.95	510	22	6.70

Table 7 – Pre-treatment Covariate Balance - Individual level - IO

Reg Lin - Only Comunity FE	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	mean
Gender 1st respondent	0.09	0.05	1.82	0.07	510	22	1.38
Age 1st respondent	0.69	1.38	0.50	0.62	510	22	38.64
Race 1st respondent	0.13	0.09	1.47	0.14	510	22	3.97
Dummy Employment 1st respondent	0.04	0.05	0.85	0.40	510	22	0.42
Dummy Health Problem 1st	0.10	0.08	1.28	0.20	510	22	0.54
Dummy long lasting diseases 1st	0.05	0.05	1.01	0.32	510	22	0.38
Dummy sanitary disiases 1st	0.01	0.04	0.28	0.78	510	22	0.30
Dummy respiratory diseases 1st	0.02	0.04	0.35	0.73	510	22	0.28
Dummy disability 1st	0.00	0.03	-0.04	0.97	510	22	0.90
Hospital visits 1st	-0.05	0.05	-1.07	0.28	510	22	0.62
Financial Expexctation	0.06	0.04	1.25	0.21	510	22	0.75
Dream housing	-0.01	0.04	-0.28	0.78	510	22	1.80
Institutional afiliation	-0.01	0.03	-0.21	0.83	510	22	0.75
Authority claim-making	0.01	0.04	0.14	0.89	510	22	0.20
Leader claim-making	0.06	0.04	1.61	0.11	510	22	0.22
Elections 2018	0.00	0.03	-0.04	0.97	510	22	0.13
Elections 2016	0.03	0.03	0.86	0.39	510	22	0.12

5 Qualitative Methods

In the previous methodological chapters, I presented a mixed-methods framework for integrating the quantitative and qualitative methods. Now, I further describe the qualitative methods that complement the quantitative field experiment. Although in Cresswell's experimental mixed-methods design quantitative methods represent the core methodology, qualitative data can be crucial in many stages of the research. I briefly detail why and when I relyed on qualitative data in the present chapter.

5.1 Initial Stages

The first time that my research team and I used qualitative data was in the field experiment planning stage. The biggest challenges were threefold: (1) gather sufficient information on how to create a righteous and ethical selection of program beneficiaries; (2) start exploring research themes that were of mutual interest to TETO and our research team; (3) decide which variables to include in the baseline questionnaire. At this stage, we used an exploratory sequential research design because we used induction to begin formulating our hypotheses.

TETO's housing program is aimed only at poor residents living in extreme vulnerability conditions. Therefore, deciding which families merit receiving aid always raises ethical considerations. A random selection method seemed unfair for many fundamental stakeholders that work together with TETO. For many community leaders, volunteers, and residents our research was the first time that they heard about impact evaluation of social programs. Deciding by lotteries which families would benefit from the program seemed odd to many stakeholders. To circumvent resistance to the experiment and to comply with ethical measures we gathered qualitative data. This type of data helped us to better understand organizational and community dynamics that could create frictions towards implementing the lotteries. Two examples are salient. First, in organizational terms, volunteers are the ones who are responsible for giving the final word on who should receive treatment. However, before the research, there were no common guidelines for how volunteers should take this decision. In different TETO headquarters (Brazilian states) volunteers had varying criteria for deciding. This lack of clear processes would sometimes create attrition with community members that could raise ethical concerns. The second example is similar but reflects the role of community leaders who are the ones who guarantee TETO's access to slums. Some leaders could have too big discretionary power for appointing beneficiaries of the program. This could also raise ethical issues in certain moments.

To circumvent these and other dynamics, we first organized workshops in all of TETO's headquarters. The workshops were aimed at volunteers and the objective was to explain our initial plans for selecting families. In these workshops, we collected qualitative feedback on what would be better ways of conducting the lotteries. Insights from volunteers were fundamental for the design of the program implementation process. For instance, the decision of retaining priority houses for families that were the most vulnerable of the sample came from our discussions with volunteers. This type of "always takers" would receive the house before the randomization took place. This way, we avoided part of non-compliance before starting the lotteries. At the same time, we were more confident in maintaining ethical standards.

After workshops, we created focus groups with volunteers that were interested in contributing to the research. The focus groups had two objectives: decide mutual research interests and discuss possible control variables to include in the baseline questionnaire. I personally conducted five meetings. Each meeting had one or two themes of discussion that were pre-established by volunteers. All themes were necessarily related to housing policies and TETO's possible impacts. Before each meeting, I sent participants a short paper on the theme to be debated. I guided discussions based on the papers, but let arguments and points of view flow freely between volunteers. Focus groups meetings were recorded for future analysis.

The qualitative data collected in the focus groups was fundamental for many reasons. First, discussions on the possible impacts of TETO helped me and my research team to better formulate our hypothesis. Volunteers are very experienced in fieldwork and they knew better than us the context of TETO's intervention. They had incredible insights on possible mechanisms and potential outcomes. They also had good guesses on null effects and potential unexpected results of the program. Furthermore, their previous experiences guided the formulation of the baseline questionnaire. At a certain point, deciding which control variables should be included was a difficult task. What could be mediating and moderating variables? In some cases, we relied on theory, but in others, volunteers' experiences were determinant for making decisions.

5.2 Data Collection During Interventions

As mentioned in chapter 2, the housing intervention has many steps. I personally participated in each one of them and collected qualitative data most of the time. The methods that I used during the intervention process were mostly field observations followed by notes and reflections. In a sense, this is the point of the research that resembles the most an ethnographic work, as what matters the most are the insights from my experience in the field. Yet, I also relied on a qualitative survey that TETO conducted with community leaders when the Covid-19 crisis started to hit communities since I no longer could visit them myself (to comply with safety measures).

Taking part in the intervention consist of accompanying volunteers when they visit communities for any reason. I have not only built houses as a volunteer (more than once) but also visited selected families before construction day and after families were living in the new housing unit. In the pre-logistic meetings, I had the opportunity of realizing how residents organize themselves and divide tasks, what problems of coordination they face, and what happens when mistrust and lack of reciprocity arise. At the construction weekend, I could gather further insights into how families receive the intervention and experience it. On subsequent visits after treatment, it was possible for me to identify potential mediating and moderating factors.

Interviews with community leaders during the strike of the coronavirus were important in the sense that they have a broad understanding of the main difficulties that slum residents faced during the crisis. Confronting this type of qualitative data to the results of the econometric models enriches the analysis and contributes to interpreting quantitative findings. It is not always clear how a compilation of estimated coefficients acts together to form a coherent theory. Qualitative data helps to close the gaps of intricate (or ambiguous) quantitative impacts. Hence, this is the stage of the research when the convergent mixed-methods design contributes the most.

5.3 Follow-up Data Collection

In the final stages of the research, after at least one year of treatment exposure to the housing program, we conducted the second follow-up survey. At this point, we already had quantitative results of the first follow-up questionnaires and we decided to further investigate the mechanisms that could explain our causal results. Thus, we conducted 50 semi-structured interviews with slum residents that took part in our lotteries. The objective of this data collection was to understand the nuances of the treatment effects and better explain the gaps in our theories, which could not be addressed by our quantitative measures.

In Creswell's framework, this method could be considered a sequential explanatory design, as qualitative data came after quantitative models and the formulation of hypotheses. The confirmation or rejection of pre-established hypotheses followed a deductive inference logic. Interviews were aimed at complementing our theories in light of the causal explanations that were initially formulated.

To contrast the effects of treatment and non-treatment of the housing program, the strategy was to conduct interviews with both the treatment and the control group of residents. Thus, 25 interviews for each group. Because local dynamics are usually present in each community, we attempted to interview at least one control and one treated family in each slum. We also tried to maintain a balanced number of male and female interviewees. When we could not conduct interviews in a small community, we transferred interviews to larger ones. Table 8 resumes the balance between treatment/control through communities.

Table 8 – Interviews conducted in each community

	QUALITATIVE INTERVIEWS		
	TREATMENT	**CONTROL**	**Observation**
COMMUNITY NAME	**Interview Conducted**	**Interview Conducted**	
BEMFICA	2	2	
CAPADOCIA	1	1	
CAROLINA DE JESUS	1	2	
CIC	1	1	
COLOMBIA		1	Unable to interview treatment
FAVORITA	1	1	
FAZENDA CAJUEIRO	1	1	
FAZENDINHA	2	2	
GUARANY	1		Unable to interview control
MANUEL FAUSTINO	1	1	
PARQUE DAS MISSOES	1		Unable to interview control
PATRIA LIVRE	1	1	
PIQUETE	1	1	
RAMPINHA		1	Unable to interview treatment
RIBEIRAO	2	2	
TERRA NOSSA	2	2	
TIRADENTES	1	1	
TRIBO	1	1	
VERDINHAS		1	Unable to interview treatment
VILA BEIRA MAR	1		Unable to interview control
VILA NOVA - COLOMBO	1		Unable to interview control
VILA VITORIA	2	2	
ZEQUINHA	1	1	
TOTAL INTERVIEWS	**25**	**25**	

Part III

Emergency Housing Policies: discussing impacts

6 Impacts on Covid-19 preventive measures

6.1 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 (BOURDIEU; RICHARDSON, 1986; COLEMAN, 1988; PUTNAM; LEONARDI; NANETTI, 1994; FUKUYAMA, 1995).

Although a growing literature addresses how variations in social capital may affect citizen's adherence to health measures (BAI; JIN; WAN, 2020; BARRIOS et al., 2020; BARTSCHER et al., 2020; BORGONOVI; ANDRIEU, 2020; DING et al., 2020; FRASER; ALDRICH, 2020; KOKUBUN, 2020; KUCHLER; RUSSEL; STROEBEL, 2020; MIAO; ZENG; SHI, 2020; VARSHNEY; 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. In the present chapter, I evaluate how TETO's 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; 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 chapter 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).

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

As metioned in previous chapters, the NGO 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 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; JIN; WAN, 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; JIN; WAN, 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.

6.2 Background and Hypotheses

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 [2], 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; ORSINI, 2020), unorganized physical distancing and self-quarantine measures disrupted production chains, especially in the service sector, where most of the jobs 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 [3]. 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 first round follow-up interviews (unemployment rate in Brazil was around 12.5% at the same period [4]).

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; 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

2 FioCruz epidemiological bulletin of Slums: <https://portal.fiocruz.br/sites/portal.fiocruz.br/files/documentos/boletim_socioepidemiologicos_covid_nas_favelas_1.pdf>

3 Locomotiva Institute bulletin: <https://www.slideshare.net/ILocomotiva/pandemia-na-favela?from_action=save>

4 see PNAD-COVID19 (IBGE-BRASIL, 2020): <https://covid19.ibge.gov.br/pnad-covid/>

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; 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 (YIP et al., 2007; CARPIANO; MOORE, 2020; WU, 2020). In the Covid-19 studies, for instance, (BAI; JIN; WAN, 2020) compare the impacts of civic norms and social networks. They find that while civic norms facilitate cooperation and 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; JIN; WAN, 2020; BARRIOS et al., 2020; BARTSCHER et al., 2020; BORGONOVI; ANDRIEU, 2020; DING et al., 2020; FRASER; ALDRICH, 2020; KOKUBUN, 2020; KUCHLER; RUSSEL; STROEBEL, 2020; MIAO; ZENG; SHI, 2020; VARSHNEY; 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; MORSE; TSAI, 2017; TSAI; MORSE; BLAIR, 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 (YIP et al., 2007; CARPIANO; MOORE, 2020; WU, 2020).

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 CDD 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; JIN; WAN, 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 U.S.'s Moving to Opportunities Experiment reduced voter turnout among adults by breaking their ties to local communities. (BARNHARDT; FIELD; PANDE, 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; 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 (about one year when first round follow-up had been conducted) 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.

6.3 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, Liebman & Katz (2007) propose, 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; 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 [5]. 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 [6].

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; LIEBMAN; KATZ, 2007). In all tables Model 1 holds for the pre-specified fixed-effects specification, Model 2 is Lin's specification, and Model 3 follows Gibbons and coauthors.

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

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

I begin by showing results for the preventive behavior hypothesis (H1). The first row of Table 9 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 0.139 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 0.240 to 0.245 standard deviations more than the average of the control group. When it comes to washing hands, beneficiaries also seem to comply more (from 0.144 to 0.170 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 aldermen 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 10 only marginally confirm the hypothesis that the program would increase solidarity from benefited families. Although coefficient are positive, p-values are close to the 10% level of significance threshold, leading me to conclude that the effects are indeed positive, but with the caveat that impacts are not global nor substantial. Most individual outcomes are 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 only marginally 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 11 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. I further discuss these results in light of the qualitative data.

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 claim-making are mostly null results, while solidarity is marginally positive. 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 12 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.

Table 9 – Intention to Treat Estimates (Initial Offer) - Preventive Behavior

Dependent Variable	Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Preventive Behavior Index	-0.256	0.139	0.164	0.153
	[0.291]	(0.122)	(0.125)	(0.123)
		(0.258)	(0.192)	(0.213)
Number of times leaving home (scale)	-1.403	0.013	-0.018	-0.013
	[1.088]	(0.120)	(0.119)	(0.117)
		(0.915)	(0.881)	(0.914)
Number of reasons to leave home (scale)	-1.497	0.112	0.141	0.131
	[0.528]	(0.114)	(0.116)	(0.117)
		(0.330)	(0.226)	(0.262)
Hours out of home (scale)	-2.479	0.098	0.086	0.080
	[1.599]	(0.119)	(0.115)	(0.114)
		(0.410)	(0.457)	(0.483)
Soft social distance measures (dummy)	0.119	0.240	0.245	0.241
	[0.324]	(0.130)*	(0.135)*	(0.133)*
		(0.066)	(0.070)	(0.070)
Sanitary/cleaning measures (scale)	2.044	-0.013	0.012	0.011
	[1.043]	(0.108)	(0.098)	(0.098)
		(0.902)	(0.899)	(0.908)
Washing hands (dummy)	0.843	0.144	0.158	0.170
	[0.364]	(0.099)	(0.093)*	(0.093)*
		(0.146)	(0.090)	(0.068)
Using mask (scale)	-1.089	-0.091	-0.043	-0.049
	[0.300]	(0.104)	(0.099)	(0.102)
		(0.382)	(0.665)	(0.628)
Plan on leaving home next week (dummy)	0.266	-0.082	-0.074	-0.097
	[0.411]	(0.116)	(0.110)	(0.108)
		(0.477)	(0.502)	(0.367)
Stay more ate home (dummy)	0.894	0.020	0.011	0.010
	[0.308]	(0.121)	(0.118)	(0.117)
		(0.870)	(0.929)	(0.932)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefficient, robust standard error, and p-value, in that order. Robust standard errors, clustered at household 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; SERRATO; URBANCIC, 2018). Outcome variables have been standardized according to (KLING; LIEBMAN; KATZ, 2007). Fixed-Effects are lottery dummies for each community-lottery.

Table 10 – Intention to Treat Estimates (Initial Offer) - Solidarity

Dependent Variable	Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Ties and Solidarity index	-1.228	0.208	0.192	0.196
	[0.446]	(0.109)*	(0.118)	(0.117)*
		(0.058)	(0.103)	(0.094)
If ill, can count on family	-1.711	0.020	0.006	0.035
	[0.787]	(0.115)	(0.111)	(0.110)
		(0.864)	(0.959)	(0.750)
If ill, can count on friends	-2.264	0.270	0.213	0.226
	[0.729]	(0.107)**	(0.111)*	(0.112)**
		(0.012)	(0.055)	(0.044)
If ill, can count on community members	-2.013	0.118	0.108	0.108
	[0.699]	(0.111)	(0.122)	(0.121)
		(0.292)	(0.374)	(0.375)
If ill, can count on church members	-1.893	0.262	0.292	0.290
	[0.687]	(0.105)**	(0.103)***	(0.102)***
		(0.013)	(0.005)	(0.005)
If ill, can count on community leaders	-1.946	0.153	0.116	0.115
	[0.753]	(0.117)	(0.116)	(0.116)
		(0.191)	(0.317)	(0.320)
If ill, can count on alderman	-2.698	-0.104	-0.066	-0.064
	[0.567]	(0.117)	(0.17)	(0.115)
		(0.375)	(0.573)	(0.576)
Provided aid to other families	1.475	0.150	0.159	0.143
	[1.503]	(0.113)	(0.122)	(0.124)
		(0.187)	(0.192)	(0.248)
Received aid from other families	1.225	-0.057	-0.082	-0.093
	[1.523]	(0.100)	(0.095)	(0.096)
		(0.564)	(0.390)	(0.334)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefficient, robust standard error, and p-value, in that order. Robust standard errors, clustered at household 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; SERRATO; URBANCIC, 2018). Outcome variables have been standardized according to (KLING; LIEBMAN; KATZ, 2007). Fixed-Effects are lottery dummies for each community-lottery.

Table 11 – Intention to Treat Estimates (Initial Offer) - Claim making

Dependent Variable	Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Claim-making index	-1.126	-0.011	-0.012	-0.012
	[0.180]	(0.105)	(0.105)	(0.104)
		(0.913)	(0.912)	(0.909)
Claimed aid from government	-1.936	-0.002	-0.011	-0.030
	[0.244]	(0.117)	(0.108)	(0.100)
		(0.988)	(0.918)	(0.760)
Claimed aid from community leader	-1.799	-0.034	-0.062	-0.067
	[0.400]	(0.108)	(0.106)	(0.108)
		(0.757)	(0.556)	(0.536)
Claimed aid from NGO	-1.782	-0.003	-0.043	-0.036
	[0.412]	(0.103)	(0.098)	(0.096)
		(0.979)	(0.659)	(0.706)
Help to receive corona voucher	-1.617	0.116	0.174	0.171
	[0.434]	(0.115)	(0.116)	(0.116)
		(0.317)	(0.134)	(0.140)
Received aid from NGO	-1.441	0.046	0.011	0.026
	[0.492]	(0.109)	(0.109)	(0.109)
		(0.669)	(0.923)	(0.811)
Received other government aid	1.817	-0.155	-0.099	-0.095
	[0.387]	(0.115)	(0.112)	(0.110)
		(0.180)	(0.376)	(0.387)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefficient, robust standard error, and p-value, in that order. Robust standard errors, clustered at household 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; SERRATO; URBANCIC, 2018). Outcome variables have been standardized according to (KLING; LIEBMAN; KATZ, 2007). Fixed-Effects are lottery dummies for each community-lottery.

6.4 Mechanisms: quantitative and qualitative evidence

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.

Table 12 – Intention to Treat Estimates (Initial Offer) - Evaluation of key actors

Dependent Variable	Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Evaluation index	-1.746 [0.692]	0.446 (0.098)*** (0.000)	0.456 (0.101)*** (0.000)	0.449 (0.100)*** (0.000)
How evaluate leaders	-2.754 [1.683]	0.216 (0.100)** (0.032)	0.213 (0.108)** (0.049)	0.192 (0.107)* (0.073)
Knowledge about TETO	0.902 [0.297]	0.167 (0.093)* (0.074)	0.178 (0.101)* (0.078)	0.174 (0.103)* (0.090)
How evaluate TETO	-1.571 [1.034]	0.298 (0.093)*** (0.001)	0.322 (0.087)*** (0.000)	0.323 (0.086)*** (0.000)
How evaluate Government	-3.561 [1.647]	0.217 (0.112)* (0.055)	0.204 (0.111)* (0.066)	0.215 (0.111)* (0.053)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefficient, robust standard error, and p-value, in that order. Robust standard errors, clustered at household 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; SERRATO; URBANCIC, 2018). Outcome variables have been standardized according to (KLING; LIEBMAN; KATZ, 2007). Fixed-Effects are lottery dummies for each community-lottery.

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 first 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 13 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 two and three of Table 13 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 (0.150 to 0.158 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 tend to be even larger.

Table 13 – Intention to Treat estimates (Intention to Treat) - Mobility Mechanism

Dependent Variable	Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Remains in the community	0.907 [0.291]	0.108 (0.100) (0.281)	0.158 (0.087)* (0.070)	0.150 (0.090)* (0.097)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefficient, robust standard error, and p-value, in that order. Robust standard errors, clustered at household 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; SERRATO; URBANCIC, 2018). Outcome variables have been standardized according to (KLING; LIEBMAN; KATZ, 2007). Fixed-Effects are lottery dummies for each community-lottery.

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 14 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 still keeping 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.

Qualitative data further illustrates the nature of social connections promoted by TETO. During the pandemic, networks of mutual help were fundamental in providing aid for families whose providers became unemployed. A key element in such local networks were the community leaders who work with TETO. In our interviews, we identify how the housing program brings benefited families closer to leaders. TETO and other organizations have raised campaigns funds to buy aid packages for families in need, and leaders were responsible for distributing those packages. Thus, having a close relationship with leaders could facilitate the delivery. Not only do residents get to know the leaders better because of the housing program also the opposite direction is true. Especially in large communities, it is hard for leaders to reach everyone. The following dialogue illustrates this point:

> *Interview Nm. 26 (Treatment unit)*
>
> Q -As a leader, has she helped you in anything besides this house issue?
>
> A -Yes, she helped me. When we went to build the house, she came to talk to me, and now, with the pandemic, the donations of what we needed and such.
>
> Q -What kind of other help has she already given you, besides the house?
>
> A – Basic basket, when the NGO Teto manages to raise funds, then she always comes to check if we need it. And then it helps.
>
> Q -And she's still helping you then?
>
> A -Yes.

We also asked what beneficiaries could remember from their meetings with TETO's volunteers. What were the main topics of the conversations [7]? 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. In our interviews, we asked respondents more about what issues were discussed in the meetings other than how to build the houses. The next two dialogues show the types of social interaction that happen in the meetings:

> *Interview Nm. 31 (Treatment unit)*

[7] This was a multiple choice question.

> Q - In the construction process, before starting construction, were there meetings, workshops, conversations addressing topics in general?
>
> A - There was, there was, that was.
>
> Q - What was the theme? What were you talking about?
>
> A - I remember one person talking about prejudice, about those who live in the community, these things. Then there was a racism thing too, these prejudices of today, right? About sexualism. There was, yes. There were games. I wasn't there in all of the games, so I remember I went indeed with the volunteers.
>
> *Interview Nm. 15 (Treatment unit)*
>
> Q – And what did you think of these meetings, of this workshop, what did you think? It was good, wasn't it?
>
> A - Very good, very good.
>
> Q – Why?
>
> A – How do we say? It was good because we can get to know each other, the ideas of others as well, which until then, everyone is silent, each one alone for themselves, and so we could get to know the will of one, the will of the other, what encourages them, what incentivizes them. And it was great, I liked it.
>
> Q – And how did you organize yourselves to build the houses? Each one did one thing, how was it?
>
> A – Each one did something, I stayed in the kitchen myself, I was one of the cooks, me, Maria and Sandra.

Interviews reinforced our previous perceptions that meetings are not only about teaching families how to build the houses. They connect people in some stances, even though this connection can be ephemeral. Meetings also serve as an opportunity for enhancing values of cooperation and community engagement among participants. They practice collective action by dividing task and asking friends, family and neighbors to help in the construction. Naturally, the ties that get strengthen the most are those previous connections with friends and members of institutionalized organizations (such as churches). Quantitative and qualitative analysis confirm this.

> *Interview Nm. 42 (Treatment unit)*
>
> Q – Well, where did you spend the night when after you destroyed your shack?
>
> A – I stayed at my neighbor's house, he lived on rent and he was still building a new home, right? He was finishing building his house on his land. Then he made two rooms, I stayed in one of the rooms.
>
> Q – Was it? You asked and he consented? How was that?
>
> A – Yeah, we asked, right, to be able to take the furniture there, then he helped us, because he's evangelical too, right? He is a church brother. Then he helped, then we took the furniture there, then I stayed there for two days, after two days I built my TETO house and took my furniture back.
>
> Q – The house also comes without electricity. Did you manage to install electricity afterward?

> A - Yes.
>
> Q – Who helped you?
>
> A – A brother from the church helped my husband.

> *Interview Nm. 31 (Treatment unit)*
>
> Q - Do you remember, for example, who was responsible for which task? Who was going to build, who was going to cook. Had these divisions?
>
> A - About my house, there was. The Teto volunteers and the community volunteers, there were about five people. They went there, and: "Here, look, a little TETO shirt for you, you're helping". And even people who won the house, most were people who won the house before from TETO, you know? They were there helping me.

In sum, 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 of Covid-19 have null results. In the next section I use previous theories to discuss more about this.

Table 14 – Intention to Treat estimates - Trust Mechanism

Dependent Variable	Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Trust index	0.683 [0.884]	0.206 (0.114)* (0.072)	0.180 (0.107)* (0.092)	0.186 (0.106)* (0.078)
Trust government information	0.523 [1.113]	0.089 (0.113) (0.432)	0.068 (0.112) (0.546)	0.084 (0.114) (0.457)
Trust community leaders' information	0.841 [1.115]	0.241 (0.109)** (0.028)	0.220 (0.096)** (0.023)	0.213 (0.095)** (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 coefficient, robust standard error, and p-value, in that order. Robust standard errors, clustered at household 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; SERRATO; URBANCIC, 2018). Outcome variables have been standardized according to (KLING; LIEBMAN; KATZ, 2007). Fixed-Effects are lottery dummies for each community-lottery.

6.5 Discussion

Social capital may be central to increase compliance with government directives or mitigating the fallout during pandemics or natural disasters. In the present chapter, I test to what extent this view stands. During the housing 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 or claim-making among benefited families. Solidarity is only marginally impacted and mostly due to higher bonds with close relationships, like friends and members of institutionalized organizations (e.g. church). The intention-to-treat estimates are only statistically positive for a few measures 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; JIN; WAN, 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; JIN; WAN, 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; MORSE; TSAI, 2017; TSAI; MORSE; BLAIR, 2020). Because leaders lack sanctioning powers and in the best case scenario are limited to make suggestions for community members to take precautions, actual effects are 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; GÓMEZ, 2020) - Brazil is also among the worst countries in the world in terms of spreading fake news related to Covid-19 (BIANCOVILLI; 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; LANDOLT, 1996; CLEAVER, 2005). In fact, social capital could even have negative consequences (PORTES; 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 chapter 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 solidarity, prevention, 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.

7 Impacts on psychological well-being

7.1 Introduction

The link between poverty and psychological well-being has become a growing field in economics and behavioral sciences. Low material standards may have particular psychological consequences that lead to poverty traps. Poverty can cause stress and negative affective states that generate "bad" economic behaviors, such as short-sighted and risk-averse decision-making (HAUSHOFER; FEHR, 2014). These behaviors contribute to a vicious cycle that traps people in poverty. Thus, understanding the mechanisms that may break this chain of misfortunes is fundamental to overcome poverty conditions. In the present chapter, I investigate how TETO's intervention may lead to "better" psychological well-being, which may lead to long-run improved economic status.

In previous chapters, I have shown that the intervention from TETO increases house quality and social interaction. This is different from other housing programs that improve house quality but destroy networks and social interaction. I show in this chapter that these features of TETO's intervention improve beneficiaries' psychological well-being, something that should not be neglected by policy makers. Theoretically, I try to compare TETO to other housing programs in a comparative perspective.

I also show that the positive impacts on subjective well-being are conditional on context: our heterogenous effects show that different levels of income matter even among the very poor. Impacts tend to be higher for lower levels of income. I also find no evidence of immediate hedonic adaptation, a feature that is commonly explored in the literature on subjective well-being.

7.2 Housing, Health, Happiness: a literature review

Scholars have long been curious about the influences of the living environment on humans' mental health and well-being. The extent to which where we live and where we interact with each other can compromise our psyche is an object of thought from various fields. From medicine and psychology to architecture and urban design, going through economics, public policy, and many more, the subject flourishes with interdisciplinarity and diversity (EVANS, 2003; CATTANEO et al., 2009; MAAS et al., 2009; FOYE; CLAPHAM; GABRIELI, 2018). To avoid the risk of getting lost in such broad literature, this section first clearly sets the boundaries of what is meant by living environment, housing, and mental health (or psychological well-being). In my definitions, I try to balance precision and enough conceptual flexibility because I want to preserve the contributions of interdisciplinary fields.

First, I define the living environment as the immediately near surrounding space outside of a person's house where she (he) can have social and physical interactions daily. By physical interactions, I mean the influences of the physical world. That is, living close to inadequate sewage disposal, suffering the consequences of storms and floods, or, even in a positive sense, enjoying a nice walk through the gardens of a nearby park. By social interaction, I mean direct and indirect connections with other people living in the same space. For the following discussions, I usually consider this space as being neighborhoods or communities (slums).

Second, to the housing concept, I focus on exploring two aspects present in the literature: (i) housing as a social policy/program; (ii) housing as a private good in economic terms. Related to the former, I compare TETO's program to other housing programs that could have been promoted either by governments or by civil society groups. For the latter, I adopt the economics perspective to emphasize the relationship between material goods (house) and the pursuit of well-being, which is closely related to the concept of utility. In classic economic terms, a good is what increases one's private utility (READ, 2007). In the following literature review, many articles implicitly consider better housing as an economic good in this sense.

Third, there is much confusion with the use of the term mental health. For the medical literature, a mentally healthy individual is one who lacks illnesses such as depression, anxiety, or related symptoms. However, some studies use the term mental health even when lacking concrete clinical diagnoses. The confusion may be due to the proximity of psychiatric disorders (medical literature) and varying measures of psychological distress and negative affect (psychology literature). In fact, there could be causal linkages between psychiatric disorders and psychological distress (KAMIENIECKI, 2001; BEAGLEHOLE et al., 2018). I consider both psychiatric disorders and psychological distress as being part of a broad concept of mental health. Yet, throughout the text, I tend to separate them and focus on the psychology literature. Specifically, the subject of most interest is psychological well-being.

Although there have been various reviews linking mental health and housing (THOMSON; PETTICREW; MORRISON, 2001; CLARK; STANSFELD; CANDY, 2006; GIBSON et al., 2011) fewer studies have focussed on psychological well-being (CLAPHAM; FOYE; CHRISTIAN, 2018). From an economics and public policy perspective, there are many advantages of concentrating analysis on measures of well-being. If the goal is to address the impacts of certain policies, it makes total sense to quantify increases or decreases in beneficiaries' well-being. In the housing literature, Clapham (2010) has argued in favor of using well-being to judge the success of such policies. Economists and other social scientists have at least two ways of measuring individual well-being and, by extent, social well-being. Revealed preferences are long considered to be the preferential method (KAHNEMAN; KRUEGER, 2006). However, the growing field of behavioral economics, policy psychology, and correlates have increasingly been using stated preferences to measure welfare (KAHNEMAN; KRUEGER, 2006; READ, 2007; BENJAMIN et al., 2014). If choices can be inconsistent or sometimes even irrational, asking people how they feel and make decisions should add to the debate. By using valid and reliable measures of subjective assessments, economists are succeeding in showing the advantages of this method[1]. As Kahneman & Krueger (2006) point out, self-reported measures of well-being could be useful complements to traditional welfare analysis because they are direct and simple. They further argue in favor of shifting the importance of income in determining a person's welfare evaluation towards his or her evaluation of relative rank in society.

One measure that has gathered increased attention is the so-called subjective well-being (SWB). Although subjective well-being may resemble the economic concept of utility, psychologists adopt a different definition. In Kahneman and Kruger's terms: "subjective well-being measures features of individuals' perceptions of their experiences". Diener et al. (1999) consider subjective well-being as "a broad category of phenomena that includes people's emotional responses, domain satisfactions and global judgments of life satisfaction". From the myriad of possible formulations, three components of SWB prevail among previous studies on housing policies. The first, and perhaps most common, is life evaluation, sometimes also known as life satisfaction, which refers to the thoughts that people have when they think about their life (KAHNEMAN; DEATON, 2010). The second is emotional well-being, which addresses people's everyday emotional experiences - the quality and frequency of joy, sadness, happiness, stress, anxiety, and other feelings (KAHNEMAN; DEATON, 2010). Finally, some authors also evaluate housing projects by asking beneficiaries the extent to which they are satisfied with the quality of their houses (floor, roof, walls, and other features of their residency). This subjective evaluation is called domain satisfaction because it narrows evaluations towards certain aspects of life (DIENER et al., 2018).

Dividing subjective well-being into different components may advance our knowledge because they are subject to varying psychological processes that may lead to divergent outcomes (KAHNEMAN; DEATON, 2010; DIENER et al., 2018). For instance, because emotional well-being deals with emotions and moods, it can be highly reactive to external influences, but at the same time quickly adaptative. On the other hand, life and domain satisfaction work through cognitive processes that lead to less automatic evaluations ((LUHMANN et al., 2012)). Accordingly, life events such as marriage should trigger a

[1] See Diener, Inglehart & Tay (2013) for a discussion of validity and reliability. The authors argue that if measures are valid and reliable self-reports may show good correlations to some types of revealed preferences. See also (EVANS; WELLS; MOCH, 2003) for a similar discussion.

higher response in the affective component in the short-term with diminishing effects in the mid-term, while cognitive response should last longer due to reflexive thoughts about life (in other words, hedonic adaptation should be faster for emotional well-being).

However, if it is theoretically and empirically clear that different components of SWB usually lead to distinct outcomes, scholars find mixed evidence on the direction and intensity of estimates. After a thorough meta-analysis of subjective well-being and adaptation to life events, Luhmann et al. (2012) conclude that the rate of adaptation is not always faster for emotional well-being, it actually depends on the type of life event being studied (e.g., marriage, bereavement, reemployment, retirement, childbirth). The debate around the moderating effects of income on SWB also highlights why we still need more theoretical formulations and empirical findings. Kahneman & Deaton (2010) find that a high income predicts better life satisfaction, but effects on emotional well-being are null beyond the threshold of $75,000 per year. In contrast, Stevenson & Wolfers (2013) find no income limit for increases in their measures of subjective well-being. In fact, the debate on whether money buys happiness is far from settled (CLARK; FRIJTERS; SHIELDS, 2008; HEADEY; MUFFELS; WOODEN, 2008; KAHNEMAN; DEATON, 2010), but nuanced views of the mechanisms underlying SWB have been recently pushing the field further. I now turn to the housing literature to show how the three components of SWB appear in previous studies and how to fill possible gaps.

Housing policies can either relocate targeted beneficiaries from low living standard places into new neighborhoods or promote *in situ* improvements at the current land plot. They can also secure homeownership or provide vouchers that subsidize rents. Most programs have a mix of these policies, which may potentially influence mental health and well-being outcomes in their own manners.

Perhaps, the most well-studied housing program is the Moving to Opportunity (MTO), which randomly selected American families to receive subsidized housing vouchers to live in low-poverty areas. Many studies have shown differences between control families that remained in poor neighborhoods and the treated families that moved to the rich regions of the evaluated cities. Leventhal & Brooks-Gunn (2003) find significant short-term effects (3 years) of less psychological distress among parents who moved to low-poverty neighborhoods and significantly lower rates of anxiety/depression and dependency problems among treated boys. Kling, Liebman & Katz (2007) also find positive effects on mental health measures among treated adults, but after 4-7 years (longer run). They also find positive effects for female youth and negative effects for male youth in a series of outcomes. In a more recent study, Nguyen et al. (2016) reinforce the role of age and gender in their findings. They report more beneficial treatment effects on psychologic distress among girls, but find heterogeneous effects among treated adolescents. Taken together, all these studies reveal that the direction of neighborhood effects is not obvious. Adults and children have specific impacts, while male adolescents react differently than females to changes in their surrounding social structures (living environment). Although all authors give plausible explanations for these results, they still need more evidence on the underlying mechanisms and processes. The subjective well-being literature could help disentangle these processes, but as far as my review goes, I could only find one study that relates MTO to SWB. Ludwig et al. (2012) show positive long-term effects (10-15 years) of MTO on SWB, but they only use life satisfaction measures.

Critics of relocating programs such as MTO argue that this type of program risks disrupting social ties and networks that people create at the neighborhood level (BARNHARDT; FIELD; PANDE, 2017). Weakening social interaction would result in the loss of informal insurance. Consequently, people could not only suffer socio-economic impacts but also negative psychological shocks, as social relationships have been thought of as being strongly correlated to SWB (ARGYLE et al., 1999; MYERS, 2003; DEVOTO et al., 2012). The alternative, improving local amenities and housing quality without relocation, should supposedly solve the problem because people could stay in the communities where they belong. However, it is also not clear whether these initiatives always work. At most, the evidence seems to be mixed.

Moore et al. (2018) have systematically reviewed how changes in the built environment influence mental health and well-being. Their review was divided between studies on "urban regeneration" and "improving green infrastructure". They find no evidence of built environment impacting mental health and only weak positive influences on well-being (quality of life measures). Concerning housing quality improvements, Cattaneo et al. (2009) find that replacing dirt floors with cement floors in Mexico significantly increased the welfare of adults. On the other hand, Foye (2017) finds only weak evidence of the size of living space increasing subjective well-being. Effects were driven by men while women experienced no effects. Both studies use measures of life satisfaction and domain satisfaction toward house quality, but only Foye's study investigates emotional well-being. In an older review, Evans, Wells & Moch (2003) conclude that most studies suggest a positive correlation between housing quality and psychological well-being. Yet, unlike Foye (2017) and Cattaneo et al. (2009), many of the reviewed studies rely on observational data and lack more robust methodologies.

Policies that promote homeownership may also increase happiness and well-being. In this case, opposing mechanisms may be at stake. In certain contexts, housing tenure represents higher social status (GURNEY, 1999; LEGUIZAMON, 2010; FRANK, 2013; FOYE, 2017). Recent findings from the SWB literature suggest that some of the underlying psychological mechanisms are relational. People tend to compare themselves to their peers before making self-assessments of quality of life and life satisfaction (CARBONELL, 2005; LUTTMER, 2005; CLARK; FRIJTERS; SHIELDS, 2008; CLARK; WESTERGÅRD-

NIELSEN; KRISTENSEN, 2009). In this sense, the house would then become a positional good (FOYE; CLAPHAM; GABRIELI, 2018). Moreover, cultural contexts play a fundamental role in determining social standards that need to be fulfilled in a happy life ((DIENER; DIENER, 1995); (OISHI et al., 2009)). Thus, in some cultures, housing tenure could increase SWB due to social status in relation to others. However, homeownership could also have unintended consequences. For instance, pursuing homeownership can compromise one's financial situation, leading to mental health deterioration. Zumbro (2014) found that becoming a homeowner increases the life satisfaction of german families with a low financial burden but decreases when the financial burden is high. In sum, after a thorough review, Clapham, Foye & Christian (2018) conclude that homeownership has mixed effects on SWB.

Finally, in any type of the above policies mediating factors play a major role in the impacts of mental health. I highlight at least two that are intrinsically related to poverty since TETO's program focuses on the urban poor. People living in substandard housing conditions invariably suffer from insecurity and lack of control of their lives. Insecurity could not only be related to crime exposure but also other types of land and tenure insecurity. Dwellers that lack tenured housing may suffer more from evictions and show more geographic mobility, which has been shown to be a factor that correlates with psychological distress, especially for children (BARTLETT, 1998; BRONFENBRENNER; EVANS, 2000). Lack of adequate sanitation infrastructure and the threat of physical hazards like storms and floods can also engender anxiety, worry, and stress (WELLS; EVANS, 1996; WELLS NANCY M & EVANS, 2003). To what respects life control, poor housing may reduce mastery and contribute to a general sense of helplessness ((EVANS; WELLS; MOCH, 2003)). All of these together could increase feelings of low self-efficacy ((EVANS; WELLS; MOCH, 2003)).

In TETO's intervention, many of these mechanisms may be at play. Two recent studies that also evaluate the impacts of TETO have found positive causal effects of the program on beneficiaries' subjective well-being in three South American countries (Mexico, Uruguay, and El Salvador). Galiani et al. (2017) finds that TETO increases overall life satisfaction and satisfaction with housing infrastructure (domain), while Galiani, Gertler & Undurraga (2018) further investigates how long these effects last. Galiani, Gertler & Undurraga (2018) conclude that after 28 months the positive effects completely vanish due to hedonic adaptations. In the next section, I discuss how my findings may theoretically and empirically complement the above articles. One thing to keep in mind is that both articles emphasize cognitive measures of SWB. They ask questions about general life satisfaction and domain satisfaction related to house components, such as the floor, roof, windows, and others.

7.3 Theoretical Conceptions of TETO Impact on Subjective Well-being

The housing program sponsored by the NGO TETO has the interesting feature of being an *in situ* house improvement intervention and at the same time a social enhancing activity. As better explained in previous chapters, TETO promotes social interaction and networks among beneficiaries. By the end of the program, it is expected that participants successfully cooperate with neighbors and volunteers to deliver all the houses on time. Through this process, those who receive the new housing unit may learn new social skills that are valuable for their lives after the end of the program. The combo of higher housing quality and better social interaction may impact the mental health of families.

Previous studies on the effects of TETO's intervention focus on the quality improvement of the housing units (GALIANI et al., 2017; GALIANI; GERTLER; UNDURRAGA, 2018). In the present study, I expand the understanding of how TETO may improve well-being among the targeted families. I also change focus on the cognitive components of subjective well-being towards the more emotional components. Further exploring the differentiating mechanisms of psychological well-being is precisely the call that recent articles on the topic have made (DIENER et al., 2018). More than focusing on housing quality, I also test to what extent increasing social and community ties could work as possible mechanisms. In addition, I attempt to compare my results to Galiani, Gertler & Undurraga (2018) in themes such as hedonic adaptation and moderating effects of income.

The first necessary theorization is the role of housing quality, which is the primary goal of the NGO. The literature review suggests that indeed better house quality improves SWB, but the majority of the articles only ask questions that are directly related to this domain. Most notably, when questions of general life satisfaction appear, they usually follow up from housing domain inquiry. This could lead to instrumental bias in the composition of questionnaires ((KAHNEMAN; KRUEGER, 2006)). Thus, I test whether housing quality is also linked to emotional well-being without priming respondents with housing satisfaction questions.

There are good reasons to believe that better house quality improves cognitive as well as emotional subjective well-being. Which component should be more sensitive is still an open question. It could be the case that the aspect of the house as being an asset in one's life would trigger a cognitive response as in the income literature (CLARK; FRIJTERS; SHIELDS, 2008; DIENER et al., 2010). People always have the option of selling the house for a higher price after quality improvements or if local amenities rise the local market demand curve. In my conversations with TETO's staff and volunteers, I discovered that it is not completely uncommon for beneficiaries to sell their houses after a while. On the other hand, people can have emotional attachments to their homes, which would raise positive feelings of enjoyment and gratification. Furthermore, concerns of insecurity towards basic needs could be correlated to negative feelings and emotional pain (KAHNEMAN; DEATON, 2010). Because TETO does provide minimum housing quality standards such as having a floor other than dirt and a protective roof, benefited families may experience a reduction in negative feelings due to less exposure to physical risks (floods, storms, sanitary diseases, rat and insects infestations). Thus, cognitive components of SWB should be associated with the economic aspects of having a house while the emotional component should be associated with the fulfillment of basic needs (DIENER et al., 2010).

The theorization that social interaction affects subjective well-being fits especially well in TETO's program. People that cultivate strong ties with friends, family, and community members, the types of bonds that TETO hopes to enhance, could become relatively happier than less connected individuals. As Lucas et al. (2000) point out, in a wide variety of countries positive affect is associated with sociability and affiliation. The happiest individuals are usually the ones who have supportive social relations (DIENER; SELIGMAN, 2002). By receiving a new home, benefitted families may increase social interactions through at least two mechanisms. The program itself allows families to get better connected with community leaders and other neighbors that also receive the houses. Evidence suggests that some recipients also keep in touch with TETO's volunteers. Additionally, previous networks may be strengthened because close relatives are the ones to whom beneficiaries ask for help on the construction day and in the pre-logistic days before that. The second mechanism is that receiving a better quality and safe house can reduce geographic mobility. Life in slums can be harsh, and people constantly seek better living conditions (EVANS; WELLS; MOCH, 2003). Consequently, constant mobility between different slums contributes to weak community ties. I empirically test both of these mechanisms using quantitative and qualitative data.

Another aspect of SWE that is worth exploring in TETO's context is the issue of hedonic adaptation. Part of the literature professes that the cognitive components of well-being should have more lasting effects than emotional components (see (LUHMANN et al., 2012) a review). I take advantage of the fact that some slums have been exposed to treatment for a longer period than others. Then, I estimate the heterogeneous effects of time exposure on measures of emotional well-being. This could be a good comparison to Galiani, Gertler & Undurraga (2018) study on TETO in other Latin American Countries. Lastly, the moderating effects of income can vary between different components of SBW, as mentioned before. By comparing the same program in a different country and context I hope to contribute to this growing literature. Galiani, Gertler & Undurraga (2018) find that adaptation is even across low and high-income groups, suggesting that adaptation is mostly hedonic than being related to relative income.

7.4 Measures of Subjective Well-being

The measures of subjective well-being were adapted from Kahneman & Deaton (2010). These authors use the well-validated Gallup-Healthways Well-Being Index (GHWBI), which has different versions of questionnaires focusing on a variety of subjective well-being components. Of especial interest is the emotional well-being questionnaire that asks the frequency of positive and negative emotions (e.g., happiness, worry, loneliness, stress). Although GHWBI serves as an inspiration, two adaptations were necessary. First, I adapted the phrasing of the questions to match the cultural and socioeconomic context of TETO recipients. Second, I included two additional measures of stress/depression and sleep quality [2].

The original phrasing is the following: "Did you experience the following feelings during a lot of the day yesterday?" Responses were on a yes or no basis. I kept the yes or no basis but changed the wording to the following: "Thinking back to yesterday, would you say you felt FEELING for most of the day?" Feelings were: happy, worried, sad, and lonely. To complement the index I included a question of sleep quality using a Likert scale (great, good, regular, bad, very bad). I also asked if the recipient makes use of any remedy for depression and the extent to which he (she) feels more stress than normal (yes or no answer). All questions were standardized to take part in a global index of subjective well-being with equal weights for each component (KLING; LIEBMAN; KATZ, 2007). Variables that address negative feelings had the signal inverted to match positive feelings.

Although I report results for each component of the global index, the most important metric is the index itself. This means that when interpreting results from each component one must be careful not to make hasty generalizations. As the literature suggests, emotions are highly sensitive to external influences (KIM-PRIETO et al., 2005). For this same reason, I chose to position the mental health block of questions far from the housing questions. I did not want domain assessments of housing program to play a major influence on the emotional well-being metrics.

7.5 Main Quantitative Results

In this chapter, I first present the main quantitative impacts of TETO's housing program on the measures of subjective well-being. Then, I present mechanisms that possibly explain the positive impacts on SWB and discuss them in light of previous theories. From the methodology chapter, the main results focus on the intention-to-treat initial offer estimator (ITT-IO), which gives a sense of how the program performs. I keep presenting

[2] I included sleep quality because there is evidence that it can impact subjective well-being (BESSONE et al., 2021).

results for three econometric models: the pre-specified fixed-effects model; Lin (2013) interacted fixed-effects model; and Gibbons, Serrato & Urbancic (2018) IWE estimator.

The first row of Table 15 presents the estimated coefficients for the Subjective Well-being Index (Z-score, following Kling, Liebman & Katz (2007)). In the first parenthesis of each block of results, robust standard errors, and the second parenthesis, p-values. In all three models, the results are positive and statistically significant at 10% and 5%. As can be observed, the emotional well-being of treated families rises by between 0.180 to 0.237 standard deviations (SDs). This is a substantial increase, considering that it represents about twice the gap in SWB between households below and above the median per capita income. As a matter of comparison, Galiani et al. (2017) find that TETO increases satisfaction with housing quality by between 0.5 to 0.63 SDs, and general life satisfaction by 0.4 SDs. The increase in life satisfaction in their study represents 3.5 times the gap in SWB between households above and below the median income. The average per capita monthly income in Galiani et al. (2017) is around US$60 for the control group at the baseline, while in the present study it is around US$90. Although monthly income differs in both samples, results are similar, even considering different measures of subjective well-being. The returns on investment of both studies also seem to be higher when compared to other types of housing programs, such as the Moving to Opportunity in the US and the Piso Firme program in Mexico (CATTANEO et al., 2009; LUDWIG et al., 2012).

To what respects the underlying variables that compose the SWB index, it is interesting to notice that significant impacts of the housing program are more salient among the indicators that reflect negative feelings (stress, depression, worry, and loneliness). Although evaluating each component isolated from the rest of the index is not ideal, it calls attention that positive assessments such as happiness and better sleep quality are less pronounced. This may be due to the fact that TETO serves more as an emergency relief than as a permanent housing solution. As Diener et al. (2010) and Scitovsky (1976) theorize, negative feelings should be inversely associated with the fulfillment of basic needs, while positive feelings should increase with social-psychological rewards. Speculatively, in a context of extreme vulnerability caused by structural poverty and aggravated by the Covid-19 crisis, the housing unit may be more efficient in reducing psychological distress than increasing positive feelings at one hand. On the other hand, it could be argued that TETO's social program component should work in favor of positive emotions. The following analysis of mechanisms helps to disentangle this apparent ambiguity.

The first mechanism that comes to mind is the extent to which the program improves housing quality. It has been shown from previous studies that TETO's housing unit presents substantial improvements in the quality of floors, walls, and roofs (GALIANI et al., 2017; GALIANI; GERTLER; UNDURRAGA, 2018). In Table 16, I extended these measures to test whether the unit increases other aspects of housing quality. Because the research was being conducted in the context of Covid-19, three out of six measures relate to sanitary conditions, which is something that TETO does not provide, but that families usually seek for themselves. In addition to them, I included the frequency of rat infestation, possession of durable goods (TV and refrigerator), and the extent to which beneficiaries evaluate their house in comparison to their neighbors. As expected, it can be seen in Table 16 that all the three measures of housing sanitary infrastructure were not impacted by the intervention. Most likely because families re-build their bathrooms and access to water to match the same conditions that they had before TETO's program. However, TETO slightly increases the protection against rat infestation. Although coefficients are only statistically significant in model 3 from Table 16, the remainder of econometric specifications in the appendix corroborate model 3. Protection against rats is also salient in the qualitative interviews with recipients of the program. These results point towards the effectiveness of the housing structure provided by TETO. Better walls, floors, and roofs should indeed protect not only from storms but against animal infestations as well.

Nevertheless, this is still a limited housing quality improvement. The housing unit consists of a wooden-made block with a single room and nothing else. As TETO argues, the unit aims to relieve emergency conditions. It should be hard to think that effects on SWB would be substantial. Surprisingly, the intervention results either in higher life satisfaction and housing satisfaction or higher emotional well-being. One feature that can help to explain these outcomes is the social nature of programs like this. Notice in Table 16 that benefited families consider TETO's unit to be better than their neighbors' housing units. The size of the effect is large and it amounts to about 0.2 SDs. Perhaps, in addition to the slight house quality improvements, what could be driving the positive results on SWE is the change in relative housing status within local communities. By comparing their new homes to the average units in their close neighborhood, TETO recipients may feel better and evaluate life more positively. This reasoning fits with previous researchers on SWE that highlight the relational and social nature of psychological evaluations, especially in terms of emotions.

What is certainly not driving the increase in well-being are the economic variables. The relationship between income or wealth and measures of subjective well-being is widely studied (DIENER et al., 2010; KAHNEMAN; DEATON, 2010). Although the correlation between income and SWE is usually positive, higher income could not drive TETO's outcomes because there is no evidence that the intervention impacts economic variables at the household level. Table 17 corroborates the findings from Galiani et al. (2017) by showing null effects in three measures of economic vulnerability during the pandemic. Treated families are no different from control families in terms of earning less money during the crisis, nor having difficulties in buying food or diversifying basic consumption items. Hence, it is not by a mechanism of reduced economic vulnerability that beneficiaries have their SWB increased. This leads us to explore other possible mechanisms to complement housing quality and housing status.

Table 18 presents results for measures of social ties and solidarity between slum residents during the pandemic. The first six dependent variables are measures of the extent to which respondents believe that they can count on other persons to care for them in case they have health issues. Persons to be considered are the following: family members, friends, community members, church members (if applicable), community leaders, and aldermen. The last two variables of the Z-score ask if respondents provided any type of aid to other community families or if received aid from other families. The Ties and Solidarity index gives a sense of the social integration experienced by respondents during the crisis. The subjacent hypothesis here is that TETO's program increases social engagement, which in turn may work as a mechanism for increasing SWB. Table 18 shows a statistically significant effect on the Z-score only at the 10% level. Effects are likely to be driven by stronger involvement with friends and church members, while other personal relationships are equal between treated and non-treated families. Effects on aid are null, probably because the level of vulnerability is similar between both groups.

Considering the nuances of the bundled treatment that TETO promotes, it is not surprising that effects are only positive for friends and church members. Counting on others presupposes trust relationships, especially when it comes to health issues. In the pre-construction meetings, volunteers constantly incentivize beneficiaries to engage their networks in helping with the program. Naturally, the first persons that beneficiaries try to engage are the ones whom they trust the most, the ones that are close to them. Family, friends, and members of religious congregations are the ones that probably fit this criterion. Because most people appeal to the family when facing health issues, effects are null for this group, as either control and treated families will first count on family members to solve their problems. However, it is interesting that treated families reported counting more on friends and church members than control families. This effect can only come from the social aspect of TETO's program if we are to consider that housing quality alone should not increase trust and networks. The social facet of the program cannot be despised. Theoretically speaking, individuals who nourish good social relationships tend to be happier. Hence, it is suggestive that if TETO increases social ties we should see improvements in SWB, even if these ties were already established with people of close contact.

Finally, one more possible mechanism that may be influencing community integration, social ties, and thus subjective well-being is the reduction in geographic mobility that TETO produces. Table 19 reports estimates of the dichotomous variable of staying in the community in the follow-up survey. Dwellers who remain in the same community as in the baseline interview receive a value of 1, while those who move out receive a value of

0. Estimates suggest that TETO increases the probability of staying in the community. Assuming that frequently moving from one place to another weakens one's networks and social ties, TETO provides a good opportunity for families to establish roots in the places where they live. Of course, these should only account for a minor part of the overall effect on SWB, but it seems to be an interesting subject for future studies.

Table 15 – Intention to Treat Estimates (Initial Offer) - Subjective Well-being

Dependent Variable	Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Subjective Well-being Index	0.691	0.180	0.226	0.237
	[0.355]	(0.105)*	(0.100)**	(0.100)**
		(0.087)	(0.024)	(0.018)
Happiness	-1.236	0.122	0.141	0.141
	[0.423]	(0.109)	(0.104)	(0.107)
		(0.265)	(0.177)	(0.189)
Worried	1.428	0.202	0.241	0.260
	[0.496]	(0.115)*	(0.114)**	(0.113)**
		(0.079)	(0.035)	(0.021)
Sadness	1.706	-0.012	0.015	0.019
	[0.455]	(0.107)	(0.101)	(0.102)
		(0.914)	(0.883)	(0.852)
Loneliness	1.648	0.119	0.174	0.174
	[0.479]	(0.109)	(0.105)*	(0.104)*
		(0.277)	(0.098)	(0.094)
Sleep quality	-2.373	0.045	0.079	0.077
	[1.117]	(0.109)	(0.110)	(0.110)
		(0.683)	(0.474)	(0.486)
Depression medication	1.907	0.189	0.182	0.194
	[0.291]	(0.091)**	(0.086)**	(0.082)**
		(0.040)	(0.035)	(0.018)
Stress	1.753	0.129	0.165	0.183
	[0.431]	(0.106)	(0.098)*	(0.097)*
		(0.221)	(0.093)	(0.060)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefficient, robust standard error, and p-value, in that order. Robust standard errors, clustered at household 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; SERRATO; URBANCIC, 2018). Outcome variables have been standardized according to (KLING; LIEBMAN; KATZ, 2007). Fixed-Effects are lottery dummies for each community-lottery.

Table 16 – Intention to Treat Estimates (Initial Offer) - Housing Quality

Dependent Variable	Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Housing Quality index	0.220	0.118	0.136	0.138
	[0.383]	(0.099)	(0.096)	(0.095)
		(0.235)	(0.157)	(0.149)
Frequency of rats at home	-2.098	0.150	0.138	0.147
	[1.567]	(0.095)	(0.088)	(0.087)*
		(0.116)	(0.117)	(0.093)
Have sink at home (scale)	1.680	0.022	0.017	0.006
	[0.387]	(0.106)	(0.101)	(0.100)
		(0.834)	(0.865)	(0.949)
Have bathroom w/ toilet and shower (scale)	1.308	-0.079	-0.050	-0.061
	[0.563]	(0.109)	(0.098)	(0.098)
		(0.467)	(0.611)	(0.537)
Share toilet with neighbors	-1.069	0.063	0.072	0.077
	[0.252]	(0.105)	(0.096)	(0.096)
		(0.550)	(0.453)	(0.423)
Comparison with neighbors' house	-1.896	0.163	0.208	0.197
	[0.657]	(0.112)	(0.113)*	(0.113)*
		(0.145)	(0.068)	(0.080)
TV and refrigerator (scale)	3.394	0.025	0.009	0.033
	[1.040]	(0.101)	(0.095)	(0.096)
		(0.807)	(0.923)	(0.730)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefficient, robust standard error, and p-value, in that order. Robust standard errors, clustered at household 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; SERRATO; URBANCIC, 2018). Outcome variables have been standardized according to (KLING; LIEBMAN; KATZ, 2007). Fixed-Effects are lottery dummies for each community-lottery.

Table 17 – Intention to Treat Estimates (Initial Offer) - Vulnerability

Dependent Variable	Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Vulnerability Index	-1.425	-0.132	-0.096	-0.119
	[0.409]	(0.120)	(0.119)	(0.117)
		(0.271)	(0.424)	(0.309)
Earning less money during crisis	-1.314	-0.174	-0.097	-0.111
	[0.608]	(0.127)	(0.117)	(0.113)
		(0.172)	(0.406)	(0.325)
Hard time buying food	-1.547	-0.022	0.001	-0.030
	[0.499]	(0.115)	(0.117)	0.117
		0.847	(0.992)	(0.796)
Worse food balance	-1.415	-0.108	-0.124	-0.133
	[0.494]	(0.116)	(0.121)	(0.119)
		(0.355)	(0.306)	(0.263)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefficient, robust standard error, and p-value, in that order. Robust standard errors, clustered at household 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; SERRATO; URBANCIC, 2018). Outcome variables have been standardized according to (KLING; LIEBMAN; KATZ, 2007). Fixed-Effects are lottery dummies for each community-lottery.

Table 18 – Intention to Treat Estimates (Initial Offer) - Ties & Solidarity

Dependent Variable	Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Ties and Solidarity index	-1.228	0.208	0.192	0.196
	[0.446]	(0.109)*	(0.118)	(0.117)*
		(0.058)	(0.103)	(0.094)
If ill, can count on family	-1.711	0.020	0.006	0.035
	[0.787]	(0.115)	(0.111)	(0.110)
		(0.864)	(0.959)	(0.750)
If ill, can count on friends	-2.264	0.270	0.213	0.226
	[0.729]	(0.107)**	(0.111)*	(0.112)**
		(0.012)	(0.055)	(0.044)
If ill, can count on community members	-2.013	0.118	0.108	0.108
	[0.699]	(0.111)	(0.122)	(0.121)
		(0.292)	(0.374)	(0.375)
If ill, can count on church members	-1.893	0.262	0.292	0.290
	[0.687]	(0.105)**	(0.103)***	(0.102)***
		(0.013)	(0.005)	(0.005)
If ill, can count on community leaders	-1.946	0.153	0.116	0.115
	[0.753]	(0.117)	(0.116)	(0.116)
		(0.191)	(0.317)	(0.320)
If ill, can count on alderman	-2.698	-0.104	-0.066	-0.064
	[0.567]	(0.117)	(0.17)	(0.115)
		(0.375)	(0.573)	(0.576)
Provided aid to other families	1.475	0.150	0.159	0.143
	[1.503]	(0.113)	(0.122)	(0.124)
		(0.187)	(0.192)	(0.248)
Received aid from other families	1.225	-0.057	-0.082	-0.093
	[1.523]	(0.100)	(0.095)	(0.096)
		(0.564)	(0.390)	(0.334)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefficient, robust standard error, and p-value, in that order. Robust standard errors, clustered at household 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; SERRATO; URBANCIC, 2018). Outcome variables have been standardized according to (KLING; LIEBMAN; KATZ, 2007). Fixed-Effects are lottery dummies for each community-lottery.

Table 19 – Intention to Treat Estimates (Initial Offer) - Geographic mobility

Dependent Variable	Control Mean [Std. Dev.]	Model 1	Model 2	Model 3
Remains in the community	0.907 [0.291]	0.108 (0.100) (0.281)	0.158 (0.087)* (0.070)	0.150 (0.090)* (0.097)
Pre-treatment controls		Yes	Yes	Yes
Fixed Effects		Yes	Yes	Yes
N. Obs		366	366	366
N. Blocks		25	25	25

Reported results: estimated coefficient, robust standard error, and p-value, in that order. Robust standard errors, clustered at household 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; SERRATO; URBANCIC, 2018). Outcome variables have been standardized according to (KLING; LIEBMAN; KATZ, 2007). Fixed-Effects are lottery dummies for each community-lottery.

7.6 Heterogeneity Analysis

As Evans, Wells & Moch (2003) and Diener et al. (2018) point out, subjective well-being is commonly correlated to moderating factors that influence the effectiveness of related policies. Among the most studied factors is the level of income or wealth of policy recipients. Although there is wide evidence in cross-country studies showing a positive and significant association between income and happiness (CLARK, 2016), fewer studies pinpoint the influence of income on emotional components of subjective well-being (KAHNEMAN; DEATON, 2010), and even to a less extent the heterogeneous treatment effects of housing policies (GALIANI; GERTLER; UNDURRAGA, 2018). I contribute to these discussions by reporting conditional average treatment effects (CATE) of TETO's intervention concerning different levels of per capita family monthly incomes. I use the following specification to do this:

$$y_{ij} = \alpha + \rho Treat_{ij} + \beta Income_{ij} + \lambda Treat \times Income_{ij} + \gamma_j + \varepsilon_{ij}$$

Where y_{ij} is the outcome for recipients in household i participating in lottery-settlement j. $Treat_{ij}$ is the randomization assignment dummy. I use the outcome variable as being the Subjective Well-being index. For $Income_{ij}$ I used the logarithm of *per capita* monthly nominal income[3]. The parameter of interest λ comes from the interaction term between treatment and income, ans it shows the moderating influence of income on treatment. The Marginal effect of treatment is the sum of parameters ρ and λ times the

[3] Recent studies consider that using the logarithm of income produces better results because quantitative dimensions of perception and judgment follow Weber's law, which states that what matters are the percent changes, not absolute changes (KAHNEMAN; DEATON, 2010).

different levels of income. Finally, the parameter γ_j is a community (slum) fixed effect and ε is the idiosyncratic error term.

By interacting the treatment variable with families' per capita income it is possible to address how income moderates the effects of the housing program on the measures of emotional well-being. Table 20 reports the results of the model above. It is interesting to notice that the parameter of interest Gamma is negative and statistically significant at the 10% level [4]. This means that higher income levels attenuate the effect of treatment on the z-score measure of emotional well-being.

Table 20 – OLS Fixed-effects with Income Interaction Term

	Estimate	Std. Error	t value	Pr(>|t|)
Treatment dummy (Initial Offer)	0.201	0.110*	1.835	0.067
Per capita Income (log)	0.040	0.066	0.601	0.548
Treatment x Income	-0.195	0.114*	-1.717	0.087

Reported estimates exclude results for fixed-effects.

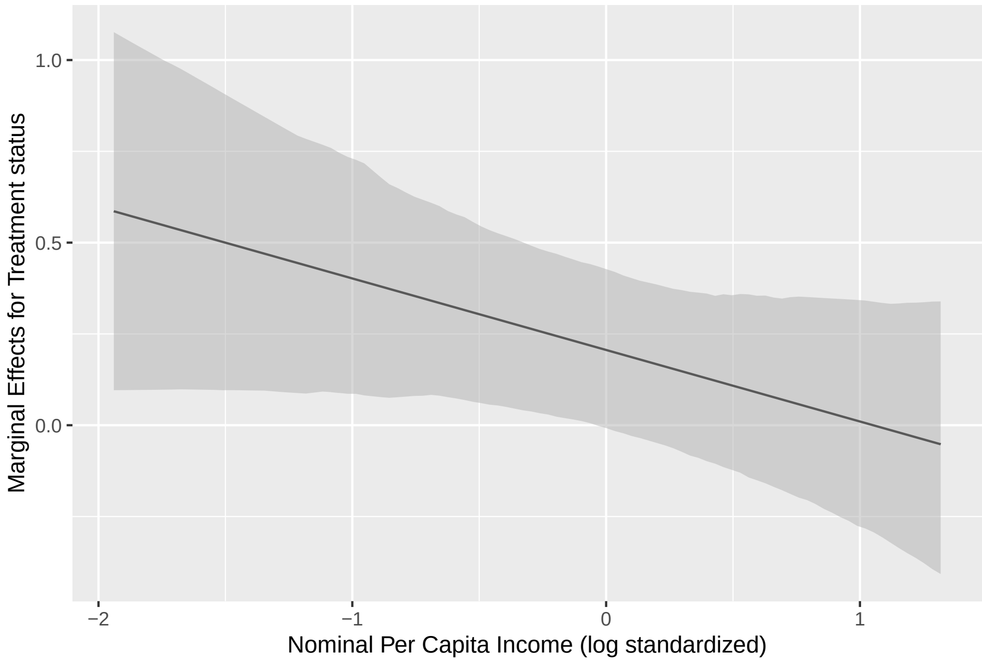
Standard errors significance: ***p<0.01, **p<0.05, *p<0.1.

The negative association between variables becomes clearer when we look at Figure 10. Notice that the figure shows how the marginal effects of treatment decline for higher values of income. Although heterogeneous effects are not causal, the evidence suggests that the poorer families are the ones who benefit the most from the housing program. In contexts of poverty like what strikes Brazilian slums, the meeting of basic needs may make more difference for the families who struggle the most to survive. The income moderating effects in TETO's context find parallels to other studies. For instance, Kahneman & Deaton (2010) report that low income exacerbates the emotional pain associated with misfortunes such as divorce, ill health, and being alone. If TETO does not increase material achievements, at least in terms of emotional gains it fulfills an important role, especially for the most vulnerable. Future studies could compare these results to other components of subjective well-being, as it is not clear that income would influence cognitive assessments in the same manner as emotions.

Another commonly explored feature of subjective well-being is the pace at which impacts on happiness return to normal values after time pass by and individuals move on with their ordinary lives. Studies on life events are particularly worried about whether people ever recover from intense negative experiences (e.g., bereavement, health traumas) or if the effects of positive events last long (e.g., marriage, childbirth). If one thing is

[4] To be more precise in the evaluation of statistical significance, I follow Gerber & Green (2012) and compute a F-test, where the unrestricted model is the one described above and the restrict model is the same except for the interaction term. The F-statistic amounts to 2.94, which yields a p-value of 0.086

Figure 10 – Estimated Coefficients of Treatment Status on Per capita Nominal Income (log)



Source: self elaboration

clear, adaptation to life events depends largely on the type of event and the measures of subjective well-being. In an extensive meta-analysis, Luhmann et al. (2012) concludes that effects are not a function of the alleged desirability of events, leaving room for further and more detailed contributions on the topic.

In Galiani, Gertler & Undurraga (2018), evidence suggests that TETO's housing program effects do not last long. The authors estimate that after 28 months of treatment exposure, the effects completely vanish (60% of the gain dissipates in eight months). By using a similar econometric approach, I also test the hedonic adaptation of TETO's program, but with different measures of SWB. Luhmann et al. (2012) argues that cognitive components of SWB should last longer than effects of affect components. My evidence in part contradicts their claim. I take advantage of the fact that TETO delivers houses in different moments for each community of the sample. Similar to the income heterogenous analysis, I interact the time of exposure variable with the treatment assignment variable to test the hedonic adaptation.

Table 21 is equal to Table 20 except that instead of interacting income it interacts treatment assignment with time exposure to treatment. Results show that varying time exposure has no moderating influence on the impacts of the program. Figure 11 reinforces this pattern, as confidence intervals are large and always intercept the zero axes, leading to the conclusion that marginal effects of treatment do not change in accordance with time exposure [5]. However, one must be cautious to interpret these results. The longest time of exposure in the sample is fourteen months, and the shortest is three months. It could still be the case that after longer periods hedonic adaptations begin to act. Nevertheless, in comparison with what Galiani, Gertler & Undurraga (2018) have found, emotional well-being seems to be at minimum as lasting as life satisfaction (and housing satisfaction) in terms of adaptation.

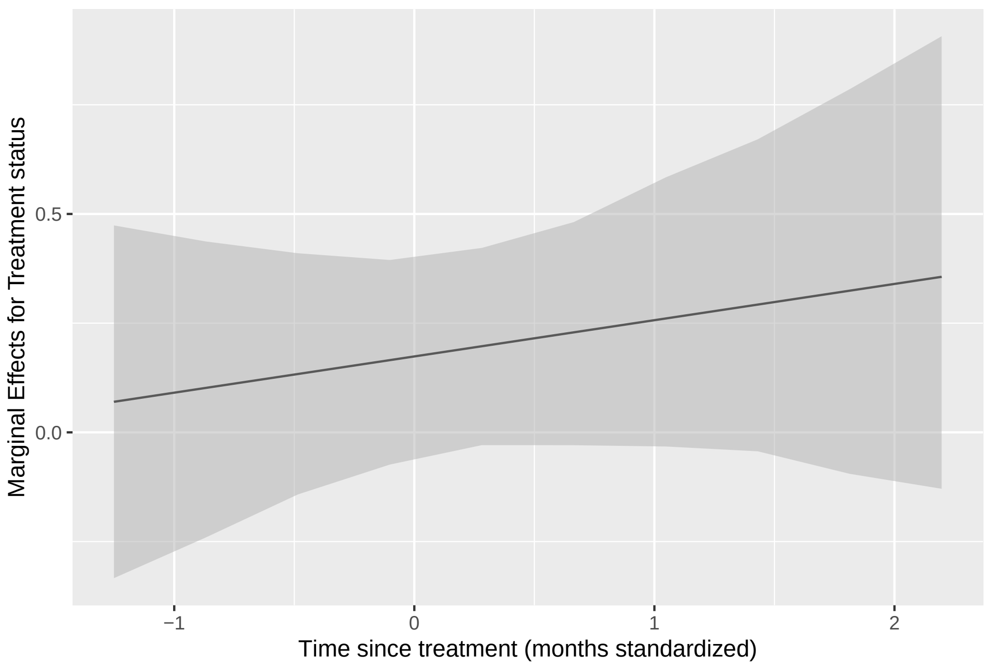
Table 21 – OLS Fixed-effects with Time of Treatment Interaction Term

	Estimate	Std. Error	t value	Pr(>|t|)
Treatment dummy (Initial Offer)	0.172	0.116	1.487	0.138
Time since Treatment (months)	0.024	0.154	0.156	0.876
Treatment x Time	0.077	0.120	0.638	0.524

Reported estimates exclude results for fixed-effects.

Standard errors significance: ***p<0.01, **p<0.05, *p<0.1.

Figure 11 – Estimated Coefficients of Treatment Status on Months after Treatment



Source: self elaboration

[5] Similarly, the F-statistic is 0.4 and p-value is 0.52.

7.7 Qualitative findings

Qualitative data was gathered to help disentangle the underlying mechanism of the intervention on subjective well-being. The first mechanism that is worth mentioning is the house quality. Although TETO does not meliorate sanitary infrastructures, the wooden-made house provides better shelter against common natural disasters. A wide range of respondents, either in the control group and in the treatment group, confirmed that housing collapse is a real risk in the slums where TETO acts. Storms and floods increase the risk of collapse or that trees fall into people's houses. Additionally, when events such as these happen, it is common that households end up without electricity because networks are informal and poorly installed. The following interviews highlights how safe the house is:

> *Interview Nm. 6 (Treatment unit)*
>
> Q – How was the experience of the first rain when you lived in this house?
>
> R – Ah, the first time I was kind of brooding, I said: "I'll wait, won't this wind blow these tiles or tear something out?"
>
> Q -What then?
>
> A -And it stayed there. The rain passed. Then I said: "Thank God, this rain has passed". When it was in the other rains I lost the fear of tearing the roof down, because it is a very safe house.

> *Interview Nm. 42 (Treatment unit)*
>
> Q – What do you think is the best thing about this house at Teto?
>
> A – From the ceiling? Ah, it was better, well it was safer, right?
>
> Q – How so? Safe in what sense?
>
> A – Rain, wind, right? Like, if it rains it doesn't get water inside, because of the underside, right? Because the house is upstairs.

Reports from our respondents lead us to think that the housing unit increases the sense of housing safety, which reduces feelings of anxiety and worries towards families' physical integrity. People from the control group are constantly afraid of losing their homes after storms and floods or even losing their lives. In a more subtable way, TETO's units are considered to be more comfortable and better looking. There were many reports (among treatment and control) that the house is beautiful and nice to live in. Notice some examples:

> *Interview Nm. 50 (Treatment unit)*[6]
>
> Q – And what do you think of Teto's house?
>
> A – I think it's very good, because it gives you comfort, it's better than those houses we make in a hurry, it's much warmer inside too, very good.

[6] This is a case of non-compliance. This woman was originally in the control group, but she divorced and now lives with her mother who has a TETO housing unit

Q – Tell me a little bit when you say she is very good, she has more comfort, what is this good, this comfort, tell me a little more?

A – Comfort because we can sleep more peacefully, in the past in the house we used to live there is a risk of trees falling down, only its wooden floor, they put stumps down and the wooden floor and then they raise the level of the house.

Q – You said that the house gives you comfort, why does it give you comfort?

R – It's a cute house, it's warmer inside, it's not in danger of falling down, nothing like that.

P – And before you had problems with insects, like cockroaches, ants or other animals.

R – A lot, cockroach, mouse there appears a lot.

Interview Nm. 31 (Treatment unit)

Q - What motivated you to enroll in the process to get the house? Why did you want? R - So, because he was... in a very good way, cute, well preserved, you know? And because it also brings improvement, because it is tall, not short, you know? Q - Is the house taller then? A - Yeah! It's not on the floor, you know? It's in a log, something like that. And like a... It's much better than a normal shack, which had to clean carpet, had to have carpet, in the latter the floor fades, then some insects would appear, things like that. With TETO it improved much more.

Q - And nowadays what do you say to me, after the years, what do you say is the best thing in the house and what is the worst thing in the house?

A - The best thing about TETO... the best thing, as I said... the best thing is the comfort it brings us. In my old shack I used to live under a sieve, sometimes it was a strange insect entering the house, but not with TETO, no, it brought me an improvement. It doesn't drip, it's cozy, there's nothing bad to talk about. It is good.

It is interesting that even residents in the control group find TETO's houses to be good-looking. From a theoretical perspective, this could connect to previous evidence on the role of social status in housing programs. Why would it matter how the house looks to others? Should it make any difference for safety and comfort? In fact, the issue of how TETO's houses are well painted and how they look cute has appeared in the great majority of our qualitative interviews. This mechanism could lead to feelings of self-esteem, as this respondent answered:

Interview Nm. 20 (Treatment unit)

Q – Did you change your feelings, thus, towards yourself after you had conquered your home?

A - A little bit, it has changed because, for me, it's a new house, I always had, thank God my little house, I always turned around here in the community I always turned around, but it wasn't as young as this is, I won like this one, gives a lift in our self-esteem.

Q – And you had this feeling like, like you said about self-esteem, that feeling that now you can do more, you know?

> A – Yeah, now it's mine, look at my house, I have my house, look at my little house, it's so cute! I got it, I was the one who got it.

In addition to safety and status, the house also generates comfort. In our qualitative analysis, the code "comfortable" was salient for at least two reasons. First, the house is well structured to the point of avoiding problems like leaks during rains, and extreme cold or extreme heat. Thus, the house seems to be a better place to live in. Second, the unit better protects residents from rats, insects, and other small animals that can carry diseases and which most people find disgusting. This could lead to a sense of dignity that indirectly impacts our measures of emotional well-being.

In summary, qualitative data reinforces the findings from the econometric models and gives us a better sense of the underlying mechanisms. House quality improvement reduces physical risks, which in turn reduces worries and stress. It also provides better comfort for families. By alleviating the pressure over basic needs, the program also impacts the emotional components of subjective well-being, a finding that is novel in the literature. The social aspect of the intervention plays an important role and fits previous theories that argue in favor of the correlation between sociability and better psychological well-being.

7.8 Discussion

A component that has been gaining relevance in policy evaluations is the extent to which they can increase recipients' subjective well-being (KAHNEMAN; KRUEGER, 2006). In a normative sense, few would argue against using well-being as one of the paramount criteria to assess the success of any given policy. However, defining well-being is an intricate enterprise that leads to broad discussions on how to measure it objectively. Thus, a simple alternative is to evaluate programs on the basis of how beneficiaries perceive the experiences they live. Instead of relying on objective measures of well-being, scholars and policy-makers can use valid and reliable scales of subjective well-being (DIENER; INGLEHART; TAY, 2013). The present chapter used this concept to investigate the impacts of TETO's housing program in comparison to alternative policies that are commonly implemented worldwide. A nuanced view of this concept helps us better understand the potentialities of programs such as the one TETO promotes.

Different from other studies, the option here was to focus on the emotional component of subjective well-being. Most studies on housing policies rely on general life satisfaction and satisfaction with house quality as the primary measures of SBW. Because TETO not only improves house quality but also promotes social interaction, using measures of emotional well-being helps to disentangle underlying mechanisms that previous studies on housing policy (or programs) find difficult to explain. For instance, although studies regarding the Moving to Opportunities program find positive effects on measures of life satisfaction, they also find that the program underperforms in subgroups of beneficiaries that are more exposed to social disconnection (e.g., male adolescents). Yet, in theoretical terms, social disconnection is more correlated with emotional well-being than it is with life satisfaction or domain satisfaction. Therefore, studying other components of subjective well-being certainly adds to the debate.

In terms of results, the present study finds estimates that are similar to other housing studies. Receiving a housing unit from TETO increases SWB by about 0.2 standard deviations, an impact that corresponds to twice the gap in SWB between households below and above the median per capita income. This is a substantial impact if we put it in perspective and compare TETO to policies that relocate beneficiaries to higher-income neighborhoods or that provide housing tenure. TETO provides an in situ intervention that only slightly increases housing quality. In other words, recipients continue to live in precarious slums and remain subject to all sorts of poverty traps. Yet, results are positive for life satisfaction, domain satisfaction, and emotional well-being (as this and previous studies show). At least to some extent, this could be considered a puzzling finding. The question is whether the underlying mechanisms presented in the study explain this outcome.

The first mechanism to be tested was housing quality improvement. From the randomized experiment, results confirm that quality improvement is limited. As expected, TETO does not meliorate any type of sanitary infrastructure. Even though it is not part of the program to provide bathrooms and access to running water, there was a belief among practitioners that by improving general housing infrastructure recipients would further invest in the housing unit. In situ observations confirm that investment exists, but it is not different from investments from the control group. In other words, sanitary infrastructure remains at the same level that before the intervention. On the other hand, TETO provides a nice and safe unit, as the qualitative analysis points out. The wooden house is well structured and works as a proper shelter from natural disasters such as floods, intense storms, and even rat infestations. This leads beneficiaries to evaluate their houses better when compared to the neighbors, an estimate that is statistically different from the control group. Interesting to notice that this is a relational assessment of one's local social position within the community concerning housing conditions. At least in the emotional scope, status seems to be a relevant driver of human behavior, as previous studies on subjective well-being suggest. Evidence from our qualitative data points out that TETO's units are viewed as "good-looking" and "cute", features that I interpret as being relational in the local context. In terms of social position among other slum residents, having a better looking house increases self-esteem, data suggests.

As for the second mechanism, the extent to which the program encourages social connections, both quantitative and qualitative data confirm that TETO indeed produces relevant incentives for social interactions. In theory, this could explain part of the positive results on emotional well-being. Positive feelings are the ones that may be more affected by social interaction. If we interpret that the entire TETO housing program is a life event analogous to winning the lottery or getting married, the social component of TETO intervention seems to be fundamental, since beneficiaries have extremely positive memories from the day they build and receive the housing unit and about the people who helped them to achieve the goal of having their own homes. The geographic mobility mechanism is a step in stone towards more community engagement and social connections.

Finally, our heterogeneous analysis contributes to open a future road of studies. Although the relationship between income (or wealth) and subjective well-being has been extensively studied, there are still avenues of research in particular context of extreme material deprivation or in times of great social disruption (pandemics and natural disasters). Like previous studies, my findings suggest that lower income is more correlated to the emotional pain associated with misfortunes. However, the types of misfortunes that I study are different from most studies that focus on life events such as divorce, ill heath, or bereavement. My context is that of the global economic and health crisis of the Covid-19 pandemic. The quantitative analysis shows that the effects of the housing program are higher among the poorer residents. The qualitative data gives sense of this finding by showing how a better housing unit helps alleviating pressure by fulfilling basic desires of very poor slum residents.

8 Final Remarks

Competing theories of development consider that slums can be places that trap residents in poverty conditions or can be places full of opportunities (GLAESER, 2011). In the present dissertation, I discuss to what extent an NGO-led housing program, in the poorest Brazilian favelas, helps beneficiaries overcome poverty. The fact that it is an NGO that promotes the program has its particularities. Results must always be interpreted with this in mind. Overall, the NGO TETO is a fundamental player in the social dynamic of the slums where it works. During the coranavírus pandemic, when many families struggled with above normal levels of vulnerability, TETO's programs were important initiatives to alleviate poverty pressures. The present dissertation has focused on the most well-known of TETO's programs, the emergency housing program.

My findings contribute to a growing literature on Community Driven Development programs (CDDs). The work developed by TETO is a bottom-up approach because it tries to engage residents of the community to contribute to the housing program. More than that, the housing program is a first step to involve residents in subsequent programs sponsored by TETO. In opposition to top-down policies, where all decisions come from elites that have limited knowledge of local problems, CDDs believe that engagement and social interaction are fundamental for the success of the policies. TETO follows these beliefs in the housing program, and the results are relevant.

Through a mixed-methods methodology, I show that positive results were driven by the bundled characteristic of the program. TETO not only increases house quality but also incentivizes social interaction between recipients. The social skills learned in the program, and the networks formed along the way, lead to better psychological well-being, better community and social ties, and bigger trust in community leaders. Together, these results represent important step-in-stones in overcoming poverty traps because they circumvent well-known barriers to development. From an individual perspective, psychological distress is believed to reduce peoples' confidence in seeking a better future (BANERJEE; BANERJEE; DUFLO, 2011). From a collective perspective, stronger social capital is thought to be a fundamental driver of development (PUTNAM et al., 2000; OSTROM; AHN, 2009).

However, TETO's housing program is also limited in its potential impacts. For instance, in terms of protection against the coronavirus pandemic, the effects of the housing program are null. If policy-makers believed that better housing conditions would create incentives for social distancing and other self-quarantine measures, my evidence suggests that better shelter is not enough. Even after TETO's housing and aid interventions, slum residents still need more resources to comply with preventive measures. Vulnerability remains high for most families, and they need access to better public policies. In terms of improving economic perspectives, the TETO housing program produces almost no results. Besides, our observational data suggest that most residents are distrustful of government authorities, a theme that merits more attention in future studies because the lack of government accountability may dampen the delivery of local public policies.

Finally, after putting all significant and null impacts on the same basket, the final conclusion can not be other than a positive net effect of TETO. In communities that lack the most basic resources, where individuals are subjected to all sorts of barriers to development, where crime and exploration are the rules, TETO represents a string of hope among these difficulties. Although many challenges remain in these slums, TETO shows possible paths for alleviating poverty. Paths that relies on local collective action, through the active involvement of community members and the capacitation of local leaderships. Infrastructure projects such as housing can bring at the same time better material conditions and improved social arrangements.

Bibliography

ABDULKADIROĞLU, A.; HU, W.; PATHAK, P. A. *Small high schools and student achievement: Lottery-based evidence from New York City*. [S.l.], 2013. Citado na página 39.

ALLCOTT, H. et al. Polarization and public health: Partisan differences in social distancing during the coronavirus pandemic. *NBER Working Paper*, n. w26946, 2020. Citado na página 57.

ANGRIST, J. D. et al. *Stand and deliver: Effects of Boston's charter high schools on college preparation, entry*. [S.l.], 2013. Citado na página 39.

ARGYLE, M. et al. Causes and consequences of subjective well-being.in: Kahneman, d., diener, e., & schwarz, n. (eds.). *Well-being: The foundations of hedonic psychology*, Russell Sage Foundation New York, p. 353–373, 1999. Citado na página 78.

AVDEENKO, A.; GILLIGAN, M. J. International interventions to build social capital: evidence from a field experiment in sudan. *American Political Science Review*, Cambridge University Press, v. 109, n. 3, p. 427–449, 2015. Citado na página 21.

BAI, J. J.; JIN, W.; WAN, C. The impact of social capital on individual responses to covid-19 pandemic: Evidence from social distancing. *Wang and Wan, Chi, The Impact of Social Capital on Individual Responses to COVID-19 Pandemic: Evidence from Social Distancing (May 23, 2020)*, 2020. Citado 5 vezes nas páginas 53, 54, 56, 57, and 72.

BANERJEE, A. V.; BANERJEE, A.; DUFLO, E. *Poor economics: A radical rethinking of the way to fight global poverty*. [S.l.]: Public Affairs, 2011. Citado 2 vezes nas páginas 17 and 99.

BARBERIA, L. G.; GÓMEZ, E. J. Political and institutional perils of brazil's covid-19 crisis. *The Lancet*, Elsevier, v. 396, n. 10248, p. 367–368, 2020. Citado na página 72.

BARNHARDT, S.; FIELD, E.; PANDE, R. Moving to opportunity or isolation? network effects of a randomized housing lottery in urban india. *American Economic Journal: Applied Economics*, v. 9, n. 1, p. 1–32, 2017. Citado 2 vezes nas páginas 59 and 78.

BARRIOS, J. M. et al. *Civic capital and social distancing during the covid-19 pandemic*. [S.l.], 2020. Citado 2 vezes nas páginas 53 and 56.

BARTLETT, S. Does inadequate housing perpetuate children's poverty? *Childhood*, Sage Publications London, v. 5, n. 4, p. 403–420, 1998. Citado na página 79.

BARTSCHER, A. K. et al. Social capital and the spread of covid-19: Insights from european countries. CESifo Working Paper, 2020. Citado 2 vezes nas páginas 53 and 56.

BEAGLEHOLE, B. et al. Psychological distress and psychiatric disorder after natural disasters: systematic review and meta-analysis. *The British Journal of Psychiatry*, Cambridge University Press, v. 213, n. 6, p. 716–722, 2018. Citado na página 75.

BENJAMIN, D. J. et al. Beyond happiness and satisfaction: Toward well-being indices based on stated preference. *American Economic Review*, v. 104, n. 9, p. 2698–2735, 2014. Citado na página 76.

BENJAMINI, Y.; HOCHBERG, Y. Controlling the false discovery rate: a practical and powerful approach to multiple testing. *Journal of the Royal statistical society: series B (Methodological)*, Wiley Online Library, v. 57, n. 1, p. 289–300, 1995. Citado na página 60.

BESSONE, P. et al. The economic consequences of increasing sleep among the urban poor. *The Quarterly Journal of Economics*, Oxford University Press, v. 136, n. 3, p. 1887–1941, 2021. Citado na página 82.

BIANCOVILLI, P.; JURBERG, C. When governments spread lies, the fight is against two viruses: A study on the novel coronavirus pandemic in brazil. *medRxiv*, Cold Spring Harbor Laboratory Press, 2020. Citado na página 72.

BLAIR, R. A.; MORSE, B. S.; TSAI, L. L. Public health and public trust: Survey evidence from the ebola virus disease epidemic in liberia. *Social Science & Medicine*, Elsevier, v. 172, p. 89–97, 2017. Citado 2 vezes nas páginas 56 and 72.

BORGONOVI, F.; ANDRIEU, E. Bowling together by bowling alone: Social capital and covid-19. *Covid Economics*, v. 17, p. 73–96, 2020. Citado 2 vezes nas páginas 53 and 56.

BOURDIEU, P.; RICHARDSON, J. G. The forms of capital. New York, Greenwood, 1986. Citado na página 53.

BRANDSEN, T.; VERSCHUERE, B.; STEEN, T. *Co-production and co-creation: Engaging citizens in public services*. [S.l.]: Routledge, 2018. Citado na página 21.

BRONFENBRENNER, U.; EVANS, G. W. Developmental science in the 21st century: Emerging questions, theoretical models, research designs and empirical findings. *Social development*, Blackwell Publishers Ltd. Oxford, UK and Boston, USA, v. 9, n. 1, p. 115–125, 2000. Citado na página 79.

CARBONELL, A. Ferrer-i. Income and well-being: an empirical analysis of the comparison income effect. *Journal of public economics*, Elsevier, v. 89, n. 5-6, p. 997–1019, 2005. Citado 2 vezes nas páginas 78 and 79.

CARPIANO, R. M.; MOORE, S. *So What's Next? Closing Thoughts for this Special Issue and Future Steps for Social Capital and Public Health*. [S.l.]: Elsevier, 2020. Citado 2 vezes nas páginas 56 and 57.

CATTANEO, M. D. et al. Housing, health, and happiness. *American Economic Journal: Economic Policy*, v. 1, n. 1, p. 75–105, 2009. Citado 3 vezes nas páginas 74, 78, and 83.

CHAISEMARTIN, C. D.; BEHAGHEL, L. *Estimating the effect of treatments allocated by randomized waiting lists*. 2018. 1457–1458 p. Citado na página 40.

CHAISEMARTIN, C. D.; BEHAGHEL, L. Estimating the effect of treatments allocated by randomized waiting lists. *Econometrica*, Wiley Online Library, v. 88, n. 4, p. 1453–1477, 2020. Citado 3 vezes nas páginas 39, 40, and 42.

CHAISEMARTIN, C. de; BEHAGHEL, L. *Estimating the Effect of Treatments Allocated by Randomized Waiting Lists.* [S.l.], 2019. (Working Paper Series, 26282). Citado na página 39.

CHERRYHOLMES, C. H. Notes on pragmatism and scientific realism. *Educational researcher*, Sage Publications Sage CA: Thousand Oaks, CA, v. 21, n. 6, p. 13–17, 1992. Citado na página 31.

CLAPHAM, D. Happiness, well-being and housing policy. *Policy & Politics*, Policy Press, v. 38, n. 2, p. 253–267, 2010. Citado na página 75.

CLAPHAM, D.; FOYE, C.; CHRISTIAN, J. The concept of subjective well-being in housing research. *Housing, Theory and Society*, Taylor & Francis, v. 35, n. 3, p. 261–280, 2018. Citado 2 vezes nas páginas 75 and 79.

CLARK, A. E. Adaptation and the easterlin paradox. In: *Advances in happiness research*. [S.l.]: Springer, 2016. p. 75–94. Citado na página 90.

CLARK, A. E.; FRIJTERS, P.; SHIELDS, M. A. Relative income, happiness, and utility: An explanation for the easterlin paradox and other puzzles. *Journal of Economic literature*, v. 46, n. 1, p. 95–144, 2008. Citado 4 vezes nas páginas 77, 78, 79, and 80.

CLARK, A. E.; WESTERGÅRD-NIELSEN, N.; KRISTENSEN, N. Economic satisfaction and income rank in small neighbourhoods. *Journal of the European Economic Association*, Oxford University Press, v. 7, n. 2-3, p. 519–527, 2009. Citado 2 vezes nas páginas 78 and 79.

CLARK, C.; STANSFELD, S.; CANDY, B. A systematic review on the effect of the physical environment on mental health. *Epidemiology*, LWW, v. 17, n. 6, p. S527, 2006. Citado na página 75.

CLEAVER, F. The inequality of social capital and the reproduction of chronic poverty. *World development*, Elsevier, v. 33, n. 6, p. 893–906, 2005. Citado 3 vezes nas páginas 55, 56, and 72.

COLEMAN, J. S. Social capital in the creation of human capital. *American journal of sociology*, University of Chicago Press, v. 94, p. S95–S120, 1988. Citado 3 vezes nas páginas 53, 56, and 72.

CORBURN, J. et al. Slum health: arresting covid-19 and improving well-being in urban informal settlements. *Journal of Urban Health*, Springer, p. 1–10, 2020. Citado 2 vezes nas páginas 53 and 55.

COX, K. R. Housing tenure and neighborhood activism. *Urban Affairs Quarterly*, Sage Publications Sage CA: Thousand Oaks, CA, v. 18, n. 1, p. 107–129, 1982. Citado na página 58.

CRESWELL, J. W.; CLARK, V. L. P. *Designing and conducting mixed methods research*. [S.l.]: Sage publications, 2017. Citado 4 vezes nas páginas 30, 32, 33, and 34.

DASGUPTA, A.; BEARD, V. A. Community driven development, collective action and elite capture in indonesia. *Development and change*, Wiley Online Library, v. 38, n. 2, p. 229–249, 2007. Citado na página 21.

DASGUPTA, P. Trust as a commodity. *Trust: Making and breaking cooperative relations*, Citeseer, v. 4, p. 49–72, 2000. Citado na página 72.

DEVOTO, F. et al. Happiness on tap: Piped water adoption in urban morocco. *American Economic Journal: Economic Policy*, v. 4, n. 4, p. 68–99, 2012. Citado na página 78.

DIENER, E.; DIENER, C. The wealth of nations revisited: Income and quality of life. *Social Indicators Research*, Springer, v. 36, n. 3, p. 275–286, 1995. Citado na página 79.

DIENER, E.; INGLEHART, R.; TAY, L. Theory and validity of life satisfaction scales. *Social Indicators Research*, Springer, v. 112, n. 3, p. 497–527, 2013. Citado 2 vezes nas páginas 76 and 96.

DIENER, E. et al. Advances and open questions in the science of subjective well-being. *Collabra: Psychology*, University of California Press, v. 4, n. 1, 2018. Citado 3 vezes nas páginas 76, 80, and 90.

DIENER, E. et al. Wealth and happiness across the world: material prosperity predicts life evaluation, whereas psychosocial prosperity predicts positive feeling. *Journal of personality and social psychology*, American Psychological Association, v. 99, n. 1, p. 52, 2010. Citado 4 vezes nas páginas 80, 81, 83, and 84.

DIENER, E.; SELIGMAN, M. E. Very happy people. *Psychological science*, SAGE Publications Sage CA: Los Angeles, CA, v. 13, n. 1, p. 81–84, 2002. Citado na página 81.

DIENER, E. et al. Subjective well-being: Three decades of progress. *Psychological bulletin*, American Psychological Association, v. 125, n. 2, p. 276, 1999. Citado na página 76.

DING, W. et al. Social distancing and social capital: Why us counties respond differently to covid-19. *Available at SSRN 3624495*, 2020. Citado 4 vezes nas páginas 53, 54, 56, and 72.

DiPasquale, D.; GLAESER, E. Incentives and social capital: Are homeowners better citizens? v. 45, n. 2, p. 354–384, 1999. Citado na página 59.

DONGIER, P. et al. Community driven development. *World Bank Poverty Reduction Strategy Paper*, n. 1, 2003. Citado na página 17.

EVANS, G. W. The built environment and mental health. *Journal of urban health*, Springer, v. 80, n. 4, p. 536–555, 2003. Citado na página 74.

EVANS, G. W.; WELLS, N. M.; MOCH, A. Housing and mental health: A review of the evidence and a methodological and conceptual critique. *Journal of Social Issues*, Wiley Online Library, v. 59, n. 3, p. 475–500, 2003. Citado 5 vezes nas páginas 76, 78, 79, 81, and 90.

FEILZER, M. Doing mixed methods research pragmatically: Implications for the rediscovery of pragmatism as a research paradigm. *Journal of mixed methods research*, Sage Publications Sage CA: Los Angeles, CA, v. 4, n. 1, p. 6–16, 2010. Citado na página 31.

FOYE, C. The relationship between size of living space and subjective well-being. *Journal of Happiness Studies*, Springer, v. 18, n. 2, p. 427–461, 2017. Citado na página 78.

FOYE, C.; CLAPHAM, D.; GABRIELI, T. Home-ownership as a social norm and positional good: Subjective wellbeing evidence from panel data. *Urban Studies*, SAGE Publications Sage UK: London, England, v. 55, n. 6, p. 1290–1312, 2018. Citado 2 vezes nas páginas 74 and 79.

FRANK, R. *Falling behind: How rising inequality harms the middle class (Vol. 4)*. [S.l.]: University of California Press, 2013. Citado na página 78.

FRASER, T.; ALDRICH, D. P. Social ties, mobility, and covid-19 spread in japan. 2020. Citado 2 vezes nas páginas 53 and 56.

FUKUYAMA, F. *Trust: The social virtues and the creation of prosperity*. [S.l.]: Free press New York, 1995. v. 99. Citado na página 53.

GALIANI, S.; GERTLER, P. J.; UNDURRAGA, R. The half-life of happiness: Hedonic adaptation in the subjective well-being of poor slum dwellers to the satisfaction of basic housing needs. *Journal of the European Economic Association*, Oxford University Press, v. 16, n. 4, p. 1189–1233, 2018. Citado 7 vezes nas páginas 79, 80, 81, 83, 90, 92, and 93.

GALIANI, S. et al. Shelter from the storm: Upgrading housing infrastructure in latin american slums. *Journal of urban economics*, Elsevier, v. 98, p. 187–213, 2017. Citado 4 vezes nas páginas 79, 80, 83, and 84.

GAY, C. Moving to opportunity: The political effects of a housing mobility experiment. *Urban Affairs Review*, Sage Publications Sage CA: Los Angeles, CA, v. 48, n. 2, p. 147–179, 2012. Citado na página 58.

GERBER, A. S.; GREEN, D. P. *Field experiments: Design, analysis, and interpretation*. [S.l.]: WW Norton, 2012. Citado na página 91.

GIBBONS, C. E.; SERRATO, J. C. S.; URBANCIC, M. B. Broken or fixed effects? *Journal of Econometric Methods*, De Gruyter, v. 8, n. 1, 2018. Citado 15 vezes nas páginas 36, 37, 39, 63, 64, 65, 66, 67, 71, 83, 86, 87, 88, 89, and 90.

GIBSON, M. et al. Housing and health inequalities: a synthesis of systematic reviews of interventions aimed at different pathways linking housing and health. *Health & place*, Elsevier, v. 17, n. 1, p. 175–184, 2011. Citado na página 75.

GLAESER, E. *Triumph of the city: How urban spaces make us human*. [S.l.]: Pan Macmillan, 2011. Citado 2 vezes nas páginas 58 and 99.

GREENE, J. C.; CARACELLI, V. J. *Advances in mixed-method evaluation: The challenges and benefits of integrating diverse paradigms*. [S.l.]: Jossey-Bass„ 1997. Citado na página 30.

GURNEY, C. M. Pride and prejudice: Discourses of normalisation in public and private accounts of home ownership. *Housing studies*, Taylor & Francis, v. 14, n. 2, p. 163–183, 1999. Citado na página 78.

HAUSHOFER, J.; FEHR, E. On the psychology of poverty. *science*, American Association for the Advancement of Science, v. 344, n. 6186, p. 862–867, 2014. Citado na página 74.

HEADEY, B.; MUFFELS, R.; WOODEN, M. Money does not buy happiness: Or does it? a reassessment based on the combined effects of wealth, income and consumption. *Social Indicators Research*, Springer, v. 87, n. 1, p. 65–82, 2008. Citado na página 77.

JOHNSON, R. B.; ONWUEGBUZIE, A. J. Mixed methods research: A research paradigm whose time has come. *Educational researcher*, Sage Publications Sage CA: Thousand Oaks, CA, v. 33, n. 7, p. 14–26, 2004. Citado na página 30.

KAHNEMAN, D.; DEATON, A. High income improves evaluation of life but not emotional well-being. *Proceedings of the national academy of sciences*, National Acad Sciences, v. 107, n. 38, p. 16489–16493, 2010. Citado 7 vezes nas páginas 76, 77, 81, 82, 84, 90, and 91.

KAHNEMAN, D.; KRUEGER, A. B. Developments in the measurement of subjective well-being. *Journal of Economic perspectives*, v. 20, n. 1, p. 3–24, 2006. Citado 3 vezes nas páginas 76, 80, and 96.

KAMIENIECKI, G. W. Prevalence of psychological distress and psychiatric disorders among homeless youth in australia: a comparative review. *Australian & New Zealand Journal of Psychiatry*, Sage Publications Sage UK: London, England, v. 35, n. 3, p. 352–358, 2001. Citado na página 75.

KIM-PRIETO, C. et al. Integrating the diverse definitions of happiness: A time-sequential framework of subjective well-being. *Journal of happiness Studies*, Springer, v. 6, n. 3, p. 261–300, 2005. Citado na página 82.

KLING, J. R.; LIEBMAN, J. B.; KATZ, L. F. Experimental analysis of neighborhood effects. *Econometrica*, Wiley Online Library, v. 75, n. 1, p. 83–119, 2007. Citado 15 vezes nas páginas 60, 63, 64, 65, 66, 67, 71, 77, 82, 83, 86, 87, 88, 89, and 90.

KOKUBUN, K. Social capital may mediate the relationship between social distance and covid-19 prevalence. *arXiv preprint arXiv:2007.09939*, 2020. Citado 2 vezes nas páginas 53 and 56.

KUCHLER, T.; RUSSEL, D.; STROEBEL, J. *The geographic spread of COVID-19 correlates with structure of social networks as measured by Facebook*. [S.l.], 2020. Citado 2 vezes nas páginas 53 and 56.

LEGUIZAMON, S. The influence of reference group house size on house price. *Real estate economics*, Wiley Online Library, v. 38, n. 3, p. 507–527, 2010. Citado na página 78.

LEVENTHAL, T.; BROOKS-GUNN, J. Moving to opportunity: an experimental study of neighborhood effects on mental health. *American journal of public health*, American Public Health Association, v. 93, n. 9, p. 1576–1582, 2003. Citado na página 77.

LIN, W. Agnostic notes on regression adjustments to experimental data: Reexamining freedman's critique. *Ann. Appl. Stat.*, v. 7, n. 1, p. 295–318, 03 2013. Citado 16 vezes nas páginas 36, 37, 39, 44, 63, 64, 65, 66, 67, 71, 83, 86, 87, 88, 89, and 90.

LUCAS, R. E. et al. Cross-cultural evidence for the fundamental features of extraversion. *Journal of personality and social psychology*, American Psychological Association, v. 79, n. 3, p. 452, 2000. Citado na página 81.

LUDWIG, J. et al. Neighborhood effects on the long-term well-being of low-income adults. *Science*, American Association for the Advancement of Science, v. 337, n. 6101, p. 1505–1510, 2012. Citado 2 vezes nas páginas 78 and 83.

LUHMANN, M. et al. Subjective well-being and adaptation to life events: a meta-analysis. *Journal of personality and social psychology*, American Psychological Association, v. 102, n. 3, p. 592, 2012. Citado 4 vezes nas páginas 76, 77, 81, and 92.

LUTTMER, E. F. Neighbors as negatives: Relative earnings and well-being. *The Quarterly journal of economics*, MIT Press, v. 120, n. 3, p. 963–1002, 2005. Citado 2 vezes nas páginas 78 and 79.

MAAS, J. et al. Morbidity is related to a green living environment. *Journal of Epidemiology & Community Health*, BMJ Publishing Group Ltd, v. 63, n. 12, p. 967–973, 2009. Citado na página 74.

MIAO, J.; ZENG, D.; SHI, Z. Can neighborhoods protect residents from mental distress during the covid-19 pandemic? evidence from wuhan. *Chinese Sociological Review*, Taylor & Francis, p. 1–26, 2020. Citado 2 vezes nas páginas 53 and 56.

MOORE, T. et al. The effects of changes to the built environment on the mental health and well-being of adults: systematic review. *Health & place*, Elsevier, v. 53, p. 237–257, 2018. Citado na página 78.

MYERS, D. C. Close relationships and quality of life. in: Kahneman, d., diener, e., & schwarz, n. (eds.). *Well-being: Foundations of hedonic psychology*, Russell Sage Foundation New York, NY, p. 374, 2003. Citado na página 78.

NGUYEN, Q. C. et al. Heterogeneous effects of housing vouchers on the mental health of us adolescents. *American journal of public health*, American Public Health Association, v. 106, n. 4, p. 755–762, 2016. Citado na página 77.

OISHI, S. et al. Cross-cultural variations in predictors of life satisfaction: Perspectives from needs and values. In: *Culture and well-being*. [S.l.]: Springer, 2009. p. 109–127. Citado na página 79.

ORTEGA, F.; ORSINI, M. Governing covid-19 without government in brazil: Ignorance, neoliberal authoritarianism, and the collapse of public health leadership. *Global public health*, Taylor & Francis, v. 15, n. 9, p. 1257–1277, 2020. Citado na página 55.

OSTROM, E. Social capital: a fad or a fundamental concept. *Social capital: A multifaceted perspective*, v. 172, n. 173, p. 195–98, 2000. Citado na página 59.

OSTROM, E.; AHN, T.-K. The meaning of social capital and its link to collective action. p. 17 – 35, 2009. Citado 2 vezes nas páginas 56 and 99.

PEIRCE, C. S. *Collected papers of charles sanders peirce*. [S.l.]: Harvard University Press, 1974. v. 2. Citado na página 31.

PORTES, A. Social capital: Its origins and applications in modern sociology. *Annual review of sociology*, Annual Reviews 4139 El Camino Way, PO Box 10139, Palo Alto, CA 94303-0139, USA, v. 24, n. 1, p. 1–24, 1998. Citado na página 57.

PORTES, A.; LANDOLT, P. The downside of social capital. Washington, DC: The American Prospect, 1996. Citado 2 vezes nas páginas 72 and 73.

PUTNAM, R. D.; LEONARDI, R.; NANETTI, R. Y. *Making Democracy Work: Civic Traditions in Modern Italy*. [S.l.]: Princeton University Press, 1994. Citado na página 53.

PUTNAM, R. D. et al. *Bowling alone: The collapse and revival of American community*. [S.l.]: Simon and schuster, 2000. Citado 3 vezes nas páginas 17, 56, and 99.

READ, D. Experienced utility: utility theory from jeremy bentham to daniel kahneman. *Thinking & Reasoning*, Taylor & Francis, v. 13, n. 1, p. 45–61, 2007. Citado 2 vezes nas páginas 75 and 76.

RORTY, R. et al. *Philosophy and social hope*. [S.l.]: Penguin UK, 1999. Citado na página 31.

SACHS, J. D. *The end of poverty: Economic possibilities for our time*. [S.l.]: Penguin, 2006. Citado na página 17.

SCITOVSKY, T. The joyless economy: An inquiry into human satisfaction and consumer dissatisfaction. Oxford U Press, 1976. Citado na página 83.

SIMPSON, B. Pragmatism: A philosophy of practice. *The SAGE handbook of qualitative business and management research methods: History and traditions*, Sage, p. 54–68, 2017. Citado 2 vezes nas páginas 30 and 31.

STEVENSON, B.; WOLFERS, J. Subjective well-being and income: Is there any evidence of satiation? *American Economic Review*, v. 103, n. 3, p. 598–604, 2013. Citado na página 77.

TASHAKKORI, A.; TEDDLIE, C. The past and future of mixed methods research: From data triangulation to mixed model designs. *Handbook of mixed methods in social and behavioral research*, p. 671–701, 2003. Citado na página 30.

TASHAKKORI, A.; TEDDLIE, C.; TEDDLIE, C. B. *Mixed methodology: Combining qualitative and quantitative approaches*. [S.l.]: sage, 1998. v. 46. Citado na página 30.

THOMSON, H.; PETTICREW, M.; MORRISON, D. Health effects of housing improvement: systematic review of intervention studies. *Bmj*, British Medical Journal Publishing Group, v. 323, n. 7306, p. 187–190, 2001. Citado na página 75.

TORFING, J.; TRIANTAFILLOU, P. *Enhancing public innovation by transforming public governance*. [S.l.]: Cambridge University Press, 2016. Citado na página 21.

TSAI, L. L.; MORSE, B. S.; BLAIR, R. A. Building credibility and cooperation in low-trust settings: persuasion and source accountability in liberia during the 2014–2015 ebola crisis. *Comparative Political Studies*, SAGE Publications Sage CA: Los Angeles, CA, p. 0010414019897698, 2020. Citado 2 vezes nas páginas 56 and 72.

VARSHNEY, L. R.; SOCHER, R. Covid-19 growth rate decreases with social capital. *medRxiv*, Cold Spring Harbor Laboratory Press, 2020. Citado 2 vezes nas páginas 53 and 56.

WASDANI, K. P.; PRASAD, A. The impossibility of social distancing among the urban poor: the case of an indian slum in the times of covid-19. *Local Environment*, Taylor & Francis, v. 25, n. 5, p. 414–418, 2020. Citado 2 vezes nas páginas 53 and 55.

WELLS, N. M.; EVANS, G. W. Home injuries of people over age 65: risk perceptions of the elderly and of those who design for them. *Journal of environmental psychology*, Elsevier, v. 16, n. 3, p. 247–257, 1996. Citado na página 79.

WELLS NANCY M & EVANS, G. W. Physical stressors. in r. f. ballesteros (ed.). *Encyclopedia of psychological assessment*, London: Sage, 2003. Citado na página 79.

WEST, M. R. et al. Promise and paradox: Measuring students' non-cognitive skills and the impact of schooling. *Educational Evaluation and Policy Analysis*, Sage Publications Sage CA: Los Angeles, CA, v. 38, n. 1, p. 148–170, 2016. Citado na página 39.

WOOLCOCK, M. Social capital and economic development: Toward a theoretical synthesis and policy framework. *Theory and society*, JSTOR, v. 27, n. 2, p. 151–208, 1998. Citado 2 vezes nas páginas 56 and 73.

WU, C. Social capital and covid-19: a multidimensional and multilevel approach. *Chinese Sociological Review*, Taylor & Francis, p. 1–28, 2020. Citado 4 vezes nas páginas 53, 54, 56, and 57.

WU, C. et al. Social capital, trust, and state coronavirus testing. *Contexts*, 2020. Citado na página 53.

YIP, W. et al. Does social capital enhance health and well-being? evidence from rural china. *Social science & medicine*, Elsevier, v. 64, n. 1, p. 35–49, 2007. Citado 2 vezes nas páginas 56 and 57.

ZUMBRO, T. The relationship between homeownership and life satisfaction in germany. *Housing Studies*, Taylor & Francis, v. 29, n. 3, p. 319–338, 2014. Citado na página 79.

Appendix

.1 Regressions for Initial Offer

Table 22 – Preventive Behavior

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.139	0.122	1.133	0.258	366	25	
Standard Reg. with Cov. IPW	0.181	0.127	1.428	0.154	366	25	
Reg Lin - Only Comunity FE	0.146	0.125	1.168	0.244	366	25	
Reg Lin - All Cov.	0.164	0.125	1.308	0.192	366	25	
IWE - Without Covariate	0.146	0.123	1.187	0.235	366	25	
IWE - With Covariate	0.153	0.123	1.246	0.213	366	25	
RWE - Without Covariate	0.151	0.125	1.212	0.225	366	25	
RWE - With Covariate	0.153	0.126	1.216	0.224	366	25	
2SLS with FE, no covariates	0.376	0.325	1.159	0.247	366	25	

Table 23 – Prev Behav P16

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.013	0.120	0.107	0.915	366	25	-1.403	1.088	1
Standard Reg. with Cov. IPW	0.035	0.122	0.288	0.774	366	25	-1.403	1.088	1
Reg Lin - Only Comunity FE	-0.031	0.117	-0.268	0.789	366	25	-1.403	1.088	1
Reg Lin - All Cov.	-0.018	0.119	-0.150	0.881	366	25	-1.403	1.088	1
IWE - Without Covariate	-0.031	0.115	-0.273	0.784	366	25	-1.403	1.088	1
IWE - With Covariate	-0.013	0.117	-0.108	0.914	366	25	-1.403	1.088	1
RWE - Without Covariate	-0.043	0.123	-0.353	0.724	366	25	-1.403	1.088	1
RWE - With Covariate	-0.032	0.128	-0.250	0.803	366	25	-1.403	1.088	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
2SLS with FE, no covariates	-0.008	0.310	-0.026	0.979	366	25	-1.403	1.088	1

Table 24 – Prev Behav P17

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.112	0.114	0.976	0.330	366	25	-1.497	0.528	1
Standard Reg. with Cov. IPW	0.132	0.121	1.084	0.279	366	25	-1.497	0.528	1
Reg Lin - Only Comunity FE	0.138	0.116	1.185	0.237	366	25	-1.497	0.528	1
Reg Lin - All Cov.	0.141	0.116	1.212	0.226	366	25	-1.497	0.528	1
IWE - Without Covariate	0.138	0.116	1.187	0.235	366	25	-1.497	0.528	1
IWE - With Covariate	0.131	0.117	1.122	0.262	366	25	-1.497	0.528	1
RWE - Without Covariate	0.146	0.120	1.215	0.225	366	25	-1.497	0.528	1
RWE - With Covariate	0.130	0.123	1.064	0.288	366	25	-1.497	0.528	1
2SLS with FE, no covariates	0.307	0.314	0.978	0.329	366	25	-1.497	0.528	1

Table 25 – Prev Behav P18

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.098	0.119	0.824	0.410	366	25	-2.479	1.599	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov. IPW	0.124	0.116	1.066	0.287	366	25	-2.479	1.599	1
Reg Lin - Only Comunity FE	0.058	0.115	0.505	0.614	366	25	-2.479	1.599	1
Reg Lin - All Cov.	0.086	0.115	0.744	0.457	366	25	-2.479	1.599	1
IWE - Without Covariate	0.058	0.114	0.510	0.610	366	25	-2.479	1.599	1
IWE - With Covariate	0.080	0.114	0.701	0.483	366	25	-2.479	1.599	1
RWE - Without Covariate	0.056	0.119	0.469	0.639	366	25	-2.479	1.599	1
RWE - With Covariate	0.076	0.119	0.638	0.523	366	25	-2.479	1.599	1
2SLS with FE, no covariates	0.205	0.323	0.634	0.527	366	25	-2.479	1.599	1

Table 26 – Prev Behav P19_iso

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.240	0.130	1.843	0.066	366	25	0.119	0.324	0.595
Standard Reg. with Cov. IPW	0.216	0.135	1.593	0.112	366	25	0.119	0.324	1.000
Reg Lin - Only Comunity FE	0.229	0.130	1.768	0.078	366	25	0.119	0.324	0.702
Reg Lin - All Cov.	0.245	0.135	1.816	0.070	366	25	0.119	0.324	0.632

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
IWE - Without Covariate	0.229	0.130	1.767	0.077	366	25	0.119	0.324	0.695
IWE - With Covariate	0.241	0.133	1.815	0.070	366	25	0.119	0.324	0.626
RWE - Without Covariate	0.234	0.134	1.752	0.080	366	25	0.119	0.324	0.718
RWE - With Covariate	0.246	0.139	1.770	0.077	366	25	0.119	0.324	0.690
2SLS with FE, no covariates	0.633	0.332	1.904	0.058	366	25	0.119	0.324	0.519

Table 27 – Prev Behav P19_clean

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.013	0.108	-0.124	0.902	366	25	2.044	1.043	1
Standard Reg. with Cov. IPW	-0.051	0.113	-0.446	0.656	366	25	2.044	1.043	1
Reg Lin - Only Comunity FE	0.044	0.099	0.443	0.658	366	25	2.044	1.043	1
Reg Lin - All Cov.	0.012	0.098	0.126	0.899	366	25	2.044	1.043	1
IWE - Without Covariate	0.044	0.098	0.446	0.656	366	25	2.044	1.043	1
IWE - With Covariate	0.011	0.098	0.115	0.908	366	25	2.044	1.043	1
RWE - Without Covariate	0.044	0.106	0.413	0.679	366	25	2.044	1.043	1
RWE - With Covariate	0.006	0.107	0.059	0.953	366	25	2.044	1.043	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
2SLS with FE, no covariates	0.046	0.303	0.150	0.881	366	25	2.044	1.043	1

Table 28 – Prev Behav P20

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.144	0.099	1.458	0.146	366	25	0.843	0.364	1.000
Standard Reg. with Cov. IPW	0.194	0.102	1.910	0.057	366	25	0.843	0.364	0.512
Reg Lin - Only Comunity FE	0.189	0.091	2.081	0.038	366	25	0.843	0.364	0.344
Reg Lin - All Cov.	0.158	0.093	1.697	0.090	366	25	0.843	0.364	0.814
IWE - Without Covariate	0.189	0.091	2.083	0.037	366	25	0.843	0.364	0.336
IWE - With Covariate	0.170	0.093	1.824	0.068	366	25	0.843	0.364	0.614
RWE - Without Covariate	0.195	0.092	2.103	0.035	366	25	0.843	0.364	0.319
RWE - With Covariate	0.170	0.094	1.805	0.071	366	25	0.843	0.364	0.640
2SLS with FE, no covariates	0.444	0.295	1.504	0.134	366	25	0.843	0.364	1.000

Table 29 – Prev Behav P21

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	- 0.091	0.104	- 0.876	0.382	366	25	-1.089	0.3	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov. IPW	-0.019	0.123	-0.157	0.875	366	25	-1.089	0.3	1
Reg Lin - Only Comunity FE	-0.064	0.102	-0.631	0.529	366	25	-1.089	0.3	1
Reg Lin - All Cov.	-0.043	0.099	-0.433	0.665	366	25	-1.089	0.3	1
IWE - Without Covariate	-0.064	0.104	-0.620	0.536	366	25	-1.089	0.3	1
IWE - With Covariate	-0.049	0.102	-0.485	0.628	366	25	-1.089	0.3	1
RWE - Without Covariate	-0.062	0.108	-0.578	0.563	366	25	-1.089	0.3	1
RWE - With Covariate	-0.035	0.109	-0.323	0.747	366	25	-1.089	0.3	1
2SLS with FE, no covariates	-0.253	0.313	-0.808	0.419	366	25	-1.089	0.3	1

Table 30 – Prev Behav P22

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.082	0.116	-0.712	0.477	366	25	0.266	0.411	1
Standard Reg. with Cov. IPW	-0.066	0.108	-0.608	0.543	366	25	0.266	0.411	1
Reg Lin - Only Comunity FE	-0.112	0.105	-1.065	0.288	366	25	0.266	0.411	1
Reg Lin - All Cov.	-0.074	0.110	-0.672	0.502	366	25	0.266	0.411	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
IWE - Without Covariate	-0.112	0.105	-1.069	0.285	366	25	0.266	0.411	1
IWE - With Covariate	-0.097	0.108	-0.901	0.367	366	25	0.266	0.411	1
RWE - Without Covariate	-0.105	0.109	-0.962	0.336	366	25	0.266	0.411	1
RWE - With Covariate	-0.087	0.112	-0.778	0.437	366	25	0.266	0.411	1
2SLS with FE, no covariates	-0.251	0.310	-0.809	0.419	366	25	0.266	0.411	1

Table 31 – Prev Behav P27

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.020	0.121	0.164	0.870	366	25	0.894	0.308	1
Standard Reg. with Cov. IPW	0.005	0.120	0.044	0.965	366	25	0.894	0.308	1
Reg Lin - Only Comunity FE	0.010	0.122	0.081	0.935	366	25	0.894	0.308	1
Reg Lin - All Cov.	0.011	0.118	0.089	0.929	366	25	0.894	0.308	1
IWE - Without Covariate	0.010	0.118	0.084	0.933	366	25	0.894	0.308	1
IWE - With Covariate	0.010	0.117	0.086	0.932	366	25	0.894	0.308	1
RWE - Without Covariate	0.014	0.124	0.111	0.911	366	25	0.894	0.308	1
RWE - With Covariate	0.009	0.122	0.072	0.942	366	25	0.894	0.308	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
2SLS with FE, no covariates	0.065	0.315	0.207	0.836	366	25	0.894	0.308	1

Table 32 – Mental health

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.180	0.105	1.718	0.087	366	25	
Standard Reg. with Cov. IPW	0.290	0.106	2.732	0.007	366	25	
Reg Lin - Only Comunity FE	0.258	0.098	2.633	0.009	366	25	
Reg Lin - All Cov.	0.226	0.100	2.267	0.024	366	25	
IWE - Without Covariate	0.258	0.098	2.625	0.009	366	25	
IWE - With Covariate	0.237	0.100	2.366	0.018	366	25	
RWE - Without Covariate	0.264	0.105	2.516	0.012	366	25	
RWE - With Covariate	0.237	0.107	2.214	0.027	366	25	
2SLS with FE, no covariates	0.536	0.311	1.726	0.085	366	25	

Table 33 – Mental Health P28

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.122	0.109	1.116	0.265	366	25	-1.236	0.423	1.000
Standard Reg. with Cov. IPW	0.133	0.108	1.231	0.219	366	25	-1.236	0.423	1.000
Reg Lin - Only Comunity FE	0.156	0.105	1.496	0.135	366	25	-1.236	0.423	0.948
Reg Lin - All Cov.	0.141	0.104	1.354	0.177	366	25	-1.236	0.423	1.000
IWE - Without Covariate	0.156	0.105	1.496	0.135	366	25	-1.236	0.423	0.943
IWE - With Covariate	0.141	0.107	1.314	0.189	366	25	-1.236	0.423	1.000

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
RWE - Without Covariate	0.157	0.109	1.440	0.150	366	25	-1.236	0.423	1.000
RWE - With Covariate	0.142	0.113	1.250	0.211	366	25	-1.236	0.423	1.000
2SLS with FE, no covariates	0.377	0.303	1.241	0.216	366	25	-1.236	0.423	1.000

Table 34 – Mental Health P29

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.202	0.115	1.759	0.079	366	25	1.428	0.496	0.556
Standard Reg. with Cov. IPW	0.234	0.117	1.994	0.047	366	25	1.428	0.496	0.329
Reg Lin - Only Comunity FE	0.283	0.112	2.529	0.012	366	25	1.428	0.496	0.083
Reg Lin - All Cov.	0.241	0.114	2.121	0.035	366	25	1.428	0.496	0.242
IWE - Without Covariate	0.283	0.111	2.555	0.011	366	25	1.428	0.496	0.074
IWE - With Covariate	0.260	0.113	2.309	0.021	366	25	1.428	0.496	0.147
RWE - Without Covariate	0.288	0.116	2.489	0.013	366	25	1.428	0.496	0.090
RWE - With Covariate	0.262	0.116	2.258	0.024	366	25	1.428	0.496	0.167
2SLS with FE, no covariates	0.607	0.324	1.872	0.062	366	25	1.428	0.496	0.434

Table 35 – Mental Health P30

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.012	0.107	-0.107	0.914	366	25	1.706	0.455	1
Standard Reg. with Cov. IPW	0.090	0.114	0.791	0.429	366	25	1.706	0.455	1
Reg Lin - Only Comunity FE	0.014	0.099	0.137	0.891	366	25	1.706	0.455	1
Reg Lin - All Cov.	0.015	0.101	0.147	0.883	366	25	1.706	0.455	1
IWE - Without Covariate	0.014	0.100	0.136	0.891	366	25	1.706	0.455	1
IWE - With Covariate	0.019	0.102	0.187	0.852	366	25	1.706	0.455	1
RWE - Without Covariate	0.017	0.112	0.148	0.882	366	25	1.706	0.455	1
RWE - With Covariate	0.028	0.115	0.246	0.806	366	25	1.706	0.455	1
2SLS with FE, no covariates	-0.021	0.296	-0.072	0.942	366	25	1.706	0.455	1

Table 36 – Mental Health P31

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.119	0.109	1.090	0.277	366	25	1.648	0.479	1.000
Standard Reg. with Cov. IPW	0.190	0.110	1.726	0.085	366	25	1.648	0.479	0.596
Reg Lin - Only Comunity FE	0.179	0.103	1.743	0.082	366	25	1.648	0.479	0.576

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Reg Lin - All Cov.	0.174	0.105	1.661	0.098	366	25	1.648	0.479	0.683
IWE - Without Covariate	0.179	0.102	1.754	0.079	366	25	1.648	0.479	0.556
IWE - With Covariate	0.174	0.104	1.673	0.094	366	25	1.648	0.479	0.660
RWE - Without Covariate	0.183	0.109	1.678	0.093	366	25	1.648	0.479	0.653
RWE - With Covariate	0.170	0.112	1.515	0.130	366	25	1.648	0.479	0.908
2SLS with FE, no covariates	0.327	0.300	1.091	0.276	366	25	1.648	0.479	1.000

Table 37 – Mental Health P32

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.045	0.109	0.409	0.683	366	25	-2.373	1.117	1.000
Standard Reg. with Cov. IPW	0.189	0.113	1.671	0.096	366	25	-2.373	1.117	0.669
Reg Lin - Only Comunity FE	0.117	0.110	1.071	0.285	366	25	-2.373	1.117	1.000
Reg Lin - All Cov.	0.079	0.110	0.717	0.474	366	25	-2.373	1.117	1.000
IWE - Without Covariate	0.117	0.110	1.065	0.287	366	25	-2.373	1.117	1.000
IWE - With Covariate	0.077	0.110	0.696	0.486	366	25	-2.373	1.117	1.000
RWE - Without Covariate	0.124	0.115	1.077	0.282	366	25	-2.373	1.117	1.000

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
RWE - With Covariate	0.073	0.115	0.631	0.528	366	25	-2.373	1.117	1.000
2SLS with FE, no covariates	0.208	0.307	0.677	0.499	366	25	-2.373	1.117	1.000

Table 38 – Mental Health P33

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.189	0.091	2.066	0.040	366	25	1.907	0.291	0.277
Standard Reg. with Cov. IPW	0.223	0.087	2.564	0.011	366	25	1.907	0.291	0.075
Reg Lin - Only Comunity FE	0.210	0.084	2.497	0.013	366	25	1.907	0.291	0.091
Reg Lin - All Cov.	0.182	0.086	2.113	0.035	366	25	1.907	0.291	0.247
IWE - Without Covariate	0.210	0.081	2.597	0.009	366	25	1.907	0.291	0.066
IWE - With Covariate	0.194	0.082	2.364	0.018	366	25	1.907	0.291	0.127
RWE - Without Covariate	0.216	0.084	2.562	0.010	366	25	1.907	0.291	0.073
RWE - With Covariate	0.189	0.083	2.270	0.023	366	25	1.907	0.291	0.162
2SLS with FE, no covariates	0.530	0.296	1.794	0.074	366	25	1.907	0.291	0.516

Table 39 – Mental Health P34

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.129	0.106	1.225	0.221	366	25	1.753	0.431	1.000
Standard Reg. with Cov. IPW	0.218	0.104	2.101	0.036	366	25	1.753	0.431	0.254
Reg Lin - Only Comunity FE	0.181	0.093	1.946	0.052	366	25	1.753	0.431	0.367
Reg Lin - All Cov.	0.165	0.098	1.686	0.093	366	25	1.753	0.431	0.649
IWE - Without Covariate	0.181	0.093	1.949	0.051	366	25	1.753	0.431	0.359
IWE - With Covariate	0.183	0.097	1.879	0.060	366	25	1.753	0.431	0.421
RWE - Without Covariate	0.180	0.099	1.809	0.070	366	25	1.753	0.431	0.493
RWE - With Covariate	0.180	0.103	1.750	0.080	366	25	1.753	0.431	0.561
2SLS with FE, no covariates	0.341	0.306	1.114	0.266	366	25	1.753	0.431	1.000

Table 40 – Claim-making

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	-0.011	0.105	-0.109	0.913	366	25	
Standard Reg. with Cov. IPW	-0.121	0.113	-1.072	0.285	366	25	
Reg Lin - Only Comunity FE	-0.015	0.102	-0.149	0.882	366	25	
Reg Lin - All Cov.	-0.012	0.105	-0.110	0.912	366	25	
IWE - Without Covariate	-0.015	0.103	-0.148	0.882	366	25	
IWE - With Covariate	-0.012	0.104	-0.114	0.909	366	25	
RWE - Without Covariate	-0.022	0.105	-0.208	0.835	366	25	
RWE - With Covariate	-0.017	0.107	-0.159	0.873	366	25	
2SLS with FE, no covariates	-0.017	0.299	-0.057	0.954	366	25	

Table 41 – Claim_P55

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.002	0.117	-0.015	0.988	366	25	-1.936	0.244	1
Standard Reg. with Cov. IPW	0.012	0.130	0.094	0.925	366	25	-1.936	0.244	1
Reg Lin - Only Comunity FE	-0.038	0.100	-0.376	0.707	366	25	-1.936	0.244	1
Reg Lin - All Cov.	-0.011	0.108	-0.102	0.918	366	25	-1.936	0.244	1
IWE - Without Covariate	-0.038	0.096	-0.391	0.696	366	25	-1.936	0.244	1
IWE - With Covariate	-0.030	0.100	-0.305	0.760	366	25	-1.936	0.244	1
RWE - Without Covariate	-0.056	0.100	-0.555	0.579	366	25	-1.936	0.244	1
RWE - With Covariate	-0.043	0.103	-0.420	0.674	366	25	-1.936	0.244	1
2SLS with FE, no covariates	-0.022	0.317	-0.070	0.944	366	25	-1.936	0.244	1

Table 42 – Claim_P56

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.034	0.108	-0.310	0.757	366	25	-1.799	0.4	1
Standard Reg. with Cov. IPW	-0.109	0.114	-0.961	0.337	366	25	-1.799	0.4	1
Reg Lin - Only Comunity FE	-0.050	0.103	-0.484	0.629	366	25	-1.799	0.4	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Reg Lin - All Cov.	-0.062	0.106	-0.589	0.556	366	25	-1.799	0.4	1
IWE - Without Covariate	-0.050	0.105	-0.475	0.634	366	25	-1.799	0.4	1
IWE - With Covariate	-0.067	0.108	-0.619	0.536	366	25	-1.799	0.4	1
RWE - Without Covariate	-0.060	0.109	-0.550	0.582	366	25	-1.799	0.4	1
RWE - With Covariate	-0.081	0.112	-0.720	0.472	366	25	-1.799	0.4	1
2SLS with FE, no covariates	-0.042	0.309	-0.136	0.892	366	25	-1.799	0.4	1

Table 43 – Claim_P57

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.003	0.103	-0.027	0.979	366	25	-1.782	0.412	1
Standard Reg. with Cov. IPW	-0.108	0.113	-0.955	0.340	366	25	-1.782	0.412	1
Reg Lin - Only Comunity FE	-0.029	0.097	-0.297	0.767	366	25	-1.782	0.412	1
Reg Lin - All Cov.	-0.043	0.098	-0.442	0.659	366	25	-1.782	0.412	1
IWE - Without Covariate	-0.029	0.096	-0.300	0.764	366	25	-1.782	0.412	1
IWE - With Covariate	-0.036	0.096	-0.377	0.706	366	25	-1.782	0.412	1
RWE - Without Covariate	-0.033	0.112	-0.296	0.767	366	25	-1.782	0.412	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
RWE - With Covariate	-0.038	0.112	-0.335	0.738	366	25	-1.782	0.412	1
2SLS with FE, no covariates	0.029	0.293	0.100	0.921	366	25	-1.782	0.412	1

Table 44 – Claim_P46

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.116	0.115	1.001	0.317	366	25	-1.617	0.434	1.000
Standard Reg. with Cov. IPW	0.089	0.122	0.729	0.466	366	25	-1.617	0.434	1.000
Reg Lin - Only Comunity FE	0.152	0.114	1.330	0.184	366	25	-1.617	0.434	1.000
Reg Lin - All Cov.	0.174	0.116	1.502	0.134	366	25	-1.617	0.434	0.804
IWE - Without Covariate	0.152	0.115	1.321	0.186	366	25	-1.617	0.434	1.000
IWE - With Covariate	0.171	0.116	1.477	0.140	366	25	-1.617	0.434	0.837
RWE - Without Covariate	0.153	0.126	1.212	0.226	366	25	-1.617	0.434	1.000
RWE - With Covariate	0.173	0.126	1.372	0.170	366	25	-1.617	0.434	1.000
2SLS with FE, no covariates	0.297	0.320	0.930	0.353	366	25	-1.617	0.434	1.000

Table 45 – Claim_P47

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.046	0.109	0.428	0.669	366	25	-1.441	0.492	1
Standard Reg. with Cov. IPW	-0.065	0.116	-0.558	0.577	366	25	-1.441	0.492	1
Reg Lin - Only Comunity FE	0.034	0.108	0.310	0.756	366	25	-1.441	0.492	1
Reg Lin - All Cov.	0.011	0.109	0.096	0.923	366	25	-1.441	0.492	1
IWE - Without Covariate	0.034	0.107	0.312	0.755	366	25	-1.441	0.492	1
IWE - With Covariate	0.026	0.109	0.239	0.811	366	25	-1.441	0.492	1
RWE - Without Covariate	0.040	0.111	0.358	0.721	366	25	-1.441	0.492	1
RWE - With Covariate	0.026	0.110	0.241	0.810	366	25	-1.441	0.492	1
2SLS with FE, no covariates	0.128	0.293	0.436	0.663	366	25	-1.441	0.492	1

Table 46 – Claim_P48

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.155	0.115	-1.343	0.180	366	25	1.817	0.387	1.000
Standard Reg. with Cov. IPW	-0.143	0.122	-1.174	0.241	366	25	1.817	0.387	1.000
Reg Lin - Only Comunity FE	-0.110	0.107	-1.024	0.307	366	25	1.817	0.387	1.000

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Reg Lin - All Cov.	- 0.099	0.112	- 0.886	0.376	366	25	1.817	0.387	1.000
IWE - Without Covariate	- 0.110	0.107	- 1.026	0.305	366	25	1.817	0.387	1.000
IWE - With Covariate	- 0.095	0.110	- 0.865	0.387	366	25	1.817	0.387	1.000
RWE - Without Covariate	- 0.103	0.111	- 0.925	0.355	366	25	1.817	0.387	1.000
RWE - With Covariate	- 0.084	0.114	- 0.737	0.461	366	25	1.817	0.387	1.000
2SLS with FE, no covariates	- 0.436	0.312	- 1.396	0.164	366	25	1.817	0.387	0.982

Table 47 – Solidarity

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.208	0.109	1.905	0.058	366	25	
Standard Reg. with Cov. IPW	0.197	0.116	1.687	0.092	366	25	
Reg Lin - Only Comunity FE	0.225	0.114	1.968	0.050	366	25	
Reg Lin - All Cov.	0.192	0.118	1.632	0.103	366	25	
IWE - Without Covariate	0.225	0.115	1.959	0.050	366	25	
IWE - With Covariate	0.196	0.117	1.676	0.094	366	25	
RWE - Without Covariate	0.234	0.118	1.978	0.048	366	25	
RWE - With Covariate	0.188	0.119	1.573	0.116	366	25	
2SLS with FE, no covariates	0.626	0.318	1.968	0.050	366	25	

Table 48 – P50_1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.020	0.115	0.171	0.864	366	25	-1.711	0.787	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov. IPW	0.061	0.114	0.529	0.597	366	25	-1.711	0.787	1
Reg Lin - Only Comunity FE	0.036	0.109	0.327	0.744	366	25	-1.711	0.787	1
Reg Lin - All Cov.	0.006	0.111	0.051	0.959	366	25	-1.711	0.787	1
IWE - Without Covariate	0.036	0.108	0.331	0.741	366	25	-1.711	0.787	1
IWE - With Covariate	0.035	0.110	0.318	0.750	366	25	-1.711	0.787	1
RWE - Without Covariate	0.041	0.113	0.360	0.719	366	25	-1.711	0.787	1
RWE - With Covariate	0.041	0.115	0.357	0.721	366	25	-1.711	0.787	1
2SLS with FE, no covariates	0.061	0.309	0.196	0.845	366	25	-1.711	0.787	1

Table 49 – P50_2

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.270	0.107	2.532	0.012	366	25	-2.264	0.729	0.094
Standard Reg. with Cov. IPW	0.258	0.115	2.242	0.026	366	25	-2.264	0.729	0.205
Reg Lin - Only Comunity FE	0.249	0.111	2.247	0.025	366	25	-2.264	0.729	0.202
Reg Lin - All Cov.	0.213	0.111	1.923	0.055	366	25	-2.264	0.729	0.442

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
IWE - Without Covariate	0.249	0.112	2.217	0.027	366	25	-2.264	0.729	0.213
IWE - With Covariate	0.226	0.112	2.010	0.044	366	25	-2.264	0.729	0.355
RWE - Without Covariate	0.251	0.116	2.164	0.030	366	25	-2.264	0.729	0.243
RWE - With Covariate	0.212	0.118	1.804	0.071	366	25	-2.264	0.729	0.570
2SLS with FE, no covariates	0.768	0.320	2.397	0.017	366	25	-2.264	0.729	0.136

Table 50 – P50_3

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.118	0.111	1.055	0.292	366	25	-2.013	0.699	1
Standard Reg. with Cov. IPW	0.131	0.114	1.148	0.252	366	25	-2.013	0.699	1
Reg Lin - Only Comunity FE	0.115	0.118	0.974	0.331	366	25	-2.013	0.699	1
Reg Lin - All Cov.	0.108	0.122	0.890	0.374	366	25	-2.013	0.699	1
IWE - Without Covariate	0.115	0.119	0.970	0.332	366	25	-2.013	0.699	1
IWE - With Covariate	0.108	0.121	0.887	0.375	366	25	-2.013	0.699	1
RWE - Without Covariate	0.116	0.119	0.975	0.329	366	25	-2.013	0.699	1
RWE - With Covariate	0.101	0.119	0.849	0.396	366	25	-2.013	0.699	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
2SLS with FE, no covariates	0.311	0.299	1.038	0.300	366	25	-2.013	0.699	1

Table 51 – P50_4

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.262	0.105	2.485	0.013	366	25	-1.893	0.687	0.107
Standard Reg. with Cov. IPW	0.266	0.112	2.378	0.018	366	25	-1.893	0.687	0.143
Reg Lin - Only Comunity FE	0.290	0.101	2.877	0.004	366	25	-1.893	0.687	0.034
Reg Lin - All Cov.	0.292	0.103	2.845	0.005	366	25	-1.893	0.687	0.038
IWE - Without Covariate	0.290	0.101	2.885	0.004	366	25	-1.893	0.687	0.031
IWE - With Covariate	0.290	0.102	2.837	0.005	366	25	-1.893	0.687	0.036
RWE - Without Covariate	0.307	0.111	2.764	0.006	366	25	-1.893	0.687	0.046
RWE - With Covariate	0.289	0.113	2.559	0.010	366	25	-1.893	0.687	0.084
2SLS with FE, no covariates	0.705	0.318	2.219	0.027	366	25	-1.893	0.687	0.217

Table 52 – P50_5

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.153	0.117	1.310	0.191	366	25	-1.946	0.753	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov. IPW	0.122	0.123	0.995	0.320	366	25	-1.946	0.753	1
Reg Lin - Only Comunity FE	0.135	0.114	1.182	0.238	366	25	-1.946	0.753	1
Reg Lin - All Cov.	0.116	0.116	1.001	0.317	366	25	-1.946	0.753	1
IWE - Without Covariate	0.135	0.114	1.183	0.237	366	25	-1.946	0.753	1
IWE - With Covariate	0.115	0.116	0.995	0.320	366	25	-1.946	0.753	1
RWE - Without Covariate	0.142	0.127	1.122	0.262	366	25	-1.946	0.753	1
RWE - With Covariate	0.112	0.126	0.886	0.376	366	25	-1.946	0.753	1
2SLS with FE, no covariates	0.452	0.309	1.462	0.145	366	25	-1.946	0.753	1

Table 53 – P50_6

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.104	0.117	-0.888	0.375	366	25	-2.698	0.567	1
Standard Reg. with Cov. IPW	-0.037	0.124	-0.303	0.762	366	25	-2.698	0.567	1
Reg Lin - Only Comunity FE	-0.068	0.117	-0.584	0.560	366	25	-2.698	0.567	1
Reg Lin - All Cov.	-0.066	0.117	-0.565	0.573	366	25	-2.698	0.567	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
IWE - Without Covariate	-0.068	0.114	-0.595	0.552	366	25	-2.698	0.567	1
IWE - With Covariate	-0.064	0.115	-0.559	0.576	366	25	-2.698	0.567	1
RWE - Without Covariate	-0.061	0.125	-0.490	0.624	366	25	-2.698	0.567	1
RWE - With Covariate	-0.070	0.122	-0.570	0.569	366	25	-2.698	0.567	1
2SLS with FE, no covariates	-0.304	0.311	-0.975	0.330	366	25	-2.698	0.567	1

Table 54 – P51

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.150	0.113	1.322	0.187	366	25	1.475	1.503	1.000
Standard Reg. with Cov. IPW	0.097	0.120	0.803	0.422	366	25	1.475	1.503	1.000
Reg Lin - Only Comunity FE	0.172	0.119	1.444	0.150	366	25	1.475	1.503	1.000
Reg Lin - All Cov.	0.159	0.122	1.306	0.192	366	25	1.475	1.503	1.000
IWE - Without Covariate	0.172	0.119	1.450	0.147	366	25	1.475	1.503	1.000
IWE - With Covariate	0.143	0.124	1.155	0.248	366	25	1.475	1.503	1.000
RWE - Without Covariate	0.167	0.123	1.360	0.174	366	25	1.475	1.503	1.000
RWE - With Covariate	0.138	0.130	1.063	0.288	366	25	1.475	1.503	1.000

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
2SLS with FE, no covariates	0.494	0.312	1.584	0.114	366	25	1.475	1.503	0.914

Table 55 – P52

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.057	0.100	-0.577	0.564	366	25	1.225	1.523	1
Standard Reg. with Cov. IPW	-0.133	0.108	-1.229	0.220	366	25	1.225	1.523	1
Reg Lin - Only Comunity FE	-0.056	0.096	-0.589	0.556	366	25	1.225	1.523	1
Reg Lin - All Cov.	-0.082	0.095	-0.861	0.390	366	25	1.225	1.523	1
IWE - Without Covariate	-0.056	0.096	-0.588	0.556	366	25	1.225	1.523	1
IWE - With Covariate	-0.093	0.096	-0.965	0.334	366	25	1.225	1.523	1
RWE - Without Covariate	-0.055	0.097	-0.565	0.572	366	25	1.225	1.523	1
RWE - With Covariate	-0.095	0.099	-0.963	0.335	366	25	1.225	1.523	1
2SLS with FE, no covariates	-0.054	0.294	-0.184	0.854	366	25	1.225	1.523	1

Table 56 – NGO Accountability

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.054	0.110	0.494	0.622	366	25	
Standard Reg. with Cov. IPW	-0.039	0.115	-0.340	0.734	366	25	
Reg Lin - Only Comunity FE	0.066	0.099	0.665	0.507	366	25	

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Reg Lin - All Cov.	0.060	0.102	0.590	0.555	366	25	
IWE - Without Covariate	0.066	0.095	0.687	0.492	366	25	
IWE - With Covariate	0.075	0.098	0.762	0.446	366	25	
RWE - Without Covariate	0.069	0.102	0.680	0.497	366	25	
RWE - With Covariate	0.077	0.105	0.730	0.466	366	25	
2SLS with FE, no covariates	0.159	0.292	0.546	0.585	366	25	

Table 57 – NGO Accountability P53

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.081	0.122	-0.663	0.507	366	25	1.617	0.789	1
Standard Reg. with Cov. IPW	-0.151	0.122	-1.237	0.217	366	25	1.617	0.789	1
Reg Lin - Only Comunity FE	-0.066	0.115	-0.578	0.564	366	25	1.617	0.789	1
Reg Lin - All Cov.	-0.030	0.113	-0.264	0.792	366	25	1.617	0.789	1
IWE - Without Covariate	-0.066	0.113	-0.588	0.557	366	25	1.617	0.789	1
IWE - With Covariate	-0.040	0.113	-0.351	0.726	366	25	1.617	0.789	1
RWE - Without Covariate	-0.065	0.115	-0.565	0.572	366	25	1.617	0.789	1
RWE - With Covariate	-0.035	0.117	-0.301	0.763	366	25	1.617	0.789	1
2SLS with FE, no covariates	-0.236	0.314	-0.752	0.452	366	25	1.617	0.789	1

Table 58 – NGO Accountability P54

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.260	0.111	2.332	0.020	366	25	1.182	0.906	0.101
Standard Reg. with Cov. IPW	0.277	0.111	2.486	0.013	366	25	1.182	0.906	0.067
Reg Lin - Only Comunity FE	0.282	0.100	2.813	0.005	366	25	1.182	0.906	0.026
Reg Lin - All Cov.	0.284	0.103	2.770	0.006	366	25	1.182	0.906	0.030
IWE - Without Covariate	0.282	0.098	2.868	0.004	366	25	1.182	0.906	0.021
IWE - With Covariate	0.311	0.102	3.057	0.002	366	25	1.182	0.906	0.011
RWE - Without Covariate	0.286	0.102	2.806	0.005	366	25	1.182	0.906	0.025
RWE - With Covariate	0.315	0.106	2.976	0.003	366	25	1.182	0.906	0.015
2SLS with FE, no covariates	0.650	0.309	2.104	0.036	366	25	1.182	0.906	0.181

Table 59 – NGO Accountability P55

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.002	0.117	0.015	0.988	366	25	0.936	0.244	1
Standard Reg. with Cov. IPW	- 0.012	0.130	- 0.094	0.925	366	25	0.936	0.244	1
Reg Lin - Only Comunity FE	0.038	0.100	0.376	0.707	366	25	0.936	0.244	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Reg Lin - All Cov.	0.011	0.108	0.102	0.918	366	25	0.936	0.244	1
IWE - Without Covariate	0.038	0.096	0.391	0.696	366	25	0.936	0.244	1
IWE - With Covariate	0.030	0.100	0.305	0.760	366	25	0.936	0.244	1
RWE - Without Covariate	0.056	0.100	0.555	0.579	366	25	0.936	0.244	1
RWE - With Covariate	0.043	0.103	0.420	0.674	366	25	0.936	0.244	1
2SLS with FE, no covariates	0.022	0.317	0.070	0.944	366	25	0.936	0.244	1

Table 60 – NGO Accountability P56

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.034	0.108	-0.310	0.757	366	25	0.201	0.4	1
Standard Reg. with Cov. IPW	-0.109	0.114	-0.961	0.337	366	25	0.201	0.4	1
Reg Lin - Only Comunity FE	-0.050	0.103	-0.484	0.629	366	25	0.201	0.4	1
Reg Lin - All Cov.	-0.062	0.106	-0.589	0.556	366	25	0.201	0.4	1
IWE - Without Covariate	-0.050	0.105	-0.475	0.634	366	25	0.201	0.4	1
IWE - With Covariate	-0.067	0.108	-0.619	0.536	366	25	0.201	0.4	1
RWE - Without Covariate	-0.060	0.109	-0.550	0.582	366	25	0.201	0.4	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
RWE - With Covariate	-0.081	0.112	-0.720	0.472	366	25	0.201	0.4	1
2SLS with FE, no covariates	-0.042	0.309	-0.136	0.892	366	25	0.201	0.4	1

Table 61 – NGO Accountability P57

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.003	0.103	-0.027	0.979	366	25	0.218	0.412	1
Standard Reg. with Cov. IPW	-0.108	0.113	-0.955	0.340	366	25	0.218	0.412	1
Reg Lin - Only Comunity FE	-0.029	0.097	-0.297	0.767	366	25	0.218	0.412	1
Reg Lin - All Cov.	-0.043	0.098	-0.442	0.659	366	25	0.218	0.412	1
IWE - Without Covariate	-0.029	0.096	-0.300	0.764	366	25	0.218	0.412	1
IWE - With Covariate	-0.036	0.096	-0.377	0.706	366	25	0.218	0.412	1
RWE - Without Covariate	-0.033	0.112	-0.296	0.767	366	25	0.218	0.412	1
RWE - With Covariate	-0.038	0.112	-0.335	0.738	366	25	0.218	0.412	1
2SLS with FE, no covariates	0.029	0.293	0.100	0.921	366	25	0.218	0.412	1

Table 62 – Trust

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.206	0.114	1.805	0.072	366	25	
Standard Reg. with Cov. IPW	0.214	0.117	1.829	0.068	366	25	
Reg Lin - Only Comunity FE	0.184	0.105	1.755	0.080	366	25	
Reg Lin - All Cov.	0.180	0.107	1.688	0.092	366	25	
IWE - Without Covariate	0.184	0.105	1.764	0.078	366	25	
IWE - With Covariate	0.186	0.106	1.762	0.078	366	25	
RWE - Without Covariate	0.181	0.123	1.468	0.142	366	25	
RWE - With Covariate	0.174	0.126	1.383	0.167	366	25	
2SLS with FE, no covariates	0.556	0.307	1.813	0.071	366	25	

Table 63 – Trust P58_1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.089	0.113	0.786	0.432	366	25	0.525	1.113	0.864
Standard Reg. with Cov. IPW	0.139	0.115	1.207	0.228	366	25	0.525	1.113	0.457
Reg Lin - Only Comunity FE	0.066	0.110	0.601	0.548	366	25	0.525	1.113	1.000
Reg Lin - All Cov.	0.068	0.112	0.605	0.546	366	25	0.525	1.113	1.000
IWE - Without Covariate	0.066	0.111	0.600	0.549	366	25	0.525	1.113	1.000
IWE - With Covariate	0.084	0.114	0.744	0.457	366	25	0.525	1.113	0.914
RWE - Without Covariate	0.064	0.118	0.544	0.587	366	25	0.525	1.113	1.000
RWE - With Covariate	0.081	0.121	0.668	0.504	366	25	0.525	1.113	1.000
2SLS with FE, no covariates	0.185	0.310	0.597	0.551	366	25	0.525	1.113	1.000

Table 64 – Trust P59_1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.241	0.109	2.200	0.028	366	25	0.841	1.115	0.057
Standard Reg. with Cov. IPW	0.204	0.117	1.743	0.082	366	25	0.841	1.115	0.164
Reg Lin - Only Comunity FE	0.228	0.094	2.416	0.016	366	25	0.841	1.115	0.032
Reg Lin - All Cov.	0.220	0.096	2.284	0.023	366	25	0.841	1.115	0.046
IWE - Without Covariate	0.228	0.094	2.425	0.015	366	25	0.841	1.115	0.031
IWE - With Covariate	0.213	0.095	2.246	0.025	366	25	0.841	1.115	0.049
RWE - Without Covariate	0.224	0.120	1.869	0.062	366	25	0.841	1.115	0.123
RWE - With Covariate	0.197	0.123	1.606	0.108	366	25	0.841	1.115	0.217
2SLS with FE, no covariates	0.702	0.303	2.319	0.021	366	25	0.841	1.115	0.042

Table 65 – Policy competence

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	-0.003	0.109	-0.027	0.978	366	25	
Standard Reg. with Cov. IPW	0.016	0.111	0.145	0.885	366	25	
Reg Lin - Only Comunity FE	0.065	0.104	0.630	0.529	366	25	
Reg Lin - All Cov.	0.039	0.107	0.365	0.716	366	25	
IWE - Without Covariate	0.065	0.104	0.629	0.530	366	25	
IWE - With Covariate	0.051	0.106	0.477	0.634	366	25	
RWE - Without Covariate	0.068	0.111	0.614	0.540	366	25	
RWE - With Covariate	0.066	0.111	0.595	0.552	366	25	
2SLS with FE, no covariates	0.041	0.302	0.137	0.891	366	25	

Table 66 – Pol_comp_P44

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.082	0.109	0.754	0.452	366	25	-1.208	0.406	1.000
Standard Reg. with Cov. IPW	0.071	0.107	0.664	0.507	366	25	-1.208	0.406	1.000
Reg Lin - Only Comunity FE	0.136	0.103	1.323	0.187	366	25	-1.208	0.406	0.560
Reg Lin - All Cov.	0.096	0.104	0.926	0.355	366	25	-1.208	0.406	1.000
IWE - Without Covariate	0.136	0.103	1.317	0.188	366	25	-1.208	0.406	0.563
IWE - With Covariate	0.102	0.104	0.990	0.322	366	25	-1.208	0.406	0.967
RWE - Without Covariate	0.137	0.107	1.277	0.201	366	25	-1.208	0.406	0.604
RWE - With Covariate	0.113	0.107	1.060	0.289	366	25	-1.208	0.406	0.867
2SLS with FE, no covariates	0.311	0.312	1.000	0.318	366	25	-1.208	0.406	0.955

Table 67 – Pol_comp_P45

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.067	0.112	0.595	0.552	366	25	-1.432	0.496	1
Standard Reg. with Cov. IPW	0.104	0.116	0.890	0.374	366	25	-1.432	0.496	1
Reg Lin - Only Comunity FE	0.101	0.112	0.906	0.365	366	25	-1.432	0.496	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Reg Lin - All Cov.	0.079	0.113	0.702	0.483	366	25	-1.432	0.496	1
IWE - Without Covariate	0.101	0.112	0.902	0.367	366	25	-1.432	0.496	1
IWE - With Covariate	0.092	0.114	0.808	0.419	366	25	-1.432	0.496	1
RWE - Without Covariate	0.099	0.116	0.852	0.394	366	25	-1.432	0.496	1
RWE - With Covariate	0.100	0.116	0.862	0.389	366	25	-1.432	0.496	1
2SLS with FE, no covariates	0.205	0.306	0.670	0.504	366	25	-1.432	0.496	1

Table 68 – Pol_comp_P48

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.155	0.115	-1.343	0.180	366	25	1.817	0.387	0.541
Standard Reg. with Cov. IPW	-0.143	0.122	-1.174	0.241	366	25	1.817	0.387	0.724
Reg Lin - Only Comunity FE	-0.110	0.107	-1.024	0.307	366	25	1.817	0.387	0.920
Reg Lin - All Cov.	-0.099	0.112	-0.886	0.376	366	25	1.817	0.387	1.000
IWE - Without Covariate	-0.110	0.107	-1.026	0.305	366	25	1.817	0.387	0.915
IWE - With Covariate	-0.095	0.110	-0.865	0.387	366	25	1.817	0.387	1.000
RWE - Without Covariate	-0.103	0.111	-0.925	0.355	366	25	1.817	0.387	1.000

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
RWE - With Covariate	-0.084	0.114	-0.737	0.461	366	25	1.817	0.387	1.000
2SLS with FE, no covariates	-0.436	0.312	-1.396	0.164	366	25	1.817	0.387	0.491

Table 69 – Loockdown Favor

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	-0.008	0.123	-0.066	0.948	366	25	
Standard Reg. with Cov. IPW	0.030	0.121	0.245	0.807	366	25	
Reg Lin - Only Comunity FE	0.045	0.110	0.406	0.685	366	25	
Reg Lin - All Cov.	0.027	0.116	0.230	0.819	366	25	
IWE - Without Covariate	0.045	0.109	0.410	0.682	366	25	
IWE - With Covariate	0.053	0.115	0.462	0.644	366	25	
RWE - Without Covariate	0.049	0.114	0.429	0.668	366	25	
RWE - With Covariate	0.050	0.120	0.416	0.678	366	25	
2SLS with FE, no covariates	-0.040	0.330	-0.123	0.903	366	25	

Table 70 – Housing Quality

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.118	0.099	1.188	0.235	366	25	
Standard Reg. with Cov. IPW	0.137	0.113	1.214	0.225	366	25	
Reg Lin - Only Comunity FE	0.127	0.093	1.371	0.171	366	25	
Reg Lin - All Cov.	0.136	0.096	1.418	0.157	366	25	
IWE - Without Covariate	0.127	0.093	1.373	0.170	366	25	
IWE - With Covariate	0.138	0.095	1.444	0.149	366	25	
RWE - Without Covariate	0.125	0.096	1.300	0.194	366	25	
RWE - With Covariate	0.131	0.099	1.329	0.184	366	25	
2SLS with FE, no covariates	0.273	0.291	0.939	0.348	366	25	

Table 71 – Housing Quality P8

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.150	0.095	1.577	0.116	366	25	-2.098	1.567	0.695
Standard Reg. with Cov. IPW	0.207	0.120	1.724	0.086	366	25	-2.098	1.567	0.514
Reg Lin - Only Comunity FE	0.161	0.088	1.829	0.068	366	25	-2.098	1.567	0.410
Reg Lin - All Cov.	0.138	0.088	1.570	0.117	366	25	-2.098	1.567	0.704
IWE - Without Covariate	0.161	0.087	1.840	0.066	366	25	-2.098	1.567	0.395
IWE - With Covariate	0.147	0.087	1.678	0.093	366	25	-2.098	1.567	0.560
RWE - Without Covariate	0.158	0.091	1.737	0.082	366	25	-2.098	1.567	0.494
RWE - With Covariate	0.145	0.094	1.553	0.120	366	25	-2.098	1.567	0.722
2SLS with FE, no covariates	0.435	0.277	1.570	0.117	366	25	-2.098	1.567	0.705

Table 72 – Housing Quality P9

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.022	0.106	0.210	0.834	366	25	1.68	0.387	1
Standard Reg. with Cov. IPW	0.022	0.118	0.186	0.852	366	25	1.68	0.387	1
Reg Lin - Only Comunity FE	0.006	0.099	0.063	0.949	366	25	1.68	0.387	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Reg Lin - All Cov.	0.017	0.101	0.170	0.865	366	25	1.68	0.387	1
IWE - Without Covariate	0.006	0.098	0.064	0.949	366	25	1.68	0.387	1
IWE - With Covariate	0.006	0.100	0.064	0.949	366	25	1.68	0.387	1
RWE - Without Covariate	0.013	0.107	0.119	0.905	366	25	1.68	0.387	1
RWE - With Covariate	0.009	0.110	0.079	0.937	366	25	1.68	0.387	1
2SLS with FE, no covariates	0.022	0.295	0.073	0.942	366	25	1.68	0.387	1

Table 73 – Housing Quality P10

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.079	0.109	-0.728	0.467	366	25	1.308	0.563	1
Standard Reg. with Cov. IPW	-0.072	0.118	-0.607	0.544	366	25	1.308	0.563	1
Reg Lin - Only Comunity FE	-0.073	0.098	-0.747	0.455	366	25	1.308	0.563	1
Reg Lin - All Cov.	-0.050	0.098	-0.510	0.611	366	25	1.308	0.563	1
IWE - Without Covariate	-0.073	0.097	-0.756	0.450	366	25	1.308	0.563	1
IWE - With Covariate	-0.061	0.098	-0.617	0.537	366	25	1.308	0.563	1
RWE - Without Covariate	-0.078	0.100	-0.782	0.434	366	25	1.308	0.563	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
RWE - With Covariate	-0.071	0.101	-0.705	0.481	366	25	1.308	0.563	1
2SLS with FE, no covariates	-0.255	0.306	-0.835	0.404	366	25	1.308	0.563	1

Table 74 – Housing Quality P11

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.063	0.105	0.599	0.550	366	25	-1.069	0.252	1
Standard Reg. with Cov. IPW	0.058	0.117	0.497	0.620	366	25	-1.069	0.252	1
Reg Lin - Only Comunity FE	0.055	0.095	0.576	0.565	366	25	-1.069	0.252	1
Reg Lin - All Cov.	0.072	0.096	0.752	0.453	366	25	-1.069	0.252	1
IWE - Without Covariate	0.055	0.095	0.576	0.565	366	25	-1.069	0.252	1
IWE - With Covariate	0.077	0.096	0.801	0.423	366	25	-1.069	0.252	1
RWE - Without Covariate	0.059	0.099	0.598	0.550	366	25	-1.069	0.252	1
RWE - With Covariate	0.076	0.099	0.766	0.444	366	25	-1.069	0.252	1
2SLS with FE, no covariates	0.084	0.310	0.271	0.787	366	25	-1.069	0.252	1

Table 75 – Housing Quality P12

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.163	0.112	1.459	0.145	366	25	-1.896	0.657	0.872
Standard Reg. with Cov. IPW	0.199	0.114	1.755	0.080	366	25	-1.896	0.657	0.481
Reg Lin - Only Comunity FE	0.214	0.112	1.914	0.056	366	25	-1.896	0.657	0.339
Reg Lin - All Cov.	0.208	0.113	1.834	0.068	366	25	-1.896	0.657	0.405
IWE - Without Covariate	0.214	0.111	1.918	0.055	366	25	-1.896	0.657	0.331
IWE - With Covariate	0.197	0.113	1.750	0.080	366	25	-1.896	0.657	0.481
RWE - Without Covariate	0.212	0.115	1.849	0.064	366	25	-1.896	0.657	0.387
RWE - With Covariate	0.198	0.118	1.684	0.092	366	25	-1.896	0.657	0.553
2SLS with FE, no covariates	0.496	0.289	1.715	0.087	366	25	-1.896	0.657	0.524

Table 76 – Housing Quality P15

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.025	0.101	0.245	0.807	366	25	3.394	1.04	1
Standard Reg. with Cov. IPW	-0.016	0.114	-0.138	0.890	366	25	3.394	1.04	1
Reg Lin - Only Comunity FE	0.008	0.096	0.088	0.930	366	25	3.394	1.04	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Reg Lin - All Cov.	0.009	0.095	0.096	0.923	366	25	3.394	1.04	1
IWE - Without Covariate	0.008	0.095	0.089	0.929	366	25	3.394	1.04	1
IWE - With Covariate	0.033	0.096	0.346	0.730	366	25	3.394	1.04	1
RWE - Without Covariate	0.001	0.099	0.008	0.994	366	25	3.394	1.04	1
RWE - With Covariate	0.026	0.102	0.256	0.798	366	25	3.394	1.04	1
2SLS with FE, no covariates	0.014	0.266	0.052	0.958	366	25	3.394	1.04	1

Table 77 – Like Stay Home

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.119	0.115	1.042	0.298	366	25	
Standard Reg. with Cov. IPW	0.173	0.117	1.481	0.140	366	25	
Reg Lin - Only Comunity FE	0.153	0.111	1.383	0.168	366	25	
Reg Lin - All Cov.	0.163	0.114	1.438	0.151	366	25	
IWE - Without Covariate	0.153	0.110	1.392	0.164	366	25	
IWE - With Covariate	0.168	0.112	1.504	0.133	366	25	
RWE - Without Covariate	0.157	0.123	1.280	0.200	366	25	
RWE - With Covariate	0.182	0.123	1.477	0.140	366	25	
2SLS with FE, no covariates	0.290	0.300	0.967	0.334	366	25	

Table 78 – Low-housing quality is one of the worst aspects of being at home

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	-0.206	0.071	-2.916	0.004	366	25	
Standard Reg. with Cov. IPW	-0.165	0.054	-3.051	0.002	366	25	
Reg Lin - Only Comunity FE	-0.206	0.065	-3.153	0.002	366	25	
Reg Lin - All Cov.	-0.197	0.064	-3.080	0.002	366	25	
IWE - Without Covariate	-0.206	0.065	-3.153	0.002	366	25	

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
IWE - With Covariate	-0.185	0.062	-2.991	0.003	366	25	
RWE - Without Covariate	-0.210	0.069	-3.045	0.002	366	25	
RWE - With Covariate	-0.186	0.066	-2.833	0.005	366	25	
2SLS with FE, no covariates	-0.589	0.265	-2.221	0.027	366	25	

Table 79 – Access to Clean Water

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.101	0.104	0.970	0.333	366	25	
Standard Reg. with Cov. IPW	0.120	0.114	1.048	0.295	366	25	
Reg Lin - Only Comunity FE	0.079	0.102	0.775	0.439	366	25	
Reg Lin - All Cov.	0.094	0.105	0.902	0.368	366	25	
IWE - Without Covariate	0.079	0.103	0.772	0.440	366	25	
IWE - With Covariate	0.094	0.103	0.905	0.365	366	25	
RWE - Without Covariate	0.078	0.102	0.762	0.446	366	25	
RWE - With Covariate	0.100	0.104	0.968	0.333	366	25	
2SLS with FE, no covariates	0.247	0.284	0.869	0.385	366	25	

Table 80 – Vulnerability

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	-0.132	0.120	-1.103	0.271	366	25	
Standard Reg. with Cov. IPW	-0.146	0.122	-1.196	0.233	366	25	
Reg Lin - Only Comunity FE	-0.102	0.116	-0.880	0.380	366	25	
Reg Lin - All Cov.	-0.096	0.119	-0.801	0.424	366	25	
IWE - Without Covariate	-0.102	0.114	-0.893	0.372	366	25	
IWE - With Covariate	-0.119	0.117	-1.018	0.309	366	25	
RWE - Without Covariate	-0.097	0.115	-0.844	0.399	366	25	
RWE - With Covariate	-0.126	0.119	-1.058	0.290	366	25	
2SLS with FE, no covariates	-0.312	0.317	-0.983	0.327	366	25	

Table 81 – Vulnerab_P39

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.174	0.127	-1.367	0.172	366	25	-1.314	0.608	0.517
Standard Reg. with Cov. IPW	-0.147	0.131	-1.127	0.260	366	25	-1.314	0.608	0.781
Reg Lin - Only Comunity FE	-0.113	0.113	-1.004	0.316	366	25	-1.314	0.608	0.949
Reg Lin - All Cov.	-0.097	0.117	-0.832	0.406	366	25	-1.314	0.608	1.000
IWE - Without Covariate	-0.113	0.110	-1.030	0.303	366	25	-1.314	0.608	0.910
IWE - With Covariate	-0.111	0.113	-0.984	0.325	366	25	-1.314	0.608	0.976
RWE - Without Covariate	-0.102	0.118	-0.859	0.390	366	25	-1.314	0.608	1.000
RWE - With Covariate	-0.110	0.122	-0.901	0.367	366	25	-1.314	0.608	1.000
2SLS with FE, no covariates	-0.476	0.341	-1.396	0.164	366	25	-1.314	0.608	0.491

Table 82 – Vulnerab_P40

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.022	0.115	-0.193	0.847	366	25	-1.547	0.499	1
Standard Reg. with Cov. IPW	-0.077	0.117	-0.660	0.510	366	25	-1.547	0.499	1
Reg Lin - Only Comunity FE	-0.007	0.116	-0.057	0.954	366	25	-1.547	0.499	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Reg Lin - All Cov.	0.001	0.117	0.010	0.992	366	25	-1.547	0.499	1
IWE - Without Covariate	-0.007	0.115	-0.057	0.954	366	25	-1.547	0.499	1
IWE - With Covariate	-0.030	0.117	-0.259	0.796	366	25	-1.547	0.499	1
RWE - Without Covariate	-0.010	0.117	-0.086	0.931	366	25	-1.547	0.499	1
RWE - With Covariate	-0.041	0.120	-0.345	0.730	366	25	-1.547	0.499	1
2SLS with FE, no covariates	0.002	0.309	0.007	0.994	366	25	-1.547	0.499	1

Table 83 – Vulnerab_P41

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	-0.108	0.116	-0.926	0.355	366	25	-1.415	0.494	1.000
Standard Reg. with Cov. IPW	-0.111	0.117	-0.955	0.340	366	25	-1.415	0.494	1.000
Reg Lin - Only Comunity FE	-0.115	0.118	-0.967	0.334	366	25	-1.415	0.494	1.000
Reg Lin - All Cov.	-0.124	0.121	-1.026	0.306	366	25	-1.415	0.494	0.917
IWE - Without Covariate	-0.115	0.118	-0.972	0.331	366	25	-1.415	0.494	0.993
IWE - With Covariate	-0.133	0.119	-1.118	0.263	366	25	-1.415	0.494	0.790
RWE - Without Covariate	-0.112	0.120	-0.939	0.348	366	25	-1.415	0.494	1.000

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
RWE - With Covariate	-0.138	0.120	-1.148	0.251	366	25	-1.415	0.494	0.753
2SLS with FE, no covariates	-0.243	0.314	-0.776	0.439	366	25	-1.415	0.494	1.000

Table 84 – Worked last week

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.080	0.117	0.682	0.496	366	25	
Standard Reg. with Cov. IPW	0.029	0.124	0.237	0.813	366	25	
Reg Lin - Only Comunity FE	0.094	0.113	0.832	0.406	366	25	
Reg Lin - All Cov.	0.058	0.119	0.492	0.623	366	25	
IWE - Without Covariate	0.094	0.112	0.840	0.401	366	25	
IWE - With Covariate	0.068	0.116	0.588	0.557	366	25	
RWE - Without Covariate	0.089	0.118	0.754	0.451	366	25	
RWE - With Covariate	0.062	0.121	0.517	0.605	366	25	
2SLS with FE, no covariates	0.282	0.312	0.906	0.366	366	25	

Table 85 – Able to Work from Home

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.093	0.118	0.793	0.429	366	25	
Standard Reg. with Cov. IPW	0.110	0.123	0.893	0.373	366	25	
Reg Lin - Only Comunity FE	0.087	0.121	0.716	0.475	366	25	
Reg Lin - All Cov.	0.112	0.122	0.913	0.362	366	25	
IWE - Without Covariate	0.087	0.120	0.722	0.470	366	25	
IWE - With Covariate	0.092	0.122	0.752	0.452	366	25	
RWE - Without Covariate	0.089	0.124	0.721	0.471	366	25	
RWE - With Covariate	0.101	0.124	0.814	0.415	366	25	
2SLS with FE, no covariates	0.258	0.315	0.817	0.415	366	25	

Table 86 – Coronavirus Index

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.091	0.093	0.984	0.326	366	25	

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks
Standard Reg. with Cov. IPW	0.136	0.108	1.258	0.209	366	25
Reg Lin - Only Comunity FE	0.077	0.085	0.915	0.361	366	25
Reg Lin - All Cov.	0.057	0.087	0.657	0.512	366	25
IWE - Without Covariate	0.077	0.088	0.884	0.377	366	25
IWE - With Covariate	0.069	0.089	0.781	0.435	366	25
RWE - Without Covariate	0.080	0.102	0.789	0.430	366	25
RWE - With Covariate	0.070	0.103	0.679	0.497	366	25
2SLS with FE, no covariates	0.251	0.288	0.869	0.385	366	25

Table 87 – Coronavirus_P2

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.048	0.101	0.475	0.635	366	25	1.83	0.376	1
Standard Reg. with Cov. IPW	0.084	0.118	0.710	0.478	366	25	1.83	0.376	1
Reg Lin - Only Comunity FE	-0.011	0.093	-0.119	0.905	366	25	1.83	0.376	1
Reg Lin - All Cov.	0.007	0.095	0.075	0.940	366	25	1.83	0.376	1
IWE - Without Covariate	-0.011	0.094	-0.118	0.906	366	25	1.83	0.376	1
IWE - With Covariate	0.002	0.097	0.023	0.981	366	25	1.83	0.376	1
RWE - Without Covariate	-0.008	0.111	-0.072	0.943	366	25	1.83	0.376	1
RWE - With Covariate	0.000	0.116	-0.001	0.999	366	25	1.83	0.376	1
2SLS with FE, no covariates	0.072	0.289	0.250	0.803	366	25	1.83	0.376	1

Table 88 – Coronavirus_P3

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.053	0.088	0.602	0.548	366	25	-0.538	1.686	1
Standard Reg. with Cov. IPW	0.094	0.102	0.923	0.357	366	25	-0.538	1.686	1
Reg Lin - Only Comunity FE	0.037	0.095	0.393	0.695	366	25	-0.538	1.686	1
Reg Lin - All Cov.	0.068	0.094	0.715	0.475	366	25	-0.538	1.686	1
IWE - Without Covariate	0.037	0.096	0.390	0.697	366	25	-0.538	1.686	1
IWE - With Covariate	0.056	0.098	0.576	0.565	366	25	-0.538	1.686	1
RWE - Without Covariate	0.044	0.112	0.391	0.696	366	25	-0.538	1.686	1
RWE - With Covariate	0.055	0.113	0.484	0.628	366	25	-0.538	1.686	1
2SLS with FE, no covariates	0.080	0.279	0.286	0.775	366	25	-0.538	1.686	1

Table 89 – Coronavirus_P4

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.065	0.113	0.573	0.567	366	25	2.103	0.759	1
Standard Reg. with Cov. IPW	0.039	0.115	0.340	0.734	366	25	2.103	0.759	1
Reg Lin - Only Comunity FE	0.075	0.105	0.713	0.476	366	25	2.103	0.759	1

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Reg Lin - All Cov.	0.004	0.103	0.038	0.970	366	25	2.103	0.759	1
IWE - Without Covariate	0.075	0.105	0.714	0.475	366	25	2.103	0.759	1
IWE - With Covariate	0.037	0.105	0.353	0.724	366	25	2.103	0.759	1
RWE - Without Covariate	0.072	0.115	0.628	0.530	366	25	2.103	0.759	1
RWE - With Covariate	0.041	0.115	0.351	0.726	366	25	2.103	0.759	1
2SLS with FE, no covariates	0.272	0.312	0.872	0.384	366	25	2.103	0.759	1

Table 90 – Coronavirus_P5

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.085	0.086	0.997	0.319	366	25	-0.254	0.668	1.000
Standard Reg. with Cov. IPW	0.157	0.102	1.531	0.127	366	25	-0.254	0.668	0.506
Reg Lin - Only Comunity FE	0.112	0.086	1.305	0.193	366	25	-0.254	0.668	0.771
Reg Lin - All Cov.	0.079	0.094	0.839	0.402	366	25	-0.254	0.668	1.000
IWE - Without Covariate	0.112	0.092	1.222	0.222	366	25	-0.254	0.668	0.887
IWE - With Covariate	0.095	0.095	1.005	0.315	366	25	-0.254	0.668	1.000
RWE - Without Covariate	0.113	0.096	1.184	0.236	366	25	-0.254	0.668	0.945

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
RWE - With Covariate	0.098	0.098	1.000	0.318	366	25	-0.254	0.668	1.000
2SLS with FE, no covariates	0.266	0.290	0.916	0.360	366	25	-0.254	0.668	1.000

Table 91 – Teto Index

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.446	0.098	4.540	0	366	25	
Standard Reg. with Cov. IPW	0.488	0.107	4.555	0	366	25	
Reg Lin - Only Comunity FE	0.452	0.099	4.568	0	366	25	
Reg Lin - All Cov.	0.456	0.101	4.506	0	366	25	
IWE - Without Covariate	0.452	0.099	4.583	0	366	25	
IWE - With Covariate	0.449	0.100	4.503	0	366	25	
RWE - Without Covariate	0.450	0.100	4.492	0	366	25	
RWE - With Covariate	0.444	0.101	4.411	0	366	25	
2SLS with FE, no covariates	1.234	0.330	3.745	0	366	25	

Table 92 – Teto P60

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.216	0.100	2.159	0.032	366	25	-2.754	1.683	0.126
Standard Reg. with Cov. IPW	0.138	0.116	1.194	0.233	366	25	-2.754	1.683	0.933
Reg Lin - Only Comunity FE	0.212	0.107	1.976	0.049	366	25	-2.754	1.683	0.196
Reg Lin - All Cov.	0.213	0.108	1.975	0.049	366	25	-2.754	1.683	0.196
IWE - Without Covariate	0.212	0.107	1.977	0.048	366	25	-2.754	1.683	0.192

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
IWE - With Covariate	0.192	0.107	1.794	0.073	366	25	-2.754	1.683	0.291
RWE - Without Covariate	0.211	0.111	1.899	0.058	366	25	-2.754	1.683	0.230
RWE - With Covariate	0.185	0.112	1.656	0.098	366	25	-2.754	1.683	0.391
2SLS with FE, no covariates	0.653	0.297	2.196	0.029	366	25	-2.754	1.683	0.115

Table 93 – Teto P61

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.167	0.093	1.792	0.074	366	25	0.902	0.297	0.296
Standard Reg. with Cov. IPW	0.281	0.112	2.503	0.013	366	25	0.902	0.297	0.051
Reg Lin - Only Comunity FE	0.159	0.100	1.588	0.113	366	25	0.902	0.297	0.453
Reg Lin - All Cov.	0.178	0.101	1.770	0.078	366	25	0.902	0.297	0.310
IWE - Without Covariate	0.159	0.100	1.587	0.112	366	25	0.902	0.297	0.450
IWE - With Covariate	0.174	0.103	1.698	0.090	366	25	0.902	0.297	0.358
RWE - Without Covariate	0.151	0.102	1.478	0.139	366	25	0.902	0.297	0.557
RWE - With Covariate	0.160	0.102	1.574	0.116	366	25	0.902	0.297	0.462
2SLS with FE, no covariates	0.427	0.288	1.483	0.139	366	25	0.902	0.297	0.556

Table 94 – Teto P62

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.298	0.093	3.203	0.001	366	25	-1.571	1.034	0.006
Standard Reg. with Cov. IPW	0.299	0.097	3.087	0.002	366	25	-1.571	1.034	0.009
Reg Lin - Only Comunity FE	0.315	0.083	3.793	0.000	366	25	-1.571	1.034	0.001
Reg Lin - All Cov.	0.322	0.087	3.686	0.000	366	25	-1.571	1.034	0.001
IWE - Without Covariate	0.315	0.082	3.837	0.000	366	25	-1.571	1.034	0.000
IWE - With Covariate	0.323	0.086	3.739	0.000	366	25	-1.571	1.034	0.001
RWE - Without Covariate	0.320	0.085	3.751	0.000	366	25	-1.571	1.034	0.001
RWE - With Covariate	0.338	0.090	3.765	0.000	366	25	-1.571	1.034	0.001
2SLS with FE, no covariates	0.811	0.294	2.756	0.006	366	25	-1.571	1.034	0.025

Table 95 – Teto P63

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Standard Reg. with Cov.	0.217	0.112	1.927	0.055	366	25	-3.561	1.647	0.219
Standard Reg. with Cov. IPW	0.265	0.116	2.290	0.023	366	25	-3.561	1.647	0.090
Reg Lin - Only Comunity FE	0.223	0.109	2.046	0.041	366	25	-3.561	1.647	0.166

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Control Mean	Control SD	Adj. BH
Reg Lin - All Cov.	0.204	0.111	1.842	0.066	366	25	-3.561	1.647	0.265
IWE - Without Covariate	0.223	0.109	2.044	0.041	366	25	-3.561	1.647	0.164
IWE - With Covariate	0.215	0.111	1.933	0.053	366	25	-3.561	1.647	0.213
RWE - Without Covariate	0.225	0.117	1.918	0.055	366	25	-3.561	1.647	0.221
RWE - With Covariate	0.211	0.119	1.775	0.076	366	25	-3.561	1.647	0.304
2SLS with FE, no covariates	0.596	0.314	1.897	0.059	366	25	-3.561	1.647	0.235

Table 96 – Home mobility

	Estimate	Std. Error	t value	Pr(>|t|)	N. obs.	N. blocks	Co
Standard Reg. with Cov.	0.108	0.100	1.080	0.281	366	25	
Standard Reg. with Cov. IPW	0.020	0.099	0.197	0.844	366	25	
Reg Lin - Only Comunity FE	0.134	0.089	1.514	0.131	366	25	
Reg Lin - All Cov.	0.158	0.087	1.814	0.070	366	25	
IWE - Without Covariate	0.134	0.091	1.478	0.139	366	25	
IWE - With Covariate	0.150	0.090	1.660	0.097	366	25	
RWE - Without Covariate	0.124	0.095	1.298	0.194	366	25	
RWE - With Covariate	0.147	0.098	1.498	0.134	366	25	
2SLS with FE, no covariates	0.286	0.286	1.002	0.317	366	25	