After the Flood

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Abstract

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This doctoral dissertation studies farm exit and returns to housing for rural communities living under high incidence of poverty. The analytical framework leverages on a natural experiment that triggered an extensive government intervention, where widespread reconstruction and shelter provision plausibly caused structural transformation of agriculture and advanced living conditions.

Chapter 1 outlines the overall research design pursued in order to evaluate the effects of the program, starting with a backdrop that ascribes the intervention. Next, it introduces the survey employed in order to gather data on the study area. This is followed by the configuration of the sampling frame, detailing power calculations and the algorithm that randomly selects

observations. Finally, it depicts the identification strategy that enables causal interpretation of differential treatment conditions which are examined in detail in Chapters 2 and 3.

Chapter 2 analyzes households' decision to exit agriculture, arguing that an aggregate demand shock, associated with the intervention, increased the value of outside options among farmers. Relatively low farm earnings, relaxed credit constraints, and changes in risk attitudes after the disaster, prompted factor reallocations that underlay baseline suboptimal outcomes. Findings reflect structural transformation, with baseline farmers engaging in nonfarm activities —through work or enterprises— at a significantly higher rate relative to their counterparts in a village where no intervention took place.

Chapter 3 —joint work with Rachel Heath— documents health and labor market responses to dwelling provision among households that, albeit inhabiting identical infrastructures after the intervention, exhibit heterogenous baseline access to adequate housing. Results indicate that shelter enhancements reduced the incidence of health shocks and quarterly doctor visits. It also improved self-reported satisfaction across standard wellbeing measures (life, health, and housing), underpinning psychological benefits associated with receiving a new house. These effects are particularly robust among families that initially experienced qualitative housing shortages.

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DEDICATION

To the anonymous families of the Colombian countryside, whose stoicism gave me purpose.

Chapter 1. AFTER THE FLOOD

1.1 INTRODUCTION

Although the initial effects of natural disasters are indisputably negative, these events can lead to overall welfare gains for affected communities. For instance, the extensive provision of subsidies after the 2008 Wenchuan earthquake led to substantial increases in household income (Park and Wang, 2017); disaster relief funds boosted local economic activity on Mexican regions hit by natural disasters (Janvry *et al*., 2016); and the distribution of aid in Indonesian regions affected by earthquakes derived in long-run improvements on income and infrastructure (Gignoux and Menéndez, 2016).

Governmental responses involve more than just the substantial transfer of resources to affected communities; they can also lead to second order gains. For example, the reception of a new house following a catastrophe might relax monetary and time constraints, such that beneficiary families could reallocate endowments toward economic activities that yield higher returns. Nakamura *et al*. (2016) find that reallocation of labor arising from a volcanic explosion in Iceland increased lifetime earnings and education for young adults. Similarly, Hornbeck and Keniston, (2014) show that positive externalities from widespread simultaneous reconstruction after the 1872 Boston fire resulted on increased land value

These unconventional views challenge any prior beliefs associated with the effects of calamities, characterized by systematic drawbacks of core welfare indicators, such as consumption, wealth, human capital accumulation and health, among others[1]. At the same time, it

[1] The literature on this topic has consistently shown that natural disasters deteriorate welfare. Droughts, floods, mudslides, and earthquakes, lead to reductions in consumption and savings (Sawada and Shimizutani, 2008); increased borrowing (Del Ninno, *et al*., 2003); higher unemployment (Lynham *et al*., 2017); reductions in human capital accumulation and increased child labor (Jacoby and Skoufias, 1997); worse health and less wealth (Caruso, 2017); alterations in agricultural production (Taraz, 2015; Deressa *et al*., 2009); child malnourishment; decreases in health

motivates the study of post-disaster governmental interventions, since these measures potentially generate double benefits: first, they allow disturbed households to buffer the losses caused by the catastrophe; and second, they can lead to reductions in rural poverty through considerable transfers of resources, enhancement of allocative efficiency, and the generation of positive consequences.

However, and despite the increasing frequency, duration and intensity of natural disasters[2], scientific evidence regarding the effectiveness of these programs is rather limited. Therefore, and exploiting a quasi-random assignment framework, this study seeks to examine the effects of an integrated intervention aimed at alleviating the effects of a major flood. This is accomplished by comparing individuals and families residing in two villages that exhibit similar pre-disaster characteristics, but that were separated by a dike that broke as a consequence of torrential rainfall.

There are several ways in which this study contributes to the existing literature. Firstly, it extends the analysis of governmental responses beyond welfare considerations associated with sizeable transfers of resources. In fact, it analyzes the ways in which the post-disaster intervention allows households to overcome pre-existing frictions that advent sub-optimal outcomes. Furthermore, it takes into account the impact on subjective wellbeing, an aspect that has received limited attention. Additionally, it examines long term implications of disaster mitigation.

Beyond the latter, this research is relevant because it provides evidence regarding the effectives of widespread programs aimed at alleviating rural poverty, contributing to an ongoing discussion pertaining to ways in which rural communities can be uplifted from poverty. It does so, generating information regarding the effectiveness of interventions that involve extensive,

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utilization services (Baez and Santos, 2007); breakdown of informal risk-sharing institutions upon which poor people heavily rely on (Takasaki, 2017); loss of productive assets; income reduction; increased dependency on government transfers (Sawada, 2007); long-lasting increases in risk aversion and increased impatience (Cassar *et al.*, 2017); reduction in weight and height; increased socio-emotional problems; and deteriorated cognitive ability (Brando and Santos, 2015; Rosales-Rueda, 2016).

[2] Several studies illustrate this trend (Emanuel, 2013; World Bank, 2013; Mendelsohn *et al.*, 2012; Kellenberg and Mobarak, 2011; World Bank and United Nations, 2010; Cavallo and Noy, 2009; UNISDR, 2009; Stromberg, 2007).

simultaneous provision of public goods[3], for which evaluations have yielded mixed results[4]. Most importantly, if in fact the post-disaster response led to net welfare gains, then these results would highlight the importance of having these protocols in place before a calamity occurs, enhancing public policies established for the mitigation of the negative effects of calamities, especially if we take into account that these interventions are increasingly difficult to implement during an emergency (Skoufias, 2003).

The rest of this paper proceeds as follows: Section 1.2 provides background information about the calamity that triggered a large-scale governmental project on the Colombian Caribbean Coast. Section 1.3 discusses the data and sources of information used for this investigation. Section 1.4 presents the identification strategy pursued to correctly estimate the effect associated with the post-disaster intervention. Section 1.5 explains the analytical framework employed to decompose the overall effect corresponding to the event. Section 1.6 presents the research ideas that will be developed within this research project. Section 1.7 concludes.

1.2 BACKGROUND

In 2010 Colombia was the third country most affected by climatological shocks[5]. That year *La Niña* phenomenon[6] brought torrential rainfall that accounted for one of the worst natural disasters the country has ever experienced. Widespread floods across the country caused losses equivalent

[3] One example of this type of intervention is the Millennial Villages Project. Although this similar initiative was established over a decade ago, the impact of this program on the welfare of Sub-Saharan African communities is still actively debated.
[4] For example, Lenz *et al*. (2017) find mixed results regarding the effectiveness of large-scale infrastructure investment towards poverty alleviation.
[5] This rank is based on the Global Climate Risk Index, which is calculated since 2006 by German Watch and assesses the degree to which countries have been affected by extreme weather events. https://germanwatch.org/en/cri
[6] *La Niña* is a climatological event characterized by unusually cold temperatures on the Equatorial Pacific Ocean, which translates into considerable increases in rainfall in the Tropics. https://www.climate.gov/enso

to 2% of the national income[7]; damaged 4% of schools and dwellings; affected 7% of the country's population and 93% of municipalities (Sanchez-Jabba, 2011). One of the worst tragedies associated with this event occurred on the Colombian Caribbean Coast, on the State of Atlántico, where overflowing waters caused the rupture of a dike, inundating an entire region.

Figure 1.1. Rupture of the Dike in 2010

1.2.1 *Study Area*

The area of study is composed by the municipalities of Santa Lucía, Suan, Campo de la Cruz, Candelaria and Manatí. These predominantly rural towns conform a sub-region of 120 square miles known as South Atlántico (Figure 1.2), inhabited by 65,000 people[8] and whose economy mainly relies agricultural activities, such as farming, livestock breeding and fishing (Aguilera, 2006).

This territory is characterized by a strident incidence of poverty. Data corresponding to the 2005 Colombian General Census shows that poverty rates, expressed through the unsatisfied basic needs index[9], range from 54% to 74%, depending on which town is considered. This makes South

[7] Calculations corresponding to the losses and damages associated with this calamity were performed by the Economic Commission for Latin America and the Caribbean and the Inter-American Development Bank in IDB (2012).
[8] According to the population projections made by the Colombian National Department of Statistics.
[9] The Unsatisfied Basic Needs index is calculated through the weighing of socioeconomic indicators related to economic dependency, dwelling characteristics, child schooling, and overcrowding. If households do not reach a

Atlántico the poorest region of the State (Figure 1.3) and thus, an area particularly vulnerable to the effects of natural disasters[10].

As part of the Magdalena River basin[11], a dip composed by a system of swamps and lagoons that usually floods during rainy periods, South Atlántico used to experience periodic inundations. However, with the construction of levees and dikes during the second half of the 20th century in order to facilitate the practice of agricultural activities, the area was sealed, permanently preventing any seasonal flooding (Mogollon, 2013).

Nonetheless, the 30th of November of 2010, amidst pouring rainfall and overflowing waters, a levee protecting South Atlántico broke, allowing a total of 2.2 cubic kilometers of water to enter the area uninterruptedly for nearly two months. Given the extent of the emergency, the entire population had to be evacuated, and the villages remained under water for several months. The flood displaced families, halted agricultural production, interrupted schooling, and destroyed vital infrastructure and productive capital, triggering a persistent state of emergency that led to a noticeable reduction in welfare (Sanchez-Jabba, 2011).

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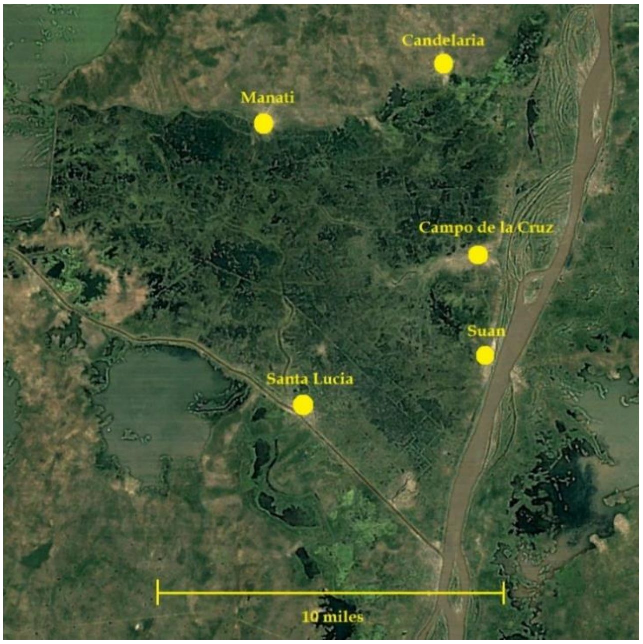
certain threshold, they are classified as poor. This poverty indicator is commonly employed in Latin America and specific details regarding its calculation can be found on the website of the Colombian National Department of Statistics.
https://www.dane.gov.co/index.php/estadisticas-por-tema/pobreza-y-condiciones-de-vida/necesidades-basicas-insatisfechas-nbi

[10] Jalan and Ravallion (1999) show that poor rural households are most vulnerable to the effects of natural disasters, since they have limited access to formal insurance and any pre-existing informal arrangements fail when this type of shocks occur. Similarly, Sakai *et al.* (2017) find that calamities disproportionately affect the rural poor, whose income is reduced due to plunges in agricultural prices and their inability to reallocate consumption.

[11] The Magdalena River is the main river of Colombia. It transects the Western part of the country, flowing northward until it mouths on the Caribbean Sea.



Note: Shaded area corresponds to the flooded area.

Figure 1.2. Area of Study

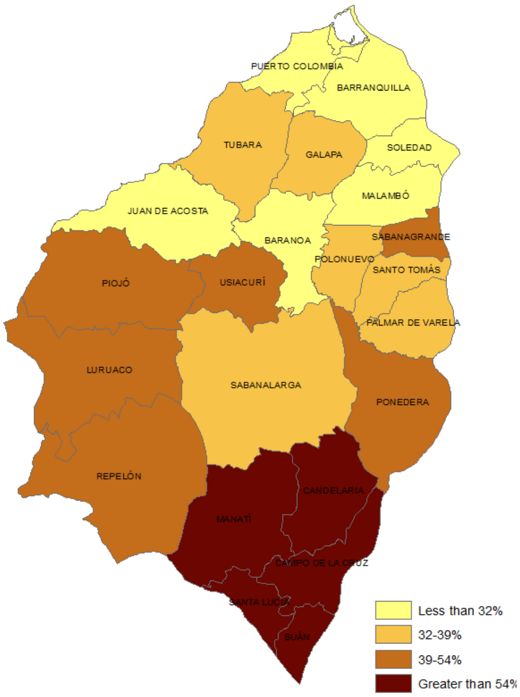


Figure 1.3. Unsatisfied Basic Needs in the State of Atlántico (2005)

1.2.2 *Government Response*

La Niña phenomenon of 2010 triggered a broad government response intended to alleviate the negative effects caused by the calamity. The strategy adopted by the Colombian Government was structured in three phases: the first phase was activated immediately after the disaster and focused on the distribution of humanitarian aid and preserving the integrity of the affected communities; the second phase, 1-2 years after the flood, consisted on the rehabilitation of the damaged infrastructure; the third phase, 3-4 years after the disaster and in which this study concentrates, was dedicated to reconstruction and the reestablishment of the socioeconomic conditions of the population (Sánchez-Jabba, 2014).

In this context, it is worth mentioning that although several areas of Colombia were affected by the climatological shock, the flood in South Atlántico was unique for two reasons: first, it was not attributable to a gradual rise on the water level as in other regions, but unexpected altogether in a tsunami-like catastrophe; second, once the fissure on the dike was sealed, the region trapped vast amounts of water, which remained repressed for nearly two years, thus prolonging the negative effects associated with the tragedy (Sanchez-Jabba, 2011).

Consequently, the Colombian government implemented an unprecedented post-disaster intervention that went beyond the restoration of the pre-existing conditions. Following a compensating principle toward this historically neglected area, the response to the disaster incorporated an added-value approach that included sizeable monetary transfers and simultaneous construction of schools, hospitals, aqueducts, parks, sewerage systems and houses, providing infrastructure that in several cases was lacking before the event (Figure 1.4).

2009 **2015**

Houses

School

Hospital

Figure 1.4. Governmental Intervention in Santa Lucia – South Atlántico (2013-2015)

1.3 DATA

Since the existing institutional data corresponding to the study area lacks information that extends beyond the basic sociodemographic attributes, this study involved the collection of data through a survey specifically designed —between October 2016 and March 2018— for this investigation, which was formulated following the guidelines and parameters established by the World Bank Living Standards Measurement Study and the Colombian Longitudinal Survey. It contains current and retrospective information —between 2008 and 2018— on households' sociodemographics, assets, dwelling characteristics, credit access, migration, flood damage, enterprises, and other indicators relevant to the proposed hypotheses. The data was collected between March-May 2018 in Colombia through a survey team composed by 17 enumerators and 2 field coordinators. Supplementary information includes the 2005 Colombian census and the Government Welfare database —SISBEN— which were used to check pre-flood trends and perform power calculations.

Due to budgetary restrictions, only two villages were included in data collection efforts. The treatment village was selected based on proximity to a dike that failed in late 2010. To select the counterfactual, the treatment was compared against a set of 27 plausible control villages on variables such as: i) outcomes, ii) treatment conditions, and iii) sociodemographic indicators. The control village was finally chosen based on i) the number of statistically significant baseline differences, and ii) the magnitude of the differences in each variable analyzed, minimizing the squared sum. Households that did not reside on the area of study during the period 2008-2018 were excluded from the analysis, since changes on behavior could not be categorically attributed to the intervention.

The questionnaire experienced various adjustments arising from three pilot testing rounds. The first pilot, a contextualization exercise, was undertaken in November 2016 and sought basic information related to the objective population, assessing the feasibility of the preconceived hypotheses. A second pilot round took place on January 2018, which evaluated logic of the questionnaire and analyzed respondents' reaction toward disclosure of potentially sensitive information. The final testing round was conducted in February 2018, which employed the final draft of the survey and was coupled with institutional work, wherein the academic project was presented to the communities residing on the study area.

Figure 1.5. Data Collection

1.4 SAMPLING

1.4.1 *Control Group*

Proper identification of the effect corresponding to the post-disaster intervention requires the proposition of a suitable counterfactual for Santa Lucía, the treatment village[12] (red on Figure 1.6). To do so, this town was compared against a set of 27 plausible control villages within a 32-mile radius, and which already exhibited similar pre-flood climatological, geographical,

[12] The resources available for this project only allow the inclusion of one flooded village into the treatment group. In particular, Santa Lucía was selected as the treatment because annual visits to this town have facilitated a broad documentation of the effects of the flood and the subsequent intervention (see Sánchez-Jabba, 2011). Nevertheless, results are not expected to change with respect to other villages, since all towns in South Atlántico received considerable investments as part of the governmental response to the calamity.

socioeconomic, and administrative characteristics[13]. Furthermore, employing 2005 Census and SISBEN[14] data to profoundly analyze pre-flood sociodemographic trends we find that San Cristobal (blue of Figure 1.6), located on the neighboring State of Bolívar, represents the most feasible alternative associated with the proposition of a control group[15]. Table 1 shows that these two municipalities exhibit pre-disaster similarities on crucial indicators, such as aqueduct coverage, school attendance, electricity coverage, asset holdings, education level, unemployment rate, and dwelling characteristics, such that the overall socioeconomic situation among these villages was not divergent.

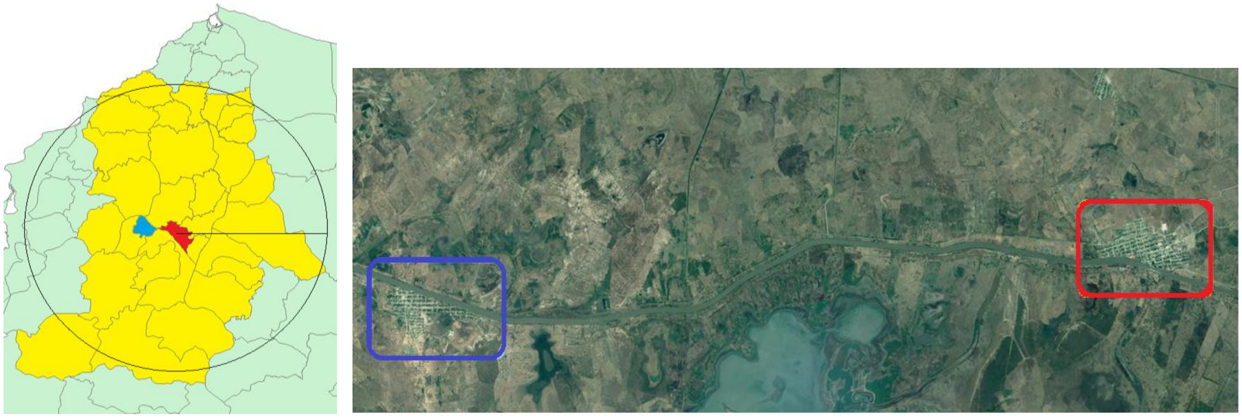
Besides the correspondence on the aforementioned attributes, the selection of San Cristóbal as the counterfactual favors the estimation of the intervention effects since it limits treatment spillovers. Located across the dike that broke in 2010, it is expected that residents in San Cristobal would not have benefited from the program in Santa Lucia because the dike actually represents both, a geographical and political boundary, imposing monetary and administrative restrictions that hinder any benefits derived from accessing the treatment[16].

[13] The proposition of the set of 27 feasible control groups closely follows the classification of productive regions of the Colombian Caribbean Coast, illustrated on Viloria de la Hoz (2006), and which groups these towns into sub-regions based on a qualitative analysis of these attributes.

[14] The SISBEN database determines families' access to governmental welfare programs. It might suffer from self-report bias arising from the strategic incentive to misreport information to gain broader rents (see Niehaus *et al.,* 2013 for a discussion regarding enforceability of targeted assistance).

[15] Pre-flood comparisons between Santa Lucía and each of the feasible counterfactual villages are presented in Appendix A1.

[16] This restriction becomes even more binding if we take into account that San Cristóbal also presents high incidence of poverty per the NBI Index, which undermines the possibility of accessing services on other villages. For example, Bryan *et al*. (2014) argue that for individuals who are close to subsistence, migration becomes increasingly costly. Similarly, Jayachandran (2006) shows that the wage elasticity is increasing in migration costs, signaling that there are migration constraints for poor households in developing countries.



Note: Municipalities in yellow correspond to plausible counterfactuals for Santa Lucía (red); San Cristóbal (blue) is the control village.

Figure 1.6. Counterfactual for Santa Lucía

One critical aspect related to the selection of the control group requires the treatment administration to be confined to Santa Lucía. This is corroborated by examining the degree to which the flood affected each village, since the incidence of the disaster determines access to the intervention. Considering that 95% of Colombian municipalities, especially on the Caribbean Coast, were affected by *La Niña* phenomenon of 2010, we should expect to see losses on both groups. However, a comparison of damages between the treatment and control quickly reveals the degree of devastation to which the former was exposed. Table 2 shows that the incidence of this climatological shock was much higher in Santa Lucía, where the flood destroyed a sizeable quantity of buildings, animals and crops.

Table 1.1. Pre-flood Comparisons

	2005 (Census)				2009 (SISBEN)			
	Santa Lucía	**San Cristóbal**	**Diff.**		**Santa Lucía**	**San Cristóbal**	**Diff.**	
Population	10,696	4.936	-		10.892	5.868	-	
Households	2.039	900	-		1.641	965	-	
Aqueduct coverage	88.76%	93.52%	-0.048	***	91.54%	86.93%	0.046	***
Sewage system coverage	30.29%	0.51%	0.298	***	22.82%	0.23%	0.226	***
School attendance	19.01%	16.94%	0.021	***	20.19%	25.52%	-0.053	***
Garbage collection	25.93%	52.72%	-0.268	***	65.78%	31.63%	0,341	***
Cooking with propone gas	18.09%	32.03%	-0.139	***	4.06%	17.26%	-0.132	***
Electricity coverage	97.75%	98.05%	-0.003		99.91%	97.85%	0.020	***
Couples in free union, married	26.36%	33.86%	-0.075	***	31.09%	32.14%	-0.011	
Washer holding	20.45%	15.01%	0.054	***	12.57%	12.45%	0.001	
Refrigerator holding	47.58%	40.54%	0.070	***	40.66%	38.03%	0.026	*
Education level; none-primary	42.17%	44.29%	-0.021	**	73.18%	64.23%	0.089	***
Education level; secondary	30.47%	30.56%	-0.001		23.63%	33.16%	-0.095	***
Occupied people	15.54%	16.12%	-0.006		14.86%	11.77%	0.031	***
Material of wall; block, brick	88.10%	90.75%	-0.026	*	87.49%	87.44%	0.001	
Material of wall; mud	9.13%	6.58%	0.025	**	8.55%	9.79%	-0.012	
Material of floors; wood	26.85%	12.02%	0.148	***	18.89%	14.71%	0.042	***
Material of floors; cement, gravel	53.31%	77.18%	-0.239	***	59.77%	71.93%	-0.121	***
Material of floors; sand	19.44%	10.79%	0.086	***	20.56%	13.36%	0.072	***
Housing tenure; rental	9.17%	8.51%	0.007		10.62%	10.47%	0.001	
Housing tenure; paid own	74.31%	77.98%	-0.037	*	69.18%	73.29%	-0.041	***
Type of housing; house/apartment	99.74%	99.08%	0,007	*	97.71%	99.04%	-0.013	***
Number of rooms in dwelling	3.69	3.07	0,620	*	3.16	3.14	0.021	
Number of people in dwelling	5.24	4.94	0.302	***	8.81	7.74	1.077	***

Significance: *p<0.10, **p<0.05, ***p<0.01

Table 1.2. Damages caused by La Niña phenomenon of 2010

		Santa Lucía	San Cristóbal
Property (destroyed)	Dwellings	1,343	95
	Farms	1,014	527
	Shops	143	6
	Lot	318	46
	Total	**2,818**	**674**
Animals (killed)	Mules	1,007	171
	Pork	8,987	1,159
	Goats	2,785	518
	Fish	320,072	3,791
	Chicken	34,253	9,140
	Cows	9,075	1,478
	Other minor species	5,007	978
	Total	**381,186**	**17,235**
Crops (hectares)	Yucca	499	415
	Pumpkin	248	143
	Corn	350	313
	Guava	213	104
	Mango	203	182
	Plantains	176	200
	Beans	139	183
	Papaya	131	87
	Lemon	123	79
	Melon	100	150
	Soursop	40	30
	Total	**2,222**	**1,886**

Source: National Office for Disaster Risk Management.

Based on the latter, it comes as no surprise that the post-disaster intervention focused on the uplift of Santa Lucía. In effect, we can see that this village considerably benefited from investments in education, health, housing, and sanitation, besides receiving considerable monetary transfers (Table 3), averaging a public spending of $5,151 per household over the period 2012-2016. San Cristóbal on the other hand, averaged a spending of $1,135 per household, half of which accrues exclusively to families favored by housing projects.

Table 1.3. Reconstruction Efforts (2012-2016)

	Santa Lucía			San Cristóbal		
Project	Items		Investment	Items		Investment
Housing	1	$	2,721,199	1	$	1,113,668
Schools	2	$	920,443	0		0
Hospitals	2	$	2,528,237	0		0
Aqueduct	2	$	479,750	0		0
Sewerage	4	$	4,073,437	0		0
Infrastructure	8	$	5,269,667	0		0
Economic incentives	2	$	161,492	0		0
Cash transfers	4	$	3,309,000	2	$	1,291,667
Total		**$**	**19,463,225**		**$**	**2,405,335**

Source: Fund for Climate Change Adaptation and Office for Social Prosperity.

1.4.2 *Power Calculations*

Once the control group was selected, power calculations were performed in order to establish the minimum sample size required to ensure a detectable treatment effect. Since this procedure was performed prior to data collection, without access to information regarding the outcome variables introduced in subsequent chapters, we opted for an approximation through a wealth index, which incorporates many of the sociodemographic variables employed for baseline checks between municipalities and represents a continuous measure, as opposed to many of the binary and categorical variables that describe preflood household characteristics.

The main hypothesis of analysis, and on which the power calculations are based, is expressed as: H0: Mean value treatment = Average value control = 0, that is to say that the difference between the groups of analysis is equal to zero, so that no change is attributed to the program. On the other hand, the alternative hypothesis (H1: Mean value treatment = Mean value control = 0) is one that proves that the average values in the treatment and control groups are different, thanks to an intervention in the target population. How close or far the averages are, depends on the sample size necessary to reject each null hypothesis, in favor of the alternative.

This means that as the averages approach, the sample size increases, and with it the probability of finding the smallest difference detectable.

It must be noted that, not finding significant differences between groups may arise due to the null impact of intervention, but also to an imprecise estimation of the mean values in the variable of interest; the latter results in type I and II errors, respectively. The first, false positive, is committed when an evaluation concludes that the program had an impact, when the actual effect is non-existent; whereas type II error appears at the moment when the evaluation concludes that the program has not had an impact, when the opposite is true. These errors can also be expressed as probabilities and rewritten as follows: Pr (reject H0 | H0 is true) for type I and Pr (not reject H0 | H0 is false) for type II error. The first notation is linked to the definition of the level of significance, and in which it is very common to use values such as 1% or 5%. On the other hand, the second definition of probability is inversely related to the power of evaluation. This means that minimizing the probability of type II error is equivalent to maximizing power through an increase in the sample size, so that there is greater certainty that not only units with the same characteristics are observed.

1.4.3 *Wealth Index*

In performing this procedure, we use three baseline datasets composed of socioeconomic variables, which conform the wealth index according to the correlation fit among them. The first source of information consists of the National Department of Planning (NDP) database for access to welfare programs —SISBEN— for 35 villages in the study area; the second source comes from the 2005 Census data for 65 villages —also in the area of study. However, in order to compare and homogenize results, there was a third calculation employing the census information for the same set of 35 villages contained in NDP dataset. For each case, all villages are included in the index to generate a standardized normal distribution. Nonetheless, for simulations, the database excludes all villages except Santa Lucia (treatment), San Cristobal and or Soplaviento (controls).

The variables with the most robust correlation fit, and which ultimately compose the index are: floor material, sewerage, telephone tenure, energy, gas, garbage, aqueduct, water, sanitary, use of sanitary, energy for cooking, refrigerator tenure, washer tenure, tv tenure, shower, number of rooms. According to Table 4, the mean value of the wealth index for Santa Lucia (treatment), employing the NDP dataset, is different from the control groups (San Cristobal; Soplaviento; pooled). Their values, however, are homogenized when using the 2005 census information, particularly when employing the broader set composed of 65 villages.

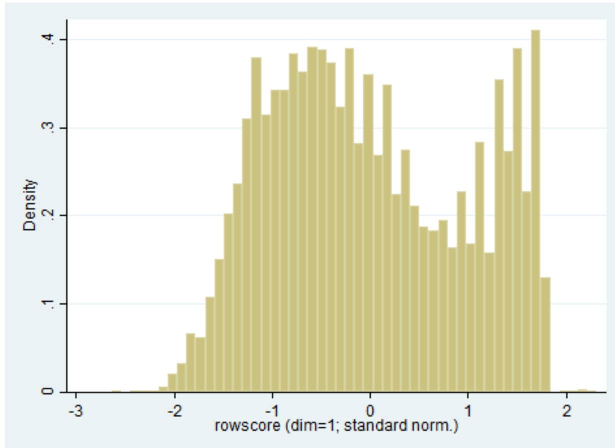
Table 1.4. Wealth Index

Villages	**Mean**		
	NDP	**Census***	**Census****
Santa Lucia	-0,245	-0,045	-0,450
San Cristobal	0,400	-0,460	-0,125
Soplaviento	0,094	-0,494	-0,312
San Cristobal-Soplaviento	0,246	-0,478	-0,214

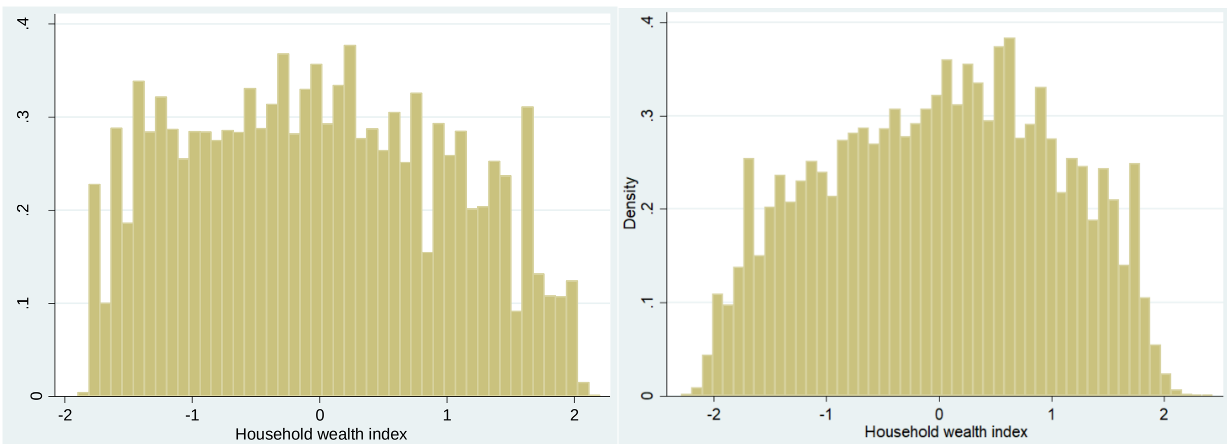
*35 villages

**65 villages

The latter is corroborated when analyzing the index distribution, displayed on Figure 1.7. We can observe that, when using NDP information —Panel (a)— the index does not approach a normal distribution, which is reflected on the differences in means shown in the above table, deeming this dataset as less desirable in order to perform power calculations. Panel (b), however, where census information is employed, exhibits a behavior that closely approximates a normal distribution, bolstering the latter as the choice for determining the sample size, especially when using the set of 65 villages.



Panel (a) —NDP



35 villages **65 villages**

Panel (b) —2005 Census

Figure 1.7. Household Wealth Index Distribution

In order to form an expectation regarding the sample size before performing power calculations with the wealth index, we simulate the sample size using the mean and standard deviation of each of the variables that encompass the wealth index, with results displayed on Table 1.5. In the case of Santa Lucia vs San Cristobal, the percentiles 50% and 75% suggest that the sample size ranges between 364-660 households with NDP dataset, and 266-1500 households with the 2005 Census. Analyzing Santa Lucia vs Soplaviento distribution, it evidences a bigger range between 564-1084 (NDP) and 601-1120 (Census) households with 50% and 75% of percentiles.

Finally, San Cristobal and Soplaviento as one control group shows a concentration of 352-802 (NDP) and 270-597 households (Census) in the percentiles like the last groups.

Table 1.5. Sample Size

Percentiles	San Cristobal		Soplaviento		San Cristobal-Soplaviento	
	NDP	Census	NDP	Census	NDP	Census
1%	38	37	34	40	48	40
5%	54	84	74	70	96	80
10%	78	142	204	256	146	112
25%	136	266	564	601	352	270
50%	364	609	1084	1120	802	597
75%	660	884	4904	1306	1696	1040
90%	964	1250	9996	1450	2588	1548
95%	1164	1555	12084	1650	2972	1748
99%	1432	1708	14184	1894	3372	1948

It is important to clarify that, because calculations are performed at an aggregate level, the error terms might not be independent. Therefore, households from the same village might have correlated unobserved characteristics, reason for which it is necessary to measure within and between intra-class correlation. According the results displayed on Table 1.6, intra-class correlation across all villages express 9.6% (NDP dataset), 48.7% and 42.32% (Census) of total variation in socioeconomic attributes, while 5% and 6% come from variation between households of the same village. Despite of the levels of intra-class correlation, it proceeds to account for within village correlation by using clustering units at the village level. According to these results, intra-class correlations across all villages express 9.6% (NDP dataset), 48.7% and 42.32% (Census) of total variation in socioeconomic attributes, while 5% and 6% come from variation between households of the same village. Despite of the levels of intra-class correlation, it proceeds to account for within village correlation by using clustering units at the village level.

Table 1.6. Intraclass Correlation

NDP		Census*		Census**	
H	**V**	**H**	**V**	**H**	**V**
0,058	0,096	0,063	0,487	0,050	0,423

Note: H= household level; V=village level

According to Table 1.7, the outcomes indicate that for a balanced design, the minimum sample size required for rejecting null hypothesis that the index mean value in Santa Lucia is equal to that of San Cristobal, consists of; 72 when correcting for intra-class correlation and 64 without adjustment (NDP); 616 when is corrected and 84 without adjustment (Census 35 villages); and 242 (corrected) and 106 (uncorrected)**.** For Soplaviento as a control, the minimum sample size required to detect an effect would be; 678 with the correction and 250 without (NDP); 684 (corrected) and 70 (uncorrected) by using Census with 35 villages; and 1016 when correcting for intra-class correlation and 550 without adjustment (Census 65 villages). Combining both villages into one control group requires a sample of; 162 and 116 (NDP), respectively; 780 (corrected) and 76 (uncorrected) by employing Census 35 villages; and 648 when correcting and 350 without correcting (Census 65 villages). The final sample size includes 1,718 households (987 on the treatment village), 534 observations above the most conservative estimate of 1,184 households —with replacement— according to the power calculations outlined above.

Table 1.7. Power Calculations

Villages	NDP			Census (35 villages)			Census (65 villages)			Design
	n1	n2	N	n1	n2	N	n1	n2	N	n1/n2
St.Lucia vs St.Cristobal	32	32	**64**	42	42	**84**	53	53	**106**	1.0
	36	36	**72**	308	308	**616**	121	121	**242**	
	26	39	**65**	33	50	**83**	42	63	**105**	1.5
	32	48	**80**	226	342	**568**	78	117	**195**	
	22	44	**66**	29	58	**87**	37	74	**111**	2.0
	27	54	**81**	255	510	**765**	84	168	**252**	
St.Lucia vs Soplaviento	125	125	**250**	35	35	**70**	275	275	**550**	1.0
	339	339	**678**	342	342	**684**	508	508	**1016**	
	100	150	**250**	28	42	**70**	215	323	**538**	1.5
	283	425	**708**	288	432	**720**	397	596	**993**	
	88	176	**264**	25	50	**75**	185	370	**555**	2.0
	275	550	**825**	342	684	**1026**	342	684	**1026**	

1.4.4 *Random Selection*

After calculating the appropriate sample size for the treatment and control groups, the next step consisted on randomly selecting the sample, according to preestablished grids on each village. For this purpose, we divided municipalities into spatial configurations that closely follow the geographical arrangements established by the National Department of Statistics of Colombia pertaining to census data collection. Within each block, dwellings were assigned a unique identifier, from which households were randomly chosen in order to conform the sample. Since observations were selected from heterogeneously populated grids, we assigned differential weights to each block when drawing the sample. This procedure included a sample correction resulting from discarding or substituting dwellings for which an interview could not be conducted upon visit to the unit. The margin of error was calculated with the following equation and information:

$$ME = \sqrt{\frac{(pqz^2)(N-n)}{n(N-1)}} \tag{1.1}$$

Where *N* corresponds to the overall population estimate, which we obtain from the National Department of Statistics; *n* corresponds to the sample size; *p* is the probability of being interviewed on a particular village; *q* corresponds to the complement of p; and *z* is a normal probability distribution parameter.

According to the calculations; in the balanced design the sample size for San Cristobal is 308 households and Santa Lucia 308; in the unbalanced design (1.5) the data set is 226 for the control group (San Cristóbal) and 342 for the treatment one (Santa Lucía); and in the unbalanced design (2.0) the required sample is 255 for San Cristobal and 510 for Santa Lucia.

Table 1.8. Adjusted Sample Size

Balanced Design						
Village	**Confidence level**	$Z_{\alpha/2}$	**Sample size**	**Population**	**Margin of error**	**Sample size (Adjusted)**
San Cristobal	95%	1.96	308	975	4.3%	321
Santa Lucia	95%	1.96	308	1600	3.96%	320
Total			**616**			**641**

Unbalanced Design (1.5)						
Village	**Confidence level**	$Z_{\alpha/2}$	**Sample size**	**Population**	**Margin of error**	**Sample size (Adjusted)**
San Cristobal	95%	1.96	226	975	4.82%	237
Santa Lucia	95%	1.96	342	1600	3.85%	355
Total			**568**			**592**

Unbalanced Design (2.0)						
Village	**Confidence level**	$Z_{\alpha/2}$	**Sample size**	**Population**	**Margin of error**	**Sample size (Adjusted)**
San Cristobal	95%	1.96	255	975	4.64%	267
Santa Lucia	95%	1.96	510	1600	3.34%	527
Total			**765**			**794**

The STATA commands used for randomly drawing the sample were ***runiform*** and ***sample***. The first one randomly assigns an order for households within each grid; the second one determines the number of households to be selected in each block according to the size of each grid. The process was the following:

```
tempvar u
gen `u'=runiform()
sort Manzana `u'
bys Manzana: gen order= _n
bys Manzana (order): list Manzana order Identificador, noobs sepby(Manzana)
```

Where *manzana* corresponds to each of the predefined grids from which units were randomly selected. The above is the algorithm for ordering each dwelling within the block. As we can see on Figure 1.8, within each grid the algorithm generates a random order that indicates the specific dwelling to be interviewed.

```
-> Manzana = 001

Manzana   order   Identi~r

    001       1       0014
    001       2       0017
    001       3       0013
    001       4       0012
    001       5       0018
    001       6       0011
    001       7       0016
    001       8       0019
    001       9       0015
```

```
-> Manzana = 003

Manzana   order   Identi~r

    003       1       0032
    003       2       0035
    003       3       0034
    003       4       0031
    003       5       0033
```

```
-> Manzana = 090

Manzana   order   Identi~r

    090       1       0904
    090       2       0902
    090       3       0905
    090       4       0901
    090       5       0903
    090       6       0906
    090       7       0907
```

Figure 1.8. Random Sample

The next step consists of establishing the number of households to survey on each block. For this, we implemented the command ***sample***, which determines a certain number of observations to select, without replacement, after considering the frequency and size of each group. In order to apply this command, it was necessary to calculate the proportion corresponding to the sample size over the estimation reflecting the overall population, determining the percentage of observations to select in the ***sample*** command for each design. The total of dwellings in San Cristobal are 975 and Santa Lucia 1600, for which we obtain the following results:

Table 1.9. Random Selection

Village	**Balanced (1.0)**		**Unbalanced (1.5)**		**Unbalanced (2.0)**	
	N°	**%**	**N°**	**%**	**N°**	**%**
San Cristobal	308/975*	**31.6%**	226/975	**23.2%**	255/975	**26.2%**
Santa Lucia	308/1600*	**19.3%**	342/1600	**21.4%**	510/1600	**31.9%**

*Total number of dwellings in each village

```
/*Balanced=1*/
sample 31.6, by (Manzana)
tab Manzana
count
list
```

```
/*Unbalanced=1.5*/
sample 23.2, by (Manzana)
tab Manzana
count
list
```

```
/*Unbalanced=2.0*/
sample 26.2, by (Manzana)
tab Manzana
count
list
```

In the case of San Cristobal (control), 31.6% of the total population was selected to be surveyed; 19.3% in Santa Lucia (treatment). The “by (Manzana)” indicator distributes that percentage across blocks according to the particular weights assigned by the algorithm, thus generating the distribution of households to be included in the final sample. It is worth mentioning that we used the command ***runiform*** for randomization instead of ***sample*** because the former allows us to keep the whole dataset upon selection, enabling the implementation of the margin of error —explained earlier— in the case of discarded observations.

1.5 ANALYTICS

1.5.1 *Confounding Factors*

Per the identification strategy introduced in Section 1.2, the government intervention was confined to a village located North of a dike that failed in late 2010, in the midst of a climatological shock that inundated the entire region. The counterfactual then, consists of households who reside on a village South of the ruptured dike, where no intervention took place.

Although flood incidence was similar on both villages, households located North experienced a flash flood that triggered extensive media attention and hence, forced the government to concentrate resources on this area. The South side, on the other hand, experienced a gradual rise on water levels from torrential rain, receiving less attention and resources. Hence, the levee breach was used as an instrument to assign conditions, as it triggered the intervention on one side while leaving the other unattended.

For each village: shock intensity was measured through reports on losses/damages concerning dwellings, appliances, plots, crops, animals, and employment. Media coverage was approximated relating results from internet searches and news-reports regarding the event. And the extension of the intervention was illustrated via government resources assigned to reconstruction and grants.

The identification strategy assumes that a potential flood effect would not confound the results, as the incidence of the climatological shock was presumably similar, such that households on both sides would exhibit similar responses, thus preserving baseline trends. Nevertheless, and to further scrutinize and eliminate any flood effect, I exploited heterogeneity on flood damage within villages, establishing if farm-related losses influence farm exit through a simple regression. In case of evidence suggesting behavioral responses, comparisons were limited to groups that report the same level of farm losses.

1.5.2 *Robustness Checks*

Despite the previous assumption, the proposed analytical framework potentially suffers from bias arising from the fact that the comparison group still experienced slightly less flooding and was not hit by a flash flood. Nonetheless, said bias can be characterized exploiting differences on flood and treatment intensity by incorporating additional villages into the analysis.

As was mentioned earlier, the climatological shock affected most of the villages on the Colombian Caribbean Coast, providing a source of exogenous variation on flood incidence. Additionally, the Government intervention was concentrated on five villages located North of the levee that failed in late 2010, generating additional measures of treatment intensity. To evaluate the bias, the main hypothesis was tested using a broader sample, consisting of cross-sectional waves of census data corresponding to 2005 and 2018, respectively. This favors a more rigorous treatment effect analysis, since villages ultimately represent the observational unit targeted by the intervention. It also helps characterization, generating treatment effect bounds by ranging the control group from entirely flooded to virtually non-flooded villages.

The above-mentioned approach, however, does impose certain restrictions. Firstly, there is limited availability of treatment condition indicators, specifically access to PXMF. Next, baseline similarities decrease as more municipalities are included in the analysis, mostly as a result of geographical factors, which affects the parallel trends assumption. Also, the census sample is restricted to a subsample when analyzing outcomes related to enterprises, thus reducing statistical power. Finally, and most importantly, questions on the census are not asked retrospectively, discarding events between 2005-2018 that might have influenced household behavior. Nonetheless, it is still expected that these additional checks constitute a useful mechanism to describe any potential bias on the reduced-form specification.

1.5.3 *Attrition*

The natural disaster might have prompted families to relocate elsewhere, leaving an endogenous sample and thus, generating attrition bias. Attrition rate was measured through migration reports corresponding to the household questionnaire applied on the area of study, as well as the 2018 census information. The first method inquires for migration within and between households, directly asking families for migration history of all members, relating reasons and year of migration. It also asks households if they know of any other families who abandoned the village between 2008-2018, also relating reasons and year of relocation. The second approach employs census information that tracks families who left the village and currently reside elsewhere, asking about residence history within the last 5 years.

Anecdotal evidence suggests that the vast majority of families remained on the area of study after the flood. In fact, the attrition rate is expected to be less than or equal to 5%. However, anticipating otherwise, data collection involved gathering contact information of families and individuals who reportedly relocated elsewhere. If the attrition rate is above 5%, a follow-up telephone survey will be conducted with missing participants in order to restore balance and reduce attrition as much as possible.

Besides the latter, a set of additional measures were undertaken to address this potential issue. First, assuming that attrition was independent of potential outcomes and exclusively associated with observable factors (such as baseline socioeconomic characteristics that determine ability to relocate upon a shock), conditioning on these variables would generate unbiased estimates. Additionally, and seeking to depict the possible bias, I calculated treatment effect bounds following the procedures proposed by Manski (1990), Lee (2009) and Behaghel *et al*. (2015), generating lower and upper limits for the true intent-to-treat effect.

The main hypothesis of this study can be tested through 2 conditions: employment and grants. To test their validity, I conducted an F-test, where the null hypothesis states that none of the conditions influences farm exit. If the null hypothesis is rejected, at least one of the conditions is statistically significant. To minimize the possibility of a false discovery when assessing the validity of each individual channel, I performed multiple testing following Bonferroni's correction, such that the criterion used to determine relevance meets the following requirement:

$$\text{Reject } H_0 \text{ if } p_i \leq {}^{\alpha}/_{n} \tag{1.13}$$

Where p_i is the p-value associated with hypothesis test i; α is the significance level, set at 0.05; and n are the number of individual hypotheses, 2 in this case. With this correction, the null hypothesis will be considered false if $p_i \leq 0.025$.

1.5.4 *Differential Treatment*

One of the major components of the governmental response consisted on the housing intervention toward families whose residence was affected by the calamity. Specifically, dwellings that experienced minor damages received up to $1,200 worth of repairs, while houses with structural damage were reconstructed or relocated on new improved neighborhoods (Sanchez-Jabba, 2014). This can be exploited to detect differential effects arising from variations on the relative exposure to the treatment. To do so, the treatment group will be split into two sub-groups: Treatment 1 (T1), which includes households that were offered the full extent of the intervention, including a new house; and Treatment 2 (T2), which comprises families who exclusively benefited from the extensive provision of public goods[17].

Within T1, the offer of a new dwelling from the government represented a plausibly exogenous event, determined by pre-flood residence with respect to the town's main road, which bisects Santa Lucía and acts as a levee that protects houses to the South (Picture 3)[18]. The intake of the new house, however, constituted an endogenous decision influenced by pre-established socioeconomic characteristics, which led to rejections of the dwelling solution offered by the government. Therefore, estimates obtained through this analytical framework correspond to causal intend-to-treat effects. In particular, families who inhabited low quality buildings (i.e. mud houses) essentially had to accept the new house because their home was destroyed, leaving them with no outside option (this group will be labeled T1A). On the other hand, families offered a new home, but who lived in higher quality buildings (i.e. cement and concrete), rejected the offer because it

[17] This categorization facilitates the decomposition of the impact associated with the intervention. For example, if we compare outcomes between T1 and the control group, we would be estimating the overall effect of the intervention. However, if the treatment group is exclusively composed by T2, we would narrow down the effect toward the extensive provision of public infrastructure. Moreover, if we subtract the public goods effect from the overall intervention effect, we would be effectively estimating the impact associated with the provision of housing to the rural poor.

[18] Since the flood hit Santa Lucía from the North, dwellings located North of the road suffered greater structural damages, enduring a tsunami-like event that resulted on the offer new dwellings for families residing on this side of town. On the other hand, houses on the protected side experienced a gradual rise on the water level due to leakages and overflow, which led to minor dwelling repairs.

did not adjust to their preferences and they could return to their homes once the water was evacuated[19], even if their building was structurally damaged (this group will be labeled T1R) [20].

Figure 1.9. Main road in Santa Lucia (2011)

Acceptance of the housing solution, as well as the likelihood of being offered a government dwelling, can be predicted through an index based on observable attributes, such as initial dwelling characteristics, savings, and overall income. This favors the use of alternative identification strategies, which do not rely on outcomes that are difficult to measure retrospectively or suffer from recall bias. For instance, employing a regression discontinuity design, the modeling of new dwelling intake allows within village comparisons between households that display analogous pre-flood characteristics, but who diverge on the access to the full extent of the treatment as a result of heterogenous valuation of their outside option. Furthermore, between village cross-sectional

[19] These families rejected the housing intervention because they had to surrender the land where the damaged dwelling was located; the house offered by the government did not have enough room for several families; did not have a backyard that could be used for cultivation; and because the material of the new dwelling was of less quality than expected.
[20] By rejecting the offer, these families waived the reception of aid toward the reconstruction of their house, being excluded from any further distribution of resources associated with the dwelling intervention. This might have generated morale effects arising from perceived unfairness on T1R, who reported lower levels of life-satisfaction and happiness during informal follow-up interviews.

comparisons can be conducted by matching observational units with a similar propensity to be offered a new house.

This study also analyzed spillover effects resulting from changes in motivation. Following a conceptual framework similar to Breza *et al*. (2015)[21], we might expect differential responses for members of families who chose not to accept a new dwelling. These households had to repair their house with private resources or live in a severely damaged building. The latter might have led them to experience opposite effects with respect to their favored counterparts, generating perception of unfairness and abandonment by the Government due to their exclusion from the reconstruction of houses.

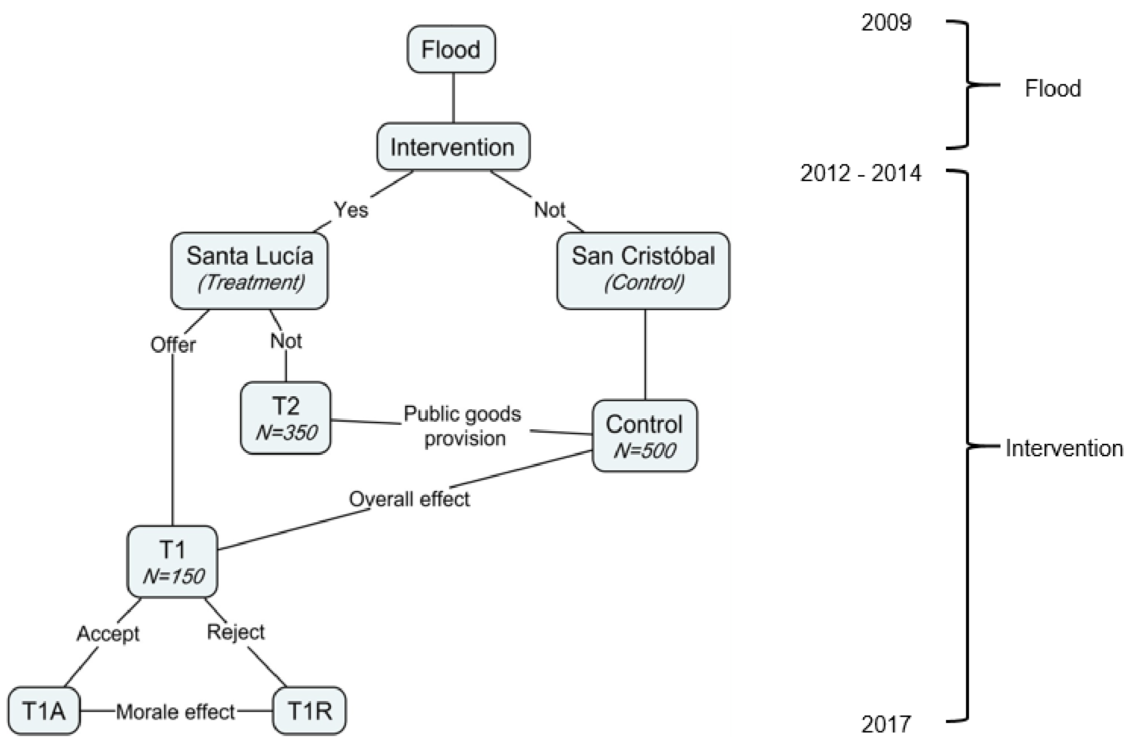


Figure 1.10. Analytical Framework

[21] Breza *et al*. (2015) find that unequal pay in otherwise similar individuals leads to distortion on effort choices.

1.6 EMPIRICS

All families residing in Santa Lucia at the time of the flood benefitted from the extensive provision of public goods and transfers. However, some families received a greater extent of the treatment, expressed through the reception of a new government dwelling. Therefore, the main empirical specification compares households who received a new house (T1) against their counterparts on the control village, keeping track of factor reallocation through changes on enterprises, occupation, crops and hours worked. The estimating equation is following:

$$Y_{iht} = \alpha + \gamma_h + \beta_1 NewHouse_h + \beta_2 Post_t + \beta_3 NewHouse_h x Post_t + \phi \mathbf{X}_{ih} + \varepsilon_{iht} \quad (1.14)$$

where Y_{iht} denotes the outcome of interest for individual *i* in household *h* at time *t*; $NewHouse_h$ is an indicator function that takes the value of 1 if household *h* obtained a new dwelling from the government and 0 otherwise; $Post_t$ indicates if the individual (household) is observed after the intervention was implemented; $\mathbf{X}_{ih}$ denotes a vector of individual and household-level pre-determined controls; γ_h are household fixed effects; and ε_{iht} corresponds to an idiosyncratic disturbance term.

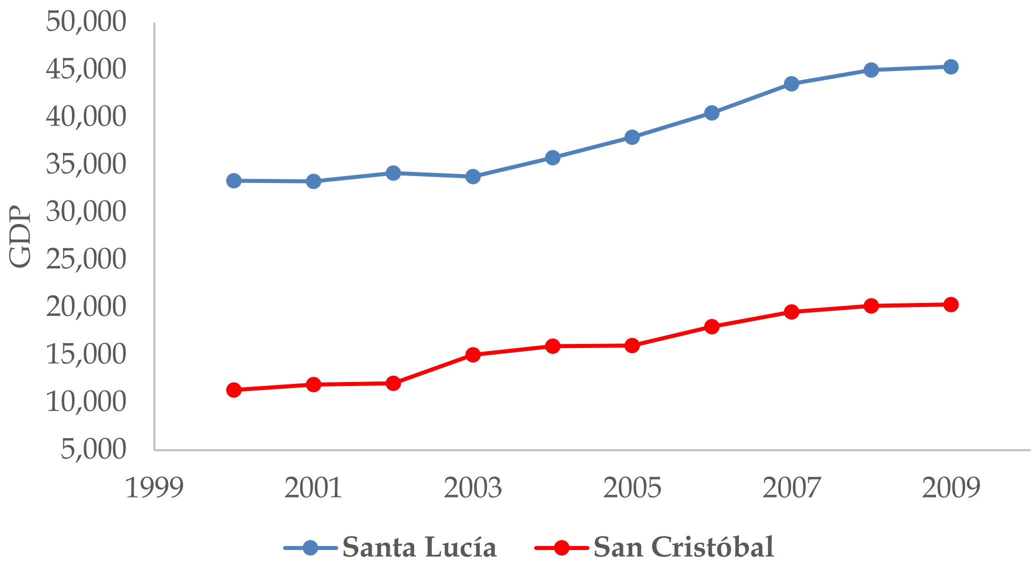
As outlined on Section 1.5, one major concern is that the take-up of the house was endogenous. Nonetheless, households located on the unprotected area who rejected the offer of a new house were included on the treatment group (T1), yielding causal intent-to-treat effects. Specifically, the decision to accept a new government dwelling was correlated with observable household characteristics, fixed at the time of the flood, such as the building's quality, savings and income. Therefore, the treatment status was instrumented with the dwelling's initial location with respect to the main road. This strategy assumes that unprotected houses located further North from the road were more vulnerable to flood damages, since they endured abreast the discharge of water.

The latter incremented the probability of being offered a new house, since the intervention depended on the dwelling's structural damage.

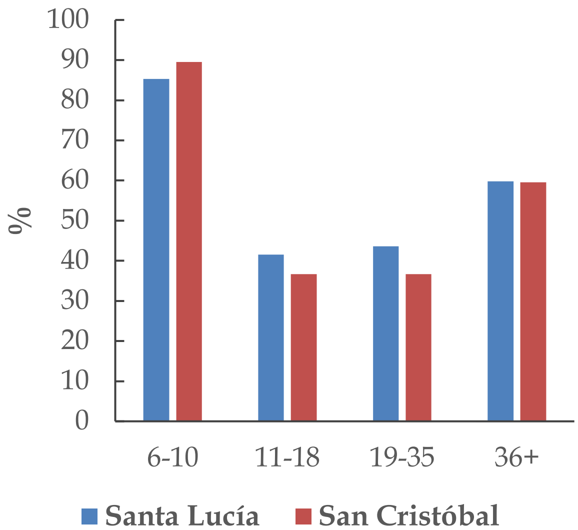
Buildings' location was tracked employing GPS coordinates (for families who did not change address) and self-reports corresponding to residence before the event. In order to test the relevance of the instrumental variable, a regression of house intake with respect to the dwelling's location yielded first-stage results. Additionally, there were no reasons to expect any correlation between the dwelling's location relative to the main road and any unobservable characteristics that influence house intake.

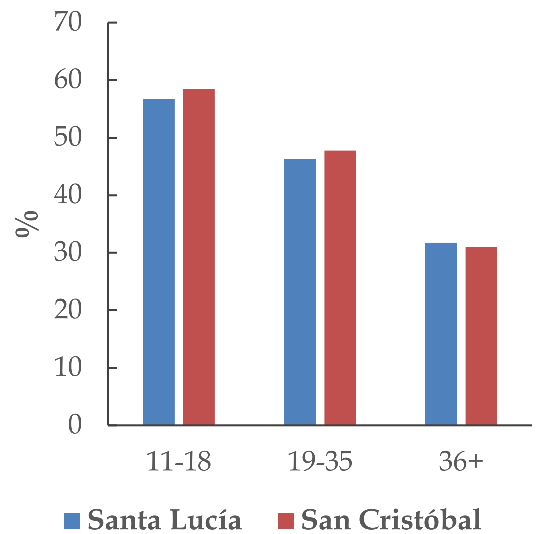
One final consideration of the empirical framework relates to the quality of the control group. Since families who were offered a new dwelling displayed distinctive socioeconomic characteristics before the flood, the sample corresponding to the control group was restricted by matching observational units of equivalent baseline attributes. To do so, a socioeconomic score variable that predicts the likelihood of being offered a new house served as matching criteria for households that otherwise diverge on the access to the housing intervention.

The difference-in-difference estimation assumes that the treatment and control groups exhibit parallel trends before the intervention was implemented, such that changes in the comparison group represent an accurate counterfactual of changes in the treatment group in the absence of the program. This is corroborated by analyzing the growth trajectories of key pre-flood economic indicators, such GDP and education levels for different age groups, as shown in Figure 1.11, where we can see the proposed variables exhibit parallel pre-disaster trends.



a. GDP (COP millions)





i. No education or Primary

ii. Secondary

b. Education Level by Age Group (% of total)

Figure 1.11. Pre-flood Growth Trends

1.6.1 *Aspirations*

New contexts provoke aspirational changes that result in behavioral responses. For example, Dai *et al*. (2014) argue that people are more likely to accomplish personal goals after salient landmarks that represent new beginnings, such as a new year or a new week. Similarly, Ray (2006) show that effort exertion is strictly related to changes in aspiration gaps —the difference between one's actual aspiration level and the level one wishes to attain.

Based on the latter, the proposition of this mediating mechanism is driven by the notion that, motivated by the fresh start embedded in a new house, beneficiary families undertook decisive steps to improve their welfare. Specifically, it is argued that the new context generated by improved housing conditions created a new aspiration window, resulting in incremental effort. This response might be reinforced by the fact that numerous families reported that the main benefit of the post-disaster intervention consisted on finally having ownership of a house[22].

Aspirations were measured through an index constructed on the basis of desired income, wealth, occupation, childbearing, enterprises, children's educational attainment and occupation. The validity of this mechanism was tested via a regression of new dwelling reception with respect to aspiration changes. It is expected that families who received a new government dwelling will exhibit statistical significance of the aspiration channel, signaling the importance of the psychological benefits associated with the intervention. To further corroborate this result, this response will also be approximated through motivational changes expressed through respondents' life satisfaction, incidence of depression, perception of governmental support and political participation.

1.6.2 *Relaxed Constraints*

The proposition of this channel is motivated by the possibility that the new house relaxed monetary and time constraints for recipient households. The test-pilot survey conducted on the area of study

[22] Information based on a test-pilot survey conducted on the area of study during October 2016.

revealed that the new house relieved dwelling expenditures. As mentioned previously, several households who obtained a new government house initially inhabited low quality dwellings, which required constant repairs. For example, during the rainy season mud-houses suffer a great extent due to leakages and erosion, forcing families into devoting considerable resources to mitigate any damages. With the reception of the new house, however, recipients did not have to worry anymore about these types of repairs. Analogously, a unit with improved sewage system, water quality, kitchen, floor and wall materials, among others, might have reduced doctor visits and thus, health related expenses arising from sanitary diseases[23].

Following a similar framework to Field (2007), this mechanism investigates if the new dwelling endowed beneficiary households with extra resources that could be reassigned toward activities that yield higher returns. The validity of this channel will be tested through a regression that considers changes on housing repairs (in time and money), health expenditures, sanitary conditions, overall health status, doctor visits, and incidence of infections, with respect to the reception of a dwelling. It is expected that recipient families will exhibit increased effort, consumption, and leisure, among other responses that signal the reallocation of additional resources.

This estimation requires the comparison between groups that exhibit similar pre-flood housing and health expenditures. It should also account for general equilibrium effects, since the construction of the new hospital might have influenced outcomes that are also affected by improved sanitary conditions resulting from a new house.

[23] Numerous studies illustrate the close relationship between water/sanitation conditions and health outcomes (Montgomery and Elimelech, 2007; Mara *et al*., 2010; Bartam *et al*., 2005).

1.7 CONCLUSION

Governmental responses to natural disasters constitute an opportunity to enhance welfare of rural communities living under high incidence of poverty. Beyond the extensive transfer of resources, which potentially result in advanced conditions relative to the status quo among beneficiaries, post-disaster programs plausibly generate second-order gains expressed through changes in households' behavior and outcomes.

This chapter describes the context of an unprecedented government intervention aimed at mitigating the impact of a natural disaster on the Colombian Caribbean Coast in late 2010. It introduces a framework that enables the subsequent analysis of Chapters 2 and 3, farm exit and returns to habitat. Initially, it depicts the events that triggered the intervention, a natural disaster that resulted in a government program aimed at mitigating the effects of the shock. Since the existing institutional information regarding communities residing on the study area suffers limitations tracking families and obtaining information that extends beyond the basic sociodemographic attributes, this project involved the collection of data through a survey specifically designed for this investigation. Said survey was applied between March and May of 2018 and was formulated following the guidelines established by the World Bank Living Standards Measurement Study and the Colombian Longitudinal Survey. It contains current and retrospective information on households' income, consumption, time use, habitat, wealth, and migration, among other essential indicators.

Chapter 1 also outlines the sampling framework followed in order to ensure the sample size included the minimum number of observations required to detect an effect. The sample size comprises 1,718 households, distributed between two villages. This was determined by performing power calculations that employ baseline sociodemographic data from the National Department of

Statistics and the database corresponding to the government's welfare programs, administered by the National Department of Planning. Within each village, households were randomly selected employing pre-defined grids, consistent with the protocols used to collect census information.

The identification strategy exploits a quasi-random assignment framework that compares, employing a difference-in-difference approach, outcomes across households that exhibit equivalent pre-determined socioeconomic characteristics, but who subsequently diverge due to the reception of transfers. The second chapter argues that the intervention stimulated aggregate demand for non-tradable goods, encouraging farm exit in response to a higher demand for small-scale services associated with widespread reconstruction, such as lodging, meals, and transportation. Capitalizing on extensive post-disaster dwelling provision, the third chapter provides the first estimates in the development literature regarding returns to housing among the rural poor, focusing on the psychological and labor market effects arising from home upgrades and tenure.

Results include a transition from a standard agrarian economy toward a semi-urban economic setting, characterized by a broader provision of small-scale services through off-farm enterprises. Specifically, the intervention plausibly relaxed pre-existing credit and information constraints related to the acquisition of new capital and skills, associated with the uptake of new economic activities. By estimating second-order gains resulting from the re-allocation of productive factors, this investigation will provide evidence regarding the efficiency of public spending aimed at mitigating the negative effects of calamities, focusing on investments that alleviate rural poverty. It is expected that this ex-post assessment highlights strategies whose adoption yield high returns, thus enhancing public policies established for effective disaster management.

There are several ways in which my doctoral dissertation contributes to the literature on this topic. Firstly, it extends the analysis of governmental responses to catastrophes beyond welfare considerations associated with sizable transfers of resources, analyzing how improvements in allocative efficiency allow households to overcome pre-existing frictions that explain baseline sub-optimal outcomes. Additionally, it examines mediating channels that have not been considered, such as relaxed resource constraints, changes in risk aversion, and psychological boosts. Employing a framework analogous to Field (2007), the first additional mechanism explores if improved dwelling quality released resources that could be reassigned toward activities that yield higher returns. Following Moya (2017) and Tian & Lemos (2017), the second alternative views reassignments, especially after the occurrence of an aggregate negative shock, as possible measures of insurance against economic activities that inherently posit a higher risk level. The last mediator reflects on pycho-emotional responses to an improved state with respect to the status-quo, especially on living conditions, which should be reflected on incremental life satisfaction, happiness, aspirations (Galiani *et al*., 2016) and therefore, on decisive steps toward welfare gains. Moreover, it enriches the existing analytical framework by i) assessing long term effects of disaster management policies and ii) recurring to primary sources of information. In particular, it analyzes changes on households' behavior and outcomes five years after the post-disaster intervention and, more precisely, eight years after the calamity, while other studies mainly consider short-run effects of 1-2 years.

Finally, this research is relevant because it will contribute to an ongoing discussion pertaining to ways in which rural communities can be uplifted from poverty, providing evidence regarding the effectiveness of interventions that involve widespread and simultaneous provision of public goods, for which existing evaluations have generated mixed results. For example, even though the Millennium Villages Project was established over a decade ago, the impact of this program on the welfare of Sub-Saharan African communities is still actively debated. Most importantly, if in fact the post-disaster response led to net welfare gains, then these results would highlight the importance of having these protocols in place before a calamity occurs, especially if we take into account that these interventions are increasingly difficult to implement during an emergency (Skoufias, 2003).

Chapter 2. QUITTING THE FARM: DETERMINANTS OF FARM EXIT IN RURAL COLOMBIA

2.1 INTRODUCTION

It is a well-established fact that the non-farm sector plays a prominent role on the economy of rural communities in developing countries. It accounts for up to 40% of employment and household income on all major continents (Lanjouw & Lanjouw, 2001; Reardon et al., 2006; Reardon et al., 2009; Davis et al., 2010). 42% of rural households in Africa operate non-farm enterprises (Nagler & Naudé, 2014). Moreover, the emergence of this sector is perceived as an important contributor for economic development, as it represents a viable mechanism toward poverty reduction (Haggblade et al., 2010; Lanjouw & Mugai, 2009; Kijima & Lanjouw, 2005; Mellor, 2000; Johnson, 2000).

It is therefore crucial that we understand the process through which rural households in developing countries decide to transition out of subsistence farming, as this can lead to the enhancement of public policies aimed at improving rural welfare. There are a variety of reasons for which a rural family may decide to operate a non-farm enterprise; the existing literature, although empirically limited, has offered some insights on this matter. Barret et al. (2001) argue that microlevel determinants of the structural transformation of agriculture, the reallocation of factors toward manufacturing and services, include diminishing marginal returns of farm labor, market frictions, risk management, and ex-post coping with adverse shocks. Other authors claim that a fixed factor of production in agriculture –land– limits this sector's ability to absorb a growing workforce, generating excess labor that, either migrates to urban areas, or engages in non-farm activities (Babatunde & Qaim, 2010; Foster & Rosenzweig, 2008; Reardon et al., 2006; Lanjouw & Lanjouw, 2001).

As recent and comprehensive datasets allow the analysis of informal enterprises on rural areas, studies have been able to partially test some of these channels. For example, Emerick (2018) showed that agricultural productivity gains caused increments in non-farm employment in India. A somewhat similar response was found by Merfeld (2018), who detected that reductions in nonfarm profitability led to a decline in nonfarm employment. Based on these recent findings, it appears that the standard neoclassical assumption of equal marginal product of labor across sectors does not hold. Specifically, low productivity of farm labor relative to the rising market wage, generates shifts in rural households' input decisions (Gollin et al., 2014). However, several aspects that explain the decline in agricultural participation are not entirely evident, leaving us with additional questions. In particular, although structural transformation appears to respond to variations on the relative returns across sectors, it is unclear whether these changes are actually explained by push factors, such as the ones described above, or pull factors that make nonfarming more profitable, such as aggregate demand shocks, or even by labor market shocks that create off-farm options. As stated by Foster & Rosenzweig (2008), the process that describes agriculture exit is still poorly understood. More specifically, and as noted by Nagler & Naudé (2014), the literature on rural enterprise productivity and how this affects households' resource allocation decisions is scant.

Seeking to contribute to the fulfillment of this gap on the literature, the present study examines rural households' decisions to leave farming, analyzing the underlying factors that motivated the transition. It is argued that a government intervention, consisting of widespread construction and conditional grants, changed households' outside option and stimulated aggregate demand for non-tradables, prompting factor reallocations in respond to comparatively higher returns. Specifically, extensive reconstruction after a natural disaster endowed agrarian households with the option of engaging in non-farm employment at the official minimum wage, an alternative that was rarely available before the event. In addition, construction vacancies attracted predominantly urban migrants, most born on the study area, which contributed to the upward shift on the overall demand for non-farm goods and services. Finally, a rural program consisting on government sponsorship of entrepreneurial initiatives, which arguably relaxed preexisting credit constraints, increased household income and demand for non-farm inputs.

The identification strategy leverages on a quasi-random assignment framework generated by a flood occurred in late 2010, which compares individuals/households that exhibit equivalent pre-determined socioeconomic characteristics. Treatment status is instrumented through location with respect to a levee that broke during the emergency, triggering the intervention on one side, while leaving the other unattended even though it also experienced significant inundation. In order to eliminate confounding effects, counterfactual selection was restricted to municipalities that experienced substantial flood damage. Although this strategy will not completely eliminate potential biases, since the flood could not be identical at different villages, it should still preserve comparative differences between groups leading up to the intervention, since households on either side would presumably exhibit similar responses as long as the intensity of the calamity was relatively equivalent.

Factor reallocations are illustrated through a standard agricultural household model, where families initially engage in farm production, non-farm employment, and non-farm enterprises respectively. The government intervention subsequently bolsters aggregate demand for non-tradables and produces labor market shocks that cause disparities on the marginal returns across sectors. Theoretical predictions dictate that families reassign labor toward non-farm activities, signaling welfare gains that arise from improvements in allocative efficiency, if the average revenue from farm production is lower than the market wage and/or the revenue corresponding to non-farm enterprises.

Since the existing institutional information regarding the communities living on the study area lacks data that extends beyond the basic sociodemographic attributes, this investigation involved the application of a survey that enables the analysis of factors that influence farm exit. The questionnaire was formulated following the guidelines and parameters established by the World Bank Living Standards Measurement Study and the Colombian Longitudinal Survey, gathering current and retrospective information on households' wealth, consumption, migration, flood damage, employment, and enterprises, while tracking key variables that capture reallocations, shifts in aggregate demand, and conditional grants.

General results depict a gradual and from transition, from a standard agrarian economy toward a semi-urban economic setting, characterized by a broader provision of small-scale services through nonfarm enterprises, which is attributed to the extensive nature of the intervention. Reassignments are motivated by two factors: i) farmers' desire to reduce income volatility after an extensive negative shock on agrarian production, resulting in the adoption of activities —materialized in nonfarm enterprises— that inherently imply a lower risk and hence, reduce income volatility; and ii) a plausible curtailment of pre-existing credit constraints related to the acquisition of new capital associated with the uptake of new economic activities.

Beyond the main contributions pertaining to the literature on agriculture exit, this study enriches our knowledge of disaster management policies by extending the analysis beyond welfare considerations associated with sizable transfers of resources, incorporating improvements arising from gains in allocative efficiency. Additionally, it provides long-term evidence concerning efficiency of public spending aimed at mitigating the negative effects of calamities, focusing on investments that alleviate rural poverty.

The rest of this paper proceeds as follows: Section 2.2 provides background information regarding the calamity that activated a large-scale government intervention on the Colombian Caribbean Coast. Section 2.3 depicts the channels that potentially explain farm exit. Section 2.4 outlines the identification strategy pursued to detect the effect of the intervention on farm outcomes. Section 2.5 discusses potential threats to the proposed identification strategy. Section 2.6 presents the data used for this investigation. Section 2.7 lays the empirical framework; Section 2.8 describes farm exit on the area of interest during the period of study. Section 2.9 presents the results. Finally, Section 2.10 concludes.

2.2 BACKGROUND

Between 2010-2011 Colombia was severely affected by climatological shocks. Torrential rainfall across the country caused floods that generated losses equivalent to 6% of the gross annual capital formation and afflicted 7% of the population (ECLAC, 2013). One of the worst tragedies associated with this event occurred on the Caribbean Coast, where the rupture of a dike caused an inundation that distressed a region inhabited by rural communities living under high incidence of poverty. The flood displaced families, halted agricultural production, interrupted schooling, and destroyed productive capital, leading to a noticeable reduction in welfare (Sanchez-Jabba, 2011). In order to mitigate the effects, the government implemented a large-scale intervention that consisted of widespread reconstruction and conditional cash transfers. Pursuing a compensatory principle toward this historically neglected area, the response included entrepreneurial grants and the construction of schools, hospitals, aqueducts, sewage systems, parks, and houses, providing infrastructure that was lacking even before the disaster (Sanchez-Jabba, 2014).

As a result, local economic activity transitioned from standard farming toward a semi-urban setting, characterized by a broader provision of small-scale services through off-farm activities. For example, extensive reconstruction endowed rural families with the option of engaging in non-farm employment, an alternative that was rarely available before the event. Data collected on the study area indicates that 25% of households residing on the treatment village had at least one member who worked at the various construction sites between 2014-2018, period in which the intervention was executed. Before the flood, more specifically between 2008-2010, the percentage of families with a member working under formal employment conditions was 1%. Increase in labor demand also attracted individuals who were previously living at nearby urban centers, with 20% of households receiving a returning member between 2014-2018, of which 10% worked in construction.

The village also experimented a substantial upsurge in the operation of non-farm enterprises, with 16% of farming families opening a non-farm business. This was presumably favored by the reception of conditional grants of up to $500 through a government program aimed at strengthening rural enterprises, *Produciendo Por Mi Futuro* (PxMF)[24]. With the establishment of PxMF many agrarian families who experienced farm-related losses had the opportunity to either i) reinstate the initial farming activity or ii) choose a new economic sector. If in effect they chose not to restore the baseline production plan, this behavior would signal welfare improvements that

[24] PxMF is a government program aimed at alleviating rural poverty for households living under vulnerable circumstances and forced displacement. Through the provision of technical and financial assistance, this program creates favorable settings for the creation of enterprises through home visits, workshops, and monetary transfers. Eligibility for the program requires potential recipients to be over 18 years old; reside on one of the villages selected for the program; and have a government welfare program score (SISBEN) below a specific threshold. Although this program was not conceived as a measure intended to alleviate the impact of the flood, it was implemented parallel to extensive reconstruction. Therefore, most of the enterprises proposed in this village correspond to families who experienced disaster-related losses.

arise from factor reallocations[25]. Furthermore, of the families who opened a non-farm business, 37% benefited from PxMF, highlighting the importance of this program in relaxing preexisting constraints associated with the uptake of new economic endeavors. A nonnegligible proportion of these new businesses, 66%, provided services related to construction, such as meals, hydration, and transportation.

Enterprises created by farmers during this period consist mostly on businesses that commercialize minor manufactures and provide small-scale services, which account for 35 and 38% of the openings, respectively. An additional share of the transition can be explained by engagement in nonfarm employment, whose share rose by 24 percentage points, up to 37% in 2018 from 13% in 2008. In particular, the construction sector displayed spectacular growth, responsible for 45% of changes in occupation.

During this period, 74% of baseline farmers switched toward nonfarm occupations and farm labor participation declined by 22 percentage points among occupied people. Most of the changes between 2008 and 2018 took place among farmers who started working on the construction, trade and service sectors, which account for 45%, 17%, and 7% of adjustments, respectively. Transition out of farming was not only sustained by widespread construction. Farm exit was also encouraged by a shift on the aggregate demand for non-tradables, generated by inbound migration, higher household income, and increased demand for non-farm inputs, which made non-farm businesses more profitable. For instance, non-farm consumption among households who received a migrant between 2014-2018 was greater when compared against those with no migration shocks. Similarly, families who benefitted from PxMF reported an income that was, on average, 13% higher than that of their counterparts who did not benefit from this program.

[25] 16% of households residing on the study area reported liquidity constraints and the absence of credit markets as the main barriers to opening enterprises.

Depending on the income elasticity of demand for non-farm goods, this would result in a greater demand for non-farm goods. Finally, among non-farm enterprises opened between 2014-2018, 67% demanded non-farm inputs, a larger percentage when compared to farm businesses.

Based on these facts, it is clear that the government response to the disaster produced a structural change on the village economy, expressed through decline in farming activities. Although income disparity between farmers and non-farmers was of $63 before the flood, by the end of the intervention the median income among non-farmers was of $264, with paid workers earning $323 and profits from non-farm businesses reaching $331. In contrast, the median income of farmers was of $261, with average farm profits of $287.

2.3 CHANNELS

Changes in profitability, caused by aggregate demand shocks, induce factor reassignments. Leveraging on the agricultural household model proposed by Singh (1986), farm exit is framed as the result of an optimization process in which households continuously assess the returns of various productive activities in order to determine efficient allocation of resources. In this particular case, a shift on the demand for non-tradables, as well as labor market shocks, cause low farm earnings relative to nonfarm production and formal employment, triggering factor reassignments in response to diverging revenues. The analysis focuses on labor reallocations, being this the most prominent input employed in household production.

Households maximize utility subject to financial and technological constraints. Farm production, involving mostly staple goods, is described by technology $x = F(L_f, A_f)$, where L_f and A_f correspond to farm labor and land respectively. Nonfarm production, which consists on the provision of small-scale services and minor manufactures, follows technology $e = N(L_n)$, where L_n denotes nonfarm labor. Nonfarm production does not require land, since nonfarm businesses are typically located in dwellings or nearby markets (Nagler & Naudé, 2014). Both, F and N are concave production functions. Labor can be perfectly substituted across sectors. Households' optimization process is therefore described by:

$$
\begin{aligned}
&\text{Max } U(x, l) \\
\text{s.t.}\quad & p_x x + w(L_f^H + L_n^H) \leq p_x F(L_f, A_f) + wL^M + p_n N(L_n) \\
& E^L \geq L_f^F + L_n^F + L^M + l \\
& L_j = L_j^F + L_j^H, j \in \{f, n\} \\
& E^A \geq A_F
\end{aligned}
\tag{2.1}
$$

Equation (2.1) corresponds to households' utility function, which exhibits the conventional properties of decreasing marginal utility and depends on consumption of staple goods x and leisure l. Households do not consume nonfarm production. The maximization is with respect to consumption, leisure, family labor assigned to farming L_f^F, family labor assigned to nonfarm production L_n^F, and labor supplied to the market L^M. The second line of equation (2.1) depicts the budget constraint for the household, where consumption expenditures and expenses on hired labor must be less or equal than revenue from farm production, nonfarm enterprises' sales, and wage income. p_x and p_n correspond to prices of farm and nonfarm goods respectively. w is the market wage. The third line of equation (2.1) describes the labor resource constrain: families' labor endowment can be allocated between farm production, nonfarm enterprises, sold at a fixed market wage, or consumed as leisure. The fourth line of equation (2.1) illustrates labor assignment to sector $j \in \{f, n\}$, which can employ family L_j^F and/or hired labor L_j^H. The last line of equation (2.1) shows the distribution of land endowment, which can only be used in farm production. Land is fixed and there are no land markets.

Following a framework that allows the inclusion of a nonfarm sector (Barret, 1996), if resource allocation within the household is pareto-efficient, the marginal product of inputs should be equalized. This induces the standard separability condition that characterizes household models, where profits are maximized without considering preferences. The resulting efficiency condition is then:

$$p_x \frac{\partial F}{\partial L_f} = p_n \frac{\partial N}{\partial L_N} = w \tag{2.2}$$

Equation (2.2) no longer holds whenever a possitive demand shock translates into an increase in the price of non-tradables or provides households with off farm options that were not available before the intervention took place. In the following subsections, I present some of the channels that can produce disturbances that trigger factor reassignments motivated by diminishing marginal returns.

2.3.1 *Income*

Depending on the income elasticity of demand for local non-tradables, an overall increase in rural household income generates an expansion of the nonfarm sector (Foster & Rosenzweig, 2004). Particularly, if there exists strong consumption complementarity between nonfarm goods and the rest, this produces a reduction in agricultural labor in order to buffer increased demand for nontraded goods (Foster & Rosenzweig, 2008). Consequently, if conditional grants for the creation of enterprises, as well as construction employment, effectively led to higher rural income, and hence consumption, we should also expect to see a positive shift on the demand for nonfarm goods. Existing evidence validates this theoretical prediction. For instance, Emerick (2018) showed that the rural non-tradable sector, mainly manufacturing, construction, and retail, expanded in response to higher income occasioned by agricultural productivity gains.

2.3.2 *Migration*

The establishment of labor-intensive industries in rural areas spur urban-rural migration encouraged by the pursuance of employment opportunities (Corno & de Walque, 2012; Hilson, 2009). Immigrants, either new or returners, often demand goods and services that stimulate the local non-tradable sector (Constant, 2014; Baghdadi & Jansen, 2010; Boustan et al., 2010; Neary 1989). In particular, newly arrived migrants require services such as housing, meals, transportation, and education (Kochin & Bae, 2018), all of which increase both, the quantity and the variety of non-tradables supplied by local enterprises (Hong & McLaren, 2015). Additionally, incremental migration of predominantly urban dwellers could have resulted in urbanization of the village, leading to higher demand for rural goods (Cali & Menon, 2012). Following from the previous channel, complementarity in consumption would have increased the demand for nonfarm goods. As such, we should expect inbound migrants into the study area — seeking construction employment or benefits associated with the government intervention — to induce a positive demand shock for nonfarm goods.

2.3.3 *Inputs*

Haggblade et al. (1989) emphasize that rural enterprises supply inputs required by local farmers. Specifically, Islam (1987) argues that as agricultural production rises farm households demand a greater quantity of nonfarm inputs. Likewise, Reardon et al. (1998) state that farm/nonfarm linkages arise whenever the former induces the latter to supply inputs required for production. Based on these premises, we should expect increased entrepreneurial activity stemming from the program aimed at strengthening rural enterprises (PxMF) to translate into a higher demand for nonfarm goods coming from both, farm and nonfarm businesses. This channel is intensified if the immigration-induced demand shock contributed to aggregate employment through augmented labor demand required to meet increasing overall spending from new residents (Bodvarsson et al., 2008; Greenwood & Hunt, 1984).

2.3.4 *Services*

Extensive production prompts rural communities to provide industry-related services. For example, mining in Africa caused farm exit among women, who observed a reduction in agricultural self-employment, accompanied by a substantial increment in provision of mining supporting services (Marlet, 2018; Kotsadam & Tolen, 2016). In a similar way, it is possible that extensive reconstruction on the study area induced the local non-tradable sector, predominantly composed by women, to form markets aimed at satisfying the demand associated with workers at the newly established industry.

2.3.5 *Labor*

In a study that estimated the shadow value of farm labor, Jacoby (1993) noticed that even though the marginal product of this input is low relative to the market wage, this factor is still employed in household production. Conceivable explanations for this fact include agency issues and transportation costs, which cause farm labor to become comparatively valuable. It can also happen as a consequence of loose labor markets, where scant labor demand imposes constraints on nonfarm labor supply.

Benjamin (1992) indicates that excess labor, expressed through involuntary unemployment, arises whenever desired labor supply exceeds off-farm opportunities and farm labor demand, causing separability to fail. This situation can be illustrated employing the agricultural household framework, where nonfarm employment is restricted such that labor supplied to the market must be less or equal to some upper bound (i.e. $L^M \leq M$). In the binding case where $L^M = M$, and assuming $p_x \frac{\partial F}{\partial L_f} < w$, the household is restrained on the amount of labor it can sell on the market, assigning excess labor toward farm production even if there would be clear gains from factor reallocations.

A labor market shock that stimulates nonfarm labor demand, such as employment vacancies associated with widespread reconstruction, might restore the separability condition, making off-farm opportunities feasible as labor markets clear. This case arises in other labor-intensive sectors, such as mining, where local communities, particularly men, provide unskilled labor to the newly established industry (Kotsadam & Tolen, 2016; Loayza & Rigolini, 2016; Brain 2017). As depicted in Figure 2.1, a possitive demand shock on an initially loose labor market causes labor to start being exchanged at the market wage w. In this case, households are no longer restrained on their nonfarm labor supply, deciding factor allocations exclusively on the merits of marginal returns.

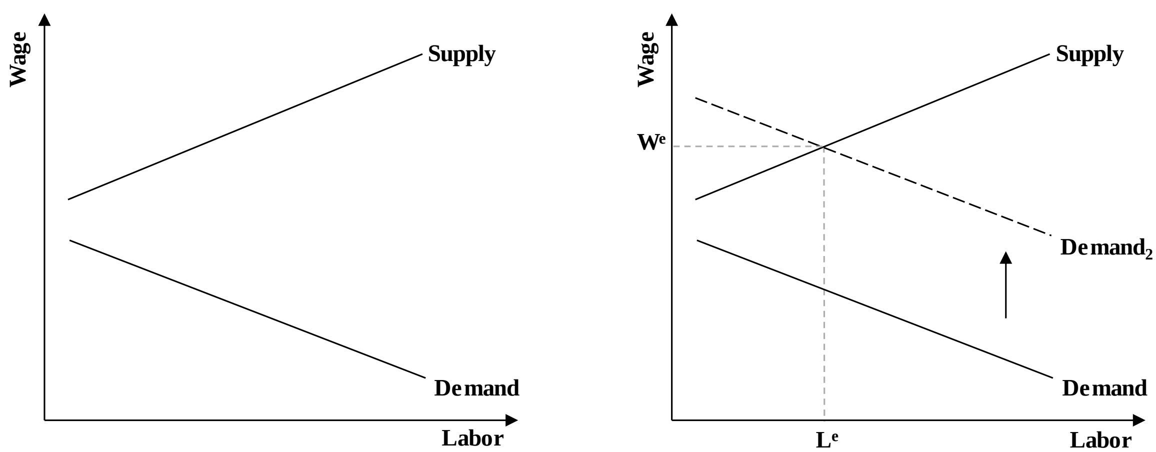


Figure 2.1. Labor Market Shock

2.3.6 *Risk*

Following Moya (2017) and Tian & Lemos (2017), the risk aversion channel views reassignments, especially after the occurrence of an aggregate negative shock, as a possible measure of insurance against economic activities that inherently posit a higher level of risk. The behavioral game applied during interviews presents participants with six different lottery choices, each positing a specific risk level[26]. It is expected that households who experienced the calamity to a larger extent will systematically choose safer lotteries.

2.4 IDENTIFICATION STRATEGY

Treatment status is instrumented through residence with respect to a dike that failed during the emergency, triggering the government intervention on one side, while leaving the other relatively unattended. Specifically, widespread construction and conditional grants were confined to Village A, where a flash flood prompted considerable media attention and therefore, attracted vast amounts of resources. Village B, the counterfactual, is located across the failed dike and experienced a gradual, yet equally destructive inundation from torrential rainfall and overflowing waters. However, due to the inconspicuous nature of the event on this village, it received extremely limited attention and resources.

Since the levee breach represented an event that could not have been reasonably predicted on either side, it constitutes an exogenous treatment assignment mechanism. The identification strategy assumes that a potential flood effect would not confound the results, since the incidence of the climatological shock was similar in both villages, such that households on either side would exhibit similar responses, thus preserving baseline trends. Relative equivalency can be examined on Table 3, which describes the intensity of the disaster through households' reports. As can be seen, approximately 80% of the population on either village had their dwelling destroyed or damaged by the inundation. 34% of households reported the loss of a family business. Farm-related

[26] Details on the risk aversion game used on this experiment can be found on the Online Appendix.

losses were greater on Village B, where 37% of families stated the loss of a farm/plot and 30% informed about the loss of farm equipment. Based on these results, one should expect residents on the control village to respond, if any, by exiting farming at a greater rate those on the "treated" group, as they suffered greater losses that inevitably diminish the return of agricultural activities.[27]

Figure 2.2. Area of Study

Table 2.1. Household Losses (2010 Flood) —% of total

	Village A	Village B	Diff.	
	% of households with loss			
Dwelling	81.47	78.56	2.91	
Enterprise	35.96	33.51	2.45	
Farm/Plot	29.18	37.18	-8.00	***
Machinery	22.21	30.53	-8.32	***
Animals	90.50	82.60	7.90	***
Crops	25.40	23.40	2.00	
Employment	33.00	18.90	14.10	***
	Units lost per household			
Cows/bulls	2.88	1.36	1.52	
Pork	2.53	1.06	1.47	***
Chicken/roosters	13.89	9.87	4.02	***
Crops (ha)	1.18	1.07	1.07	

Despite the equivalent saliency of the calamity, the post-disaster intervention focused on Village A, which benefited from considerable investments in education, health, housing,

[27] As outlined in Section 2.1, one of the push factors that account for farm exit are adverse climatological shocks (Barret et al., 2001; Lanjouw & Lanjouw; 2001).

sanitation, basic infrastructure, and conditional grants (see Table 4). Between 2012-2017 this village averaged public spending of $1,614 per household, while Village B averaged $501 per family over the same period.

Table 2.2. Reconstruction Investments (2012-2017) —USD

Sector	**Village A**	**Village B**
Housing	$ 2,005,000	$ 1,131,667
School	$ 899,333	-
Hospital	$ 2,765,333	-
Sanitation	$ 4,318,667	-
Infrastructure	$ 8,414,000	$ 2,230,667
Total	**$ 18,402,333**	**$ 3,362,333**

Source: Colombian Fund for Climate Change Adaptation

As mentioned earlier, extensive media coverage might have influenced agglomeration of resources. According to Figure 2.3, where this variable is approximated with news reports and internet searches, Village A received immense media attention after the 2010 flood. Village B, on the other hand, remained unnoticed despite experiencing comparable, or sometimes greater losses. The public interest generated by the flood, concentrated on Village A, plausibly compelled the government to strategically overinvest on the provision of goods in this village in order to maintain a favorable political image.[28]

[28] Plumper & Martin (2002) argue that in emerging democracies, such as Colombia, governments have an incentive to provide public goods, as opposed to rents, in order consolidate incumbency. In this particular case, if extensive media coverage created a favorable setting for the government to gain political support by effectively managing the disaster, we should observe overinvestment in public goods provision on this village relative to Village B, where there are no tangible returns (for the government) from doing so.

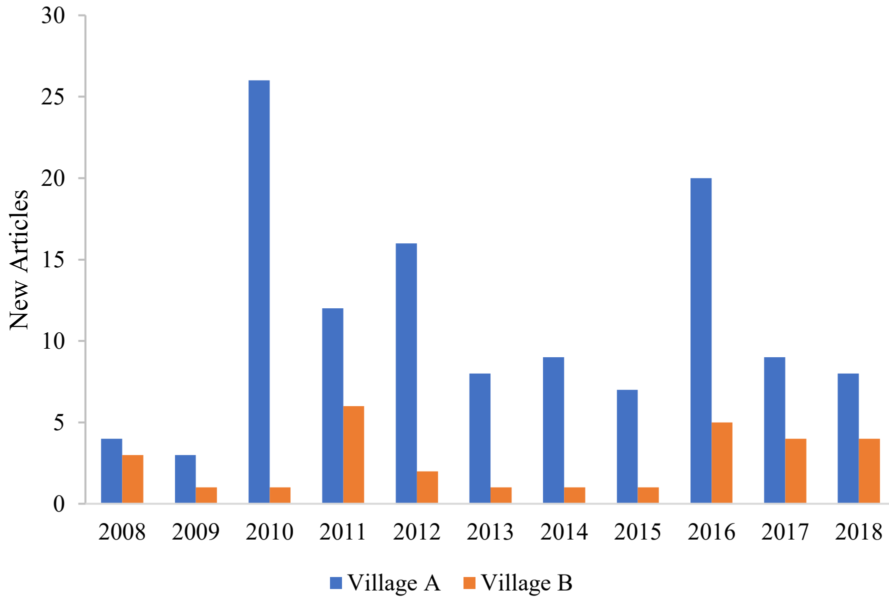


Figure 2.3. Media Coverage (2006-2018)

2.4.1 *Pre-Flood Trends*

To select the counterfactual, Village A was individually compared against a set composed of 21 plausible control villages based on i) the number of statistically significant baseline differences on sociodemographic attributes, and ii) the squared sum corresponding to the magnitude of the variables proposed in i).[29] After performing individual comparisons, Village B was selected as the one that most closely resembles baseline conditions in Village A. Employing data from the 2005 Census, as well as retrospective information collected on the study area corresponding to 2008, Table 5 shows that these municipalities exhibit pre-flood similarities on crucial socioeconomic indicators, such as water and electricity access, schooling, workforce, asset holdings, and dwelling characteristics.

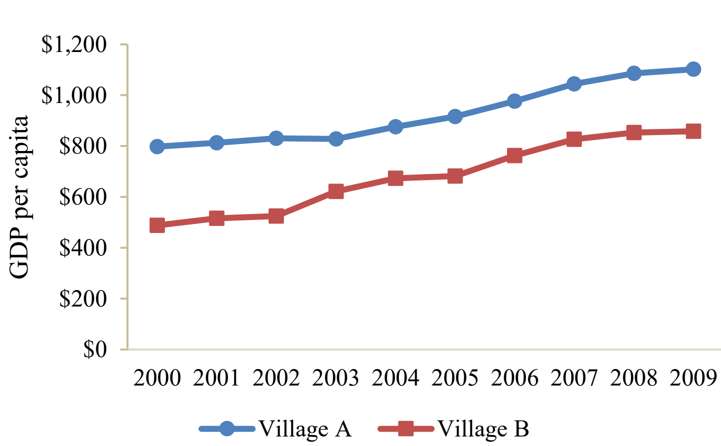
[29] Pre-flood comparisons between Village A and each of the feasible control groups are presented in Appendix A1. The proposition of feasible counterfactuals closely follows the classification of productive regions of the Colombian Caribbean Coast presented on Viloria de la Hoz (2006).

Table 2.3. Pre-flood Checks (2005-2008) —Village A and B

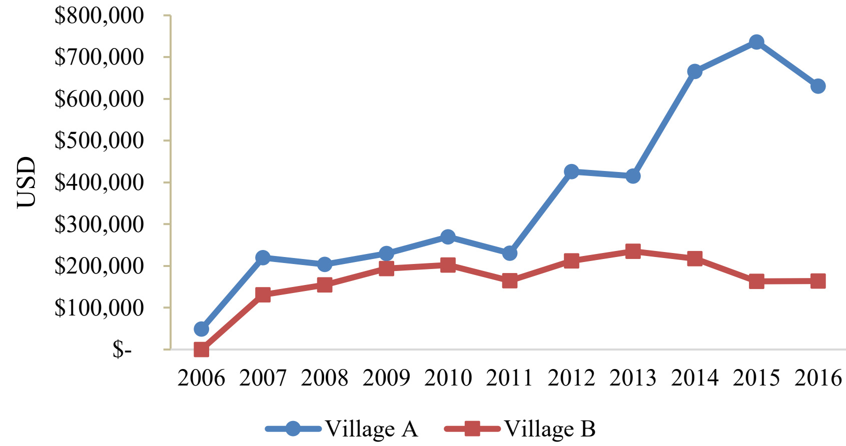
	2005				2008			
	Village A	**Village B**	**Diff.**		**Village A**	**Village B**	**Diff.**	
Water access	88.76%	93.52%	-0.05	***	87.20%	94.00%	-0.07	***
Electricity	97.75%	98.05%	0.00		96.60%	98.40%	-0.02	**
Sewage	30.29%	0.51%	0.30	***	50.50%	5.90%	0.45	***
Education level: none/primary	42.17%	44.29%	-0.02	**	49.30%	49.20%	0.00	
Workforce	72.53%	74.54%	-0.02	***	72.10%	72.60%	-0.01	
Refrigerator	47.58%	40.54%	0.07	***	65.60%	69.30%	-0.04	
Washer	20.45%	15.02%	0.05	***	48.10%	51.90%	-0.04	
Garbage collection	25.93%	52.72%	-0.27	***	32.10%	38.40%	-0.06	***
Material of walls: block, brick	88.10%	90.75%	-0.03	*	90.20%	89.90%	0.00	
Material of floors: cement, gravel	53.31%	77.18%	-0.24	***	53.10%	63.90%	-0.11	***
Dwelling tenure: own	74.31%	77.98%	-0.04	*	50.40%	60.90%	-0.10	***
Number of people in dwelling	5.24	4.94	0.30	***	5.96	5.53	0.43	

Source: 2005 Census and Ola Invernal 2018.

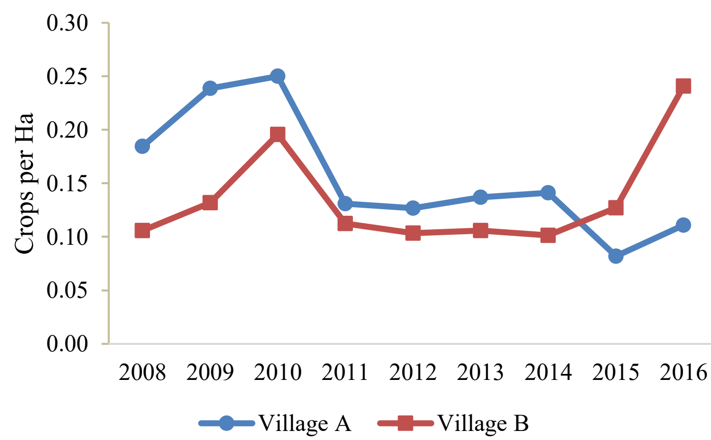
It is assumed that villages A and B exhibit parallel trends before the disaster, such that Village B represents an accurate counterfactual for Village A in the absence of the treatment. The comparisons presented on Table 3, however, are limited on their ability to depict these trends, since socioeconomic data for this region is not collected on a regular basis. To provide context regarding pre-flood trends, Figure 2.4 shows yearly administrative information that tracks baseline behavior of some relevant socioeconomic indicators, such as per capita GDP, monetary transfers, and agricultural production.



(a) GDP per capita (1999-2009) —USD



(b) Monetary transfers from welfare programs (2007-2016) — USD



(c) Agricultural production (2008-2016) —area sown per hectare

Figure 2.4. Parallel Trends (2000-2016)

In regard to per capita GDP, behavior exhibits clear baseline parallel trends, with Village A displaying higher pre-flood income, as shown in Panel (a) of Figure 2.4. Nevertheless, data corresponding to the intervention period is not available. Therefore, I turn to monetary transfers associated with government welfare programs, Panel (b), which exhibit analogous values leading up to the catastrophe, upon which there is a dramatic divergence on social welfare benefits, which include traditional conditional cash transfer programs, entrepreneurial and food security grants, and wage payments as compensation for employment loss during the emergency. These transfers constituted an important addition to household revenue. For instance, on 2015 payments on Village A averaged $65 per household, representing 46% of total income reported for 2008. Finally, I analyze total agricultural production. According to the information displayed in Panel (c) both groups exhibited a positive agricultural growth trajectory before the flood, with Village A engaging in crop sowing at greater volume than Village B. In 2011, however, both villages experienced a dramatic plunge in overall agrarian production, which is followed by a stagnation period. Nonetheless, when the intervention begins to monetize Village A's economy in late 2013 and early 2014, there was a considerable reduction in crop sowing during the following years, indicative of farm exit. This response contrasts sharply with household behavior in Village B, where farm production continued to recover, surpassing pre-flood sowing levels in 2016.

2.4.2 *Outcomes*

Behavior of outcome variables also reflects baseline similarities between villages. On Table 2.4, it can be seen that, leading up to the intervention, the composition of businesses was relatively equivalent, with farm enterprises accounting for a considerable share of the economic base. For Village A, the proportion of farm businesses decreased from 75 to 60% relative to that of nonfarm enterprises, displaying decline on farming activities prior to intervention. Village B presented a higher proportion of farm enterprises during the pre-intervention period —1.3 times greater—, yet also exhibited decline in farming.

Previous trends correspond to village-level behavior. However, an analysis among farmers reveals equivalent patterns regarding entrepreneurial composition. Among this group —and in both villages— the proportion of farm businesses was considerably higher before the flood; 3.46 and 4.5 times the size the nonfarm sector, respectively. Leading up to the intervention, we also observe a gradual decline on the participation of farm enterprises. Nonetheless, it is noteworthy that, among this group, the contribution of farmers toward the overall entrepreneurial base is the same on both villages.

The overall population, as well as farmers in particular, exhibit pre-treatment trends indicative of structural transformation, a process that has characterized Colombia's agricultural sector during the past decades, where, according to the World Bank estimates based on the United Nations Population Division's World Urbanization Prospects for 2018[30], the rural population decreased, from 53% to 19% of the overall population between 1960-2017. However, as will be shown on Section 2.10, after the intervention took place, the process was significantly greater on Village A relative to Village B. Another important finding consists on the mild effect of the 2010 flood on overall economic activity. According to the information shown on Table 2.4, composition of businesses, as well as overall entrepreneurial base, did not change in either municipality after the 2010 disaster which triggered the program on the "treatment" village, which bolsters the case relating farm exit to the demand shock generated by the intervention.

In fact, baseline behavior suggests that farming was experiencing structural transformation at a higher rate on the control village, which makes the subsequent effect on the "treatment" village

[30] https://data.worldbank.org/indicator/SP.RUR.TOTL.ZS

more meaningful. Furthermore, if the farming sector was comparatively larger on the “control”, greater farm exit on Village A relative to Village B would bolster findings, as the village without intervention had a relatively larger base from which farmers could switch following an aggregate demand shock.

Table 2.4. Enterprises —Farm and Nonfarm (2005-2013)

Year	Village A						Village B					
	All			Farmers			All			Farmers		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
2005	161	214	0.75	158	46	3.46	81	63	1.29	81	18	4.50
2006	182	238	0.76	179	49	3.64	81	69	1.17	81	18	4.50
2007	196	263	0.75	193	53	3.67	87	78	1.12	87	24	3.63
2008	224	326	0.69	221	56	3.94	96	96	1.00	96	24	4.00
2009	235	350	0.67	231	70	3.30	102	102	1.00	102	24	4.25
2010	217	340	0.64	214	63	3.39	90	108	0.83	90	27	3.33
2011	242	410	0.59	214	70	3.05	96	114	0.84	93	30	3.10
2012	259	441	0.59	214	74	2.90	102	126	0.81	93	30	3.10
2013	280	487	0.58	217	77	2.82	111	129	0.86	96	30	3.20

Columns (1), (4), (7) and (10) indicate farm enterprises

Columns (2), (5), (8) and (11) indicate nonfarm enterprises

Columns (3), (6), (9) and (12) indicate ratio of farm to nonfarm enterprises on Villages A and B

2.5 THREATS TO IDENTIFICATION

2.5.1 *Attrition*

It is not unusual for families to relocate elsewhere after a calamity. For example, 47% of pre-flood residents did not return to New Orleans one year after hurricane Katrina destroyed the city in 2005 (Sastry & Gregory, 2015). Paxson’s (2008) calculations estimate return rates at 66% in 2007. Another study, which tracked victims of the 1988 Armenia earthquake up to two years after the event, established that 25% of the population permanently moved to another country (Najarian et al., 2017). Considering the vast incidence of the disaster, it is possible that families initially residing on the study area responded by relocating. The problem with this consists in that, if there were systematic differences between those who decided to leave and those who stayed, an overwhelming displacement results in an endogenous sample that generates attrition bias.

An aggregate shock like the one experienced on the area of interest implied that the whole population was displaced, forced to temporarily migrate due to the tragedy. Consequently, migration decisions in this context pertain to the return choice made by households once conditions were safe, an aspect that is influenced by socioeconomic characteristics (Hunter, 2005). In the case of New Orleans, empirical evidence suggests sharp differences between those who returned and those who decided not to (Groen & Polivka, 2010). Hence, it is perfectly reasonable for attrition bias to arise in his particular context, with households exhibiting advantageous socioeconomic attributes separating themselves from the rest of the population. The central question is then: are attrition rates sufficiently low as to undermine any possible bias?

The sampling framework approximated attrition rates through two separate measures of migration: the first inquires for migration within households, directly asking families for migration history of all members, relating reasons and year of migration. The second asks households if they know of any other families who abandoned the village between 2008-2018, also relating reasons and year of relocation. In both cases, families are able to indicate if relocation happened as a result of a natural disaster. According to the information collected, families residing on the study area prior to the shock did not respond by leaving their villages. As mentioned earlier, the entire population was forced to evacuate during the emergency (Sanchez-Jabba, 2011).

Dumville et al. (2006), state that attrition rates of 5% or lower are not a matter for concern in studies involving randomized controlled trials. In fact, some researchers go as far as to consider acceptable studies with retention rates at 70% or above (Lyles et al., 2007; Valentine & McHugh, 2007). Taking into account that the attrition rate on the study area was well below this threshold, I conclude that the effective attrition on this study does not represent a threat to identification. Another factor that adds relief to this concern consists on the fact the all observational units lost from the sample due to the calamity did not have the treatment condition, which reduces the likelihood result significance loss due to attrition bias (Akl et al., 2012).

According to the information collected on the study area, the attrition rate is well below 5%. On Village A, 9% of households reported a member who does not reside on the area (13% on Village B). However, among former residents those who do not currently reside on Village A, 5% stated a natural disaster as the reason for migrating (1% on Village B). In fact, if we calculate the attrition rate as the ratio between the total number of people who reside elsewhere due to a natural disaster occurred between 2008-2018 and the total number of inhabitants on each village — obtained from the summation across dwellings of the total number of people who live on each unit— we find attrition rates of less than 1% on both villages, which limits any potential concerns arising from an endogenous sample after the calamity.

2.5.2 *Endogenous Assignment*

Although families living on Village B did not have access to the intervention, work at construction sites, reception of migrants, and access to PxMF within Village A are plausibly endogenous. For example, Lanjouw & Murgai (2009) state that nonfarm employment is closely associated with education and wealth. Similarly, conditional grants were targeted based age, asset holdings, dwelling characteristics, and participation on a specific government welfare program designed for families living in extreme poverty. Hence, it is possible that families that benefitted from the treatment display specific baseline attributes that separate them from the rest of the population within the same village, biasing farm exit estimates.

Table 2.5 examines possible endogeneity in treatment assignment through simple regressions where the treatment conditions discussed in Section 2.4 represent the dependent variable, and observable baseline (2008-2010) sociodemographic attributes constitute the explanatory variables. This approach assumes that there is no self-selection on unobservable factors, which constitutes a reasonable conjecture considering that work at construction sites was predominantly carried out by unskilled labor, such that unobserved characteristics, like innate ability, might have limited influence on the likelihood of finding employment or on wages. Additionally, PxMF was allocated by the central government; not the local municipality administration, limiting the incidence of unobservable factors, such as political participation or local networks.

Table 2.5. Determinants of Access to PXMF

	All	**Farmers**
Enterprise	-0.246 ***	-0.229 ***
	0.013	0.047
Income	-0.014	-0.078
	0.019	0.100
Own House	-0.001	-0.012
	0.013	0.046
Farm/Land	-0.116 ***	-0.168 ***
	0.013	0.034
Refrigerator	-0.012	0.040
	0.014	0.056
Washer	0.034 *	0.169 **
	0.018	0.079
Schooling	-0.007 **	-0.003
	0.003	0.013
N	**3868**	**1020**

Results from this exercise indicate that access to the treatment was influenced by baseline socioeconomic attributes, mainly prior operation of enterprises and the possession of farm/land, thus potentially tarnishing the identification strategy. However, it must be emphasized that comparisons between the treatment control groups in this context correspond to assessments between farmers on both villages. Since farmers are identified on the basis of their enterprise —if they operate one— and or occupation, we would expect to observe salient preflood similarities among these groups, which would not change at the time the flood occurs. This is in fact the case when analyzing baseline differences between farmers in each municipality (see Table 2.6), where we observe correspondence on the main observable socioeconomic attributes. As a result, and considering that both villages were severely affected by the shock, subsequent divergences between these groups would be attributed to the treatment.

Table 2.6. Baseline Checks (2008-2010)

	All			**Farmers**		
	Farmers	Nonfarmers	Diff.	Village A	Village B	Diff.
Income 2008 (USD)	162	285	123 ***	162	161	-2
Enterprise: Yes/No	0.490	0.568	0.078 *	0.490	0.400	-0.090
Land: Yes/No	0.168	0.174	0.006	0.168	0.218	0.050
Washer	0.176	0.174	-0.001	0.176	0.236	0.061
Schooling	9.297	10.070	0.773 ***	9.297	9.324	0.027
N	**893**	**1218**		**900**	**396**	

2.5.3 *Risk Attitudes*

Changes in risk attitudes are not really a matter for concern on the analysis unless there was some underlying reason for which this variable displayed diverging behavior among villages before the shock. I consider this to be unlikely, as villages A and B are located within close proximity of each other, such that climatological shocks would be equally experienced by both villages and, as discussed in section 2.4, they did display comparable pre-flood socioeconomic and agricultural indicators. Furthermore, and as a first measure aimed at mitigating confounding effects, if any, counterfactual selection was restricted to flooded municipalities, ensuring that any behavioral responses arising from the catastrophe would materialize on both villages with similar intensity, thus preserving relative differences.

Nonetheless, it is still possible that the negative shock generated changes in risk attitudes that explain farmers' decision to exit agriculture, diluting the results. In this case, farm exit would not occur as a consequence of diverging returns, but rather as a response to a push factor that makes farming less desirable. To address this identification concern, I will exploit heterogeneity on flood damage within villages, examining if farm-related losses influence exit by matching differential damage to household behavior on the proposed farm variables. To capture this effect, risk aversion is proxied through lottery choices and insurance take-up as a function of flood exposure following the specification:

$$\mathrm{Y}_{ih} = \alpha_h + \alpha_v + \sigma_1 \mathrm{Severity}_h + \theta \mathbf{X}_{ih,t} + \eta_{ih} \tag{2.3}$$

Where Y_{ih} is an outcome that reflects risk-aversion levels through two possible variables: L_{ih}^{d}, which corresponds to a categorical variable that ranks lottery choices d(=1 to 6) by individual *i* in household *h*; and I_{ih}, an indicator of insurance take-up after the calamity. $\mathrm{Severity}_h$ corresponds to a risk index composed from several variables that cause the damage on farm-related losses, including animals, crops, farm equipment, and plots that ca no longer be used for farming due to alterations in soil quality of prolonged flooding. $\mathbf{X}_{ih}$ is a matrix of covariates that reflect socioeconomic characteristics. Finally, the specification includes village (α_v) and household (α_h) fixed effects and an error term (η_{ih}), which is clustered at the village level.

2.6 DATA

Since the existing institutional information regarding the communities residing on the study area lacks data that extends beyond the basic sociodemographic attributes, this investigation involved the application of a retrospective survey that enables the analysis of households' production decisions between 2008-2018, which facilitates identification of the channels that affect farm exit[31]. The sample size comprises 1,739 households, of which 57.6% reside on the "treatment" village, the one that benefitted from widespread reconstruction and PxMF, "Village A" hereafter[32]. The data, collected between March-April of 2018, contains information related to households' consumption, wealth, and migration, as well as key variables that capture farm exit, factor reallocations, and shifts in aggregate demand.

2.6.1 *Farm Exit*

Farm exit is interpreted as an initially agrarian household engaging in an economic activity not related to agriculture, livestock, fishing or hunting after 2014, when the intervention was implemented. To capture this transition, the questionnaire includes several modules that trace enterprises, time use, employment, production site, investments, and migration. Any of the following changes in household behavior signal structural transformation: i) opening a non-farm

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[31] Due to budgetary restrictions, only two villages were included in data collection, which limits treatment effect analysis. For this reason, we refer to the intervention as "treatment". However, results are not expected to vary if additional villages were incorporated into the sample. In order to corroborate the external validity of our findings, Section 2.9 will present results from robustness checks employing 2018 Census data (once it becomes available), which allows the inclusion of several more villages, on both the treated and control groups.

[32] Power calculations performed employing 2005 Census data suggest that the sample size required for a minimum detectable effect over an index composed of socioeconomic variables is of 616, equally distributed between treatment and control groups, with a predictive power of 80%, $\alpha = 0.05$, and adjusted for intra-class correlation. The socioeconomic index was composed by the following variables: floor material, bathroom access, garbage disposal method, cooking source; coverage of sewerage, electricity, natural gas, and potable water; tenure of refrigerator, washer, television; and number of rooms in the dwelling. Variables were selected based on the correlation fit among them, following a selection criterion that maximized the fit among all possible combinations of variables.

enterprise[33]; ii) reduction in time allocated to farm labor; iii) shifts in production site — from a farm/plot toward a dwelling or local market; iv) engagement in nonfarm employment; v) reduction in the use of farm inputs; and vi) migration to nearby urban centers.

Enterprises and occupations are separated into farm and nonfarm. A business is classified as a farm enterprise if it sells crops or meat, including livestock breeding, produced at a farm or plot. An occupation is related to farming if the person defines their craft as farmer, peasant, laborer, rancher, cultivator, hunter, fisherman, and they carry on their job a farm or plot. The type of business—assessed through the product it sells—, as well as the year in which said business was opened, establishing if the event happened during years in which the intervention was implemented is indicated on the production module. To construct employment history, which determines household members' engagement in farm and nonfarm occupations, the questionnaire tracks baseline farmers' employment between 2008-2018, determining years where members switched between to the nonfarm sector, either by engaging in nonfarm employment or by opening a nonfarm enterprise. The change in occupation is narrowed by asking individuals about their baseline and final occupation, as well as the places in which they have worked between 2008-2018. Farmers extracted from the overall sample consist on households that were exclusively dedicated to farming before the shock. This includes those who either operated a farm enterprise or were engaged in farm employment, in accordance with the criterion outlined above.

2.6.2 *Aggregate Demand*

Shifts in aggregate demand for non-tradables and nonfarm labor are approximated through the channels proposed in Section 2.3. Rural income is measured over household reports on total

[33] The production module constructed a history of family owned businesses, tracking the type of products sold and the year the enterprise was opened.

earnings, determining with a simple regression the extent to which increments in this variable depend on the reception of PxMF. Access to PxMF was calculated using two separate filters. The first one consists on directly asking for participation on this program; the second enquires, in general terms and without explicitly alluding to the program, for reception of government aid targeted at business openings, specifically querying for the reception of technical and monetary assistance. Demand for nonfarm goods is obtained from the consumption module, where households report their monthly expenditures on goods such as gasoline, cigarettes, liquor, magazines, lottery, beauty services, internet and cellphone calls. Migration is tracked through reports corresponding to arrival of household members during the past 5 years.

To determine how the new resident population impacted demand for non-tradables, I look at the relationship between immigrant concentration, their consumption of nonfarm goods, and nonfarm business sales. The production module also tabs inputs used by firms. In areas with a higher proportion of businesses opened within the last five years, particularly among those who benefited from PxMF, we should expect to see a greater use of nonfarm inputs. Employing spatial location and types of products sold, I identify enterprises that are most likely to provide construction supporting services. Matching business location to construction sites, it follows that proximity is proportional to the demand shock experienced by households, who have a propensity to respond by opening a family business. The analysis is complemented with general information related to buyers' profile, which describes the links between firms and the markets they serve.

2.6.3 *Reallocations*

In order to typify pull factors that motivate reallocations, I calculate the average return across households' economic activities. For enterprises, either farm or non-farm, this measure is constructed by dividing reported profits over labor hours; the same procedure is followed for nonfarm employment, where income corresponds to the monthly wage. To further characterize this process, I present information related to reasons for business openings/closings. PxMF arguably relaxed preexisting credit constraints related to investments required for the aperture of new firms. To illustrate possible entry barriers, enumerators asked about baseline access to credit markets, wealth, and the reasons for which households had not switched sectors even when they reported a wish to do so.

2.6.4 *Risk Aversion*

In case evidence suggests behavioral responses arising from the exposure to the disaster, comparisons will be limited to clusters that report the same level of farm losses, such that any differences in behavior could not be attributed to changes in risk aversion. Risk attitudes will be instrumented through lottery choices and insurance take-up. In particular, lottery choice will be measured through answers on the behavioral game module, where individuals can choose among 6 options that posit differential risk levels. Insurance acquisition as a response to the flood is tracked by asking recipients about insurance take-up history, including insurance type. The severity index is a composite statistic based on self-reports of flood related losses (dwelling, enterprises, plot/land, machinery, appliances, furniture, crops, animals), impact of the disaster on household economic stability (based on a 0-10 scale), forced displacement (to another town or shelter, including duration), and employment/income loss (including spell). Socioeconomic characteristics include gender, age, marital status, educational attainment (years of schooling), relationship to household head, monthly income (wages, rental income, enterprise sales, government transfers), and wealth (dwelling characteristics, including wall/floor material, access to public services, sanitary conditions).

2.7 EMPIRICAL STRATEGY

Farm exit will be tested in a two-stage process employing a difference-in-difference approach that compares outcomes across households that exhibit equivalent pre-determined socioeconomic characteristics, but who subsequently diverge due to the application of the treatment. Households that did not reside on the area of study before the flood are be excluded from the analysis, as changes on their behavior could not be categorically attributed to the intervention. Furthermore, to narrow down the effect, the analysis focuses on households that were farming before the shock (2008-2010). The empirical analysis examines factors that plausibly influence farm exit through the following specification:

$$O_{iht} = \gamma_0 + \beta_1 V_h + \beta_2 PxMF_{ht} + \beta_3 L_{ih} + \phi R_{jht} + \alpha X_{iht} + \varepsilon_{iht} \tag{2.4}$$

Where, O_{iht} is the farm outcome of interest for individual i in household h at time t, consisting of any of the exit variables listed in section 2.4. As mentioned earlier, the intervention is instrumented through residence location with respect to the dike that failed in late 2010, V_{ih}. $PxMF_h$ is the indicator for entrepreneurial grants; L_{ih} is the scored obtained in lottery game; R_{jht} is a measure of revenues from household enterprises at time *t*. Errors, ε_{iht}, are clustered at the household level.

2.7.1 *Wealth Changes*

In order to determine how transition out of agriculture benefited household who chose to exit, I test changes on wealth and income attributable to farm exit employing the following specification:

$$Y_{iht} = \gamma_0 + \gamma_h + \beta_1 S_{ih} + \beta_2 I[T = 2] + \beta_3 I[T = 2] * S_{ih} + \boldsymbol{\mu X}_{iht} + \varepsilon_{iht} \tag{2.5}$$

Where Y_{iht} is the dependent variable that captures changes on the following indicators: i) income, and ii) a socioeconomic index constructed from household asset holdings and dwelling characteristics corresponding to the period 2014-2018. $\boldsymbol{X}_{iht}$ corresponds to a vector of sociodemographic attributes that determine wealth, including gender, age, and baseline (2008-2013) schooling, dwelling characteristics, and access to welfare programs. γ_h is a household fixed-effect that controls for household-specific unobservable preferences.

2.7.2 *Spatial Analysis*

Since aggregate demand shocks are influenced by factors external to the household, such as concentration of migrants, enterprises that demand nonfarm inputs, and proximity to markets, I perform a spatial analysis at the neighborhood level. Employing GPS coordinates, for each neighborhood in the village, I calculate the percentage of household who received a migrant, who benefited from conditional transfers, and the presence of markets. Next, I analyze the relationship between these variables and nonfarm enterprises' sales.

2.8 FARM EXIT

In order to examine farm exit on Village A relative to Village B, I analyze the behavior of nonfarm enterprises and employment among households that were exclusively dedicated farming in 2008. The first indicator —nonfarm employment—follows baseline farmers, tracking changes in occupation toward the nonfarm sector. The second measure shows the stock of farm and nonfarm enterprises among baseline farmers, accounting for business openings and closures. Table 2.7 summarizes the findings. The first result that emerges from the information displayed on the table is that both villages experienced decline in farming activities throughout the last decade. This should not be surprising, as structural transformation of agriculture generally accompanies economic development (Johnston, 1970). In a broad sense, this is a process that has characterized the agricultural sector in Colombia during the past decades, where the rural population decreased, from 53% to 19% of the overall population between 1960-2017. According to Junguito et al. (2014), the contribution of agriculture to GDP dropped from 25% in 1965 to 6% in 2012.

The relevant difference, however, relies on the rate of transformation, which was considerably higher in Village A, a fact that can be attributed to shift in aggregate demand for non-tradables, generated by widespread reconstruction and PxMF. According to the information displayed on the table, 917 and 330 households could be classified as farmers in 2008 on Villages A and B, respectively. These farmers accounted for a total of 172 and 81 farm enterprises on each village. This number remained relatively stable until 2013, when the intervention was implemented on the "treatment" village. At that point, farmers began to engage in nonfarm activities. For example, the proportion of nonfarm employment to farm employment in 2018 was 4.3 times larger, a ratio that was considerably higher compared to Village B, where it was of 1.7 [columns (5) and (6)]. Additionally, farmers on Village A added a total of 140 nonfarm enterprises, most of which were opened between 2015-2018, during the intervention phase. Among farmers, 14.5% opened a nonfarm enterprise and 74.8% engaged in nonfarm employment. By the end of the shock, more than half of the enterprises owned by baseline farmers in 2008 were nonfarm businesses, a ratio that ascended to a quarter on Village B [columns (11) and (12)].

Table 2.7. Employment and Enterprises (2008-2018) —Villages A and B

Year	Employment								Enterprises					
	Village A		Village B		A	B	A	B	Village A		Village B		A	B
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)
2008	917	0	330	0	0.00	0.00	0.42	0.30	172	0	81	0	0.00	0.00
2009	914	4	327	3	0.00	0.01	0.42	0.30	183	11	87	0	0.06	0.00
2010	896	14	321	9	0.02	0.03	0.41	0.29	201	11	90	0	0.05	0.00
2011	858	32	309	18	0.04	0.06	0.39	0.28	204	14	90	3	0.07	0.03
2012	798	84	288	27	0.11	0.09	0.36	0.26	208	18	90	3	0.08	0.03
2013	721	158	276	36	0.22	0.13	0.33	0.25	211	21	90	6	0.10	0.07
2014	676	200	276	36	0.30	0.13	0.31	0.25	215	32	90	6	0.15	0.07
2015	623	252	261	48	0.40	0.18	0.28	0.24	225	42	93	9	0.19	0.10
2016	532	343	231	75	0.64	0.32	0.24	0.21	246	63	96	12	0.26	0.13
2017	357	518	192	114	1.45	0.59	0.16	0.18	253	116	96	21	0.46	0.22
2018	161	686	102	174	4.26	1.71	0.07	0.09	263	140	96	24	0.53	0.25

This data corresponds to baseline farmers

Columns (1) and (3) indicate farm employment on Villages A and B

Columns (2) and (4) indicate nonfarm employment on Villages A and B

Columns (5) and (6) indicate ratio of nonfarm to farm employment

Columns (7) and (8) indicate farmers' share of overall employment

Columns (9) - (14) do the same for enterprises

Not surprisingly, the contribution of farmers to the overall workforce declined consistently, most noticeably in Village A. Before the intervention took place, farmers accounted for 42% and 30% of the overall workforce on each village. Yet, by 2018 that proportion had been reduced to less than 7 and 9% on each village, as shown by Columns (7) and (8) of Table 2.7. In particular, farmers' share of overall employment declined on Village A by 35 p.p., while the same variable dropped 21 p.p. on the other village. In a similar way, the percentage of farm enterprises operated by baseline farmers diminished by 33 percentage points, from 100% in 2008 to 67% in 2018; on Village B, this reduction was of 17 p.p. The rest of the variables proposed in Section 2.4 reinforce the findings expressed through enterprises and nonfarm employment. Production site gradually shifted from farms and plots toward dwellings and village markets; farm labor was also reduced; and several farmers devoted resources toward acquisition of land and machinery.

2.8.1 *Reasons*

The production module inquired for reasons related to households' decision to transition toward the nonfarm sector. Among respondents who opened an enterprise during the last five years, 42% declared volatility in income as the main driver, evidence suggestive of responses aimed at mitigating and or reducing risk. From the responses, it is clear that several households would like to switch sectors, with 82.5% of baseline farmers —who continued to operate an enterprise during the study period— stating they would like to open another type of business. Among respondents, only 2% declared an agricultural business as their desired activity if they had the chance to switch without any costs, while the rest expressed their desire to open a nonfarm enterprise (restaurants, hotels, and transportation services among the most popular choices). When asked about reasons that prevented them from doing so, 86% reported credit constraints (5% reported deficient access to inputs) as the main factor restraining them from undertaking their desired endeavor.

2.9 RESULTS

In order to characterize channels that influence farm exit, the empirical analysis estimates how each of the treatment components proposed in Section 2.3 affects farm outcomes. The first set of results consists of estimations where the dependent variable indicates whether a household member engaged in nonfarm employment. The second presents a survival analysis for enterprises operated during the study period. For both analyses, I find that nonfarm employment and the shutdown of enterprises is determined by two main factors: profitability (pull factor) and risk aversion (push factor).

2.9.1 *Nonfarm Employment*

Table 2.8 depicts results for the first specification, which detects drivers of nonfarm employment. It can be observed that higher revenues, the availability of nonfarm work alternatives, and higher levels of risk aversion are positively associated with the likelihood of belonging to the nonfarm sector at the end of the study period. The estimation controls for the incidence of shocks during the past decade —illness or disease which prevented a household member from working— and basic socioeconomics attributes, particularly asset holdings, which might affect households' decision to open enterprises or reassess the value of their outside option. Preexisting credit constrains, which accounted for 86% of responses related to barriers to switching sectors —according to responses stated in Section 2.8— were measured through access to PxMF and credit payments.

Initially, I present results where the measure that captures farm exit corresponds to the village of residence, estimation (1). As expected, living on Village A increases the likelihood of nonfarm employment, upholding the notion that a shift in the aggregate demand for non-tradables precipitates agriculture exit. However, I proceed to a decomposition of this effect by analyzing the incidence of push and pull factors. Estimation (2) presents results related to push factors: the effect of relatively higher levels of risk aversion —measured through the scores on the lottery choice module described in Section 2.6. (3) adopts a market-based approach, illustrating the impact of pull factors, such as a higher income associated with nonfarm enterprises, as well as the availability of nonfarm alternatives, expressed through work at construction sites.

Table 2.8. Farm Exit Determinants (Baseline Farmers)

(Nonfarm Employment=1)	**(1)**		**(2)**		**(3)**		**(4)**	
Village A	0.181	***						
	0.033							
Disaster			0.390	**			0.036	
			0.043				0.046	
Risk-Aversion Score			0.002				0.005	**
			0.001				0.001	
Enterprise Revenue					0.002	***	0.002	***
					0.001		0.001	
Construction					0.476	***	0.456	***
					0.022		0.032	
PxMF					0.058	**	0.076	***
					0.020		0.017	
Controls	Yes		Yes		Yes		Yes	
N	1132		1132		1132		1132	

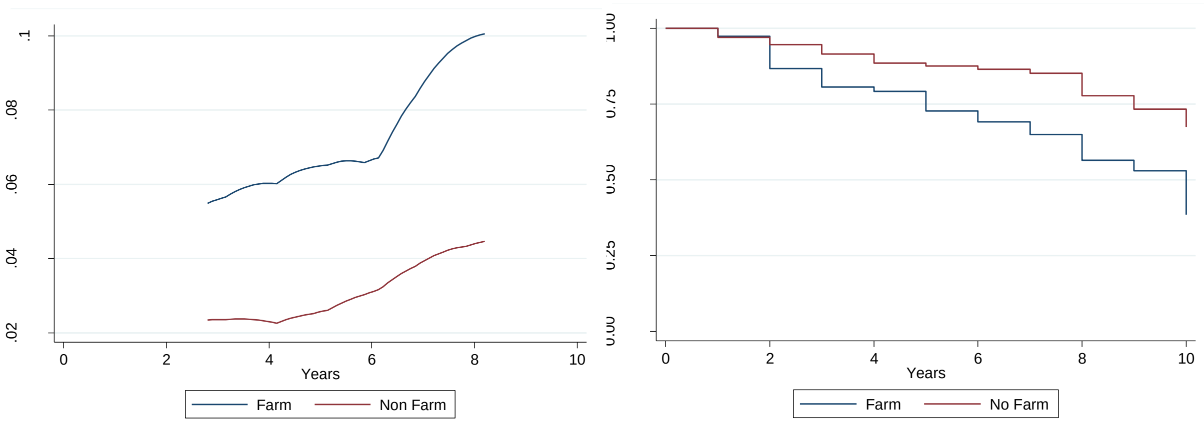
Overall, results indicate that both types of factors have a positive effect on the probability of nonfarm employment. With respect to push factors, we observe that a higher score on the lottery game —associated with higher levels of risk aversion[34]— increases take-up of nonfarm employment, which can be interpreted as behavior aimed at curbing income volatility of farming endeavors, where income might be subject to climatological shocks. The latter is bolstered by the fact that the incidence of natural disasters constitutes the most important factor influencing the likelihood of transitioning toward the nonfarm sector.

Access to PxMF is rationalized as an element that relaxes preexisting credit constraints expressed by respondents when asked about reasons that prevented them from switching toward their desired economic activity. Results suggest that this program boosts nonfarm employment, highlighting the importance of curtailing liquidity constraints in overcoming baseline suboptimal outcomes.

2.9.2 *Survival Analysis*

Next, I proceed to examine factors that influence households' decision to close enterprises, establishing the emergence of differential exit patterns according to the type of business. For this purpose, I pool all enterprises, regardless of sector, and follow them through the study period, starting in 2008 through 2018. Panel (a) of Figure 2.5 shows the hazard estimates for farm and nonfarm businesses, marked by an increasing trend in firm exit throughout the period. The salient feature, however, lies on the surging rate at which farm enterprises exit the market —relative to the nonfarm sector— particularly once the intervention was implemented on Village A (year 6).

[34] The lottery offered households an insurance choice that guaranteed a minimum score for those who wanted to evenly spread risk across alternatives. Besides this choice, the lottery incrementally offered a higher return under a waning likelihood, such that higher scores reflect higher levels of risk aversion.



Panel (a) – Hazard Estimates Panel (b) – Survival Estimates

Figure 2.5. Enterprise Shutdown (2008-2018) —Failure Event: Enterprise Shutdown

It is argued that the shutdown of farm businesses reflects a broader recomposition in production that arises as a result of the factors discussed in the previous subsection. Panel (b) shows that, by the end of the period, more than 50% of baseline farm enterprises had been closed, while three quarters of nonfarm businesses were still active. This reassignment is reflected on the behavior exhibited by the stock of enterprises (presented in Section 2.4), characterized by an upswing in nonfarm businesses between 2016-2018, contrasted with a stagnant behavior displayed by the other sector.

Aspects that determine survival are consistent with those that impact employment. Essentially, nonfarm enterprises are less likely to exit the market. The village of residence plays a prominent role on survival of firms, bolstering the argument stating that the intervention on Village A stimulated aggregate demand for non-tradables. Hazard semi-parametric estimations indicate that, among baseline farmers who transitioned toward the nonfarm sector by the end of the intervention, credit aspects constituted the main drivers of the switch. In particular, PxMF significantly increased the rate at which nonfarm enterprises were closed. However, the effect was opposite among baseline farmers who remained on the agricultural sector. These results are indicative of differential returns across alternatives, with nonfarm businesses yielding higher returns, an aspect that limits shutdowns in the nonfarm sector.

Table 2.9. Determinants of Firm Shutdown —Cox Proportional Hazard Regression

	Farmers		Nonfarmers	
Village A	-0.956	**	-0.861	
	0.482		0.560	
Disaster	0.219		0.390	
	1.055		0.484	
Risk-Aversion Score	0.012		0.016	
	0.016		0.011	
Enterprise Revenue	0.000		-0.014	**
	0.007		0.007	
PxMF	-0.949	*	0.552	*
	0.558		0.293	
Controls	YES		YES	

2.9.3 *Welfare*

Finally, I assess the potential welfare implications associated with the recomposition of households' production portfolio. Tracking production and employment over the study period and, following the second stage specification presented on the empirical section, I establish if farm exit had an effect on household income and employment. Results from this estimation corroborate that structural transformation represented a net welfare gain, and plausibly enabled families to overcome baseline suboptimal outcomes, as this event is positively correlated with income stemming from operating enterprises and the availability of nonfarm employment.

Table 2.10. Effect of Farm Exit on Income and Employment

Dependent Variable	**(1)**	**(2)**
Income	71.849 ***	
	21.734	
Employment		0.624 ***
		0.019
N	**1136**	**1143**

2.10 CONCLUSION

This chapter studies the effect of an aggregate demand shock on households' allocation of resources. It is argued that a government intervention, consisting of widespread construction and conditional grants, changed the value of outside options. In particular, low farm earnings —relative to the profitability of nonfarm enterprises— as well as changes in risk attitudes, ousted productive factors from agricultural activities toward the nonfarm sector. Overall trends indicate that the shock precipitated structural transformation, with baseline farmers engaging in nonfarm activities, either through work or enterprises, at a significantly higher rate relative to their counterparts on the village where no intervention took place.

Although existing empirical evidence suggests that, indeed structural transformation accompanies higher levels of economic development, it is also not clear whether the transition observed in this particular context —and plausibly in other low-income settings— actually represents a sustainable reconfiguration of the local economy. Specifically, whether this transition represented a transient change that faltered once the spur dwindled on the treatment village. Since the data gathered on the study area corresponds to information between one and two years after the program was implemented, it is difficult to determine if the new market economy —induced by the shock— is able to operate, at least partially, without government stimulation.

These considerations yield paramount implications for interventions in rural contexts, where extensive programs can potentially reshape local activities in response to changing returns across feasible production sets. In the case that the package constitutes a transient, yet powerful shock that wobbles economic foundations, this could ultimately result in a net loss, as agents react without anticipating the amplitude or duration of the event, or the subsequent reduction in revenue. Governments programs, especially in rural areas, should account for these potential unintended consequences. Future work on this research includes tractability of the variables that capture the sustainability of the transformation.

Chapter 3. RETURNS TO HOUSING PROVISION IN RURAL COLOMBIA

3.1 INTRODUCTION

Most of the debate regarding housing provision in developing countries has focused on the response to rapid, unplanned urbanization, a phenomenon that dramatically increases inadequate shelter. According to UN-Habitat (2011), slum population — informal, overcrowded settlements with deficient infrastructure — in Asia ascends to 504 million; in Africa, to 211 million; and in Latin America and the Caribbean, to 111 million. This represents as many as 32% of the world's urban population living under substandard housing (The Challenge of Slums, 2003), an aspect that negatively impacts health and subjective wellbeing (Cattaneo et al., 2009). Furthermore, by 2030 it is estimated that 40% of the world's population will require lodging, basic infrastructure, and services (UN-Habitat, 2011).

To address this pressing issue, countries implement projects that upgrade living conditions for deprived families, a critical aspect for economic development (Arku, 2006). For example, during the past two decades TECHO — an NGO that operates in Latin America — built almost 100,000 pre-fabricated units for impoverished families (Galiani et al., 2016). Likewise, between 1972 and 1981, 90 percent of the World Bank's shelter loans were channeled through slum upgrading developments (Arnott, 2008). In Thailand, the government —through subsidies and mortgage payments—created tenure security on among informal occupants by encouraging communities to negotiate long-term leases or purchases (Vratkapan & Perera, 2006). In India, randomized housing lotteries provide substantial subsidies that allow low-income communities to access shelter (Barnhardt et al., 2016). Shelter programs have also been implemented by governments in Indonesia (Some et al., 2009), China (Day & Cervero, 2010), and Brazil (Dasgupta

& Lall, 2009); some executed in rural areas of Malawi, Zimbabwe, Bangladesh, and Ecuador (UN-Habitat, 1995).

The existing empirical evidence categorically justifies the formulation of shelter initiatives. Improving housing conditions among poor households generates positive impacts on child health and subjective wellbeing (Galiani et al., 2016; Devoto et al., 2012; Cattaneo et al., 2009); consumption (Sodini et al., 2016); access to credit (Field & Torero, 2006); investments and educational outcomes (Galiani et al., 2010; Field 2005); health (Duflo et al., 2015) and relaxes time constraints (Devoto et al. 2012; Field, 2007)…all of which lead to welfare gains. However, the existing assessments are confined to urban contexts — where the vast majority of the interventions have taken place[35] — providing limited information about returns to shelter between rural families, whose state drastically differs from that of their urban counterparts. For example, home in urban areas generally entails access to complementarities that further improve life quality, mostly in the form of amenities (Glaerser & Gottlieb, 2006). In contrast, a rural family might view a dwelling not only as a residence, but also as the space where household production takes place (Nagler & Naude, 2014). Consequently, assuming that results obtained exclusively under urban settings can be extended toward rural contexts, especially when establishing parameters for rural housing provision, can cause severe inefficiencies.

Despite the latter, and to the best of our knowledge, an evaluation that formally examines this type of intervention has yet to be performed. As the United Nations Programme for Human Resettlements — UN-Habitat — argues on the 1995 report regarding rural housing improvements in developing countries: “Researchers have tended to neglect the problem of rural shelter, and

[35] There is a clear explanation as to why housing programs have concentrated on urban areas. Geographical dispersion — which limits economies of scale and substantially increases the marginal cost—, reduced political representation, diffused property rights, and lack of access to financial mechanisms are all factors that limit the provision of rural shelter (UN-Habitat, 1995).

there is at present little published material which policy-makers can consult for guidance." It appears that the situation has not changed since, reason for which this study presents the first estimates regarding returns to housing among rural households, focusing on an intervention implemented by the Colombian government in order to mitigate the impact of a calamity. Particularly, we leverage on a program that delivered homes to families whose dwelling was destroyed by a natural disaster, an approach usually adopted after the occurrence of a major catastrophe[36].

Besides estimating the returns to housing in a rural context, we believe this study advances the existing literature on the topic by examining shelter aspects that have substantial impacts on household welfare: infrastructure, tenure, and location. The first channel considers changes in dwelling attributes that translate into improved living conditions for the household —mainly sanitary—, measuring impact on health outcomes and analyzing how this change expands liquidity. The second channel examines home tenure, establishing how ownership motivates families to modify their behavior in terms of consumption and labor allocation. The third channel considers the environment in which the intervention takes place. Following an increasing literature related to neighborhood effects, the final aspects determines if household relocation to new neighborhoods enhanced or diminished the benefits associated with dwelling provision. While several of these dimensions have been analyzed separately by the literature, we believe this represents the first assessment to study these effects within the same context.

[36] Examples of these initiatives include the "Rural Housing Construction with Appropriate Technologies" Project in Ecuador after the 1987 earthquake (UN-Habitat, 1995); the "Road Home Program", which offered Louisiana residents grants to rebuild their houses or relocate elsewhere after hurricanes Katrina and Rita (Green & Olshansky, 2012); the reconstruction of houses in Indonesia after the 2004 tsunami (Steinberg, 2007); and the Gujarat Emergency Earthquake Reconstruction and the Marmara Earthquake Emergency Reconstruction projects following the 2001 Gujarat and 1999 Marmara earthquakes in India and Turkey, respectively (Ganapati & Anuradha, 2014). Nevertheless, there are no formal evaluations concerning the socioeconomic effects of these post-disaster housing interventions on beneficiary communities.

Existing evaluations regarding the effect of shelter improvements are hard to disentangle or contextualize, complicating the identification of the channels and their interpretation. In particular, the literature related to habitat programs has limited assessments to contexts where the intervention i) delivers simultaneous relief on many aspects that were lacking on living conditions before the intervention or ii) enhances a specific aspect of the unit. As a result, it is generally difficult to establish mechanisms that mediate changes in households' outcomes. For example, in the case of dwelling provision (Galiani et al., 2016), the resulting effect can be attributed to any of the upgraded characteristics in habitat. Although combined enhancements —such as sanitation and water access— have a positive impact on household health (Duflo et al., 2015; Checkley et al., 2008; Merchant et al., 2003), the individual effect corresponding to each aspect is hard to separate, limiting policy recommendations. As Wolf et al. (2014) put it: "The choice of a suitable approach that can differentiate health effects between different improvements in water and sanitation relative to the baseline is crucial for meaningful estimates. However, evidence from well-conducted intervention studies assessing exclusive use of adequate access and supply of safe water or universal use of effective sanitation is still very limited."

To the best of our knowledge, this is the only study to look at effects of housing provision; other interventions look at specific upgrades. In order to identify the returns to each of the specific elements improved by the intervention, we exploit two types of variation. The first variation corresponds to baseline access to adequate infrastructure among beneficiaries, which was heterogenous in the sense that some families lacked certain attributes. The second type of variation arises from in-situ vs relocated enhancements. As will be explained on the following section, some households relocated toward new neighborhoods, while other received a new dwelling on the initial land. Since several improvements are expensive and require economies of scale —such as plumbing— the resulting difference in habitat features on otherwise identical shelter can further contextualize the effect form the intervention.

The reception of a new house constituted an endogenous event influenced by predetermined socioeconomic characteristics, especially dwelling quality. To ensure that estimations are performed between comparable groups, we constructed a score variable based on pre-flood attributes, including shelter materials and initial wealth. Using this information, we matched observational units who exhibit similar propensities to receive a dwelling, but who diverge in treatment allocation due to exogenous variation in flood damage. Additionally, we instrumented treatment status through baseline residence location, a factor that considerably influenced the probability of accessing the program, as the gradient of the village diminishes in accordance to the distance relative to the main square.

By studying an intervention that simultaneously improved all shelter characteristics for households with heterogenous baseline access to adequate housing, we are able to disentangle the effects associated with habitat provision among poor populations, identifying the channels through infrastructure enhancements affect households' behavior and outcomes. Furthermore, through variations on treatment intensity —measuring the additional effect stemming from incremental upgrades in structure within the same setting—we are able to determine the degree of complementarity across feasible alternatives related to shelter improvement, informing about the plausible benefits associated with combined interventions.

Decomposition of the impact associated with housing upgrades further allows us to make contributions toward areas in which there is mixed evidence regarding the effectiveness of certain interventions. For instance, while some studies have highlighted the positive impacts from enhancing sanitary conditions (Dickinson et al., 2015; Kumar & Vollmer, 2013; Spears, 2012), others have found no evidence of a positive effect on health outcomes (Cameron et al., 2018; Patil et al., 2014; Clasen et al., 2014; Devoto et al., 2012; Pattanayak et al., 2007). Pickering et al. (2015) even find both, positive and null effects of improved household sanitation. Khanna (2008) even finds a negative impact, where piped water access increases the incidence of diarrhea. In several of these cases, the lack of effectiveness of sanitation improvements arises because they fail to consider complementarities and externalities on the solutions pertaining to water and sanitation (Duflo et al., 2015), a situation that characterizes rural areas. In fact, the implementation of sanitation-only solutions, without any sewerage complementarity, is significantly less effective than sewerage-only interventions (Wolf et al., 2014). Yet their positive impact is clear when delivered in conjunction (Cutler & Miller, 2006; Galiani et al. 2005).

From a policy perspective, this study is also relevant from a post-disaster recovery perspective. Usually, habitat is one of the critical components in determining long-term recovery after a natural disaster (Gillis et al., 2007). Hence, governments and multilateral agencies assign sizable resources seeking to mitigate damages generated in this sector. For example, during the time it operated, the Road Home Program — a federal package implemented in the United States after hurricanes Katrina and Rita — executed over US$9 billion in helping Louisiana families rebuild their home or relocate elsewhere[37]. Likewise, between 2006 and 2011 the World Bank assigned US$3.8 billion toward 68 shelter reconstruction projects across the world (Ganapati & Mukherji, 2014). In assessing the overall effectiveness of these initiatives, it is of vital importance that we establish how they affect recipient households, as they can have a broad impact, influencing health, happiness, education, and wealth.

[37] https://www.road2la.org/HAP/Default.aspx. Consulted on November 19, 2018.

The rest of this paper proceeds as follows: Section 3.2 provides background information about the housing intervention. Section 3.3 presents the channels that explain advanced household outcomes. Section 3.4 describes the dataset employed for estimations. Section 3.5 illustrates the analytical framework. We describe the prevailing habitat conditions in the study area in Section 3.6. And Section 3.7 presents the results. Finally, Section 3.8 concludes.

3.2 HOUSING PROVISION

Between 2010 and 2011 Colombia suffered the worst climatological shock ever experienced by the country (CEPAL, 2012). In the midst of torrential rainfall generated by *La Niña* phenomenon[38], overflowing waters caused the failure of a levee located on the Caribbean Coast, inundating a region inhabited by rural communities living under high incidence of poverty and whose economy relied heavily on farming (Aguilera, 2006). The flood displaced families, halted agricultural production, interrupted schooling, and destroyed vital infrastructure, triggering a persistent state of emergency that led to a noticeable reduction in welfare (Sanchez-Jabba, 2011).

Among the many sectors affected by the calamity, the habitat component was exceptionally disturbed. According to the damage assessment conducted by the Fund for Adaptation to Climate Change (FACC), a total of 362 dwellings —19.5%—were destroyed in Santa Lucia, the study village, as a result of the shock. In order to alleviate the negative impacts produced by the tragedy, the Colombian government adopted an unprecedented intervention, which included the provision of shelter to families who lost their home. As such, between 2013 and 2016, period in which the reconstruction phase was active, the agency in charge of rebuilding houses —FACC—delivered 188 dwellings, constituting the most extensive rural housing intervention on the country.

[38] *La Niña* is a climatological event characterized by unusually cold temperatures on the Equatorial Pacific Ocean, which translates into increased rainfall in the Tropics, where Colombia is located. https://www.climate.gov/enso

Figure 3.1. Houses Damaged by the Flood

Unfortunately, the availability of resources did not satisfy the demand induced by the catastrophe, since the government only replenished 52% of wrecked buildings. In fact, data collected on the area of interest indicates that 35% of households applied to receive new shelter, where 25% of applications were rejected, mostly as a result of institutional constraints. In order to receive a house, applicants were required to meet certain conditions, including proof of ownership of the destroyed unit; residence on the destroyed unit for at least three years; possession of no other property, besides the destroyed unit; and enrollment on the official government census conducted to assess losses created by the 2010-2011 rainy season. Distribution of dwellings was strictly determined by the destruction generated by the flood, which was established through FACC visits on each of the units reported as damaged on the government census. Buildings with severe damage or risk of collapse were then classified as eligible for replacement (i.e. new shelter).

Several applicants failed to meet these standards, thus excluding them from the intervention. Specifically, 33% held excess property, instantly disqualifying them from becoming recipients; 12% could not demonstrate ownership over the dwelling to be replenished; and 5% owned a dwelling that was not eligible for reposition per the assessment conducted by the government after the flood. Evidence regarding program participation suggests that, among families that did not seek a new house after the flood, 49% possessed property other than the unit damaged by the disaster; another 19% reported that their dwelling was not eligible for reposition per the damage assessment conducted by the Government amid the calamity, indicative of superior preflood habitat conditions. In fact, only 14% of households that belong this group —relative to 50% among the group who applied and most notably, 81% among eligible applicants— reported a total loss on their dwelling as a consequence of the disaster. These facts immediately signal preexisting differences between the two groups, which resulted in endogenous treatment assignment.

As a measure intended to reduce risk against future shocks, the intervention pursued relocation of families on areas with comparatively lower risk of inundation, building new neighborhoods equipped with several amenities, such as parks and roads. This measure meant that, while a share of beneficiaries —42%—resettled in new neighborhoods, another proportion obtained new shelter on their initial land. In in many cases, relocation represented an additional upgrade in living conditions for recipients, as the provision of shelter was accompanied with investments in local infrastructure than further enabled living conditions through public goods provision, such as plumbing and sewage.

Figure 3.2. Families Relocated in New Neighborhoods (2014)

3.3 CHANNELS

This section describes the channels through which housing provision in the present context plausibly affected households' behavior and outcomes. All channels rely on infrastructure upgrades as the main component that affects living conditions. The first analyzes the relationship between living conditions and health outcomes. The second mediating mechanism consists of relaxed constraints that result in efficiency gains. Exploiting additional variations on housing dimensions, we leverage on two components of shelter that were affected by the program: ownership and relocation, in order to investigate the importance of value-added benefits that might justify integral shelter programs.

3.3.1 *Health*

Since most interventions occur among families who live under substandard housing, infrastructure advancements commonly translate into superior sanitary conditions, which positively affects health outcomes (Arku, 2006). The evidence on this topic is abundant and robust, with consistent estimates that reflect the gains emanating from upgrades in sanitary conditions, most specifically on the incidence of diarrhea and child anthropometric measures (Ausburg & Rodriguez-Lesmes, 2018; Pickering et al., 2015; Kumar & Volmer, 2013; Montgomery & Elimelech, 2007; Mara et al., 2010; Watson, 2006; Bartam et al., 2005; Cutler & Miller, 2005; Jalan & Ravallion, 2003; Merchant et al., 2003).

Unfortunately, few studies have analyzed how shelter programs per se affect household wellbeing, mostly because habitat provision is expensive and thus, opportunities for formal analysis are scarce. Existing assessments include Galiani et al. (2009), who evaluate TECHO, a housing program that delivers complete habitat structures —roof, walls, and floors— among urban slum dwellers. As expected, the authors found positive effects on child health, expressed through the reduction of diarrhea. Cattaneo et al. (2009) studied a focal intervention —floor replacement from dirt to cement— documenting reduction on parasitic infections, diarrhea, and the prevalence of anemia. Along similar lines, Dickinson et al. (2015), Hammer & Spears (2013), and Spears (2012) show that household latrine adoption yields a positive impact on infant mortality and children's height/weight. Finally, Duflo et al. (2015) highlight the importance of complementarities on infrastructure-based approaches by considering simultaneous water and sanitation developments. They find a reduction on diarrhea, an advance relative to plumbing focused programs, which did not affect health outcomes.

Housing upgrades can also expand subjective wellbeing. Galiani et al. (2016) and Cattaneo et al. (2009) report positive effects in life satisfaction and mental health, documenting reductions on the incidence of depression and stress. The literature on household relocation toward better quality housing, particularly that related to the *Moving To Opportunity Program*, has found the same result (Ludwig et al., 2013; Kling et al., 2007; Katz et al., 2001).

3.3.2 *Relaxed Constraints*

Enhanced infrastructure and ownership relax household constraints through efficiency gains. For instance, plumbing decreases time spent arguing over water-related matters, endowing families with additional hours that can be reassigned in accordance to household preferences (Devoto et al., 2012). Likewise, tenure security among urban squatters reduces quarrels over land ownership, leading to increments in labor supply as people do not have to spend time arguing over property rights (Field, 2007).

Just as in the previous examples, it is possible that improvements in infrastructure relaxed liquidity constraints for beneficiary families. If in effect, and as outlined above, better salubrious conditions within the dwelling reduced the incidence of parasitic and respiratory diseases, it is possible that families decreased health-related expenditures. The latter could potentially endow households with additional resources that translate into improved welfare outcomes via increased consumption or reduced labor supply. Following the same principle, it is possible that with the reception of a new dwelling, recipients had to perform less frequent maintenance to their shelter, which was vulnerable to whether shocks. For example, during the rainy season adobe houses suffer due to leakages and erosion, forcing families into devoting considerable resources to mitigate damages.

3.3.3 *Ownership*

Tenure affects households' expenditures, investments and even labor decisions. Galiani & Schargrodsky (2010) find that secure property rights increase dwelling investments on walls, roofs, and constructed surface. Meeks (2017) shows that secure property rights translate into investments in sanitation, mainly connections to sewerage or sceptic systems; Benmelech et al. (2017) report positive effects on consumption of durable goods and Sodini et al. (2017) show that home ownership causes households to work and save more.

These results follow from a greater sense of security, where households decide to undertake costly investments under the reassurance that, through stronger property rights, any benefits streaming from these endeavors will accrue to its members and will not be enjoyed by anyone else. It is then reasonable to expect households who benefitted from sturdier property rights to report relatively higher levels of investments, changes in labor supply, and savings. In particular, first-time owners are anticipated to display greater effects, which can be related to a positive behavioral response connected to significant change on overall life conditions.

The latest aspect can be related to the psychological literature. On this regard, Dai et al. (2014) argue that the likelihood of changes in behavior increases after salient landmarks that represent new beginnings, such as a new year or a movement to a new city. On a similar note, Ray (2006) shows that effort exertion is strictly proportional to aspiration gaps, the difference between an individual's actual situation on a particular aspect of his life and the desired level that that individual wishes to attain. Based on the latter, it is possible that enriched living conditions created a new aspiration window and that, motivated by the fresh start embedded in owning a new house for the first time, beneficiary families undertook decisive steps to improve their welfare.

3.3.4 *Location*

In McIntosh et al. (2018) government expenditures on public infrastructure were suboptimal, obliging households to assume private investments in order to secure housing efficiency. Since relocated families received an extensive package, in terms of public provision of goods that affect returns to shelter, we investigate whether widespread spending in local infrastructure —expressed through relocation on better quality neighborhoods— expanded outcomes for this group in relation to those who remained on their initial location. This procedure will inform about the importance —or lack thereof— of economies of scale that enable households to effectively capture benefits arising from shelter provision, thus motivating the adoption of integral approaches on habitat-based programs.

The fulfillment of certain habitat components becomes increasingly difficult among dispersed geographical units (Meeks, 2017). For example, Devoto et al. (2012) argue that connecting dwellings to the water main is expensive and often not financed, which reinforces the view that families who did not relocate might lack this service. In this sense, it is possible that relocated families experienced housing shortage relief to a greater extent, relative those who benefitted from in-situ provision, since new neighborhoods were equipped with services that are not necessarily satisfied on the old neighborhoods. Aspects such as plumbing and connection to a sewage system depend on economies of scale and are generally are generally provided on the basis of aggregate demand, with in-situ provision having a feeble effect on overall shortage within a particular neighborhood or area. Nonetheless, with the construction of a new neighborhood, the configuration of a local and substantial demand for these services likely prompts the government to invest on these additional components.

Neighborhood analysis, based on relocation relative to in-situ conditions, informs about the value-added component arising from complementarities associated with dwelling provision, provides idea of the degree to which shelter interventions should be implemented to a greater extent, based on complementarities that arise from simultaneous enhancement. This contributes to an ongoing discussion pertaining to ways in which rural communities can be uplifted from poverty, evaluating the effectiveness of large-scale interventions that involve widespread provision of public goods, for which existing assessments have generated mixed results. For example, even though the Millennium Villages Project was established over a decade ago, the impact of this program on the welfare of Sub-Saharan African communities is still actively debated. Furthermore, the existence of economies of scale potentially induce supplementary profits that further increase welfare. For instance, Chyn (2018) and Chetty et al. (2016) argue that household resettlement under more advantageous conditions —superior neighborhoods— improves labor market outcomes and subjective wellbeing during young adulthood, a result that could be particularly robust among relocated families in the present setting. Additional benefits arising from extensive provision, not only relieving shortage in terms internal habitat components but complementing them with external infrastructure —plumbing and connection to sewage— would inform about the return to integrated interventions.

3.4 DATA

The data for this study was collected between March and May of 2018 in Santa Lucia, Colombia, one of the villages affected by the calamity and which subsequently benefited from governmental shelter provision. Employing a retrospective survey specifically designed for this investigation, we gathered information related to habitat conditions, flood damage, wealth, and other socioeconomic indicators, enabling the trace of program participation, infrastructure upgrades, and outcomes associated with overall health and household behavior.

The first element to consider consists of treatment allocation. To track this component, enumerators visited preselected residences, which immediately revealed whether a specific family lived in one of the houses provided by the government, thus indicating treatment condition. However, and anticipating possible endogeneity in allocation, the survey decomposed program access through a screening mechanism that inquired for housing application and any subsequent marginalization from the program. As an initial filter, interviewers explicitly asked households if they had requested a government dwelling. Next, among those who applied for new shelter, and attaching the observed treatment condition, we were able to separate households who were rejected or withdrew from the program, querying for reasons that accounted for exit. We verified program compliance by performing this procedure among families that did not request a new house, seeking to identify households that transitioned toward unassigned groups.

Next, we examined access to adequate housing and flood damage, factors that significantly influenced the likelihood of program participation. For baseline habitat conditions, surveyors asked families to state whether their residence in 2008 satisfied a set of elements deemed as necessary for acceptable living conditions[39]. We identify incremental upgrades through

[39] Measurement of adequate housing is explained in detail on the next section.

enumerators' reports on shelter attributes during the 2018 in-situ visits, classifying additive components as local infrastructure improvements.

The questionnaire included a special module that registered flood losses. In particular, shelter destruction is marshalled asking households if they experienced any habitat-related damages. In case of an affirmative response, the survey requested an assessment of the loss —total or partial— providing an inchoate approximation of after-shock necessity for shelter relief. Ability to cope with the disaster is ascribed to family wealth, measured through asset holdings and participation in public welfare programs; and income from several sources (sales from enterprises, wages, remittances and rents).

The survey also incorporates variables that typify the channels proposed in Section 3.3, the first of which concatenates improvements in health outcomes to enhanced sanitary conditions. Advancements in salubrious settings are attributed to developments in housing infrastructure, expressed through retrofitting upgrades in floor/wall materials, human waste disposal methods, access to potable water and a safe cooking space. Health outcomes are assessed through household reports concerning incidence of health shocks, including year in which the disturbance occurred, as well as its destabilizing effect on the family; incidence of illnesses, specifying the type of complication suffered by household members during the past month; and doctor visits during the past quarter.

The second mediating mechanism states that incremental habitat quality relaxed liquidity and time constraints for beneficiary families, who are expected spend less resources combating illnesses/diseases and fixing their dwelling. To measure this, the survey requested families to list health expenditures during the past quarter (exams, medicine, out-of-pocket expenses), as well as households' responses to health shocks, which include income forgone during sick days, increased labor supply, use of savings, and sale of assets. We also inquired for shelter maintenance, gathering information related to the frequency with which households performed repairs and shelter upkeep, tallying the costs —in terms of money and time— associated with this practice. Presuming that shelter provision effectively relaxed monetary and time constraints within recipients through the conducts stated above, we look at changes on variables that potentially absorb surplus resources. Following the standard microeconomic literature, we predict a response consistent with a pure income effect —relative prices remain unaltered— with households unrestrained to increase consumption of normal goods, captured through changes in time use and consumption of non-durable goods during the past month.

Strengthened property rights are detected through government titling among recipients who stated formal ownership, through a certified public document, of the occupied unit. According to this channel, home ownership underpins habitat investments, appraised through the value of outlays and savings/loans associated with shelter improvements. We also expect incremental property rights to translate into behavioral responses triggered by a surge in happiness, which is ascertained through subjective measures of wellbeing. These include current and retrospective reports on overall happiness —based on the life ladder concept outlined in Moya (2018)—, as well as satisfaction with life, health and habitat aspects of an individual's life. The last portion of this channel is further illustrated by studying psycho-emotional responses, reflected on an array of variables: i) aspirations, for which we collected data on the desired living standard for children, mainly educational attainment, aspired salary, and desired occupation —relative to parents' current state on each of these variables; ii) fertility, including births and pregnancy; iii) migration, which is traced by inquiring for household members who have departed the family residence during the past decade, focusing on migration motivated by the pursuit of better opportunities and employment/study prospective; iv) savings for education, entrepreneurial endeavors and purchase of durable goods; v) and lifestyle habits, consisting of consumption of alcohol, tobacco, and fruits/vegetables.

Finally, neighborhood effects are conveyed through variation in residence location across families favored by the intervention. We construct residential history through two separate conducts: GPS coordinates and reports regarding addresses held during the past decade. Although the first measurement is not available for 2008, it enables identification of relocated households. Employing satellite images and information provided by the agency in charge of constructing the government dwellings, we matched the recorded GPS coordinate with predetermined spatial grids that delineate the area corresponding to new neighborhoods. As an alternative provision, enumerators noted the address corresponding to the dwelling that was visited during the interview and, for households who stated a residential change during the past decade, they entered the preceding address and neighborhood (in case respondents could not recall the information). For observations that exhibited a change in location, information corresponding to treatment condition —following the parameters explained earlier— elicits neighborhood relocation among program beneficiaries.

3.4.1 *Assignment*

Figure 3.3 summaries the assignment framework within this quasi-experimental setting. The sample is composed by 974 households, of which 336 applied for new shelter, while 638 decided not to enroll on the program. Within the group of families that did not enroll, 47 were offered a new dwelling, which could be explained by the fact that several eligible recipients rejected the housing solution offered by the government. However, in the end 99.7% of those who dismissed program participation did not benefit from the intervention.

Out of 336 applicants, 87 were denied treatment, mainly as the result of the screening process adopted by the government. Only 1 family among those who were rejected —conditional on applying— flouted into a new house. Finally, among those who were offered a new dwelling —conditional on applying for the program— 40 rejected new shelter, adamant to surrender existing land. There is an almost perfect match between the number of families who withdrew from the program after applying and being offered a new house (40) and the amount of families who were offered a new house despite not applying for the program (47), which strongly suggests that this last group was targeted by the government in order to deliver excess housing. Overall, the resulting assignment indicates success in allocation of new houses, with the program attaining robust compliance across all groups, with virtually no households switching treatment conditions. To ensure that calculations are performed within the appropriate group, we excluded all households who inhabited government provided dwellings as tenants or who seized occupation, which account for 16.3% of final recipients.

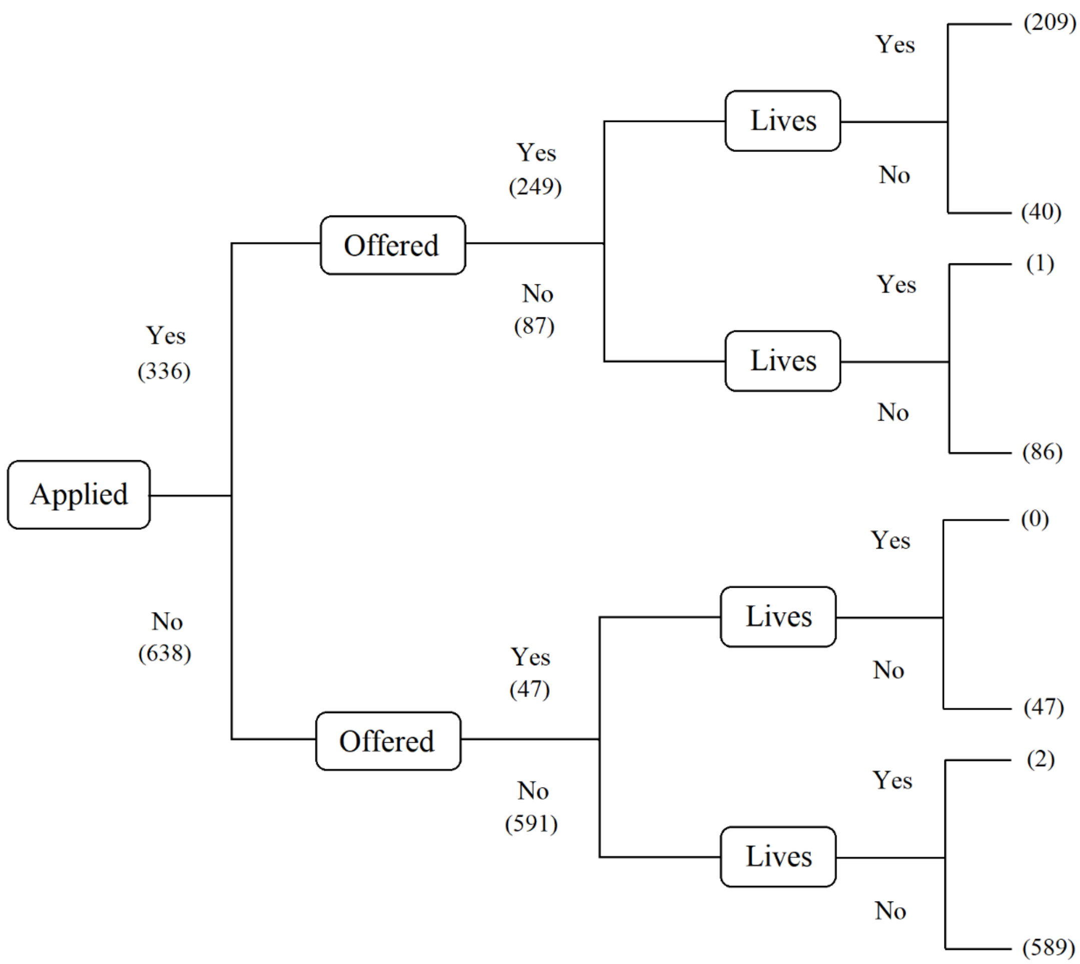


Figure 3.3. Assignment Framework

3.5 ANALYTICS

3.5.1 *Measuring Housing Shortage*

We calculate the prevalence of substandard housing conditions within each household through the construction of an index that considers the main elements required for adequate housing. For this purpose, we follow the methodology proposed by the Colombian Department of Statistics —DANE— regarding the calculation of housing shortage. According to DANE (2009), a housing deficit arises when a household fails to meet acceptable shelter standards. If a household occupies a home that fails to meet a predetermined norm that approximates the minimum conditions required for acceptable living conditions, it is referenced as experiencing housing shortage in accordance with the specific dimension considered —quantitative or qualitative.

The first metric approximates housing shortage as the deviation between households' demand for housing and the availability of units. Available units include habitat structures that are able to effectively safeguard the integrity of its dwellers. Hence, it takes into account the quality of materials that constitute the physical composition of the dwelling, primarily material of walls. Generally, this measure also contemplates overcrowding, inquiring for the number of families residing in a given unit. Nonetheless, the former will not be considered on the present study, since this situation is not uncommon in rural contexts, where lodging availability is relatively limited and thus, several families tend to live in the same house. For instance, according to the information collected on the study area, 28% of households shared residence with immediate or extended relatives in 2008; 30.4% in 2018.

The second metric —qualitative shortage— is concerned with habitat attributes that directly impact life quality among inhabitants, including quality of floors and sanitary conditions. These are gauged keeping track of household access to safe water; electricity; connection to a waste disposal system; and food preparation conditions.

It is expected that dwelling provision by the government resulted in reduced housing shortage of either type. On one hand, new dwellings, equipped pre-fabricated walls, necessarily solved quantitative shortage among beneficiaries who lacked adequate walls before the flood. Additionally, upgraded infrastructures delivered advanced floors, a kitchen, and toilet, curbing qualitative deficits.

3.5.2 *Selection Bias*

Access to the treatment was endogenous, with baseline socioeconomic conditions influencing program participation. The main determinant of treatment allocation consists of habitat damage, an exogenous shock that explains 95% of the variation induced by factors that we propose as bases of treatment allocation. Table 1 shows, after adjusting the sample size to reflect overall population[40], that destruction of the family residence increased the likelihood of program participation by 26 percentage points (pp). Estimates also indicate that families who live under deficient infrastructure, expressed through quality of walls, which determines buildings' ability to withstand structural damage, were 4.5 pp more likely of being offered a new house. However, baseline wealth, measured through an index inclusive of socioeconomic attributes, decreased the probability of program participation, albeit to a marginal extent, an aspect indicative of manipulation in the allocative mechanism.

Table 3.1. Determinants of Treatment Allocation

Treatment = 1	**(1)**	**(2)**	**(3)**
Dwelling: Loss	0.300 ***		0.260 ***
Infrastructure: Deficient		0.087 ***	0.045 **
Wealth Index		-0.104 ***	-0.013 ***
N	**1964**	**3008**	**1964**
Pseudo R-Squared	0.284	0.061	0.305

The latter can be corroborated when analyzing differences in baseline access to adequate housing and asset holdings, with the group that forewent program participation exhibiting advantageous characteristics. According to the information presented in Table 2 —adjusting

[40] This adjustment was performed following the guidelines for sample adjustment outlined by the Department of Statistics of Colombia**.**

sample size and including only families who own new shelter— families who decided to participate in the program (Column 2) displayed substantially higher incidence of substandard housing across all shelter components, highlighting prior discrepancies that deem those that did not seek program involvement (Column 1) as an unsatisfactory comparison group. These differences, which eventually rendered the groups incompatible, are also reflected on initial wealth, which determines households' ability to cope with the calamity. Additionally, this group sustained fewer shelter losses as a result of the calamity, presumably reclined on advanced infrastructure.

This, combined with the institutional requirements for program participation —surrender excess land— meant that prospective beneficiaries who did not accumulate myriad losses would actually incur on a net loss should they enter the program, as they would relinquish landholdings and structures that embedded a greater value relative to the alternative offered by the government. The latter follows from the valuation of shelter, expressed as the stated house sale reservation price, where (1) reported a significantly higher appraisal of baseline habitat. Specifically, (2) valued shelter at an average value of $6,367, while (1) stated a mean value of $30,533. Consequently, we can safely disregard the latter as a potential control group, as they display observable factors that depart from those exhibited by the treated.

Out of households that did apply for the program, 38% were either rejected or withdrew. A review of Table 2 shows significant divergencies materialized on the elevated squared sum of deviations with respect to the treatment group. Specifically, households who were sidelined from the treatment (Column 3) —conditional on program application— differed from actual beneficiaries on the basis of traits that rendered them ineligible, with 33% reporting excess property, instantly disqualifying them from becoming recipients; 12% could not demonstrate ownership over the dwelling to be replenished making it impossible for the government to provide new shelter as i) the government could not verify ownership of the infrastructure to be replenished or ii) the program required eligible families to relinquish land where the damaged structure was located, for which no property rights could be proved. Finally, 5% held a dwelling that was not eligible for reposition per the assessment conducted by the government after the flood[41]. As was the case with (1), 38% of eligible households in (3) refused to surrender land, one of the requirements to move forward on the program, thus withdrawing the program voluntarily. All this suggests that disparities in wealth caused the fragmentation on the applicant group, a fact that can be verified by analyzing baseline differences in wealth between these groups, with the excluded segment consistently displaying favorable socioeconomic characteristics.

Accordingly, we can conclude that the group that applied to participate in the program, yet did not receive a dwelling, does not represent an appropriate comparison group, as they display latent factors that differentiate them from those who ultimately benefited from the intervention. Besides, whether separation occurred as the result of excess wealth —expressed through possession of other properties—, incapability to prove ownership, or declination to surrender land, the crucial aspect consists on the fact these divergencies restricted participation and therefore, reveal underlying differences among these groups. Hence, we disregard (3) as a potential counterfactual, even if it displays relatively similar baseline characteristics.

[41] According to the information on Appendix A1, among this group the proportion of households that reported total dwelling loss ascended to 30%, which was still low compared to 81% among final recipients.

Table 3.2. Baseline Comparisons (2008-2011)

	(1)	**(2)**	**Diff.**	**(3)**	**(4)**	**Diff.**
Habitat						
Ownership: Yes	60.34	56.8	-3.5	68.4	60.7	-7.7
Walls: mud, coarse wood, plastic	8.3	12.5	4.2 **	5.3	14.1	8.9 **
Floors: Sand	16.1	22.3	6.2 **	21.1	24.5	3.5
Water: No	12.5	13.7	1.2	13.2	14.1	1.0
Electricity: No	3.1	3.6	0.4	7.9	1.8	-6.1 **
Toilet: None	21.5	28.9	7.4 **	17.1	28.8	11.7 *
Cooking Method: Wood	30.3	41.4	11.1 ***	35.5	41.7	6.2
Assets						
Farm/Land	21.2	9.5	-11.6 ***	26.3	2.5	-23.9 ***
Refrigerator	36.2	22.6	-13.6 ***	32.9	18.4	-14.5 **
Stove	32.3	19.9	-12.3 ***	28.9	20.9	-8.1
Washer	20.1	9.8	-10.2 ***	9.2	7.4	-1.9
Motorcycle	13.5	7.4	-6.0 ***	13.2	6.7	-6.4
Dwelling						
Damaged	79.31	85.1	-5.8 **	90.8	89.6	-1.2
Destroyed	14.0	50.0	36.0 ***	27.5	79.4	51.9 ***
N	**2552**	**1334**		**304**	**652**	

3.5.3 *Control Group*

The adjusted sample consists of 652 families, excluding those who lived under rent. As a conservative measure, we initially compare the treated group with households that decided not to apply for the treatment (Column 1 on Table 3), knowing that both groups display baseline differences that tarnish estimations. However, the obtention of a statistically significant effect within this framework, if any, would bolster subsequent estimations that impose restrictions on the sample that is employed as the counterfactual.

To ensure we estimate the impact using the most suitable comparison group, the identification strategy matches observational units based on a propensity score (to receive a new dwelling) determined by baseline housing conditions and flood damage, aspects that ultimately influenced the likelihood of program participation, as shown on Table 3. For each of the 652 observations contained in the treatment group, a couple was selected from the pool of households who not only did not apply for a new house but also satisfied some additional constraints: i) were not offered to participate on the program —excluding 49 observations— limiting behavioral responses associated with being offered a new house; ii) did not gain access to new shelter over the past decade, excluding 176 observations, iii) were owners of the dwelling they inhabited (i.e. did not live under rent or without property rights), excluding 62 observations, and iv) lived in the village of interest throughout the entire period (2008-2018), excluding 13 observations.

As an additional measure aimed at corroborating results, we constructed a synthetic control group from households located on a nearby village that displayed analogous baseline sociodemographic characteristics, and where no intervention took place[42]. The coupled control group — (7) in Table 3— was chosen following the same criteria employed on the extraction of the counterfactual group from (1). For each of the 652 observations contained in the treatment group, a couple was selected from a pool comprising families residing on the village from which the synthetic control group was assorted. Since the control village closely resembles the treated municipality and, taking into account that the pool of possible counterfactuals increases to 2,109 families on the alternate village, the resulting match displays stringent correspondence on relevant habitat features, minimizing preexisting differences. The only perceptible divergence between the

[42] Specifics regarding the criteria employed in order to select the village from where the synthetic control group is constructed can be found in Chapter 1 of this doctoral dissertation.

two groups surfaces when examining sewage connection, which was scant on the control village at baseline and is evident on the comparatively higher proportion of households on the comparison group with sceptic tank instead of a toilet connected to the sewage system.

The coupled control groups were chosen on the basis of observable habitat features that elicit qualitative/quantitative housing shortages, pairing comparable units on the grounds of an index that captures the probability associated with program participation (i.e. being offered a new dwelling). As can be seen on Table 3, the control group (3) presents salient similarities with respect to the treatment. In fact, when all three possible counterfactual groups are considered, (3) stands out not only as the one with less statistically significant differences regarding baseline access to adequate housing, but the one with minimal deviations from treatment values —expressed through the squared sum—, thus bolstering our choice for a control group. Since (4) is composed by families who, not only applied for the intervention but also did not withdraw from the program, the causal effects resulting from the estimations correspond to the intent-to-treat effects.

Table 3.3. Control Group

	(1)	**(2)**	**Diff**	**(3)**	**(4)**	**Diff.**	**(5)**	**(6)**	**Diff**
Ownership: Yes	62.9	60.7	-2.1	58.3	60.7	2.5	62.6	60.7	-1.8
Walls: mud, coarse wood, plastic	8.8	14.1	5.3 *	12.9	14.11	1.2	8.6	14.1	5.5
Floors: Sand	13.9	24.5	10.7 ***	23.9	24.5	0.6	16.6	24.5	8.0 *
Water: No	12.1	14.1	2.0	11.7	14.1	2.5	8.6	14.1	5.5
Electricity: No	3.3	1.8	-1.4	0.6	1.8	1.2	2.5	1.8	-0.6
Toilet: None	18.2	28.8	10.7 ***	29.4	28.8	-0.6	14.1	28.8	14.7 ***
Cooking Method: Wood	26.8	41.7	15.0 ***	41.1	41.7	0.6	42.9	41.7	-1.2
N	**1584**	**652**		**652**	**652**		**652**	**652**	
Squared Sum			**489**			**16**			**347**

The proposed matching procedure plausibly eliminates the selection bias discussed earlier under the assumption that unobservable factors have no incidence on the likelihood associated with program participation. Although this cannot be formally tested, the institutional constraints discussed above, which excluded households from treatment assignment, relate primarily to observable features, especially preexisting landholdings that a household would otherwise have to relinquish. An upheaval on this premise could arise when considering capacity to prove ownership rights over the land to be intervened or to demonstrate that a dwelling was in fact destroyed by the disaster, aspects that could be influenced by residual traits, such as ability. However, the relatively low proportion of households who reported such limitations as the primary reason for program exclusion —6% among (5)— tarnishes this issue, supporting the approach adopted in order to reduce sample bias.

It is worth mentioning that observations were also matched on flood damage, which was widespread on both villages[43]. This generates a substantial pool of observations that experienced the shock to a similar extent, curbing any responses that could potentially confound the results on variables related household behavior, underpinning the quality of the control group. Mainly, (1) and (3) reported reduced dwelling losses as a result of the flood, thus signaling an inchoate flood effect —relative to the treated group—potentially confounding the results in the case that the flood effect influenced the outcome variables proposed in this study. By scrutinizing the control group for families that also experienced shelter turmoil, we sideline any potential response corresponding to the initial shock —flood— which triggered the government intervention, isolating the effect of shelter provision to the greatest extent possible.

[43] The disaster damaged 81 and 43%, respectively, of dwellings on each village; destroyed 22 and 8%.

3.5.4 *Retrieving Exogenous Variation*

In addition to matching observations based on preflood habitat conditions, we attempt to recover the exogenous portion corresponding to decision concerning program participation. As shown on Table 1, flood damage and initial dwelling conditions explain nearly 40% of treatment allocation assortment between households who benefitted from shelter provision and those who were rejected or withdrew from the program. Following Heckman's (1979) two-step adjustment procedure, we initially predict the observable —exogenous— fraction emanating from take-up of the government dwelling —conditional on applying and being offered a new house. Next, procuring to eliminate the bias, we include the prediction as a covariate that captures the endogenous component on the decision.

Performing the operation delineated above implies the deposition of families who withdrew from the program into the treatment group, yielding conservative, yet even more reliable estimates of returns to shelter. It would also reorient the comparison group toward the same village, employing households who did not seek program participation as the counterfactual, with the resulting impact corresponding to the intent-to-treat effect. A primary reason for which a comparison group residing on the same village is preferred to a synthetic control group located elsewhere consists of the fact that the government intervention encompassed an extensive package that involved widespread provision of public goods, which could have affected outcome variables. Therefore, a control group selected within the treatment village should feeble any potential confounding effects, since all families on the treatment village were eligible to access complementary programs related to the governmental response to the calamity.

3.5.5 *Identifying Shelter Improvements*

In order to isolate the effect corresponding to specific infrastructure upgrades, we pursued a difference-in-difference approach, comparing outcomes across households that exhibit homogenous habitat conditions after the intervention, but who display differential baseline access to adequate housing. Employing retrospective information, we are able to trace pre-flood infrastructure circumstances, detecting incremental upgrades as a result of the housing program. Initially, we sort recipients into clusters with identical infrastructure. Next, we compare the gains arising from extensive shelter provision among groups that differ exclusively by a specific housing component, but who are otherwise identical in terms of baseline shelter. For each group, we estimate the effect of habitat provision relative to the control group, anticipating that, for households who lacked a particular dwelling component, the improvement on outcome variables is presumably greater. Otherwise, it would be concluded that the treatment had no effect. Since treatment groups are equivalent in terms of baseline housing, except for the one attribute being considered, we deduce that the difference on the effects reflects the benefits corresponding to marginal retrofitting upgrades, thus highlighting the exact mechanism that causes improved welfare outcomes.

Our estimation strategy follows a difference-in-difference setup that compares health outcomes and dwelling maintenance for households that benefitted from shelter provision relative to the control proposed group. The baseline reduced-form specification is the following:

$$Y_{iht} = \alpha + \beta_1(Dwelling)_h + \beta_2(Post)_t + \beta_3(Habitat)_{ht} + \gamma(Index)_{ht} + \varepsilon_{iht} \tag{3.1}$$

Whenever we have panel data, or

$$Y_{iht} = \alpha + \beta_1(Habitat)_h + +\gamma(Index)_{ht} + \varepsilon_{iht} \tag{3.2}$$

For outcomes for which we only have cross-sectional data. Although some outcomes correspond to individual-level variables, particularly those related to subjective wellbeing and labor supply, the main unit of analysis corresponds to households. Therefore, we cluster standard errors at the neighborhood level. This is motivated by the fact that random selection of observations was conducted at this level, choosing units from a set of preestablished grids that correspond to local neighborhoods.

Y_{iht} represents the outcome of individual i in household h at time t. $Dwelling$ is an indicator that signals whether household h benefitted from the housing program. $Habitat$ is the interaction between treatment and time indicator. $Index$ corresponds to a socioeconomic index composed of variables that could influence health outcomes and dwelling expenditures; it includes households' possession of assets, age, and gender. For binary outcomes, we replace the linear specification with a random utility model, specifically a probit model.

Figure 3.4 depicts the overall identification strategy, where we consider an arbitrary habitat variable $H^j_{c,t}$, which denotes the housing value in component j, at time t, for cluster c which starts at point F for the control group. The treatment group has been split into two clusters $c = \{1{,}2\}$. For the first group, the value of the habitat measure H on component j in 2008 is $H^j_{1,2008} = A$. Analogously, the measure of the habitat feature H for component j for group 2 is $H^j_{2,2008} = B$**.** Both groups have identical housing conditions in 2008, except for the difference expressed through component j.

In 2018, regardless of substandard housing incidence in 2008, both groups end up with the same type of unit, yielding a value of $H^j_{c,2018} = D$, where redressing upgrades have been implemented. We assume that the control group did not experience enabling conditions in component j, such that the value for H remains unchanged at point G. A standard difference-in-difference analysis would estimate the effect for each group as follows:

$$E[H^{j}_{1,t}] = (D - G) - (A - F) = D - A \tag{3.3}$$

$$E[H^{j}_{2,t}] = (D - G) - (B - F) = D - B \tag{3.4}$$

Since both groups are otherwise identical in terms of living conditions, the net effect corresponding to a specific upgrade on component $j = \ D - B \ - \ D - A \ = A - B$. This resulting variation in infrastructure would in turn explain disparities on the outcome variables proposed in Section 3.4.

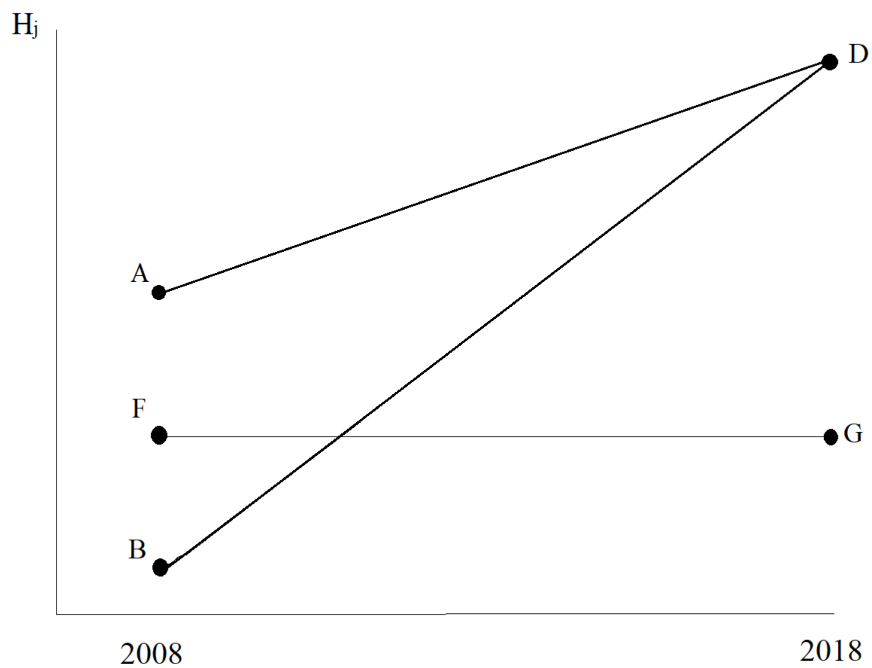


Figure 3.4. Analytical Framework

This simplified depiction disregards any type of habitat-related event on the comparison group. Nonetheless, the data suggests that in fact, the control group did not experience meaningful advancements in habitat conditions between 2008-2018. According to information gathered on the study area, 5% of households in (2) benefitted from government shelter provision during this period, all of which were discarded when performing the algorithm that coupled observations. 4% benefitted from shelter provision on the control village, where we followed the same procedure.

3.6 HABITAT

3.6.1 *Intensive Margin*

This section characterizes physical upgrades among the treatment group, arranging observations on the basis of initial living conditions, subsequently identifying marginal improvements that stem from the intervention. According to the information presented in Section 3.5, the adjusted sample comprises 648 families, from which 24% lacked any type of floor; 14% had walls made of mud, coarse wood, plastic, or cardboard; 28% did not have any type of toilet; 14% lacked access to safe water; and 41% used wood to prepare food. Electricity was not included as a subcategory due to the relatively low shortage on this component (2% on 2008; 1% in 2018).

The government provided dwellings are expected to relieve most shortages, as these units came equipped with materials —and spaces— that advanced baseline shelter attributes. Table 3.4 presents the transition matrix for each group. Indeed, most deficiencies were solved through infrastructure enhancements. For example, out of 92 families that lacked adequate walls in 2008, only 4 did not fulfill this component after receiving a new house. Similarly, 93% of treated households overcame floor deficiencies. These advances, nonetheless, are less robust when looking at qualitative aspects —water, toilet access and safe cooking space— which display a degree of complementarity with neighborhood characteristics, an aspect that varied substantially between relocated and in-situ provision.

Table 3.4. Habitat Transition Matrix —Treatment Group (2008-2018)

Shortage	**2008**	**2018**	**Reduction**
Walls	92	4	95.7
Floor	160	12	92.5
Water	92	24	73.9
Toilet	184	44	76.1
Gas	268	52	80.6

Subgroups associated with incremental shelter analysis —above— exhibit differential access to other habitat features. Mainly, the absence of a particular element does not preclude other shortages. For example, although 160 families required floors, only 24 exclusively lacked this component; the rest displayed deficiency in at least one other aspect (see Appendix A1). However, we preserved coarse categories in order to underpin statistical power, which is curtailed as we include refined, yet splintered groups. Moreover, any preexisting differences in baseline housing quality between these groups would be differenced out, which asserts confidence on the estimates resulting from this approach.

3.6.2 *Extensive Margin*

35% of treated households exhibited adequate living conditions before the shock. Since this group did not experience retrofitting upgrades —only reception of a new dwelling— it was employed in order to characterize returns to shelter provision. Using the matching algorithm described in Section 3.5, we couple observations from the non-applicant group with households in the treated group who did not benefit from incremental infrastructure.

Panel (a) of Table 3.5 tracks subjective wellbeing for treated households who benefitted exclusively from dwelling provision. According to the results shown on this table, life and housing satisfaction languished on this group despite benefitting from new shelter. Despite the latter, we cannot decisively conclude that shelter provision itself did not improve welfare, as beneficiaries did sustain considerable losses as a result of the flood, which plausibly degenerated self-reported levels of happiness and habitat satisfaction. In other words, the outcome could have been worse had the government not replenished the units that were destroyed by the disaster. However, if we examine the trend displayed among the control group, the latter does not appear to be the case, since overall satisfaction with habitat conditions diminished by the same amount.

The most persuading evidence, however, suggesting the feeble impact of habitat provision at the extensive margin, emerges when we examine the group that also experienced relief in housing shortages, where the trend reverses itself (see Panel (b) on Table 3.5). Among those who additionally received retrofitting upgrades of any type, reported happiness and habitat satisfaction increased dramatically.

These results indicate that shelter provision improves household outcomes through enhancements in physical living conditions. Hence, when deciding allocation of resources associated with housing programs, agencies in charge of implementing the intervention should prioritize households who experience severe baseline habitat deficits; not only assessing assignment based on socioeconomic indicators. Concentrated treatment intensity on these prospective beneficiaries will in turn generate second-order gains expressed through enhanced indicators of subjective wellbeing.

Table 3.5. Subjective Wellbeing — Reported Satisfaction

(a)

	Control: Matched			Treated:All		
	2008	**2018**	**Diff**	**2008**	**2018**	**Diff**
Life	6.057	5.610	-0.45	5.340	5.107	-0.23
	(0.103)	(0.093)		(0.115)	(0.099)	
Habitat	3.527	3.595	0.07	3.194	3.181	-0.04
	(0.037)	(0.036)		(0.045)	(0.049)	
N		**3392**			**2816**	

(b)

	Control: Matched			Treated: Dwelling		
	2008	**2018**	**Diff**	**2008**	**2018**	**Diff**
Life	6.370	5.948	-0.42	6.073	5.295	-0.78
	(0.166)	(0.152)		(0.179)	(0.176)	
Habitat	3.905	3.545	-0.36	3.476	3.025	-0.45
	(0.047)	(0.063)		(0.070)	(0.064)	
N		**1232**			**1084**	

(c)

	Control: Matched			Treated: Dwelling + Upgrades		
	2008	**2018**	**Diff**	**2008**	**2018**	**Diff**
Life	5.744	5.539	-0.21	4.600	5.341	0.74
	(0.136)	(0.119)		(0.131)	(0.120)	
Habitat	3.328	3.611	0.28	3.045	3.391	0.35
	(0.051)	(0.044)		(0.055)	(0.054)	
N		**2068**			**1920**	

3.6.3 *Costs*

Health shocks in the study area galore, an aspect that is probably related to prevalent substandard habitat conditions. 22% of respondent families reported a health shock during the last quarter —a health-related disturbance that prevented a household member from working; 24% informed of the incidence of a disease during the month leading up to the interview. Almost half of medical events

—42%— were related to respiratory and or parasitic illnesses. Not surprisingly, doctor visits were abundant, with an average of 8.2 quarterly medical appointments per household.

Costs associated with these events are substantial. Households were asked to estimate expenses, in time and money, resulting from health complications. Although regular medical expenditures averaged \$8.25 per month, less than 3% of monthly income, the cost of a health shocks, measured as the amount of money it cost the family to overcome the event, was of \$52.5, 17% of household income. The average number of sick days was of 8.7 days.

Expenses related to shelter maintenance also capture a significant proportion of resources. 53% of households undertake dwelling repairs, of which 68% represent annual upkeep. The average cost of maintenance is \$66, with households devoting 12.6 days per year.

3.7 RELOCATION

Figure 3.5 shows the spatial distribution of government provided dwellings (blue). Towards the Southeast sector of the village, we can see an agglomeration of such houses, corresponding to households relocated on new neighborhoods (42%), while scattered units indicate in-situ provision.

Household relocation as a result of the intervention constitutes an important element of the analysis because agglomeration of new houses creates economies of scale that further enhance habitat quality. Whereas resettled families lived in new neighborhoods, equipped with superior public infrastructure, families whose new dwelling was situated on the initial land relied on the existing public system. This transmuted into relative housing shortages. Concretely, after the intervention, 93% of relocated households were connected to a public waste disposal system, such that 98% of families in this group informed the use of a toilet with access to sewage system. On the other hand, 28% of in-situ houses lacked such connection, resulting in nearly half of families using a latrine (32%) or not having any toilet at all (16%).

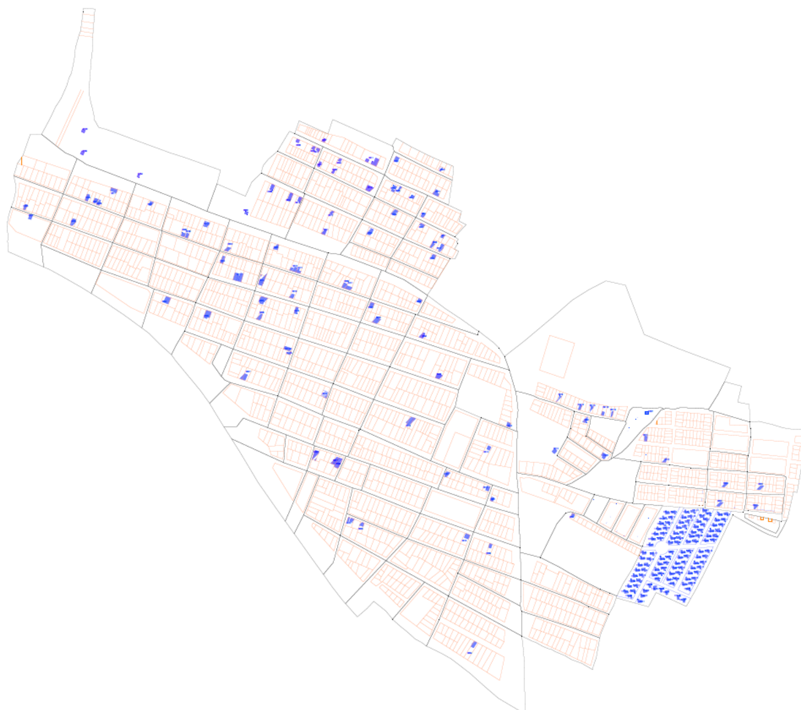


Figure 3.5. Distribution of Government Dwellings (2018)

Access to potable water, which also depends on the quality of public infrastructure, did not yield significant differences across groups. Among beneficiaries that remained on their initial land, this deficiency fell by 10 percentage points (pp.), not different from the 12 pp. reduction among those who relocated. A possible explanation for this finding lies on the fact that overall water shortage was relatively low —at 14%— among the treated, regardless of location, before the program was implemented. Between 2008-2018, the government could have invested in water connections, providing relief at a broad scale, and preserving preexisting resemblances. This notion is reinforced if we take into account that other groups displayed similar trends. For example, on the rest of the village —which did not benefit from the intervention, potable water deficiency fell by 6 pp., which would explain half of the overall reduction on the treated.

Unlike the previous case, sewage connection faltered on the rest of the village, relative to the treatment group. While overall waste disposal deficiency fell by 22 p.p. on the rest, it fell by 35 pp. among the treated. The fact that open defecation was utterly high before the intervention (54% among the treated), meant that a higher proportion of the advancement on this component could be attributed to targeted investments on public infrastructure associated with the construction of new neighborhoods.

Comparisons between relocated and in-situ families contextualize the benefits associated with integrated shelter interventions. Specifically, differences in outcomes between two families who were admitted into the program, displaying equivalent baseline characteristics; who ultimately received the same dwelling from the government, thus experiencing the same relief on habitat deficiencies; and who diverge in access to sewage as a result of post-intervention location; indicate the value-added benefits resulting from complementing housing provision with improvements in public infrastructure.

For families who received increasing marginal upgrades (i.e. relief in more than one component), the rising treatment intensity is expected to translate into greater enhancements. In particular, a nonlinear positive relationship between home advancements and outcome variables would indicate positive externalities arising from integrated approaches. Analogously, decreasing, yet positive marginal benefits arising from enabling conditions indicates the existence of specific mechanisms that enhance household welfare through dwelling provision.

A possible concern that emerges when performing calculations based on dwelling location consists of the incidence of specific household traits that favored certain families into the resettled group. This becomes an increasingly feasible concern if we take into consideration that in-situ shelter provision was undertaken during later stages of the program, when the availability of land was scarce. To address this issue, we present baseline habitat attributes for each group, procuring to detect significant discrepancies that would explain endogeneity on program rollover.

Indeed, we can see that, overall, relocated families exhibited less favorable initial living conditions which, according to the rationale proposed in Section 3.5, would signal after-flood necessity for shelter relief, influencing relocation. However, and as suggested earlier, any relative differences between these two groups would be differenced out when performing the difference-in-difference procedure. Furthermore, since relocated houses clearly displayed comparatively higher incidence of substandard housing, any net gains with respect to the in-situ group would understate the benefits originating from extensive habitat provision, further encouraging the adoption of expansive approaches.

Table 3.6. Baseline Housing —Relocated

	(9)	**(10)**	**Diff.**
Ownership: Yes	48.7	20.7	-28.0 ***
Walls: mud, coarse wood, plastic	19.2	29.3	10.1
Floors: Sand	23.1	44.8	21.8 ***
Water: No	11.5	17.2	5.7
Electricity: No	3.8	1.7	-2.1
Toilet: None	30.8	39.7	8.9
Cooking Method: Wood	33	76	42.5 ***
N	**312**	**232**	

After performing estimations under conditionally exogenous specifications (diff-in-diff and or matching), there is no evidence of an effect of variation in sewage connections, resulting from relocation, on health outcomes. Nonetheless, it appears that resettlement had a substantial positive impact on self-reported happiness and habitat satisfaction, casting light on alternative mechanisms that leverage on psychological improvements and responses to a fresh start discussed on Chapter 1.

Table 3.7. Effect of Sewage Connection

Y=Health Shocks		**All**		**Matched**	
	Relocated	Treatment	Combined	Treatment	Combined
Habitat		-1.373 ***	-1.429 ***	-1.377 ***	-1.435 ***
		(0.214)	(0.292)	(0.217)	(0.290)
Relocation	0.148		0.114		0.125
	(0.417)		(0.438)		(0.426)
Index	0.633 **	0.080	0.076	0.342 *	0.338 *
	(0.372)	(0.096)	(0.097)	(0.181)	(0.183)
N	**209**	**798**	**798**	**418**	**418**

Y=Doctor Visits		**All**		**Matched**	
	Relocated	Treatment	Combined	Treatment	Combined
Habitat		-0.992	-1.343	-1.884	-2.206
		(0.902)	(0.944)	(1.468)	(1.444)
Relocation	1.884 **		1.059		0.594
	(0.720)		(1.001)		(1.233)
Index	0.403	-0.796	-0.712	-1.637	-1.614
	(0.406)	(0.641)	(0.707)	(1.083)	(1.235)
N	**209**	**798**	**798**	**418**	**418**

Y=Illness/Disease		**All**		**Matched**	
	Relocated	Treatment	Combined	Treatment	Combined
Habitat		-0.034	-0.049	-0.032	-0.049
		(0.039)	(0.048)	(0.048)	(0.058)
Relocation	0.051		0.060		0.059
	(0.065)		(0.074)		(0.071)
Index	-0.030	-0.011	-0.006	-0.009	-0.002
	(0.033)	(0.019)	(0.019)	(0.027)	(0.026)
N	**209**	**798**	**798**	**418**	**418**

Table 3.7. Effect of Sewage Connection (Continued)

Y=Sick Days		**All**		**Matched**	
	Relocated	Treatment	Combined	Treatment	Combined
Habitat		-2.070	-3.813 **	-2.458	-3.667
		(1.700)	(1.694)	(2.431)	(2.670)
Relocation	2.972		3.714		1.786
	(2.176)		(2.243)		(2.173)
Index	-1.417	-0.584	-0.645	-2.968 **	-2.962 **
	(1.228)	(0.887)	(0.935)	(1.199)	(1.135)
N	**209**	**798**	**798**	**418**	**418**

Y=Life Satisfaction		**All**		**Matched**	
	Relocated	Treatment	Combined	Treatment	Combined
Habitat		0.736 *	-0.336	0.453	-0.614
		(0.434)	(0.413)	(0.486)	(0.475)
Relocation	2.498 **		2.499 ***		2.496 ***
	(0.863)		(0.860)		(0.864)
Index	0.269	0.292 **	0.288 **	0.293	0.289
	(0.265)	(0.129)	(0.128)	(0.185)	(0.185)
N	**209**	**798**	**798**	**418**	**418**

Y=Habitat Satisfaction		**All**		**Matched**	
	Relocated	Treatment	Combined	Treatment	Combined
Habitat		0.319	-0.219	0.289	-0.255
		(0.197)	(0.157)	(0.211)	(0.176)
Relocation	1.304 ***		1.296 ***		1.296 ***
	(0.286)		(0.289)		(0.290)
Index	0.164	0.013	0.017	0.011	0.018
	(0.129)	(0.060)	(0.058)	(0.089)	(0.086)
N	**209**	**798**	**798**	**418**	**418**

Y=Health Perception		**All**		**Matched**	
	Relocated	Treatment	Combined	Treatment	Combined
Habitat		0.333 ***	0.371 ***	0.264 **	0.284 **
		(.107)	(0.085)	(0.129)	(0.110)
Relocation	-0.133		-0.130		-0.133
	(0.220)		(0.217)		(0.220)
Index	0.020	0.076 *	0.068	0.033	0.027
	(0.089)	(0.044)	(0.044)	(0.062)	(0.062)
N	**209**	**798**	**798**	**418**	**418**

3.7.1 *Instrumenting Relocation*

Our objective consists on identifying a plausibly exogenous source of variation on relocation originating from in-situ reconstruction and resettlement in new neighborhoods. To circumvent possible endogeneity associated with the choice to relocate, we proposed an instrument based on preflood residence in order to retrieve the exogenous component of this decision. The rationale for the instrument relies on the fact that flood losses have considerable impact on the likelihood of resettlement in a new neighborhood. Specifically, total loss of the dwelling during the calamity increases the probability of relocating within the intervention by 35 percentage points.

Since flood damage was significantly determined by dwelling location of the dwelling before the flood, we propose the gradient and distance to the most elevated sector in town as a variable that would positively affect the likelihood of resettlement without necessarily being correlated with unobservable attributes that would influence this condition (see Figure 3.6).

Relocation **Flood**

Figure 3.6. Relocation and Flood Damage

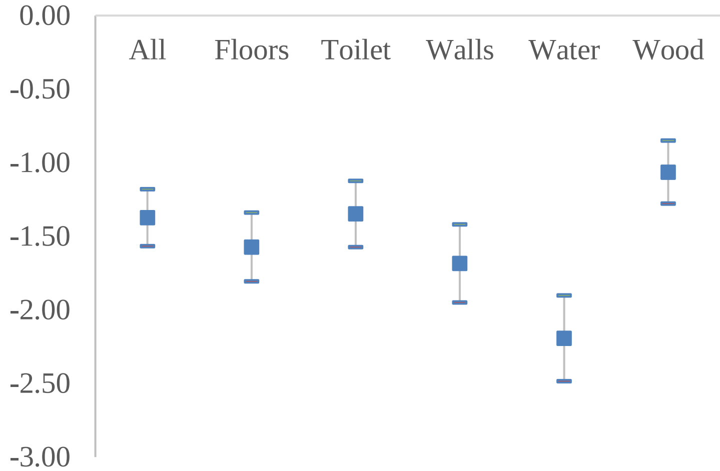
3.8 RESULTS

This section illustrates the results. Initially, for each outcome we present the effects of advanced habitat conditions without distinguishing housing shortages, which is followed by estimations based on specific baseline deficits. In Panel (a) the control group consists of households that did not apply for the program; the second panel, (b), employs observations matched from the former group; in Panel (c) the counterfactual is construed from the control village discussed on the Online Appendix, where no intervention took place.

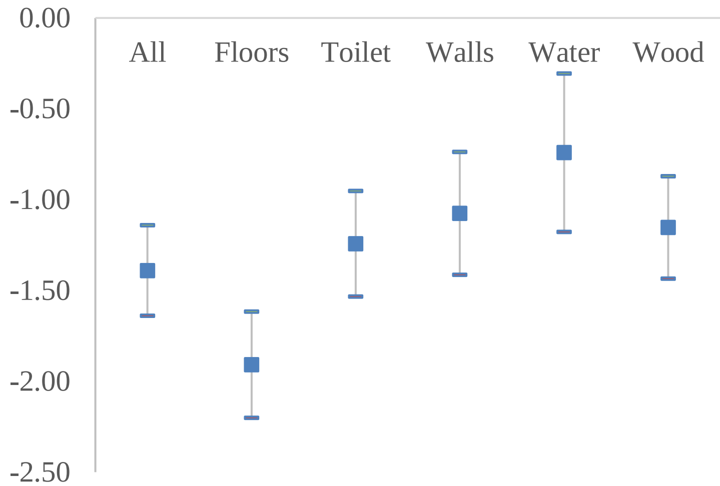
3.8.1 *Health*

Figure 3.7 graphs the confidence interval for each of the health outcomes proposed in Section 3.4. It is clear that shelter provision, through retrofitting upgrades, enhanced health indicators among beneficiary households. Relative to the control group, advanced habitat conditions reduced health shocks across all specifications by 61%, 74% and 74%% respectively. It also reduced the incidence of illnesses and diseases under specification (b). The latter is reflected on the reduction of quarterly doctor visits, which decreased by 28%, 56% and 39%, respectively. These results extend toward the subjective component of overall health, with treated families consistently reporting surges in self-reported satisfaction with life, health and housing aspects. Hence, and consistent with the effects documented in Galiani et al. (2016) and Cattaneo et al. (2009), we also conclude that dwelling provision not only has a positive impact on household health, but a dramatic effect on subjective wellbeing— underlining the psychological benefits resulting from shelter provision.

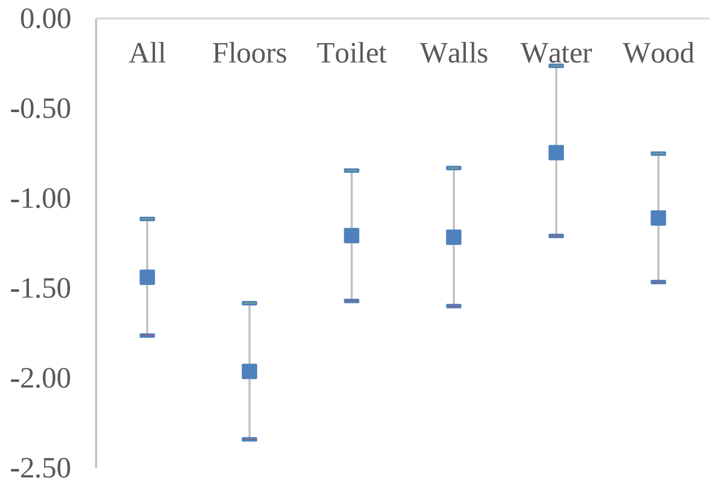
Although the positive effects of infrastructure upgrades are systematic, and statistically significant, across all empirical specifications, they are striking among households experiencing certain baseline shortages. In particular, families initially lacking adequate sanitation, expressed through the absence of any type of toilet or access to potable water, attained the greatest marginal improvements in health and subjective wellbeing indicators. Within this group, for instance, the housing intervention bettered perceived health by 1.6 points (on a 1-10 scale); those experiencing wall shortage displayed a reduction of half a point —albeit not statistically significant— on the same metric. Qualitative deficits relief, especially sanitation shortages, also displayed the most salient reductions on the incidence of illnesses/diseases, as well as quarterly doctor visits. On the other hand, households exhibiting quantitative deficit —walls— often achieved negligible improvements on the realm of outcomes, particularly those related to subjective wellbeing. We interpret this as evidence indicative of increasing returns to habitat provision within applicants experiencing qualitative baseline shortages.



(a) Control Group: Non-Applicants

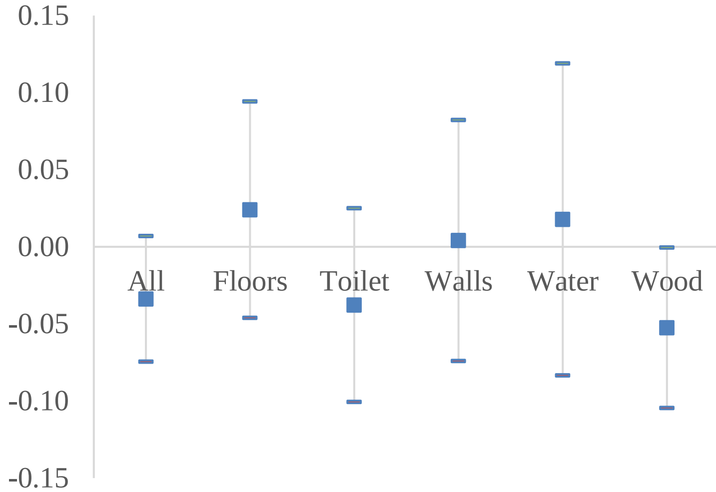


(b) Control Group: Non-Applicants (Matched)

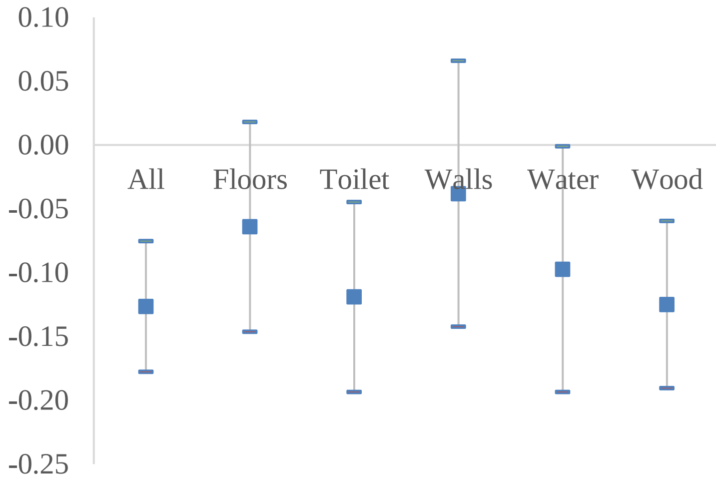


(c) Control Group: Synthetic Control

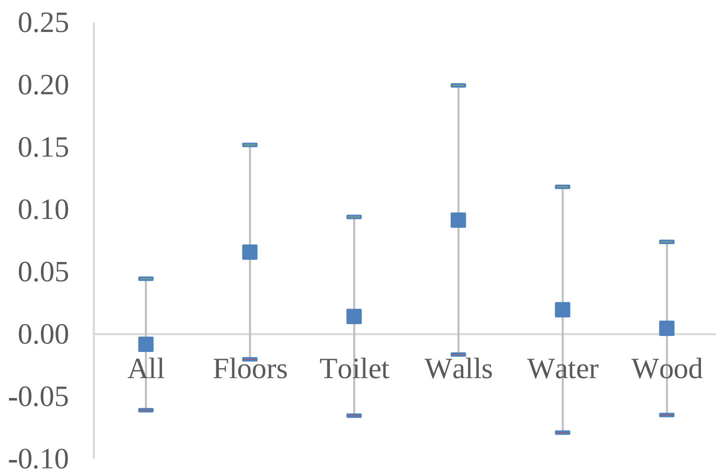
Figure 3.7. Health Shock — Since Moving to New Residence



(a) Control Group: Non-Applicants

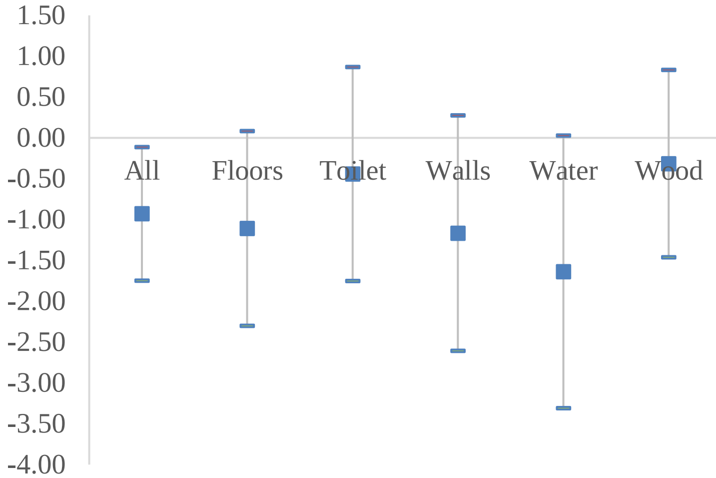


(b) Control Group: Non-Applicants (Matched)

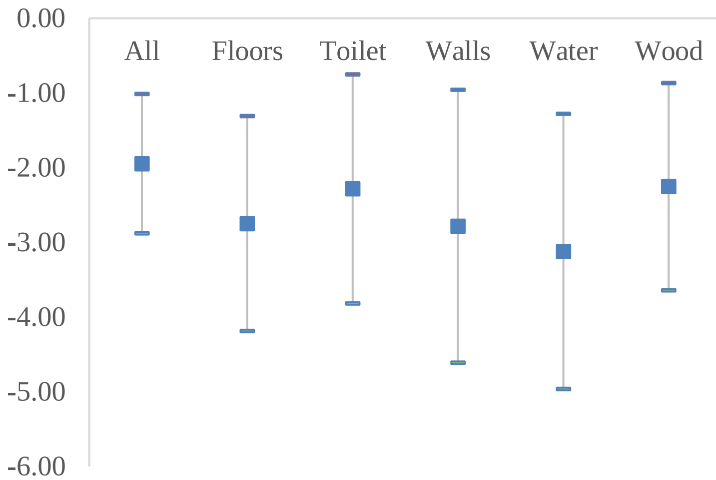


(c) Control Group: Synthetic Control

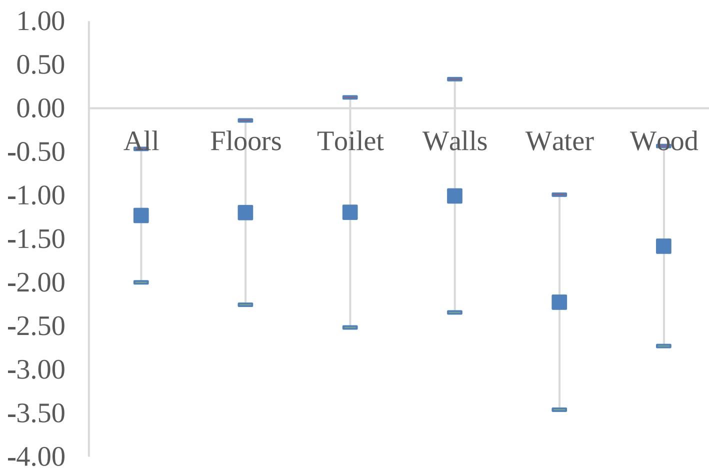
Figure 3.8. Diseases/Illnesses — Past Month



(a) Control Group: Non-Applicants

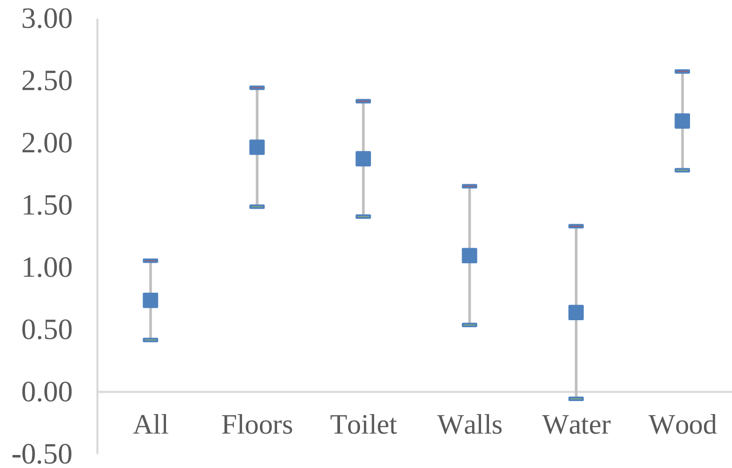


(b) Control Group: Non-Applicants (Matched)

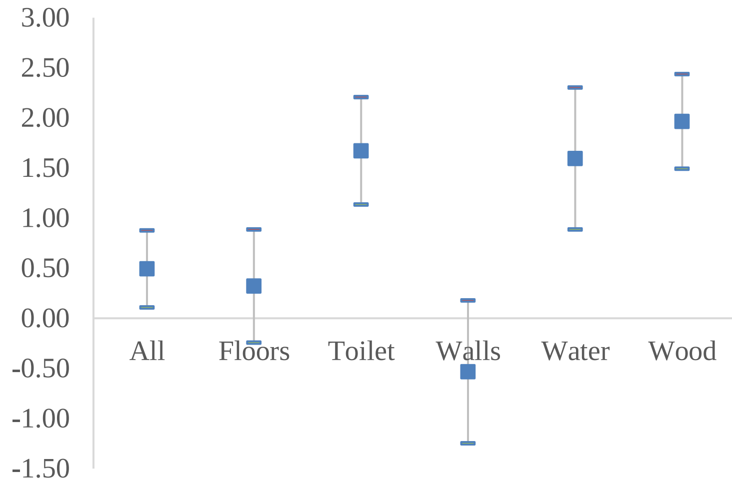


(c) Control Group: Synthetic Control

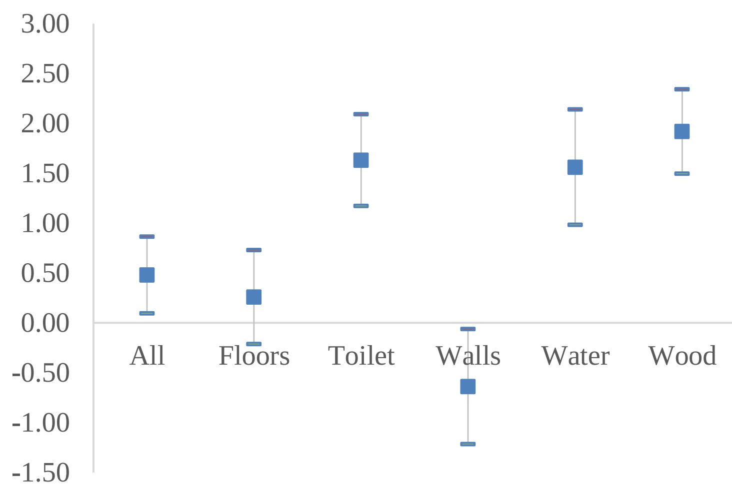
Figure 3.9. Doctor Visits — Last Quarter



(a) Control Group: Non-Applicants

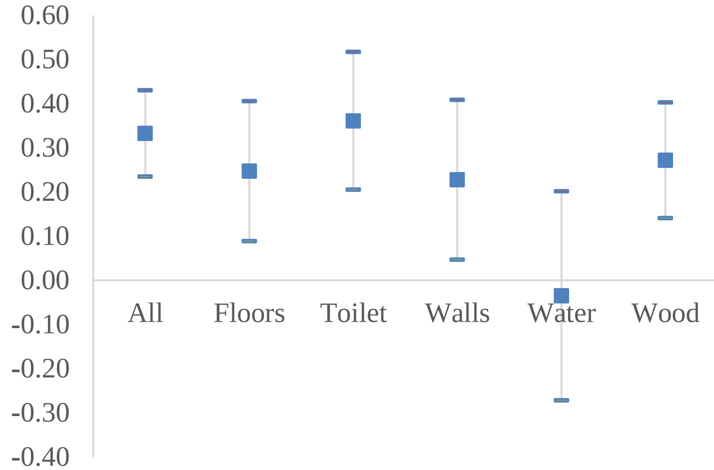


(b) Control Group: Non-Applicants (Matched)

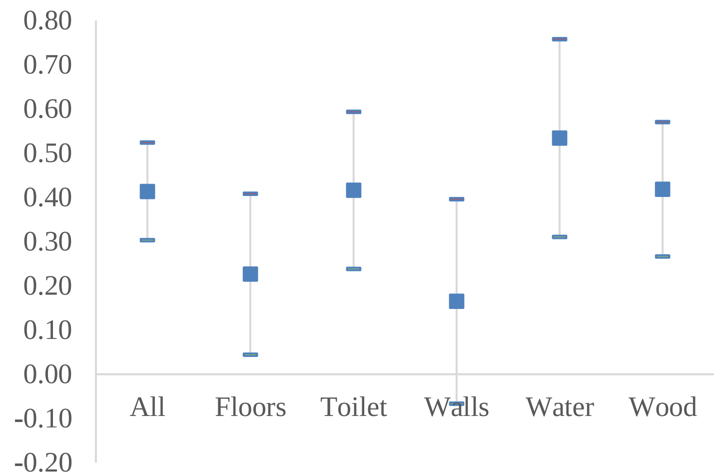


(c) Control Group: Synthetic Control

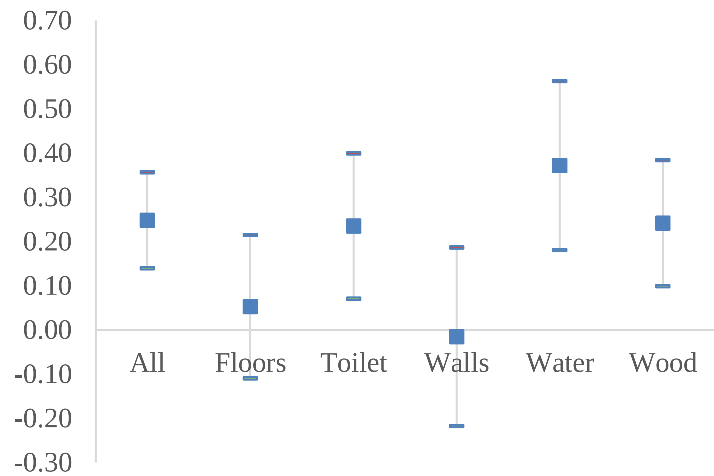
Figure 3.10. Life Satisfaction (2008-2018)



(a) Control Group: Non-Applicants

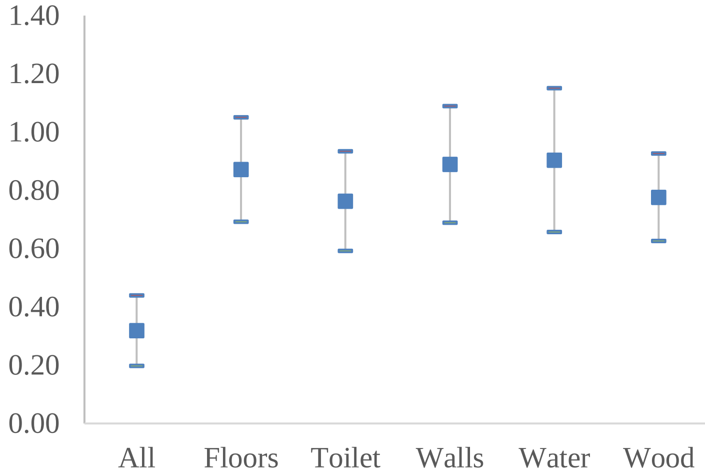


(b) Control Group: Non-Applicants (Matched)

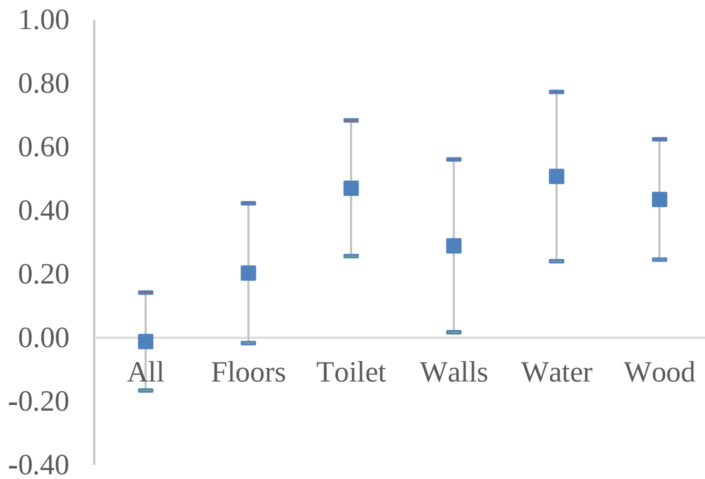


(c) Control Group: Synthetic Control

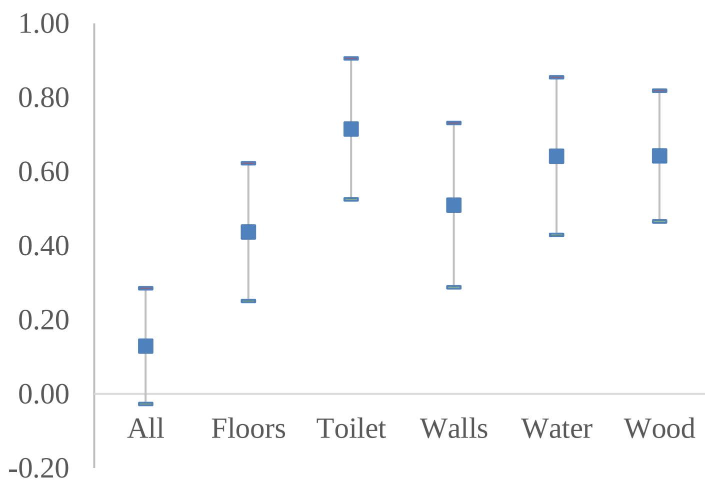
Figure 3.11. Health Satisfaction (2008-2018)



(a) Control Group: Non-Applicants



(b) Control Group: Non-Applicants (Matched)



(c) Control Group: Synthetic Control

Figure 3.12. Habitat Satisfaction (2008-2018)

3.8.2 *Repairs*

The intervention also reduced costs associated with dwelling maintenance. As we showed in Section 3.5, households devote significant resources —both in time and money— into shelter repairs. However, now inhabiting improved units, beneficiary families experience reductions at the extensive and intensive margins: in one hand, we observe a decrease in the likelihood of residential repairs, between 10 and 30 percentage points, depending on the empirical specification. In addition, we find a substitution effect on the type of maintenance, where the treatment diminishes the frequency of annual repairs —which we infer are mostly related to structural interventions— by up to 27 percentage points, compensated with an increase in monthly upkeeps, which we associate primarily with superficial adjustments (see Table 7).

Table 3.8. Effect of Treatment on Dwelling Maintenance

Maintenance	**(a)**		**(b)**		**(c)**	
Repairs	-0.19	***	-0.29	***	-0.10	***
	(0.026)		(0.028)		(0.030)	
Frequency						
Monthly	0.121	***	0.181	***	0.248	***
	(0.033)		(0.042)		(0.044)	
Quarterly	0.042	*	0.049	*	0.030	
	(0.024)		(0.027)		(0.034)	
Semesterly	-0.038		-0.035		-0.010	
	(0.027)		(0.031)		(0.027)	
Annually	-0.125	***	-0.195	***	-0.268	***
	(0.042)		(0.049)		(0.054)	
N	2,092		1,244		1,086	

3.8.3 *Additional Resources*

As the housing program curtailed households' costs, it endowed families with additional resources that could be reassigned in order to improve wellbeing. We examine this possibility in two-stages: first, we estimate the effect of the intervention on health and maintenance costs; next, we predict the reductions on the previous factors and then examine their effect on income and labor supply. According to the results displayed on Table 8, treated households observed a reduction in monthly medical expenditures by an amount that ranges between $3.12 and $3.86. The expected quarterly savings on expenses related to coping with illnesses/diseases range from $23 up to $77, a gain that represents 30% of monthly income among the treated. Monetary gains also include savings between $29 and $52 due to reduced dwelling repairs. In terms of time, we document an average reduction of two days on the total time lost due to the incidence of health shocks, as well as one day resulting from diminished time spent on shelter maintenance.

The second stage reveals strong, statistically significant, positive effects of reductions in health expenditures and maintenance cost on household income. There is no evidence indicating changes in labor supply, suggesting that marginal endowments might have been reassigned toward other components of overall household consumption. Future work on this area includes the assessment of the effect of the intervention on consumption of non-durable goods and access to credit, seeking to determine if the program relaxed preexisting liquidity or credit constraints.

Table 3.9. Additional Resources from Habitat Provision

First Stage: Health and Maintenance Costs

Treatment: Habitat	**(a)**		**(b)**		**(c)**	
Health						
Expenditures (monthly)	-3.12	**	-3.12	***	-3.86	**
	(1.499)		(1.065)		(1.882)	
Cost of illness	-60.27	*	-76.92	*	-23.42	*
	(36.067)		(40.978)		(12.347)	
Days sick	-1.83		-1.91	*	-2.59	**
	(1.239)		(1.142)		(1.206)	
Maintenance						
Time	-11.12	**	-1.10	*	0.56	
	(4.615)		(0.596)		(.0.489)	
Money	-52.22	***	-43.64	***	-29.20	***
	(8.700)		(9.137)		(6.355)	

Second Stage: Effect on Income and Labor Supply

Expected Gains	**(a)**		**(b)**		**(c)**	
Income						
Health Expenditures	45.88	***	41.20	***	22.41	***
	(6.839)		(6.709)		(5.709)	
Income Foregone	2.85	***	2.00	***	8.69	***
	(0.483)		(0.340)		(1.367)	
Shelter Maintenance (Money)	3.69	***	3.13	***	4.29	***
	(0.637)		(0.522)		(0.932)	
Labor Supply						
Days Foregone	0.08		0.11		-0.34	
	(0.217)		(0.214)		(0.209)	
Shelter Maintenance (Time)	-0.05	*	-0.52		0.57	
	(0.024)		(0.320)		(0.682)	

3.9 CONCLUSIONS AND FUTURE WORK

In this paper, we have analyzed returns to habitat in rural Colombia leveraging on a natural experiment where housing was provided to mitigate a large flood. Initial estimates correspond to the effect of retrofitting upgrades across households that experienced specific baseline housing shortages. To do so, we performed comparisons employing conditional exogenous specifications, such as difference-in-difference and or matching, in order to detect the effect of incremental shelter.

Results indicate that the intervention had a positive effect on household wellbeing through an improvement of health outcomes. The latter was particularly robust among families that initially exhibited sanitation deficits, mainly absence of potable water and a toilet connected to sewage system. Based on households' substantial medical expenditures and maintenance costs, we proceeded to examine if the intervention endowed beneficiaries with additional resources. For this purpose, we looked at the effect of reduced expenditures on overall income and labor supply. We found evidence suggestive of a positive income endowment as a result of reduced medical and maintenance costs. Nonetheless, there is no evidence of an impact on time use, expressed through labor supply.

Given the conceivable variation in sewage across groups, we suspected that relocation would positively impact health outcomes. However, after performing an initial analysis employing conditionally exogenous specifications (diff-in-diff and or matching), the latter does not appear to be the case. Since resettlement in new neighborhoods plausibly represents an endogenous decision, we proposed an instrument in order to retrieve the exogenous component of this condition, based on preflood residence. According to the information generated hitherto, the first stage is expected to yield strong correlations between preflood residence and the choice to resettle elsewhere.

Context under the parameters outlined above should enable the development of a case for validity, thus configuring a presumably acceptable instrument.

While there is a concern for a possibly weak instrument, we believe is that instrumentation might be worth pursuing, since IV would offer the cleanest estimation framework, as the control group would have also benefitted from the intervention. Just as the case with other conditional specifications, IV would inform about the expected sign of the treatment effect, bolstering our findings —contingent on consistency with previous results.

3.9.1 *Context*

Future works will focus on elucidating the extent to which relocation endogenous, ascertaining factors that influenced households' decision to move. This involves further characterization of the housing program, establishing if families exercised discretion —or modified their behavior in anticipation of differential treatment— at different stages of the intervention, especially when *in-situ* relocation became a visible alternative.

Contingent on selective sorting around relocation, a change on the relative effect of unobservables causing said assortment at the time of the flood seems unlikely. In addition, instrumentation of the above decision should limit the influence of endogenous factors. However, under the threat of weak instruments, it might be worthwhile to underpin potential controls to be included on the reduced-form estimation in order to curtail possible bias.

3.9.2 *Channel(s)*

After performing estimations under conditionally exogenous specifications (diff-in-diff and or matching), there is no evidence of an effect of sewage connections on health outcomes. We suspect results would not change under an IV framework. As an affirmative measure, I we propose to exploit further variation in the type of toilet (connected to sewage; latrine/sceptic; none) before ruling that there is no impact on health indicators.

Contingent on the latter, we could move forward exploring alternative channels, relation to home ownership. As show on the results, it appears that relocation had a substantial positive effect on self-reported happiness and habitat satisfaction. This casts light on alternative mechanisms, which leverage on the psychological improvements and responses to a fresh start discussed on Chapter 1.

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APPENDIX A

Appendix A1. Housing Shortage Decomposition

Group	Shortage	Count
1	None	236
2	Floor	24
3	Water	32
4	Toilet	28
5	Kitchen	88
6	Walls	8
7	Floor & Water	12
8	Floor & Toilet	0
9	Floor & Kitchen	12
10	Floor & Walls	24
11	Water & Toilet	4
12	Water & Kitchen	4
13	Water & Walls	0
14	Toilet & Kitchen	76
15	Toilet &Walls	0
16	Kitchen & Walls	0
17	Floor & Water & Toilet	0
18	Floor & Water & Kitchen	8
19	Floor & Water & Walls	4
20	Floor & Toilet & Kitchen	24
21	Floor & Toilet & Walls	4
22	Floor & Kitchen & Walls	8
23	Water & Toilet & Kitchen	12
24	Water & Toilet & Walls	0
25	Water & Kitchen & Walls	0
26	Toilet & Kitchen & Walls	0
27	All	4
	Total	**648**

VITA

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