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Essays on development economics

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Contents

Introduction

Recent literature on environmental, health, and labor economics, has found that policy and natural shocks affect people in a heterogeneous manner. In particular, it has shown that shocks received during early childhood affect adulthood education and labor performance outcomes, such as years of completed schooling and income. The research in these fields has also found that, large shocks like hurricanes and earthquakes have a negative effect on vulnerable populations (i.e. rural populations). This dissertation is presented as a compendium of three essays that study the effects of environmental and policy shocks on early childhood physical development and labor supply outcomes.

The first essay investigates how shocks affect the early stages of life. The main question addressed is how children's physical development is affected by environmental shocks during gestation and first year of life. To answer this question, the chapter looks at the effects of tropical storms (natural shocks) occurring during gestation and first two years after birth on anthropometric measurements. As a potential transmission channel, the chapter studies the effects of tropical storms on the agricultural production. The potential transmission channel discussed is that a negative shock on agricultural production can affect the source of income or food produced for the consumption of expectant mothers, thereby affecting their nourishment. Also, following the discussion presented by the literature, it is possible that for strong storms the levels of stress suffered by expectant mothers affect the correct development of the fetus. Both channels can explain the results found. These transmission channels are particularly interesting in the context of Jamaica. The country's geographic location makes it susceptible to tropical storms. At the same time its economy is highly sustained by tourism and agricultural and mineral commodity trade, which increase the income generation and food security vulnerable to storm related shocks.

The essay builds on the literature examining the effects of policies and shocks on early childhood on adult performance in labor, education, and health. Almond (2006) studies the effects of the influenza pandemic on adult outcomes such as income and socioeconomic position. Imberman et al. (2012) looks at the effects of hurricane Katrina on children's school performance and Wilde et al. (2014) investigate the effect of heat waves on health and school performance of children conceived at the time of these events. Other studies related to shocks from natural disasters like hurricanes and earthquakes include Sotomayor (2013), Hoddinott and Kinsey (2001), Baez and Santos (2007), Frankenberg et al. (2013) and Currie and Rossin-Slater (2013). Still other literature focuses on the effects of exposure to alcohol in pregnancy (Von Hinke Kessler Scholder et al. (2014)), religion (Schultz-Nielsen et al. (2014)), migration (Lavy et al. (2016)), economic hardship (Kiernan and Huerta (2008)):, Gutierrez (2013) and Case et al. (2002)) among others. The common factor in the literature is that stressful events suffered by the mother, culture, and health related habits can generate negative impacts in their children through reduction of their own nutrition and the effects of cortisol in the fetus.

This chapter investigates the "fetal origins" hypothesis using information on date of birth for children ages 0 to 59 months old. Using a Difference-in-Differences approach, we find that the effect of storms which did not reach the hurricane status is positive on the Weight-for-Age z-score during gestation, but stronger events could harm children's development. Children living in coastal-rural areas hit by small storms during their second trimester of gestation would attain 1.2 SD more in Weight-for-Age score. In the other hand, this indicator has a reduction of 1.75 SD when children are hit by hurricanes in their third quarter of gestation.

In the second essay, I study the effects of environmental shocks on the labor market. In particular, I address the question if climate-related shocks affecting the risk of formally employed men moving to the informal sector. Are those men working in formal sector at risk of falling into informality due to damage from tropical storms on small Caribbean economies? These questions are of extreme importance for these development countries that are annually stricken by tropical storms or hurricanes. The proposed discussion takes the approach of Acevedo (2015) where individuals change behavior in labour supply to cope with the negative effects of weather shocks as adaptation mechanism to smooth consumption.

There is no evidence in the literature about how individual's decisions to work in the informal market are affected by natural disasters. The evidence on how the allocation of time is affected by weather shocks has been recently studied using exogenous variation in time and geographical level, principally using data extracted from rainfall (Acevedo (2015)) or temperature (Zivin and Neidell (2014)). These authors have found that floods are associated with an increase in unemployment (result biased against women) and a reduction in labor income. On the other hand, an increase in temperatures is associated with a reduction in labor supply. More related to tropical storms is Belasen and Polachek (2008) study on hurricane affected workers in the state of Florida. In this research, authors found that earnings are positively impacted with respect to the hurricane category and that, on the contrary, employment is negatively affected. Using data from Mexico Rodríguez-Oreggia (2013) found that the potential destruction due to hurricanes might increase employment rates, mainly on blue-collar jobs related to reconstruction. Spencer and Polachek (2015), using data on crop production, found that there is a negative relationship between agricultural productivity and hurricanes, which is more pronounced for crops above ground. This evidence supports the negative effect of weather shocks and elements of the general equilibrium of labor markets in developing economies. However, no evidence has been found on the effect on transitions patterns from formal labor markets.

This research also is in accordance with the literature on labor supply. The informal sector has been studied widely in the Latin American and Caribbean region. In particular, the efforts have been concentrated on measuring its size using approaches such as those mentioned by Alderslade et al. (2006) including: Electricity consumption (Basbay et al. (2016)), currency demand (Kamau and Lin (2016)), nightlight (Ghosh et al. (2009)) and micro-data (Gasparini and Tornarolli (2007)). The intention of this set of studies is to shed light on the size of the informal sector: however, as Caldera Sánchez et al. (2011) remind us, more research with better data is needed. Although substantial effort has been dedicated to measuring the informal sector, not much has been done to characterize individuals that constitute the sector or how external factors (like weather shocks) may affect their decision to remain in, or to switch to, formality. The latter is of particular interest since it will provide information to help design public policy to prevent potential switches between sectors or, in the best-case scenario, help individuals from the informal sector to transition to the formality. This essay helps to narrow the evidence gap of the effects negative shocks (weather shocks) on the decision that workers take regarding their formality status.

This essay contributes to the "New Climate-Economy Literature" (Dell et al. (2013)) in at least two ways. Firstly, it introduces a novel dataset that contains individuals' labor force information in a pseudo-panel structure, combined with tropical storm data collected from their original Geographical Information System (GIS) sources. The latter contains various geographic and physical characteristics, not only from the storms per se, but also from the topographical information of Jamaica. In addition to this data structure, I introduce an endogenous switching model to disentangle causality from tropical storms estimated trough maximum likelihood (multivariate probit estimation). This method departs from the standard analysis of environmental shocks (Dell et al. (2013)) to study labor market outcome in this literature.

As expected, an increase in the probability of falling into informality is positively associated with the hurricanes' strength. I find there is a quadratic relationship between the storms' accumulated strength and the probability of remaining informal and to fall into informality if the individual was initially formally employed. The relation depicts an exponential pattern where small tropical storms and tropical depressions have almost zero or negative effect, but when storms passed the threshold to be defined as hurricanes, the probability associated with the storms' accumulative strength is positive. The probability of remaining in informality ranges between 1 percent and 11 percent, and the probability of falling into informality when that individual was formally employed in previous period goes from 1 percent to 12 percent. These effects have been considered as lower bounds of the true effect, since the relationship between the destruction variable and the actual probability is not perfect. Findings suggest a further discussion on how to protect not only informal population, but how to support those formally employed so that they do not fall into the informality. Although not explored in depth here, it is possible that the hurricanes affect the probability of switching through their effect on the general equilibrium components of the labor market: physical capital destruction, crop demise, and floods. There is not enough data to understand and test the transmission channel, however the extension of this essay includes the exploration of the heterogeneous effect on the industrial sector, which sheds some light on the actual transmission channels. The Jamaican context is an excellent laboratory to study informality behavior since this issue is pervasive in the economy and its geo-location makes the country vulnerable to a different class of storms.

The third essay studies the impact of a health universalization policy on labor supply. According to the literature, such policies can function as contingency measures to reduce the risk people may suffer from negative health shocks. We present evidence on the effects of free public healthcare arising from a context not previously studied. Indeed, the Jamaican no-user-fee policy and context differs from previous studies in several aspects. First, the policy did not have any demographic targeting mechanism. This allows for investigation of the health effects on the economically active population (21-64 years old), which contrasts with previous studies focusing either on children (Tanaka (2014)) or the elderly (Card et al. (2009); Shigeoka (2014)), due to policy design. Second, the policy did not include targeting mechanisms related to employment or formality status. As such, incentives to switch from the formal to the informal sector to benefit from the policy were absent. Third, employer-sponsored health insurance in Jamaica is optional and limited. As a result, motivation to participate in the labor force is presumably unrelated to a pure motivation to access affordable health insurance. In other words, “employment lock” is unlikely to exist. To the extent of our knowledge, this study is the first assessing the effects of free public healthcare on health outcomes and labor market dynamics among the economically active population in the absence of both incentives to become informal and “employment lock”.

Disentangling causality between the policy and health or labor market outcomes is problematic. This problem exists because before and after comparisons would confound pre-existing trends with the program’s effect. Therefore, to disentangle causality from secular trends, we used data from two household level surveys: the Jamaica Labor Force Survey and the Survey of Living Conditions. We stacked yearly waves of these surveys from 2002 until 2012 as a district level panel. Then, we implemented a difference-in-differences strategy controlling for time invariant unobservable characteristics at the district level and exploiting two sources of variation. The first source is the timing of the policy enactment (i.e., before vs. after policy adoption), while the second is the cross-sectional individual level variation in the availability of health insurance (i.e., individuals without vs. individuals with access to formal health insurance).

Our main findings suggest a reduced likelihood of suffering illnesses associated with inability to carry out normal activities equivalent to two percentage points (or 28.6 percent with respect to the baseline mean). At the intensive margin, we find that the number of days where people were unable to perform normal activities due to illnesses suffered within the previous four weeks decreased by 0.17 days (equivalent to 34 percent with respect to the baseline mean). Therefore, there is evidence that the policy increased the general health of the population and, as suggested by Strauss and Thomas (1998), this could have translated into increased labor supply.

These three essays investigate the effects of shocks on humans. As described, variation from environmental shocks in terms of geographical, time, and intensity as well as variation in timing of policy implementation are exploited to test how some outcomes related to social welfare are affected. The investigation is possible thanks to the characteristics of Jamaica, a country-island located in the Atlantic Basin that is surrounded by Cuba to the North, Haiti and Dominican Republic to the East and Belize, Honduras and Caiman Islands to the West.

With a population of 2.7 million and a per capita GDP of US$8,872, Jamaica is a middle-income, small island economy.[1] Located in the Caribbean hurricane belt, Jamaica has averaged two storm events per year between 1990 and 2012. The frequency of storm events seem to be increasing, as there were 29 events in the 2000s compared to 11 in the 1990s.

Jamaica is the third largest island in the Caribbean after Cuba and Hispaniola. The country has a varied topography, comprising the eastern mountains, with maximum elevation of 7,402 feet, the central valleys and plateaus, and the coastal plains, where most of the population resides. The urban population comprised 53.9 percent of the total in 2011, compared to 49.7 percent in 1991 and 52.1 percent in 2001.

Historically, Jamaica's economic growth has been slower than that of other countries in the region. Average annual economic growth over the observation period 1993-2012 in Jamaica was 0.54 percent, compared to 3.2 percent for the Latin America and the Caribbean region. The period includes three periods of economic contraction, 1996-98, 2008-10, and 2012.

Unemployment over the whole period averaged 13.5 percent, peaking at 16.5 percent in 1997 and falling below 10 percent only in 2006 and 2007 (9.6 and 9.4 percent, respectively). The headcount ratio of poverty has declined from a peak of over 40 percent in 1991 to 9.9 percent in 2007. The recession accompanying the world economic downturn led to a renewed increase to 19.9 percent in 2012.

Jamaica's economy is highly dependent on services, particularly tourism, for which the country is famous. The contribution of agriculture to total GDP has been low for

[1] Population is from 2011 Jamaica census, GDP is 2015 per capita purchasing power parity (PPP) from the World Development Indicators.

an extended period, at 8.3 percent in the 1990s, further declining to 6.8 percent by 2012. Over the same period, industry's contribution to GDP declined from 37 to 21 percent of total GDP, while services increased from 55 to over 70 percent.

Agriculture remains an important sector, however, as it employs a disproportionate share of the labour force, especially in rural areas. In 2012, agriculture employed on average 16.7 percent of the classifiable workforce, second only to wholesale and retail repair of motor vehicle and equipment, which averaged 20 percent. These characteristics make the country vulnerable to the present and future challenges, in physical and social terms, that the climate change will impose.

Chapter 1

The Effects of Tropical Storms and Hurricanes during Pregnancy on Early Childhood Development: Evidence from Jamaica[1]

Abstract.

This study analyzes the effects of exposure to hurricanes and tropical storms during pregnancy on children's anthropometric measures taken within the first five years of life. It combines destruction indexes at the district level with 13 yearly rounds of household level surveys from Jamaica. The empirical strategy exploits variation arising from the storms' timing and intensity across different cohorts within the same district. The findings suggest that when expectant mothers living in coastal-rural areas are affected by at least two hurricanes, their children are 56 percentage points more likely to show low birth weight. Furthermore, these children also experience negative impacts on anthropometric measures taken within the first five years of life equivalent to 1.88 standard deviations in weight-for-age and 1.4 standard deviations in weight-for-height.

JEL classification: I12, J13, O15, Q54.

Key words: Jamaica, anthropometric measurements, tropical storms.

[1]This paper is co-authored with Diether Beuermann.

1.1 Introduction.

This study analyses the effects of weather shocks suffered during pregnancy on humans' early physical development. The study provides evidence related to the "fetal origins" hypothesis pioneered by Barker (1990) focusing on the effects of tropical storms and hurricanes that occur during gestation periods on anthropometric measurements taken during the first five years of life.[2] We exploit anthropometric measures taken from children born between 1988 and 2012 in Jamaica along with geocoded information of tropical storms occurred during their pregnancy periods.

This paper is related to the literature on the socioeconomic effects of natural disasters like hurricanes and earthquakes (Baez and Santos (2007); Caruso and Miller (2015); Currie and Rossin-Slater (2013); Frankenberg et al. (2013); Hoddinott and Kinsey (2001); Imberman et al. (2012); Sotomayor (2013)). Our study is also related to several papers that explore the effects of different situations experienced during pregnancy on children's outcomes. See Case et al. (2002), Gutierrez (2013), and Kiernan and Huerta (2008) for economic hardship; Von Hinke Kessler Scholder et al. (2014) for exposure to alcohol; and Schultz-Nielsen et al. (2014) for nutritional deficiency. More generally, our study is related to the set of studies exploring the medium- and long-term consequences of shocks suffered during early stages of development. Almond and Currie (2011) and Almond et al. (2017) provide comprehensive reviews of these studies. The common factor in the literature is that stressful events suffered during early stages of development generate negative impacts over the short, medium, and long term.

A strand of this literature focuses on the stress suffered by expectant mothers and its effect on in-utero development. Agüero (2014) and Hu and Li (2016a) investigate the effects of high temperatures on birth weight and adult height; Almond and Mazumder (2011) study the effects of nutritional deprivation during Ramadan on birth weight. Camacho (2008) and Glynn et al. (2001) study the effects of acts

[2] The hypothesis argues that the intra-uterine environment (especially nutrition) "programs" the fetus to have specific metabolic characteristics, which can lead to future disease.

of terrorism and natural disasters as sources of mothers' stress on birth weight; and Lavy et al. (2016) focus on stressful migration episodes. Overall, these studies find negative effects that could last throughout the children's lives.

We contribute to the international literature investigating the effects of tropical storms and hurricanes suffered during pregnancy on early childhood physical development. Our strategy allows an exploration of the possibility of nonlinear effects with respect to the intensity of the destruction suffered during pregnancy. To do so, we use a large sample of storms that affect the North Atlantic region, accompanied by hurricanes between categories 1 and 2 and tropical depressions and storms (which are weaker than hurricanes but that are accompanied by more precipitation). To our knowledge, this is the first study exploring these issues in a country in the Caribbean, a region that is exposed to recurrent weather shocks.

Our findings suggest that expectant mothers living in coastal-rural areas exposed to an average of at least two hurricanes during their second trimester of gestation are 56 percentage points more likely to deliver a baby with low birth weight (i.e., below 2.5 kg). In addition, this exposure during the third trimester of gestation causes a reduction in children's weight-for-age -ZWA- (weight-for-height -ZWH-) measured within the first five years of life, equivalent to 1.88 (1.4) standard deviations. Considering the well-documented deleterious effects of low birth weight on educational and labour market outcomes (Black et al. (2007); Currie and Moretti (2007); Oreopoulos et al. (2008); and Royer (2009)) our findings highlight the importance of having appropriate safety nets in place to assist expectant mothers who experience such events. Taking the findings in Black et al. (2007) as a benchmark, our estimates imply that Jamaican children exposed to two or more hurricanes during their mother's pregnancy will be 2.76 percent less likely to graduate from high school, will have 2.76 percent lower IQ (for boys), and will have 2.07 percent lower earnings in adulthood compared to similar children who did not experience these shocks while in utero.

The remainder of the document is organised as follows. Section 2 provides a brief background on the Jamaican economy. Section 3 describes the data, while Section

4 shows the construction of the destruction measures based on the storms' physical characteristics. The empirical strategy is developed in Section 5. Results are discussed in Section 6, Section 7 analyses the robustness of our results, and Section 8 concludes.

1.2 Country Background

With a population of 2.7 million and a per capita GDP of US$8,872, Jamaica is a middle-income, small island economy.[3] Located in the Caribbean hurricane belt, Jamaica has averaged two storm events per year between 1990 and 2012. The frequency of storm events seem to be increasing, as there were 29 events in the 2000s compared to 11 in the 1990s.

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1.3 Data.

The data for our study are derived from two sources. First, we use the yearly rounds of Jamaica's Survey of Living Conditions (SLC) from 1993 to 2012.[4] The SLC is a nationally representative survey executed every year on a sub-sample of households interviewed in the second quarter of the Labour Force Survey (the "April LFS"). The SLC contains information on individuals' sociodemographic characteristics, district of residence, and a detailed module for children under five which collects birth weight, anthropometric measures, and vaccination status.[5] Appendix 1 contains detailed descriptions of the LFS and the SLC designs.

Using the SLC collected data on height, weight, age, and the World Health Organization (WHO) z-scores tables, we calculated standardised measures of weight-for-height, weight-for-age, and height-for-age. The total sample size is roughly 14,000 children under 60 months of age after selecting only individuals with standardised scores between -5 and 5. Of these, 2,569 reside in the coastal-rural region.

Tropical storms particularly affect households located in the coastal-rural region in two ways. First, poverty rates are higher in rural areas, and second, proximity to the coast makes them particularly vulnerable to storms coming from the ocean. Their lack of consumption-smoothing mechanisms (precautionary savings and/or access to

[4]We did not use the 1994, 1995, 2003, 2005, and 2009 rounds as they did not include the anthropometric module. Since 2001 and 2011 were census years, the SLC was not executed.

[5]The April LFS execution period is between April and June. The SLC execution period regularly goes from June to November visiting a nationally representative subsample of the April LFS.

sources of finance), coupled with their dependence on natural resources, generate an increasing exposure to risk and an inability to cope with it, as Hallagate et al. (2015) suggest.

Table 1.1 presents descriptive statistics for both the complete and the coastal-rural samples. The coastal-rural area differs from the complete sample in some respects. Children from the coastal-rural area live in households with fewer members and with heads who are less likely to have tertiary education. In terms of outcomes, while the average birth weight of children living in the coastal-rural area is similar to that of the full sample, the incidence of low birth weight is higher in coastal-rural areas. Children in Jamaica on average appear to have higher birth weight than the world average, as their anthropometric z-scores are higher by approximately 0.20 standard deviations. However, the complete sample has better weight-for height and worse height-for-age than children living in coastal-rural areas. Vaccination rates of children residing in coastal-rural areas and the full sample are the same.

Our second data source is the International Best Track Archive for Climate Stewardship (IBTrACS), managed by the National Oceanic and Atmospheric Administration (NOAA). This dataset contains information on every tropical storm between 1969 and 2014, including date, trajectory, maximum sustained wind, radius of maximum speed, and minimum central pressure (mb). This information is collected every six hours during the storm's lifespan and will be used to build the wind field model that is the basis for the calculation of destruction measures. Appendix Table B1 shows the dates of the storms included in our analyses, the maximum wind speed, and the category of the storm (Saffir-Simpson Scale).

1.3.1 Wind Filed Model and Storm destruction variable.

Following Strobl (2012), who based his analysis on Boose et al. (2004), we calculated an approximation of the storms' local wind speed in every district in Jamaica. The wind field model is based on the model suggested by Holland (1980) for cyclostrophic wind and sustained wind speed as follows:

$$V_{d,s,r} = GF\left[V_m - S(1-\sin(T))\frac{V_h}{2}\right]\left[\left(\frac{R_m}{R}\right)^B \exp\left(1-\left[\frac{R_m}{R}\right]^B\right)\right]^{1/2} \tag{1.1}$$

where $V_{d,s,r}$ is the estimate of storm s wind speed, in district d, within a period of time r. G is the gust factor, F is the surface friction, V_m is the maximum sustained wind velocity that the storm reaches at any point, S is the asymmetry due to forward motion of the storm, T is the clockwise angle between the storm's forward path and the ray between the storm's center and the district's centroid d, V_h is the forward storm's speed, R_m is the radius of maximum winds, R is the length of the ray that connects the storm's center and the district's centroid d, and B is the shape of the wind profile curve-scaling parameter.[6]

Here we depart from the Strobl (2012) destruction measure. The author proposes an index that uses a set of weights to consider local characteristics like population growth. We are not interested, as Strobl (2012) is, in whether storms destroy productive capital, since we are exploiting variation at the district level, while he adds up the entire destruction at the national level to perform comparisons across time/country. Therefore, we calculate the destruction measure at the district level as follows:

$$WIND_{d,s} = \int_t^\tau V_{d,s,r}^{3.8} dr \tag{1.2}$$

where $WIND_{s,d}$ is the destruction measure estimated for storm s within district d that is equal to the summation of the values of wind field to some power. Then, for each six-hour observation, we estimate the wind field model $V_{s,d,r}$ restricted to districts that are between 0 and 500 km away from the storm.[7] The 3.8^{th} power used for the wind model follows the relation found by Strobl (2012) between total costs due to hurricanes and the maximum observed wind speeds of the hurricane.[8] To match the

[6]F, S, and B parameters were taken from Strobl (2012) and Boose et al. (2004).

[7]As mentioned in Strobl (2012), this assumption relies on the fact that major storms can reach a diameter of 1000 km.

[8]To extended explanation about the power parameter used, see Strobl (2012).

total $WIND_{s,d}$ that a fetus receives in each trimester of gestation, we use information on birth and storm dates. Information on the date of birth is contained in the SLC, and the NOAA IBTrACS storm data contains the date on the event. Therefore, we determine the total exposure of the child at each moment between the first and the third trimester of gestation.[9] The strategy to match children and storms is as follows:

Step 1. Using child i's date of birth (D_i^b) and the date of storm's observation (D_s) we know if the individual was hit in a determined period as follows:

1. If $D_s - D_i^b \in [-270, -181]$ days, individual was hit in the first trimester of gestation;
2. If $D_s - D_i^b \in [-180, -91]$ days, individual was hit in the second trimester of gestation;
3. If $D_s - D_i^b \in [-90, 0]$ days, individual was hit in the third trimester of gestation;

Therefore, we define a group of indicator variables denoting if each child i was affected by any storm during each gestation period.

Step 2. The indicators created above have a value one if a storm s hit child i in period of gestation p; while zero otherwise. We then calculate the destructive power received by child i during gestation period p coming from storm s, by multiplying the correspondent destruction variable for storm s ($WIND_{s,d}$) times the indicator correspondent to child i, living in district d, during gestation period p. Finally, we add the destructive power of all the storms over period p which hit each child i. For example, if a child was hit by four different storms in her first trimester in womb, the value of the destructive power received for that trimester is the total wind received resulting from adding the four individual destruction measures.

[9] We do not have information about the exact amount of time that the individual was in utero, so we assume that all of them were born full term (i.e., at 270 days of gestation).

1.4 Methodology

Our main question is whether environmental conditions affect children's physical development when their mothers are exposed to different combinations of tropical storms during the gestation period. To disentangle causality between environmental shocks and health outcomes, we use a destruction variable created from the storms' physical characteristics (wind speed, distance to district, trajectory, etc.), that are random and exogenous events in intensity, trajectory, and life span. We define the outcome variable as standardised versions (using WHO tables) of anthropometric measurements such as weight-for-height, weight-for-age, and height-for-age as well as birth weight (in kg) and the likelihood of being born with low birth weight (below 2.5 kg).

Following Dell, Olken, and Jones (2014), the main econometric model is as follows:

$$Z_{i,d,c} = \delta_d + \delta_c + \delta_s + \delta_d \cdot TREND + \sum_{p=1}^{3} \sum_{y=1}^{3} [\beta_{y,p} \cdot Q^{y}_{i,d,c,p}] + \mathbf{X}'\boldsymbol{\gamma} + \epsilon_{i,d,c} \tag{1.3}$$

where $Z_{i,d,c}$ is the outcome for child i born in district d who belongs to cohort c (birth month-year). δ_d and δ_c are fixed effects at the district and cohort (birth year-month) level, $\delta_d \cdot TREND$ is a district linear trend on children's birth year that absorbs long-term linear trends in the outcome that can vary depending on the district. δ_s is a fixed effect of the survey year. $Q^{\tau}_{i,d,c,p}$ is the total amount of destructive power ($WIND$) that child i, born in district d, who belongs to cohort c received during gestation trimester p elevated to the power y. Lastly, $\mathbf{X}$ is a vector of household and individual sociodemographic controls including household head's education, age, and gender, household size, number of individuals in household ages 0-5, 6-14, 15-24, and 25-49, and child's age and gender. Under this framework, estimates of $\beta_{1,p}$, $\beta_{2,p}$ and $\beta_{3,p}$ for $p \in \{1, 2, 3\}$ capture the relationship between destructive power received during gestation trimester p and the outcomes of interest.

Model (1.3) controls for all time-invariant unobserved characteristics at the district level through the district fixed effects. The cohort fixed effects control for seasonal patterns, ameliorating potential selection bias that mothers could have with respect to the timing of pregnancy decisions. The year of survey fixed effects controls for non-observable characteristics that might have affected children's measurement processes within each survey round. Lastly, time-variant characteristics at the district level are controlled through the inclusion of differential linear time trends by district.

Following the literature and the continuous nature of treatment, we allow a non-linear relationship between the destruction measure and the outcomes of interest. Maccini and Yang (2009) found a positive relationship between rainfall and some welfare-related outcomes when evaluating the effect of that shock on the first year of life. When the shock is large, like the terrorist attacks studied by Camacho (2008), the effect is negative. However, when the event is of much higher magnitude, like the tsunami in Indonesia studied by Frankenberg et al. (2013), the effect of the aid received in the aftermath of the event could push the outcome variable up, given that aid may offset the negative impact of the event. Therefore, to capture the potential nonlinearity of effects, we consider a third-degree polynomial of the destruction measure.

An important aspect of this model is the necessity of treatment variation within the same cohorts. Following Cummins (2015), there is a potential bias that affects estimation when cross-sectional data are used to estimate effects of certain treatments that are applied at the cohort level. The dependency and seasonality of the age-at-measurement given the potential correlation of the treatment with the specific cohort generate the problem. The author claims that this bias can potentially be avoided if there is variation in the treatment within the same cohort so that it is possible to disentangle the cohort effect from the treatment effect. As mentioned before, variation of district-level destruction measures within the same cohorts allows us to distinguish storm effects form cohort effects.

Potential sources of bias could arise from selection and migration. It is possible that parents self-select to give birth in the first six months of the year, when there are usually no tropical storms. The second source of selection bias is the potential effect of tropical storms on survival rates such that the resulting observed sample comprises individuals with different potential outcomes than those who were not observed because they died before the survey date. The third source of selection bias is potential migration due to storms. The final source is the assumption that the district of birth was equivalent to the district where the mothers lived during pregnancy. If the mother's location during pregnancy was different from the place of birth used to impute the district-level destruction measures, then our results could be biased.

1.5 Results.

Our results not only report point estimates, but also primarily explore the existence of heterogeneous effects with respect to the intensity of exposure. To do so, we evaluate the cubic polynomial of the estimated impact parameters from model (3) at different values of the destruction measure received within each trimester of gestation. As such, potential nonlinear relationships between total exposure to storms and outcomes of interest are presented.

The most common weather events in the Caribbean are tropical depressions and storms. Within our study period, 75 percent of the events were either depressions or storms. These events are less destructive than hurricanes in terms of wind power and gust factor. Tropical storms and depressions are characterised by a increase in rainfall but generally do not cause serious damage to crop production or road infrastructure.

Our study period contains some of the most destructive events ever registered in the Caribbean Basin. Twenty-five percent of the events are category 1 and 2 hurricanes on the Saffir-Simpson scale. Some of the most renowned hurricanes are Gilbert in 1988, Gustav in 2008, Ivan in 2004, and Mitch in 1998. These events had devastating impacts on infrastructure. These include destruction of road infrastructure, which severely hampered rapid emergency response; increased housing shortages due to reduction of inhabitable units; reduced access to energy and water; and increased food insecurity due to crop destruction.

1.6 Effects of Tropical Storms

We start assessing the estimated impacts of the average tropical storm (excluding hurricanes) that occurred within each trimester of gestation. Table 1.2 shows the estimated impacts for the full national sample suggesting the absence of effects. However, when focusing on the coastal-rural population in Table 1.3, we observe some mild positive effects of storms occurring during the second trimester of pregnancy equivalent to 0.04 kg in birth weight but no effects on the likelihood of low birth weight. In addition, we also observe positive effects on both weight-for-height and weight-for-age standardised scores measured within the first 60 months of life. These are equivalent to 0.08 (0.07) and 0.06 (0.06) standard deviations in ZWH (ZWA) following storms experienced within the second and third trimesters of pregnancy, respectively.

Therefore, we observe some positive effects on children that were hit by tropical storms in their second and third trimesters in-womb. This suggests that, when exposed to events low in power (like tropical depressions), some benefits could come with the extra rainfall fostering agricultural output. This could be translated into improved nutrition during pregnancy (either through an income or own production effect). However, the lack of data prevents us from testing and disentangling these possible transmission channels directly.[10]

1.7 Effects of an Average Hurricane

In this section, we document the estimated effects of an average storm that included one hurricane. Table 1.4 shows the estimated effects for the full national sample suggesting no discernible impacts. However, when focusing on the coastal-rural sample in Table 1.5, estimates suggest some positive effects of being exposed to an average hurricane during the second trimester of gestation. These effects are equivalent to 0.35

[10] Data on own production and income is present in the data, however the survey's framing structure does not allow us to detect how storms affect these variables. In the case of self-production and consumption of food, the recall period is four weeks, and this coupled with the fact that the survey is implemented between May and August (before storm season) makes impossible to identify storm's effect.

kg in birth weight, 0.91 standard deviations in weight-for-height, and 0.79 standard deviations in weight-for-age.

While these results may appear to be counterintuitive, the accumulated destructive power of an average storm including one hurricane falls below the median of the destructive power distribution of all storms observed in the study period. Figures 1 to 3 show the estimated effects of the storms' destructive power experienced during each trimester of gestation on the outcomes of interest. The point estimates shown in Table 1.5 correspond to the second vertical line in the figures. Therefore, the destructive power of an average hurricane is not yet an extremely destructive event. As such, the few positive effects observed could be in line with the positive effects associated with increased rainfall and agricultural output.

Some empirical support can be provided to the previous hypothesis by observing the relationship between our destruction measure and agricultural output. Figure 4 uses quarterly output data from the Ministry of Agriculture to plot the relationship between output (log(tons)) and the destruction measure (log(Wind)).[11] The figure suggests that output behaves as a concave function of the destruction measure. Depending on the crop, output behaves as either an increasing or flat function of the destruction index up to a certain threshold. Once the threshold is reached (approximately between the equivalent of one average hurricane and the combination of hurricanes and other sub-categories of storms) output starts a decreasing pattern. Once this threshold is reached, an increase of 8 log-points in the storm's destructive power is associated with a decrease of approximately 48,550 tons of total agricultural output.

1.8 Effects of Destructive Hurricanes

The study also focused on the effects of storms that included two or more hurricanes. Table 1.6 shows full-sample estimated effects associated with destruction measures

[11] Ministry of Industry, Commerce, Agriculture and Fisheries: All Island Estimates of Production for Domestic Crops http://www.moa.gov.jm/AgriData/index.php

equivalent to an average storm that included at least two hurricanes. While impacts on birth weight are negative in sign, they are imprecisely estimated. However, we observe significant impacts, suggesting an increased likelihood of the occurrence of low birth weight. It appears that pregnant women exposed to these events during their second (third) trimester increase the likelihood of delivering a baby with low birth weight by 20 (17) percentage points. These estimated effects are twice as large as the overall mean of an 8 percent incidence of low birth weight (reported in Table 1.1).

When focusing on the coastal-rural sample (Table 1.7), estimated effects are larger. The estimated impacts on birth weight are negative, around 0.7 kg for the first and second trimesters (although imprecisely estimated for the second trimester). These findings are consistent with Camacho (2008), where birth weight effects rose when exposed to shocks during the first and second trimesters of gestation. However, our results are larger. While Camacho (2008) estimated a reduction in birth weight of 11.6 grams when exposed to land mines in the second trimester of gestation, our estimate finds a reduction of 730 grams.[12] In addition, we find that the likelihood of delivering a low-birth-weight baby increases by 56 percentage points if the storm is experienced during the second trimester of gestation (effect equivalent of 5 times the overall mean of 11 percent for the coastal-rural area).

The negative effects on birth weight are particularly relevant, as previous literature has documented a long-term negative impact of lower birth weight on educational and labour market outcomes (Black et al. (2007); Currie and Moretti (2007); Oreopoulos et al. (2008); Royer (2009)). For example, taking the findings in Black et al. (2007) as a benchmark, our estimates imply that Jamaican children exposed to two or more hurricanes during the mother's pregnancy will be 2.76 percent less likely to graduate from high school, will have 2.76 percent lower IQ (for boys), and will have 2.07 percent

[12] The data from Camacho (2008) present an average birth weight of 3.153 kg while the mean for Jamaica is about 3.184 kg. However, the share of low birth weight (less than 2.5 kg) in Jamaica exceeds that of Colombia (11 percent in Jamaica's rural-coastal sample versus 7.74 percent in Colombia).

lower adult earnings when compared to similar children who did not experience these shocks while in utero. Therefore, our findings highlight the importance of having appropriate safety nets in place to assist expectant mothers experiencing such events.

Regarding weight-for-height (ZWH), we find negative effects equivalent to 1.4 standard deviations due to experiencing the storm during the third trimester of gestation. Compared to Baez and Santos (2007), who studied the effects of hurricane Mitch in Nicaragua, our result is almost three times larger than their estimate of 0.493 of a standard deviation in ZWH.

In contrast with ZWH, ZWA is a longer-term outcome since the possibility of the latter to be adjusted or improved with better subsequent nutrition is lower than the former. We observe a negative effect equivalent to 1.88 standard deviations in ZWA due to experiencing the storm within the third trimester of gestation. Compared with that of Kumar et al. (2014), our estimate is 12 times larger with respect to the negative documented effect of droughts on ZWA in India.[13]

The previous estimates correspond to the third vertical line in Figures 1 to 3. As these figures show, the relationship between the destructive power of the storms and the outcomes of interest is nonlinear. In general, we observe that the effects begin from zero to slightly positive when storms are in the left tail of the destructive power. However, as the destructive power of storms increase, the effects on the outcomes of interest turn negative (with some flattening out and even turning to positive effects for extremely destructive events on the right tail of the distribution).

The nonlinearity of the effects and the flattening of the curves for extremely catastrophic events are consistent with Frankenberg et al. (2013). In this study, the authors find a positive effect of the tsunami on children's height, suggesting that this class of extreme events could come with "massive influx of humanitarian aid and the accompanying resources following the tsunami." page 12. This behaviour could push outcome variables up since food intake could increase and, in some cases, improve.

[13]The authors found that the larger effect of drought is about 0.15 of a standard deviation when children were exposed to droughts during the gestation period.

The effects that we document are likely lower bounds due to measurement error. Our destruction measure was built using data from the six-hour interval records from NOAA archives. This is the smallest time interval available and, therefore, the exact path followed by the storm at each stage of its life cannot be exactly recreated. In addition, the measure of total exposure by an individual is calculated using the geometric distance between the storm and the centroid of the district of residency, which is not the exact location of residency. Despite these limitations, our results are robust to several sources of possible biases that we show in the next section.

1.9 Robustness Analysis

Our strategy faces several potential sources of bias. This section shows that our results are robust to them. First, parents could self-select the conception and, therefore, the birth period. Parents could decide to have their children in the first half of the year (i.e. in the non-storm season). If this were the case, we should observe a conglomeration of live births during the non-storm season. Figure 5 shows the frequencies of births by day of the year, pooling years of birth from 1988 to 2012. The figure shows that there is no season in which a discernible mass of live births is concentrated.

Second, if our strategy resembles a good source of exogenous variation, we should not observe a systematic relation between our destruction measure and characteristics that could be related with the outcomes of interest. To test for this, we run model (3) using the sociodemographic characteristics of household heads as outcomes. As can be seen in Appendix Table B1, out of 117 estimated parameters, only seven (or 6 percent) were statistically significant at the 10 percent level or lower. This provides further confidence on the conditional exogeneity of the destruction measure within our empirical strategy.

Third, although women may not choose to give birth purposely in the non-storm season, they could adapt to climatic conditions. Adaptation to storms could potentially ameliorate the estimated effects. Therefore, to test if women are adapting, we created an indicator for wall quality that equals one if walls' material is brick, concrete nog, or concrete, while zero otherwise. We then aggregated the destruction that the district where the household resides suffered in the 12 months preceding the survey date. We then estimated model (3) using this indicator as an outcome. Appendix Table C2 shows that exposure to storms within the previous 12 months do not affect the walls' materials. Therefore, we interpret this as weak evidence of adaptation mechanisms.

Fourth, as in Maccini and Yang (2009), selection in the sample of children due to differential survival might bias the results. This would arise if the most affected children had died before being surveyed and, therefore, the observed children would be an already selected sample of relatively stronger people. To test if selection is present, we show that tropical storms have no relationship with the children's likelihood of appearing in the survey. That is, we find null relationship between the storms' strength and the size of birth cohorts by district-year-season (results available upon request).

Fifth, if our results are real and are not simply reflecting a random occurrence, there should not be any relationship between our outcomes and storms not yet suffered. To test for this, we regress the outcome variables on future storms (two and three years after the real gestation period) as if they had occurred during the gestation period. Appendix Tables C3 - C5 show that less than 10 percent of all estimated parameters in these placebo tests were statistically significant at the 10 percent level or lower.[14] This provides further confidence that our results are not just driven by random chance.

Sixth, our results assume that the district where the household resided on the interview date was also the district where it resided during the pregnancy. Therefore, it is important to show that internal migration is not biasing the results. Since the SLC interviews a sub-sample of the April LFS and, as shown in Appendix 1, the LFS contains a rotating panel component; then it is possible to identify a household-level

[14] Specifically, Appendix Table 4 contains six estimated parameters statistically significant out of 60, Appendix Table C4 contains five out of 60, and Appendix Table C5 contains six out of 60.

panel in the SLC. We label this as a panel "by-chance" since the SLC per se has no panel component but rather is a random subsample of the LFS (which does have a panel component). This enabled us to identify 292 cases where we observed the mother while she was pregnant and in at least one subsequent period after the child was born.[15] For these cases, we know the exact location (i.e., the district) where the mother resided during pregnancy.

As such, to test possible biases due to migration, we implemented a bounds approach. This consisted of imputing two extreme levels of outcome values to the individuals whose mothers we do not observe during the pregnancy period. If the estimation results combining the imputed values for the non-panel individuals while using the actual outcomes for the panel individuals show the same sign as the results without imputation, then we can be more confident that potential internal migration is not biasing the results. We implement these estimations with two alternative imputations of extreme values: the 10th-90th and the 25th-75th percentiles of the outcome distribution of panel individuals. Results shown in Appendix Tables C6 - C8 reject in general the hypothesis that the point estimates found are zero.

In addition, using data from the Population Census of Jamaica in 2001 and 2011, we found that migration across parishes for children under 5 is low. In 2001, the percentage of children under 5 who were born in a different parish than the current parish of residency was 9 percent, for children under 4 it was 8.4 percent, for children under 3 it was 7.7 percent, for children under 2 it was 6.8 percent, and for children under 1 year old it was 5.7 percent. The figures from the 2011 census were 8.45 percent, 7.9 percent, 7.2 percent, 6.3 percent, and 5.3 percent, respectively.

Finally, it could be that our results were not the effects of storms suffered during pregnancy, but rather a reflection of the presence (or absence) of interventions that would have responded to storms in post-pregnancy periods. For example, if the advent of a storm during pregnancy had triggered a scarcity of vaccinations received within

[15]For this exercise, we focused on survey rounds starting from 2002 onward. This is because information regarding the mother of each child surveyed was not collected before 2002.

the first moments of life, then our results in terms of anthropometric measures could be reflecting this. To test for this, we estimate model (3) having an indicator for whether the child received the bacille Calmette-Guerin (bcg) vaccination that is supposed to be received at birth. The results (available upon request) were all insignificant, suggesting that this channel is unlikely to be pervasive.

1.10 Conclusion

We studied the effects of tropical storms and hurricanes experienced during pregnancy on birth weight and children's physical development within the first 60 months of life. Using variation in the storms' destructive power between 1987 and 2011 at the district level coupled with anthropometric measurements taken at the household level, we find evidence of nonlinear effects. When tropical storms and one average hurricane hit within the second trimester of pregnancy, some positive effects are found in birthweight as well as weight-for-height and weight-for-age standardised measures. However, when destruction indexes equivalent to at least two average hurricanes hit while pregnant, serious negative effects occur. Indeed, expectant mothers living in coastal-rural areas exposed to these shocks during their second trimester of pregnancy are 56 percentage points more likely to deliver a baby with low birth weight. In addition, this exposure during the third trimester of pregnancy causes a reduction in children's weight-for-age (weight-for-height) measured within the first 60 months of life, equivalent to 1.88 (1.4) standard deviations. Finally, for extreme events located at the top 5 percent of the destruction measure distribution, effects flatten out and sometimes even turn positive.

The nonlinearity of our results is in line with previous findings: (i) The overlapping between tropical storms and rain may boost agricultural output, implying better nutrition (through an increase in crop quantity or income), benefiting children in the womb through the mothers' nutrition (in the spirit of Maccini and Yang (2009)). Aggregate data on agricultural output provided empirical support for this possibility. (ii)

A medium to large event would generate stress to pregnant mothers and/or malnourishment due to infrastructure destruction and/or loss of agricultural output, creating an adverse environment for normal fetal development (Camacho (2008)). This is reflected in the negative effects that we document for events related to the combination of two average hurricanes. (iii) Boost of public expenditure and increase in aid and humanitarian relief funds in the aftermath of an unusually catastrophic event may push nutrition intake up immediately after the storm (Frankenberg et al. (2013)).

Although no consensus exists, climate change would imply a new tropical storm pattern.[16] Rising sea levels and humidity in the tropical region are critical factors that would exacerbate the destructiveness of storms in coastal areas. Adaptation and resilience to the potential new pattern of events by governments and civil society are, therefore, important.

From a policy perspective, our findings suggest harmful effects of negative shocks suffered in utero on birth weight and early physical development, which have been shown to be correlated in the longer term with adult productivity. Therefore, our results provide additional objective justification for considering policies aimed at protecting expectant mothers at risk of suffering environmental shocks. Policy options toward effectively coping with these risks include weather insurance schemes, food security policies, community bounding strategies, and initiatives that promote resilience and adaptability to climate change, among others.

Beyond our specific results, poverty-related vulnerability will likely increase the potential negative effects of storms. Hallagate et al. (2015) suggest that economic vulnerabilities might create more pronounced poverty traps due to climate change since the poor will not have enough tools to cope with this risk.[17] Informal settlements, insecure sources of income, and inadequate formal insurance will make it impossible

[16]Recently, a discussion by the Geophysical Fluid Dynamic Laboratory, a division of NOAA, has affirmed the existence of a relationship between global warming and the potential increase of stronger cyclones by the end of the century. This will become a clear relationship between climate change and the increase in the risk due to weather shocks.

[17]The document is open to access and can be downloaded from: https://openknowledge.worldbank.org/handle/10986/22787

for this population to overcome the negative effects of environmental shocks. Public policy discussions on unemployment insurance, improving access to public health and pregnancy checkups, and boosting conditional cash transfer programs could be a good start to think about precautionary measures to confront weather shocks of the magnitude studied in this paper.

1.11 Figures and Tables

Figure 1.1

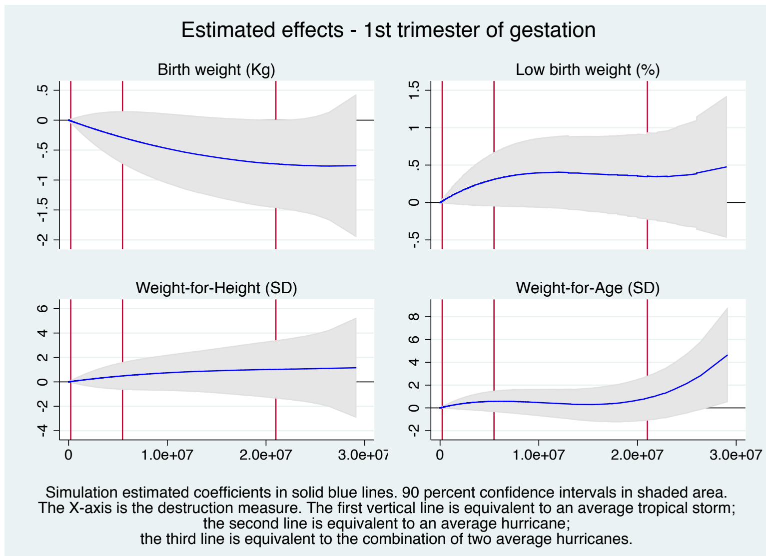


Figure 1.2

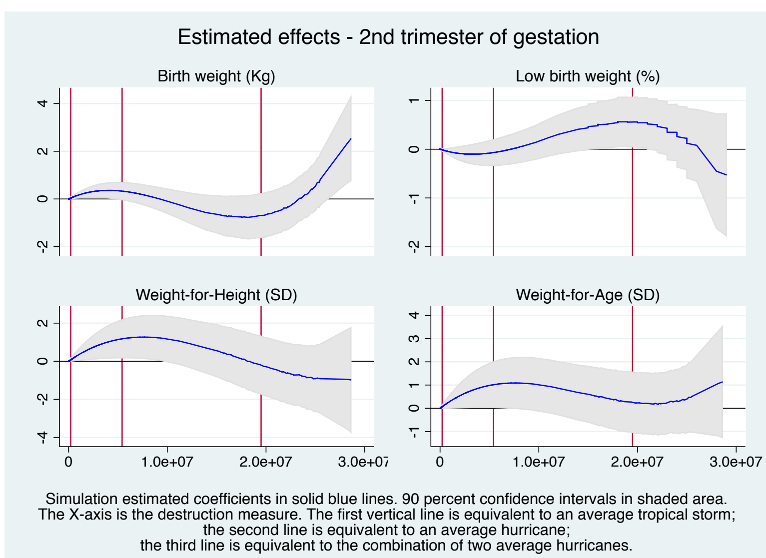
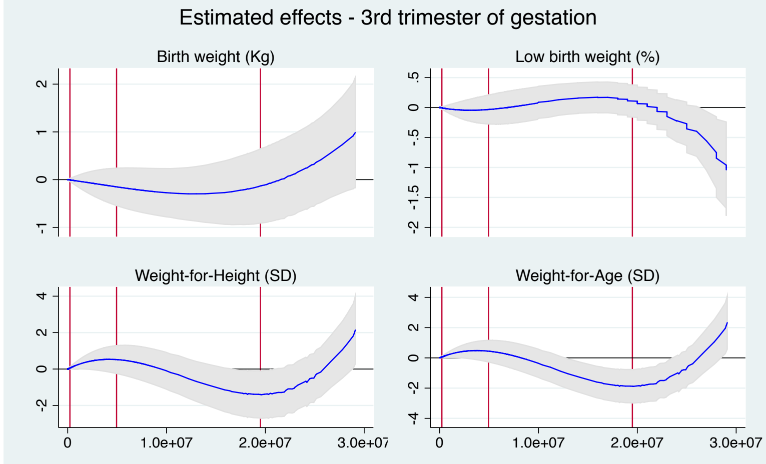
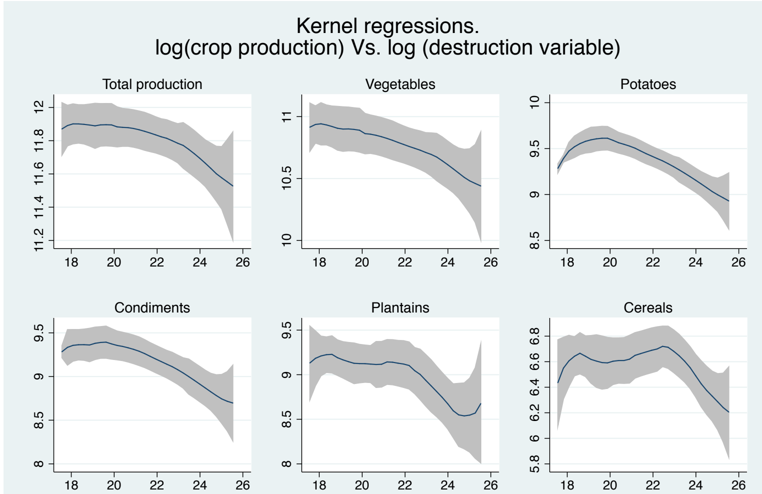


Figure 1.3



Simulation estimated coefficients in solid blue lines. 90 percent confidence intervals in shaded area.
The X-axis is the destruction measure. The first vertical line is equivalent to an average tropical storm;
the second line is equivalent to an average hurricane;
the third line is equivalent to the combination of two average hurricanes.

Figure 1.4



Kernel regressions estimates in solid blue. 90 percent confidence interval in shade region
Y-axis representes produced crop in log(tons). X-axis is the log of destruction measure.

Figure 1.5

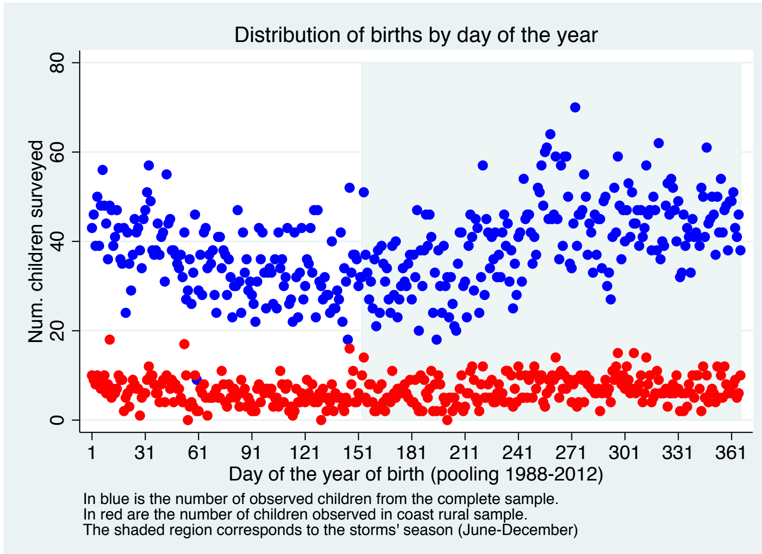

Distribution of births by day of the year
Num. children surveyed
0
20
40
60
80
1
31
61
91
121
151
181
211
241
271
301
331
361
Day of the year of birth (pooling 1988-2012)
In blue is the number of observed children from the complete sample.
In red are the number of children observed in coast rural sample.
The shaded region corresponds to the storms' season (June-December)

Table 1.1: Descriptive statistics

Variables		Complete sample	Coast-rural	Difference	
Education of household head					
	Primary	0.33	0.34	-0.01	
		(0.46)	(0.47)	(0.01)	
		13447	2459		
	Secondary incomplete	0.29	0.29	-0.001	
		(0.45)	(0.45)	(<0.01)	
		13447	2459		
	Secondary	0.33	0.34	-0.003	
		(0.47)	(0.47)	(0.01)	
		13447	2459		
	Tertiary	0.06	0.04	0.01	***
		(0.22)	(0.19)	(<0.01)	
		13447	2459		
Age of household head		43.41	43.40	0.01	
		(15.4)	(15.4)	(0.33)	
		14109	2569		
Female household head		0.54	0.53	0.01	
		(0.49)	(0.49)	(0.01)	
		14111	2569		
Household size		6.26	6.13	0.13	**
		(3.09)	(2.72)	(0.06)	
		14111	2569		
Children's characteristics					
	Age in months	30.12	30.02	0.10	
		(16.8)	(16.7)	(0.36)	
		14111	2569		
	Girl	0.50	0.50	-0.01	
		(0.50)	(0.50)	(0.01)	
		14111	2569		
Health outcomes					
	Birth weight (Kg.)	3.18	3.18	0.0008	
		(0.45)	(0.47)	(0.01)	
		10468	1840		
	Low birth weight	0.08	0.11	-0.02	***
		(0.28)	(0.31)	(<0.01)	
		10468	1840		
	Weight-for-Height	0.27	0.22	0.05	*
		(1.20)	(1.18)	(0.02)	
		11792	2139		
	Weight-for-Age	0.23	0.24	-0.02	
		(1.22)	(1.21)	(0.02)	
		12573	2279		
	Height-for-Age	0.13	0.20	-0.07	**
		(1.41)	(1.38)	(0.03)	
		12443	2251		
Vaccines					
	bcg	0.89	0.89	0.01	
		(0.30)	(0.31)	(<0.01)	
		13340	2403		
	measles	0.68	0.67	0.01	
		(0.46)	(0.47)	(0.01)	
		14111	2569		
	opv	0.59	0.58	0.01	
		(0.49)	(0.49)	(0.01)	
		14111	2569		
	dtp	0.60	0.61	-0.003	
		(0.48)	(0.48)	(0.01)	
		14111	2569		
	Complete vaccination	0.57	0.56	0.01	
		(0.49)	(0.49)	(0.01)	
		14111	2569		

Notes: Each cell correspond to:1. mean;2. sd, 3. sample size. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 1.2: Average tropical storm's effect on children's health outcomes (exclude hurricanes)- Complete sample

Gestation period	Birth weight	Low birth weight	ZWH	ZWA	ZHA
1st Trimester	-0.003	0.005	0.007	0.01	0.02
	(0.006)	(0.004)	(0.015)	(0.01)	(0.017)
2nd Trimester	0.006	-0.003	0.01	0.005	0.001
	(0.005)	(0.004)	(0.015)	(0.01)	(0.01)
3rd Trimester	0.002	-0.004	0.009	0.004	0.006
	(0.005)	(0.003)	(0.013)	(0.01)	(0.01)
Observations	*9963*	*9963*	*11255*	*11991*	*11864*

Notes: This table present the results from the estimation of equation 1.3 using the complete sample. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Controls included are: household head's education and age and a dummy for female head, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, and child's age and gender (dummyfor female). Birth weight in kilograms, low birth weight is a dummy equals to one if birth weight is lower than 2.5 kilograms, and z- scores are measured in standard deviations. Estimated standard errors, reported in parentheses, are clustered at the district level. Values for simulation were 215500 for Q1, 211261 for Q2, and 224532 for Q3 corresponding from average destruction due to the impact of non hurricane storms in the period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 1.3: Average tropical storm's effect on children's health outcomes (exclude hurricanes)- Coast-Rural

Gestation period	Birth weight	Low birth weight	ZWH	ZWA	ZHA
1st Trimester	-0.01	0.02	0.02	0.04	0.04
	(0.01)	(0.01)	(0.03)	(0.03)	(0.02)
2nd Trimester	0.04**	-0.01	0.08*	0.07*	0.003
	(0.01)	(0.01)	(0.04)	(0.04)	(0.04)
3rd Trimester	-0.01	-0.006	0.06*	0.06**	-0.01
	(0.01)	(0.01)	(0.03)	(0.02)	(0.03)
Observations	*1764*	*1764*	*2052*	*2186*	*2159*

Notes: This table present the results from the estimation of equation 1.3 using the coast rural sample. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Controls included are: household head's education and age and a dummy for female head, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, and child's age and gender (dummy for female). Birth weight in kilograms, low birth weight is a dummy equals to one if birth weight is lower than 2.5 kilograms, and z- scores are measured in standard deviations. Estimated standard errors, reported in parentheses, are clustered at the district level. Values for simulation were 215500 for Q1, 211261 for Q2, and 224532 for Q3 corresponding from average destruction due to the impact of non hurricane storms in the period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 1.4: Average storm's effect on children's health outcomes when hit by at most one hurricane - Complete sample

Gestation period	Birth weight	Low birth weight	ZWH	ZWA	ZHA
1st Trimester	-0.03	0.07	0.06	0.10	0.26
	(0.07)	(0.06)	(0.19)	(0.18)	(0.20)
2nd Trimester	0.05	0.004	0.16	0.04	0.02
	(0.07)	(0.05)	(0.19)	(0.19)	(0.20)
3rd Trimester	0.01	-0.02	0.08	0.01	0.04
	(0.06)	(0.04)	(0.16)	(0.14)	(0.16)
Observations	*9963*	*9963*	*11255*	*11991*	*11864*

Notes: This table present the results from the estimation of equation 1.3 using the complete sample. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Controls included are: household head's education and age and a dummy for female head, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, and child's age and gender (dummy for female). Birth weight in kilograms, low birth weight is a dummy equals to one if birth weight is lower than 2.5 kilograms, and z- scores are measured in standard deviations. Estimated standard errors, reported in parentheses, are clustered at the district level. Values for simulation were 5.4 mill. for Q1 and Q2, and 4.9 mill. for Q3 corresponding from median destruction due to the impact of at most one hurricanes in the period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 1.5: Average storm's effect on children's health outcomes when hit by at most one hurricane - Coast-Rural

Gestation period	Birth weight	Low birth weight	ZWH	ZWA	ZHA
1st Trimester	-0.18	0.30	0.31	0.46	0.35
	(0.17)	(0.21)	(0.17)	(0.17)	(0.17)
2nd Trimester	0.35**	-0.08	0.91**	0.79*	0.08
	(0.17)	(0.15)	-0.17	(0.17)	(0.17)
3rd Trimester	-0.12	-0.03	0.53	0.48	-0.18
	(0.17)	(0.15)	(0.17)	(0.17)	(0.17)
Observations	*1764*	*1764*	*2052*	*2186*	*2159*

Notes: This table present the results from the estimation of equation 1.3 using the coast rural sample. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Controls included are: household head's education and age and a dummy for female head, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, and child's age and gender (dummy for female). Birth weight in kilograms, low birth weight is a dummy equals to one if birth weight is lower than 2.5 kilograms, and z- scores are measured in standard deviations. Estimated standard errors, reported in parentheses, are clustered at the district level. Values for simulation were 5.4 mill. for Q1 and Q2, and 4.9 mill. for Q3 corresponding from median destruction due to the impact of at most one hurricanes in the period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 1.6: Average storm's effect on children's health outcomes when hit by two or more hurricanes - Complete sample

Gestation period	Birth weight	Low birth weight	ZWH	ZWA	ZHA
1st Trimester	-0.22	0.10	-0.22	-0.08	0.56
	(0.40)	(0.1)	(0.40)	(0.41)	(0.42)
2nd Trimester	-0.12	0.2*	-0.12	-0.08	0.32
	(0.39)	(0.11)	(0.39)	(0.36)	(0.37)
3rd Trimester	-0.16	0.17**	-0.16	-0.29	-0.31
	(0.33)	(0.08)	(0.33)	(0.31)	(0.32)
Observations	*9963*	*9963*	*11255*	*11991*	*11864*

Notes: This table present the results from the estimation of equation 1.3 using the complete sample. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Controls included are: household head's education and age and a dummy for female head, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, and child's age and gender (dummy for female). Birth weight in kilograms, low birth weight is a dummy equals to one if birth weight is lower than 2.5 kilograms, and z- scores are measured in standard deviations. Estimated standard errors, reported in parentheses, are clustered at the district level. Values for simulation were 21 mill. for Q1 and Q2 and 19.5 mill. for Q3 corresponding from median destruction due to the impact of two or more hurricanes in the same period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 1.7: Average storm's effect on children's health outcomes when hit by two or more hurricanes - Coast-Rural

Gestation period	Birth weight	Low birth weight	ZWH	ZWA	ZHA
1st Trimester	-0.73*	0.35	1.02	0.86	0.5
	(0.44)	(0.34)	(1.41)	(1.16)	(1.22)
2nd Trimester	-0.7	0.56*	-0.21	0.25	1.09
	(0.55)	(0.31)	(0.92)	(0.78)	(0.92)
3rd Trimester	-0.13	0.11	-1.4*	-1.88***	-0.52
	(0.47)	(0.16)	(0.78)	(0.66)	(0.93)
Observations	1764	1764	2052	2186	2159

Notes: This table present the results from the estimation of equation 1.3 using the coast rural sample. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Controls included are: household head's education and age and a dummy for female head, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, and child's age and gender (dummy for female). Birth weight in kilograms, low birth weight is a dummy equals to one if birth weight is lower than 2.5 kilograms, and z- scores are measured in standard deviations. Estimated standard errors, reported in parentheses, are clustered at the district level. Values for simulation were 21 mill. for Q1 and Q2 and 19.5 mill. for Q3 corresponding from median destruction due to the impact of two or more hurricanes in the same period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Chapter 2

The Effects of Natural Disasters on Labor Market: Do Hurricanes Increase Informality?

Abstract

This paper studies the probability of formally employed men falling into informality because of exposure to hurricanes and tropical storms. It combines destruction variables calculated from physical storms' characteristics at the district level with 36 quarterly rounds of Jamaica's labour force surveys. The empirical strategy exploits variation arising from the storm's timing, intensity, and geographic location within a panel-random effects endogenous choice model framework. Controlling for potential biases due to initial conditions, panel attrition and employment selection, findings suggest that hurricanes positively affect the transition probability regardless of whether the individually was initially employed in a formal or an informal job. When the marginal effects of the storm were studied, the probability of become informally employed ranges between 0.7 and 12 percent depending on the employee's initial state and the moment when the storms were suffered. These results suggest that the public and private policy agenda on adaptation to climate change should incorporate a discussion on how to offset the negative effects of hurricanes, since these events could become worse in the near future.

JEL classification: C33, E26, J01, J22, Q54.

Key words: Tropical storms, informal employment, labour market transitions, endogeneity, simulated based estimation, Jamaica.

2.1 Introduction

Are weather shocks affecting the risk that Jamaica's formally employed men will become informal? Are those men working in the formal sector at risk of falling in informality due to tropical storms in Caribbean economies? These questions are of extreme importance in developing countries that are annually afflicted by tropical storms or hurricanes. The proposed discussion follows the view of Acevedo (2015), who found that individuals change their behavior in the labour market to cope with negative effects of weather shocks as adaptation mechanism in order to smooth consumption. This finding has implications for policy discussions.

This paper updates the "new climate economy literature" -Dell et al. (2013)- in two ways. The first is that it introduces a novel dataset that contains individuals' labour force information in a pseudo-panel structure interacted with tropical storm information collected from original geographic information system (GIS) sources, containing various geographical and physical characteristics from the storms per se and from topographical information on Jamaica. In addition to this data structure, an endogenous switching model is introduced to disentangle causality from tropical storms estimated through maximum likelihood (multivariate probit estimation). This method departs from the standard analysis of environmental shocks that Dell et al. (2013) used to study labour market outcomes in the literature, since it incorporates a structural analysis of the individual's decision-making process on job options.

Big storms are associated with an increase in the probability of falling into informality. There is a non-linear relationship between the storms' accumulated strength and the probability of remaining informal and falling into informality if the individual was initially formally employed. The relationship depicts an exponential pattern where small tropical storms and tropical depressions have almost zero or negative effects; however, when storms pass the threshold defined by hurricanes, the probability associated with the storms' accumulative wind is positive. The probability of remaining informal ranges between 1 and 11 percent, and the probability of falling into informality when the individual was previously formally employed ranges between 1 and 12 percent. These effects have to be taken as the lower bounds of the true effect, since the relationship between the destruction variable and the actual probability is not perfect. These findings suggest that there should be a discussion not only on how to protect the informal population but also on how to support the formally employed population so that they do not fall into the informal sector.

The literature contains no evidence of how natural disasters affect men's decision to become informal. The evidence on how weather shocks affect the allocation of time has been recently studied using exogenous variations in time and geography, principally using rainfall -Acevedo (2015)- or temperature -Zivin and Neidell (2014)- data. These authors find that floods are associated with an increase in unemployment (a result that is biased against women) and a reduction in income from labour. On the other hand, an increase in temperature is associated with a reduction in the labour supply. More related to tropical storms is Belasen and Polachek (2008), who studied how hurricanes affected workers in Florida. They find that hurricanes positively impact earnings but negatively affect employment. Using data from Mexico, Rodríguez-Oreggia (2013) finds that the potential destruction due to hurricanes as big shocks might increase employment rates, mainly blue-collar jobs involved in reconstruction. Sharing the same geographic location as this research, Spencer and Polachek (2015), using crop production data, find a negative relationship between agricultural productivity and hurricanes that is more pronounced for crops that are above ground. This evidence favours the negative effect of weather shocks on elements of the general equilibrium of labour markets in developing economies. However, no study was found on the effect of weather shocks on transitions in employment formality patterns.

This research also fits within the literature on labour economics. The informal sector has been studied in the Latin American and Caribbean (LAC) region. The effort has focused on measuring its size using different approaches as mentioned by Alderslade et al. (2006): Electricity consumption -Basbay et al. (2016)-, currency demand -Kamau and Lin (2016)-, nightlight -Ghosh et al. (2009)-, and micro-data -Gasparini and Tornarolli (2007)-. The intention of this set of studies is to determine the size of the informal sector. However, more research and better data are needed, as stated by Caldera Sánchez et al. (2011). Although attempts have been made to measure the issue, little has been done to identify the individuals that make up the sector or how external factors (like weather shocks) affect their decision to remain in or to switch to another sector. The latter is of particular interest since it will provide the necessary evidence to inform of public policy designed to prevent potential switches between sectors or, in the best-case scenario, to help informally employed individuals shift to the formal sector. This paper helps close the evidence gap of the effects of negative shocks (weather shocks) on workers' decisions regarding their formality status.

This paper also estimates the effects of tropical storms on the transition into and out of informality. Its innovation is the use of a wind field model to approximate the actual destruction generated by tropical depressions, tropical storms, and hurricanes in a more precise way than the binary approach through government reports used in the literature on this class of events. Although the measure does not perfectly correlate with the outcome variable due to imperfect matching at the geographical location of the workers at the time of the event, it is still a good proxy for the destruction -Strobl (2012)-, allowing the method to exploit a third level of exogenous variation (apart from the geographical and timing ones) that is the intensity of the storm. This information allows me the use an endogenous switching model using a multivariate probit estimation to disentangle causality from storms. This model has been tested in other labour market issues, specifically to the characteristics that govern transitions from and to low-wage status using data from the Great Britain -Cappellari and Jenkins (2006b)-, and with regard to how informality persistence depends on the status of informality in previous periods using data from Ukraine -Akay and Khamis

(2011)-. Benchekroun et al. (2014) estimate transition probabilities across sectors using a multivariate logit. This study goes further than Kavuma et al. (2015), who studied the transitions to informality using panel data and probit models, since it controls for environmental conditions as negative shocks.

The importance of this research is based on the size of the informal sector in developing economies and its vulnerabilities. Based on Gasparini and Tornarolli (2007), the informal sector accounted between 25 percent (Suriname) and 89 percent (Haiti) of working population, positioning this issue as one of the greatest challenges in the LAC region. These authors found that informality in Jamaica accounts for 58 percent of the labour force.[1] Some authors have found that, although in different contexts, this population is vulnerable to shocks. Since public systems do not recognize them, their capacity to cope with shocks is weaker than that of formal workers. The lack of health insurance may imply that the higher costs of accessing good health centers reduces the possibility of this population to receive proper health services, worsening their productivity -Perry et al. (2007)-. Another aspect is lack of access to financing, since in the event of a negative shock, informal workers would not have the means to cope with its effects, forcing them to use their own (usually scarce) resources to do so -Patankar and Patwardhan (2015)-.

In terms of public and private action, there is much to be done. From the World Bank's Atlas of Social Protection Indicators of Resilience and Equity (ASPIRE) data, which contain information on coverage of different insurance systems available in the economy, such as for old age, disability, death of the head of household, maternity leave, sickness cash benefits, and social health, it is easy to see that Jamaica is among the bottom three in access to insurance, higher only than Honduras and Guatemala.[2] In terms of health insurance, in a recent paper, Beuermann and Pecha (2016) find

[1] This study uses micro-data to determine the size of the aspect and the most comprehensive study about it has been done using this data -Perry et al. (2007)-. The World Bank's World Development Indicators shows that informality in the region ranges between 30 percent in Costa Rica and 74.4 percent in Guatemala.

[2] To take a closer look to the data, please refer to:
http://datatopics.worldbank.org/aspire/indicator/social-insurance

that the maximum share of the population with private insurance was less than 20 percent and that the implementation of public free access to health care was associated with a significant increase in weekly hours worked, which demonstrates that such measures would help workers, otherwise uninsured, to bear negative health shocks. This evidence shows that there is space and a need to improve the public and private sectors' ability to increase the population's coping mechanisms against environmental shocks.

Climate change will increase health shocks and property losses. A recent World Bank study underscores the urgency that every economy improve their mechanisms to adapt to the consequences of climate change. The increase in temperatures, the rise in sea level, and the change in crop seasons have created a new global situation threatening the sustainability of life and business as usual. The study also finds that vulnerability to climatic events is negatively correlated with wealth, making developing economies less capable of withstanding increasingly larger natural shocks -Hallagate et al. (2015)- in contrast with developed economies, where fiscal instruments not designed for risk prevention and reduction, like unemployment insurance and public health provision, are used to mitigate the negative effects from hurricanes, as noted by Deryugina (2016). With this, the poor population, where informal workers are concentrated, will be the most negatively affected by future weather shocks. According to Patankar and Patwardhan (2015) (2015), under a flooding shock, informal workers had inadequate coping mechanisms to withstand major flood events in Mumbai.

This paper adds to the evidence on the vulnerability of informal workers to remain informal due to big storms, and shows how hurricanes increase the risk that formal workers will become informal. The paper is organized as follows: Section 2 describes the methodology and data used, Section 3 presents the results, and Section 4 concludes.

2.2 Methodology and Data.

2.2.1 Methodology: An Econometric Model of Informality Transitions

Following Cappellari and Jenkins (2002) and Cappellari and Jenkins (2006b), the structure of the panel data and the dynamic nature of the analysis present some challenges that the study must tackle. Those challenges are related to response attrition and the initial condition's problem stated by Heckman (1981b).

The first challenge concerns sample selection via response attrition. I kept only data from males for which I can reconstruct the proxy of formality in period $t-1$ and t since the research question is related to that transition only. This sample of individuals contains males who had information about occupation and those observed in the dataset but who did not have the information to construct the occupation. The potential selection to respond to the occupation questionnaire could be not random, so the estimated parameter could be biased.[3]

On the other hand, an individual's status in period t could be affected by his status in period $t-1$, another class of selection bias that could operate in this analysis. The so-called initial conditions problem introduced by Heckman could be at work in this setup in the sense that informal males could be systematically different than formally employed males. This systematic difference could imply that the propensity of initially informal people to remain informal is higher than the propensity of the formal ones to become informal.

Last but not least, a distinction must be drawn between heterogeneity and state dependence. To disentangle the participation due to characteristics from state dependence in the observed labour market state's persistence, this study allows the model to account for inter-temporal correlation between unobservable factors in the processes involved Heckman (1981a). It addresses the three challenges through estimation of a

[3] I do not take in account potential panel attrition since the nature of the data set may induce such attrition at random every 3 to 4 years due to survey design and master sample's revision. With this, the panel attrition is orthogonal to the individuals' characteristics and/or labour occupation.

four-variate probit model with endogenous selection and endogenous switching.

This study works with a sample of males from Jamaica. This is because information regarding endogenous variables that affect females' decision to work are not available. Let us assume that the sample of males can be seen in a base period, say, period t-1. The relevant information is the formality status in that period, so that only information on workers employed in formal or informal sectors is kept, a common practice in the literature -see Cappellari and Jenkins (2008).

Following the notation used by Cappellari and Jenkins (2008), for each individual $i = 1, \ldots, n$ in the data from the year $t-1$, I assume that there is a latent informality propensity $I^*_{i,t-1}$, so that observing informal status $I_{i,t-1}$ depends on whether this propensity is larger than a certain observed threshold. The initial conditions equation is defined as:

$$I^*_{i,t-1} = \boldsymbol{\alpha}'\mathbf{x}^{I_{t-1}}_{i,t-1} + u^{I_{t-1}}_{i,t-1}, \text{ where } u^{I_{t-1}}_{i,t-1} = \mu_i + \delta_{i,t-1} \sim N(0,1) \tag{2.1}$$

$$I_{i,t-1} = \mathbb{1}\{I^*_{i,t-1} > 0\} \tag{2.2}$$

From equation (2.1), $\mathbf{x}^{I_{t-1}}_{i,t-1}$ is a vector of individual characteristics and α is the vector of parameters associated to the characteristics. The error term defined by $u^{I_{t-1}}_{i,t-1}$ is the summary of unobserved differences between individuals and are assumed to be uncorrelated with observed characteristics: $u^{I_{t-1}}_{i,t-1}$is the sum of a normal time invariant individual-specific effect μ_i and a normal orthogonal white noise process $\delta_{i,t-1}$. From equation (2.2), $\mathbb{1}\{I^*_{i,t-1} > 0\}$ is an indicator function that is equal to one if the latent variable is larger than zero, without loss of generality, and zero otherwise.

With the panel nature of the data, assume that in the following period (say, quarter t) there is certain probability of an individual being retained. Again, assume that there is a latent variable $R^*_{i,t}$ that accounts for the propensity of individual i being followed or retained in the data from $t-1$ to t, disregard less the availability of information on status of employment or formality. In the same spirit as the previous set of equations, the observed retention status $R_{i,t}$ depends on the propensity to be non-negative, i.e:

$$R^*_{i,t} = \boldsymbol{\lambda}'\mathbf{x}^R_{i,t-1} + u^R_{i,t}, \text{ where } u^R_{i,t} = \theta_i + \epsilon_{i,t} \sim N(0,1) \tag{2.3}$$

$$R_{i,t} = \mathbb{1}\{R^*_{i,t} > 0\} \tag{2.4}$$

The description of equations (2.3) and (2.4) is similar to those for equations (2.1) and (2.2).[4]

To estimate transition to informality, a second condition must hold. Among the retained sample of men, they must be employed or working in period t in order to observe the information on informality state. In the same way as the previous equation on retention, the propensity of being employed or working is given by the latent variable $W^*_{i,t}$ that is a linear function of some characteristics (observed and unobserved) as follows:

$$W^*_{i,t} = \boldsymbol{\gamma}'\mathbf{x}^W_{i,t-1} + u^W_{i,t}, \text{ where } u^W_{i,t} = \omega_i + \eta_{i,t} \sim N(0,1) \tag{2.5}$$

$$W_{i,t} = \mathbb{1}\{W^*_{i,t} > 0\} \tag{2.6}$$

The description of equations (2.5) and (2.6) is similar to those for equations (2.1) and (2.2). Note that if the individual j was not followed in period t ($R_{i,t} = 0$), equation (2.5) is truncated.

Lastly, the transition equation that describes the informality status in period t is presented. Let us assume that the propensity of being informal in period t is described by the latent variable $I^*_{i,t}$ characterised by a linear index specification of base period characteristics but conditioned by the base period informality status, the so-called endogenous switching regression:

[4] Note that all the equations are parameterised in terms of base quarter t-1's covariates in order to avoid simultaneity changes in probabilities and changes in attributes.

$$I^*_{i,t} = [\boldsymbol{\beta}'_1 I_{i,t-1} + \boldsymbol{\beta}'_2(1 - I_{i,t-1})]\mathbf{x}^{I_t}_{i,t-1} + \sum_{\tau=1}^{2}[\boldsymbol{\varphi}'_{1,\tau} I_{i,t-1} + \boldsymbol{\varphi}'_{2,\tau}(1 - I_{i,t-1})]\mathbf{S}_{d,t,\tau} + u^{I_t}_{i,t}, \quad (2.7)$$

where

$$u^{I_t}_{i,t} = \nu_i + \pi_{i,t} \sim N(0, 1)$$

$$I_{i,t} = \mathbb{1}\{I^*_{i,t} > 0\} \quad (2.8)$$

Equation (2.7) contains the central point of the investigation. As can be seen, the transition equation is a linear function of some characteristics in base year ($\mathbf{x}^{I_t}_{i,t-1}$) and the storms suffered at the district d level in quarter $\tau = 1, 2$ before last interview . With this, the set of parameters $\phi_{1,\tau}$ indicates the effect of tropical storms suffered $\tau = 1, 2$ quarters before interview in period t conditional on being informal in the base period on the probability of being informal in period t.[5] The correspondent description for $\phi_{2,\tau}$ is the effect of tropical storms suffered during $\tau = 1, 2$ quarters before interview in period t conditional on being formal in the base period on the probability of being informal in period t, that is, the parameter of interest. Note that this equation is truncated for the cases $E_{i,t} = 0$ or $R_{i,t} = 0$ (not working in period t or not retained in the panel, respectively)

Figure (2.1) depicts the chain, or sequence, of events. From the data, I kept only men who were either working in the informal or the formal sector in the base period. With this, individuals are either $I_{t-1} = 1$ or $I_{t-1} = 0$. One of the hypotheses behind the use of the endogenous model is that the panel retention could be not random and that the formality status may affect the probability of being observed in the following round. With this, in the second level of the Tree, individuals can be followed in period t or not. In the case that an individual cannot be followed, the only information available is for the informality state in $t - 1$ and the information for the

[5] Note that all the equations are parameterised in terms of base quarter $t - 1$'s covariates in order to avoid simultaneity changes in probabilities and changes in attributes.

retention (since the covariates in the equation form retention are located in $t-1$).

Attrition can be defined from two sources. One important aspect to note is that there could be two different means of attrition: either an individual cannot be followed or, if followed, the information regarding employment status is missing. In both cases the non-followed and the missing in response are treated as not retained, $R_t = 0$.

On the other hand, when $R_t = 1$, working state in t is observable. For the period t I kept all of those individuals that were working in the formal or informal sector as well as the unemployed. At this point, a second truncation of the information is present since all of those that are observed as unemployed do not have information about the employment sector. If the individual is $W_t = 1$, it is possible to observe the informality state in t. Each of these groups contributes to the general likelihood of being informal in t.

The sample is divided depending on the realizations of the variables R_t, E_t, I_t, as described in Figure (2.1), these realizations are the expression of three different sets of individuals in the sample that will contribute in the way expressed in (2.1).

In this study, I will keep all those individuals that in $t-1$ were either informal or formal employees. Also, the retention will be equal to one if the individual can be followed from $t-1$ and t; however, if an individual is observed in t and its working or employment status is observed but not the sector (variable $I_{i,t}$ is missing), it will be part of the non-retained individuals (i.e $R_{i,t} = 0$).

Let us assume that the set of unobservables are jointly distributed as:

$$(u_{i,t-1}^{I_{t-1}}, u_{i,t}^{R}, u_{i,t}^{W}, u_{i,t}^{I_t}) \sim \mathcal{N}_4(\mathbf{0}, \mathbf{\Sigma}) \tag{2.9}$$

That is a four-variate normal distribution with means of zeros and variance-covariance matrix $\mathbf{\Sigma}$. Adopting a random-effects specification, the elements off diagonal of the variance-covariance matrix are the cross-equation covariance components of the time invariant individual-specific effects (μ_i, θ_i, ω_i, ν_i).

With the four-variate normal distribution assumption, the individual contribution to the likelihood in each group of table 2.1 is given by:

$$\begin{aligned}
\mathcal{L}_{A_i} &= \Phi_2(k_{1,i}\boldsymbol{\alpha}'\mathbf{x}^I_{i,t-1}, k_{2,i}\boldsymbol{\lambda}'\mathbf{x}^R_{i,t-1}; \rho_1) \\
\mathcal{L}_{B_i} &= \Phi_3(k_{1,i}\boldsymbol{\alpha}'\mathbf{x}^I_{i,t-1}, k_{2,i}\boldsymbol{\lambda}'\mathbf{x}^R_{i,t-1}, k_{3,i}\boldsymbol{\gamma}'\mathbf{x}^W_{i,t-1}; \rho_1, \rho_2, \rho_3) \\
\mathcal{L}_{C_i} &= \Phi_4(I_{i,t-1}\Psi_{1,i} + F_{i,t-1}\Psi_{2,i}; \rho_1, \rho_2, \rho_3, \rho_4, \rho_6, \rho_6), \text{ where } F_{i,t-1} = 1 - I_{i,t-1}
\end{aligned} \tag{2.10}$$

where Φ_n is the n^th-variate normal cumulative density function,

$$\Psi_{1,i} = (k_{1,i}\boldsymbol{\alpha}'\mathbf{x}^{I_{t-1}}_{i,t-1}, k_{2,i}\boldsymbol{\lambda}'\mathbf{x}^R_{i,t-1}, k_{3,i}\boldsymbol{\gamma}'\mathbf{x}^W_{i,t-1}, k_{4,i}\left[\boldsymbol{\beta}'_1\mathbf{x}^{I_t}_{i,t-1} + \sum_{\tau=1}^{2}\boldsymbol{\varphi}'_{1,\tau}\mathbf{S}_{d,t,\tau}\right])$$

and

$$\Psi_{2,i} = (k_{1,i}\boldsymbol{\alpha}'\mathbf{x}^{I_{t-1}}_{i,t-1}, k_{2,i}\boldsymbol{\lambda}'\mathbf{x}^R_{i,t-1}, k_{3,i}\boldsymbol{\gamma}'\mathbf{x}^W_{i,t-1}, k_{4,i}\left[\boldsymbol{\beta}'_2\mathbf{x}^{I_t}_{i,t-1} + \sum_{\tau=1}^{2}\boldsymbol{\varphi}'_{2,\tau}\mathbf{S}_{d,t,\tau}\right])$$

Note the change in the subindex of $\boldsymbol{\beta}$. Let us define k as the sign of i's contribution as $k_{1,i} = 2I_{i,t-1} - 1$, $k_{2,i} = 2R_{i,t} - 1$, $k_{3,i} = 2E_{i,t} - 1$, and $k_{4,i} = 2I_{i,t} - 1$.

For the estimations, I will use a quadratic version of the storm variable. In the next section, I explain how to construct the destruction variable that will account for all the physical information regarding the storms' $S_{d,t,\tau}$ variable. The idea of using a second-degree polynomial expression of the form of the destruction variable is to capture the effect of the level of the storm's strength that will inform, if the storm increases in power, how it will affect the transition probability (low level of destruction/wind versus high level). With this, the storm variables for the expressions in $\Psi_{1,i}$ and $\Psi_{2,i}$ become:

$$\sum_{\tau=1}^{2}\boldsymbol{\varphi}'_{1,\tau}\mathbf{S}_{d,t,\tau} = \sum_{\tau=1}^{2}[\varphi_{1,1,\tau}S_{d,t,\tau} + \varphi_{1,2,\tau}S^2_{d,t,\tau}] \tag{2.11}$$

and

$$\sum_{\tau=1}^{2}\boldsymbol{\varphi}'_{2,\tau}\mathbf{S}_{d,t,\tau} = \sum_{\tau=1}^{2}[\varphi_{2,1,\tau}S_{d,t,\tau} + \varphi_{2,2,\tau}S^2_{d,t,\tau}] \tag{2.12}$$

respectively.

The interpretation of the storms' parameters is as follows. If $\varphi_{1,1,\tau}$ is positive (negative), small storms affect positively (negatively) the probability of become informal in the second stage given that the individual's initial status was informal. If $\varphi_{1,2,\tau}$ is positive (negative), big storms affect positively (negatively) the probability of become informal in the second stage given that the individual's initial status was informal. If $\varphi_{2,1,\tau}$ is positive (negative), small storms affect positively (negatively) the probability of become informal in the second stage given that the individual's initial status was formal. If $\varphi_{1,2,\tau}$ is positive (negative), big storms affect positively (negatively) the probability of become informal in the second stage given that the individual's initial status was formal.

In the other hand, the covariances ρ_j are defined as:

$$\begin{aligned}
\rho_1 \equiv &\quad corr(u^I_{i,t-1}, u^R_{i,t}) = cov(\mu_i, \theta_i) \\
\rho_2 \equiv &\quad corr(u^I_{i,t-1}, u^W_{i,t}) = cov(\mu_i, \omega_i) \\
\rho_3 \equiv &\quad corr(u^R_{i,t}, u^W_{i,t}) = cov(\theta_i, \omega_i) \\
\rho_4 \equiv &\quad corr(u^I_{i,t-1}, u^I_{i,t}) = cov(\mu_i, \nu_i) \\
\rho_5 \equiv &\quad corr(u^R_{i,t}, u^I_{i,t}) = cov(\theta_i, \nu_i) \\
\rho_6 \equiv &\quad corr(u^W_{i,t}, u^I_{i,t}) = cov(\omega_i, \nu_i)
\end{aligned} \tag{2.13}$$

With these, the distribution of unobserved heterogeneity is parameterised through the cross-equation correlations. Correlation ρ_1 describes the association between unobservable individual-specific characteristics that determine the base year informality status and the panel and employment retention. A positive (negative) sign indicates that individuals who are likely to be informal in the base year are more (less) likely to be retained in the following survey compared to formal individuals. The ρ_2 describes the relationship between initial informality and the individual's employment likelihood in the following period. A positive (negative) sign indicates that informal individuals are more (less) likely to be employed in the following period compared to formal ones. The ρ_3 describes the relationship between retention and employment state. A positive (negative) sign indicates that retained individuals are more (less) likely to be or to become employees compared to those who were not retained.

An important correlation to see is ρ_4. It describes the relationship between unobservable characteristics of individuals who were informal workers in the base year and those for informal individuals in the following period. As expected, a positive sign means that initially informal workers were more likely to remain informal in the following observed period, and the contrary for a negative sign. For the least two, the definition operates in the same way as in the first two.

With these components, a four-variate probit model can be defined to estimate the transition probabilities and their components. Combining the equations from equation (2.10), a derivation of the log-likelihood contribution for any man i is represented by:

$$\log(\mathcal{L}_i) = (1 - R_{i,t}) \log(\mathcal{L}_{A_i}) + R_{i,t}(1 - W_{i,t}) \log(\mathcal{L}_{B_i}) + R_{i,t} W_{i,t} \log(\mathcal{L}_{C_i}) \qquad (2.14)$$

These elements ask for two important conditions. On the one hand, an exogenous restriction must be in place for identification purposes. In a model with no conditional cross-equation correlations, it is important to declare regressors that are relevant for the endogenous equations that are at the same time conditioning the informality process. I will mention them in the data section.

A valuable feature of the cross-equation correlation is the possibility of identifying ignorable conditions. To test the ignorability of each selection mechanism, I test if the correlations between equations are jointly not significant. If $\rho_1 = \rho_2 = \rho_4 = 0$ the initial conditions equation can be ignored from the estimation. If the case is that $\rho_1 = \rho_3 = \rho_5 = 0$, retention is ignorable and, finally, if $\rho_2 = \rho_3 = \rho_6 = 0$ employment condition can be ignored.

To estimate the model, I used simulated maximum likelihood. I base the estimation exercise on the routines designed by Cappellari and Jenkins (2006a) to calculate multivariate normal probabilities by simulation generating a code that accounts for the likelihood function described in equation (2.14). The result of equation (2.14) is what Cappellari and Jenkins (2008) call 'partial likelihood' (or 'pseudolikelihood') since there is a violation of the standard assumption of independence of the error term across observations. The data consist of repeated observations of the same men across successive pairs of quarters, since I paired couples of quarters from the LFS panel data. With this, I used clustering of the error term at the individual level to adjust the variance-covariance matrix, since in this way arbitrary correlations between observations of the same individual can be allowed.

2.2.2 Data

Labor Force Survey of Jamaica

The dataset comes from two different sources. The first is the Jamaica Labour Force Survey (LFS) for the years 2004 to 2014 with some gaps. This survey is representative at the rural and urban level, at the parish (the largest geographical division), and at the national level. The LFS is implemented quarterly and is a rotational panel on dwellings.

The LFS has a two-stage stratified random sample design. In the first stage, a selection of primary sampling units (PSUs) is made, and in the second stage there is a selection of dwellings. A PSU is an enumeration district (ED) or a combination of EDs that is selected for a sample usually defined by the previous census. The ED division is the third level of geographical desegregation where the second is constituency and the first is parish. After the random selection of PSUs, a list of the dwellings located in each PSU is executed to define the master sample for the LFS. Each ED contains a minimum of approximately 100 dwellings in rural areas and a minimum of 150 dwellings in urban communities. After the EDs are selected, a list of dwellings is created; this list is the master sample. This master sample is revised every three to four years (for representativeness purposes).

The LFS is by nature a rotational panel on dwellings. Once the selected PSUs are listed, 32 dwellings are randomly selected from each PSU. These 32 dwellings are then divided into eight groups or panels of four dwellings each. Dwellings in panels

1 to 4 are interviewed in the first quarter LFS (16 dwellings per PSU each quarter). Dwellings in panels 3 to 6 are interviewed in the second quarter LFS. Dwellings in panels 5 to 8 are interviewed in the third quarter LFS. Dwellings in panels 1, 2, 7, and 8 are interviewed in the fourth quarter LFS. In the first quarter of the following year, dwellings in panels 1 to 4 are interviewed again and the yearly cycle is repeated (Table 2.2). This rotating panel scheme with the same dwellings lasts until the master sample is revised usually every three to four years.

From these data I kept individual characteristics and formality status for males for whom the time elapsed between observations is three to four quarters. I selected this sample for two reasons: on one hand, variables that could affect the labour endogenous choice for women like fertility are not available, making it hard to test the hypothesis on this population. On the other, a three to four quarters time frame will increase the chances of having storms in between and reacting to them between observations. The set of variables used as controls contains individual characteristics like age, education, occupation (a dummy=1 if individual has a professional occupation, zero otherwise), and geographic location (a dummy=1 if individual lives in rural area, zero for urban) in others. To create a proxy for informality, that is, the main outcome variable, I used the information declared by workers in two ways.[6] The first one, the National Insurance Scheme Criteria, defines a worker as informal if he satisfies any of the following conditions:

- Declares himself as employee of the private sector and carries out his job at his family dwelling; or
- Employee of the private sector and carries out his job at a family dwelling or plantation, garden, farm, employer's house, and the number of persons working in the business is 2 to 9; or

[6] In 2015, the statistics office of Jamaica, with the support of the Inter-American Development Bank, implemented an special survey on informality using sample from the LFS. The conditions presented here to create the variable for informality are the ones that characterise the respondents in the mentioned survey. The data is not available for public use, only the information to build the informality dummy from the LFS was provided for this study.

- Employee of the private sector and carries out his job at family dwelling or plantation, garden, farm, and the number of persons working in the business is 4 to 9; or

- Employee of the private sector and carries out his job at family dwelling or plantation, garden, farm, employer's house, Industry, factory, office, and the number of persons working in the business is 5 or 6.

The second, called firm registration criteria, defines an employer as informal if he satisfies any of the following conditions:

- Declares himself as self-employed and carries out his job at family dwelling; or

- Self-employed worker and the number of persons working in the business is 2 and he carries out his job at a plantation, garden, farm; or

- Self-employed worker and the number of persons working in the business is 3 and he carries out his job at employer's house; or

- Self-employed worker and the number of persons working in the business is 1 and he carries out his job at an industry, factory, house; or

- Self-employed worker and the number of persons working in the business is 2 to 4 and he carries out his job at construction site; or

- Self-employed worker and the number of persons working in the business is 1 and carries out his job on the street with fixed local; or

- Self-employed worker and the number of persons working in the business is 1 to 2 and he carries out his job on the street with no fixed local; or

- Self-employed worker and the number of persons working in the business is 1 to 2 and carries out his job at shop or store; or

- Self-employed worker and the number of persons working in the business is 1 to 2 and carries out his job at market, stall.

With the informality variable, an instrument for equation (2.1) is needed. One of Heckman's recommendations throughout his work is to use information from the time prior to the individual's working life as instrument for initial conditions equations. This kind of information, like the parent's labour history or their economic status when he was a child, is not available in this survey. With the data available, I used as instrument for equation (2.1) a variable that contains the working status in the past five years. The effect of this variable to the transition probability is through its effect on the initial conditions equation, which at the same time affects the probability of being employed in the following period. The instruments used in equations (2.3) and (2.5) of retention and working probabilities are discussed in the following section.

Empirical average transition probabilities form Labor Force Survey of Jamaica

After describing the dataset, I present a table that describes the average transition probabilities. Table 2.3 presents the transition probabilities for the sample used and, as can be seen, there are changes after accounting for the different sources of potential attrition bias (unemployment and panel attrition). I also take into consideration the possibility of item non-response as in Cappellari and Jenkins (2008). However, the sample under this condition is negligible since out of the final 56,251 men used in the estimation, less than 0.1 percent did not respond. With this, I define the attrition group to be filled by all those men that do not have a follow-up and those with item non-response.

It seems that attrition is not random in this sample. As can be seen in panel (a), the probability of being informal in the follow-up period is almost 10 times higher for those that were informal in the base period (71.1 versus 8.3).[7] In panel (b), the large share of unemployed in the follow-up period was informal in the base period. In this

[7]his paper is not intended to identify how persistent o heterogeneous is the state dependence. This is let for future research

case, the sample used was not only individuals that have information on informality in the follow-up period but also those that became unemployed.

Lastly, panel (c) incorporates the sample that cannot be followed due to attrition. The sample in this category is particularly large for men who were informal in the base period. These descriptions of the transition probabilities show that the attrition of the sample in not necessarily negligible. However, this research will shed light if including this factor in the analysis affects males' decision making on labour supply.

Storms and Geographical Data.

Data used for tropical storms is extracted from the International Best Track Archive for Climate Stewardship (IBTrACS) from the National Oceanic and Atmospheric Administration (NOAA). This dataset contains information on every tropical storm between 1969 and 2014 including date, trajectory, maximum sustained wind, radius of maximum speed, minimum central pressure (mb), and others. This information is collected every six hours for the storm's lifespan and is used to build the wind field model that is basic for destruction index calculations. Figure 2.2 shows the tracks and wind speed (in scale of thickness) for the set of storms used. Table D1 lists information regarding the dates of each storm, the maximum wind speed, and the category of the storm (Saffir-Simpson scale).

Following the discussion on identification of the endogenous equations, I use geographic characteristics to instrument retention and working probabilities. I use as an instrument for working probability in equation (2.5) the elevation of the district with respect to the sea level, and as instrument for the retention equation I use the linear distance between geographic district's centroid and Kingston. Some evidence in the survey design and labour economics (Antonovics et al. (2000) and Lall and Mengistae (2005), respectively) have found that geographic factors like distance to the nearest urban centre and the topographical characteristics of the firms' region affect largely the probability of attrition to surveys when panel data are at work and the working probabilities and specialization of the locals, respectively. With this, the distance to

Kingston will affect the probability of being followed in the panel, and through this it will affect the probability of being employed and the transition probability but not the initial conditions. On the other hand, the elevation would affect the probability of being employed, and through this, the transition probability but not the retention probability or the initial conditions.

Wind Filed Model and Storm Destruction Variable.

Following Strobl (2012) and Boose et al. (2004), the destruction variable built for Jamaica contains an approximation of the storm's local wind speed in every district on the island. Boose et al. (2004) tested the method using data from Puerto Rico, an island 700 miles east of Jamaica. Due to the vicinity of these two countries and their similarities in terms of location and vulnerability to tropical storms, it is possible to use estimated parameters from the abovementioned authors. The wind field model is the application of the Holland (1980) equation for cyclostrophic wind and sustained wind speed.

$$V_{d,s,r} = GF\left[V_m - S(1-\sin(T))\frac{V_h}{2}\right]\left[\left(\frac{R_m}{R}\right)^B \exp\left(1-\left[\frac{R_m}{R}\right]^B\right)\right]^{1/2} \tag{2.15}$$

$V_{d,s,r}$ is the estimate of the s storm's wind speed at some district d at some point in storm's life, r. V_m is the maximum sustained wind velocity that storm s reaches at any point, T is the clock-wise angle between the storm's forward path and the ray between the storm's center, and the centroid of district d, V_h is the forward storm's speed, R_m is the radius of maximum winds, R is the length of the ray that connects the storm's center and the district's centroid d, G is the gust factor, finally F, S and B are surface friction, asymmetry due to forward motion of the storm, and the shape of the wind profile curve, scaling parameters estimated by Strobl (2012) and Boose et al. (2004) for some Caribbean islands. Figure 2.3 depict the relationship between the mentioned variables. The information on the total wind received by a specific district is contained in the variable given by:

$$WIND_{d,s} = \int_{t}^{\tau} V_{d,s,r}^{3.8} dr \tag{2.16}$$

where $WIND_{d,s}$ is the destruction variable estimated for district's centroid d and it is equal to the summation of the values of wind field to the a power of the storm s' lifespan. The GIS data contains an observation for each tropical storm every six hours, so that, for each one of them, I estimate the wind field model $V_{d,s,r}$ for storms that are between 0 and 310 miles from the closest district as depicted in Figure 2.2.[8] The 3.8th power depicts the relationship found by Strobl (2012) between total costs due to hurricanes and the maximum observed wind speeds. Figure 2.4 shows an example using hurricane Ivan's destruction proxy. In the figure, districts with red values suffer large destruction due to wind and less destructive values correspond to districts with orange coloring, finally, the black line represents the hurricane's track.

In terms of the estimation, the variable called S in equations (2.11) and (2.12) are filled with $WIND_{d,s}$ and the its square. The process is as follows:

Step 1. Using man i's quarter of survey (D_i^b) and the date of storm's observation (D_s) I know if he was hit one or two quarters before the observation in period t as follows

1. If $D_s - D_i^b \in [-3, 0]$ months, man was hit one quarter before last observation t.
2. If $D_s - D_i^b \in [-6, -4]$ months, man was hit two quarters before last observation t.

With these criteria I can define dummy variables, one per period of interest.

Step 2. The dummies crated above have a value one if a storm s hit man i in quarter q. To create the treatment as the total shock received by man i in period q, there were multiplied the correspondent destruction index for storm s times the

[8]As mentioned in Strobl (2012), this assumption relies on the fact major storms can reach a diameter of more than 600 miles.

dummy correspondent to man i quarter q. This procedure creates treatment variables with values correspondent to the destruction indexes in each position where the dummies have value equals to 1. Finally we add all the values over quarter q by each man i due that more than one storm may hit during the same quarter.

Figure 2.5 contains a graphic example of the variation in time I am exploiting. The first column depicts the quarter for which there are data available from LFS. In the second column, a dummy that informs the availability of storm in each quarter that tells about the storms suffered one or two quarters before the last interview. After that, in green are the quarter/LFS panel implemented and in red the period in which the storm is located between rounds. Take as example panels C and D in 2005q2. This sample is affected by storms between observations that are three or four quarters apart. The same analysis operates for panels E, F, G, and H. The particular advantage of the sample comprising panels A and B is that it is like a control group that did not suffer from storms between rounds. This structure defines the sample used to estimate the endogenous choice model.

2.3 Results from the Estimation of the Four-variate Probit

2.3.1 Ignorability Tests

One of the aims of the endogenous choice model is to account for potential selection and to determine whether there is a relationship in non-observables between decisions. From Table 2.4, the only non-observable correlations that are different from zero are the ones between retention in the panel and being informal in $t-1$ and the one for informal in t and working probability in t. However, this result does not indicate ignorability of conditioning decisions.

To define ignorability, the null hypothesis of correlation equal to zero should be rejected. As can be seen, the only selection equation that can be ignored from the analysis is retention.

2.3.2 The effects of tropical storms

Table 2.5 presents the results of the effect of storms on the probability of transitioning to informality. In the first column, I present the average individual marginal effect evaluated at means, in the second the parameter from the multivariate probit estimation and their correspondent standard errors. The model fits well the observed transitions, since for the initially informally employed individuals, the predicted probability of falling into informality is the same as panel (a) in Table 2.3, although the other one is larger. In terms of estimated parameters, the effect of the storms on the probability of the latent variable has a U-shape, meaning that beyond a certain threshold, the probability of become informal is positive on the destruction due to the storm, disregarding the initial status. The regression includes controls and instruments described in the previous section and the parameter estimations can be found in the Appendix E.[9]

As noted by Stewart and Swaffield (1999), Cappellari and Jenkins (2008), Mullahy (2015), and Mullahy (2016), computing marginal effects for this class of models is hard. The complication rises from the correlation across equations that makes possible that one variable not only affects the equation that it is modeled initially but also other equations for which the first one is conditioning. With this, the method used here is the one proposed by Mullahy (2016) based on the following formula that is the derivative of the four-variate normal distribution with respect to storm s:

$$\frac{\partial \Phi_4(I_{i,t-1}\Psi_{1,i} + F_{i,t-1}\Psi_{2,i}; \widehat{\rho_1}, \widehat{\rho_2}, \widehat{\rho_3}, \widehat{\rho_4}, \widehat{\rho_6}, \widehat{\rho_6})}{\partial S_{d,t,1}} =$$

[9] As technical note, I estimated the model using the *mvnp* from Cappellari and Jenkins (2006a) with 280 simulations that is the square root of the total sample used, that is the amount of simulations recommended by the authors to optimise the simulations. I used a Stata 14 MP with 4 cores in a Mac Book Pro with intel i7 processor of 2.5Ghz. I adapted the plugin designed by professor Jenkins for Macintosh due that it was not available for this platform and only restricted to Windows. You can download the c++ file for plugin and the plugin from my webpage: https://sites.google.com/site/camilopechag/home/research/stata-files

$$\phi(I_{i,t-1}(k_{4,i}\Big[\widehat{\boldsymbol{\beta}}_1'\mathbf{x}_{i,t-1}^{I_t}+\sum_{\tau=1}^{2}\widehat{\boldsymbol{\varphi}}_{1,\tau}'\mathbf{S}_{d,t,\tau}\Big])+F_{i,t-1}(k_{4,i}\Big[\widehat{\boldsymbol{\beta}}_2'\mathbf{x}_{i,t-1}^{I_t}+\sum_{\tau=1}^{2}\widehat{\boldsymbol{\varphi}}_{2,\tau}'\mathbf{S}_{d,t,\tau}\Big]))\times$$
$$\Phi_3(I_{i,t-1}\Psi_{1,i}^{-I_t}+F_{i,t-1}^{-I_t}\Psi_{2,i};\widehat{\rho}_1,\widehat{\rho}_2,\widehat{\rho}_3)\times$$
$$\widehat{\varphi_{1,1,1}}+2\widehat{\varphi_{1,2,1}}S_{d,t,1} \tag{2.17}$$

This equation computes the individual marginal effect of the storms suffered one quarter before the last interview by men that were informal in $t-1$. The same process applies for the other 3 sets of parameters (equation (2.10)). As can be seen, it is used as input the estimation from the four-variate probit and the mean of covariates to calculate the derivative.

The average individual marginal effect evaluated at the average storm seems to be negative. As mentioned before, there is a U-shaped relationship between the storms and the probability of the latent variable. When I evaluated the marginal effect at the mean of the observed storm vector, it seems that the average effect of the storm is negative and significant, although small. To better understand what could be the implications of increase in power of the storm I graph the average individual marginal effect evaluated on different values of the destruction variable.[10]

As can be seen in Figures 2.6 and 2.7, the main positive effect comes from the destruction generated by mainly large shocks. In both cases, when individuals' initial state is informal or formal, there is a threshold around 7.5 million in the destruction variable where the probability of falling into informality is positive. Compared with the values that non-hurricane storms generate, hurricanes are mainly responsible for this finding.

The potential magnitude of destruction generated by hurricanes is broadly known. These kinds of events are, by nature, one of the largest catastrophes that economies can suffer. There is evidence worldwide about losses in infrastructure: roads covered by landslides due to heavy rainfall, bridges destroyed due to the intensity of rain

[10] I only used 10 data points belonging to the upper bound of each decile of the destruction variable's distribution. This was done this way since the estimation of the individual marginal effect at each point require the simulation of the four-variate normal distribution, so that the computation of every data point in the data set would requires near to a year of computing time, per variable.

and wind power, destruction of buildings such as hospitals and schools, shoreline and costal line covered with debris and the wind-gust factor due to the storm, food production decreases due to the destruction of tall crops such maize and rice, and increases in assets prices due to scarcity are some of the results of this class of events (http://www.desinventar.org). With this, the evidence found in these pages is related with a general equilibrium effect where all these conditions could push workers into informality if they were formal or to induce them to remain informal.

The method of testing for transmission channels is not straightforward. This study has found that, on average, the effect of big storms is negative. The increase in storms' destructive power will move workers from the formal sector to informality, and this should have heterogeneous magnitude with respect to the sector in which the man was initially employed. Coastal economies commonly depend on services, such as tourism, that could be directly affected if infrastructure is not suitable to receive new costumers. Another potentially affected sector is agriculture, since this kind of shocks could imply moving people from the rural formal sector to the informal sector. Although there are no data available to test the transmission channel, a study on the heterogeneous effect by sector is needed, no only to understand the actual channels but also to encourage better targeting and design of private and public policy.

Climate change is generating conditions for worse storms to spawn. Although there is no consensus, it is true that the increase in the sea water level and temperature are elements that will help increase storms' strength and destruction power so that increasing its effect on various aspects of social welfare as the labour situation. These conditions and the increase in temperatures worldwide will create big losses in national economies, as mentioned by Carleton and Hsiang (2016).

2.4 Conclusion

In this paper I used an endogenous choice model to determine the causal effect of storms on the probability of transitioning to informality. The study uses data from a labour force survey of Jamaica and geographical and physical information from 35 storms (13 tropical depressions, 11 tropical storms, and 11 hurricanes) between 2004 and 2014. I interacted a measure for the total amount of destruction generated by a set of storms that affected the district with the males reported in the Labour Force Survey in that district. I created a pseudo panel of individuals due to the nature of the survey to describe the transition probabilities between the formal and informal sectors to informality across time. The main question was, do tropical storms have any effect on the probability of transitioning to informality? The identification strategy relies on the exogenous variation at the geographic, time, and strength of the events with which I estimated a four-variate probit to control for three potential biases: initial conditions, panel attrition, and employment selection.

I found that, regardless of the initial state of being a formal or an informal worker, the effect of tropical storms on the probability of transit to informality is positive. An advantage provided by the dataset I created is that I can test how the effect behaves as the accumulated power derived from tropical storms increases. The estimation results show that as the accumulated amount of destruction derived from the storms suffered during one to two quarters between observations, the probability of remaining informal across periods varies between 1 and 11 percent, particularly due to hurricanes. The proper probability for those men that start from a formal job varies between 1 and 12 percent. This model also controls for education, age, and occupation. This evidence would go in the opposite direction from Bosch and Maloney (2010), who find that informality is a voluntary state, since these exogenous events can be seen as pressure for workers to move, involuntarily, to informality. It is important to note that the results obtained are lower bounds of the potential effects, since the correlation between the actual location of the individual at the moment of the event is not perfectly correlated with the destruction variable; however, it is the better approximation at the moment.

Another note is that the results are average storms' effect on working men. A potential heterogeneity can be present in the initial state-industrial sector, since the service sector might behave differently than agricultural sector, for example. This study is still open and will also shed light on the actual transmission channel since at the moment it appears that the effect found is a general equilibrium effect where a certain part of the demand or inputs used by the labour market are affected by the storms. However, at this point there is not data to test this possibility.

The evidence found represents a step further in our understand of the potential effects of climate change. Jamaica, as Small Island Developing State, is threatened by the rise in the sea level and temperature fluctuation, important factors in increasing the intensity of tropical storms. As described by NOAA,

> Anthropogenic warming by the end of the twenty-first century will likely cause tropical cyclones globally to be more intense on average. This change would imply an even larger percentage increase in the destructive potential per storm, assuming no reduction in storm size.[11]

With this, the danger caused by increasingly worse storms urges for new policies on adaptability and resilience.

In terms of coping tools for workers to bear the greater risks due to environmental shocks, a discussion on prevention must take place. The literature has found that the most important instrument available to businesses to cope with negative environmental shocks is preparation. McDonald et al. (2014) study how preventive measures, formal financial instruments, and informal insurance are used as mitigation and adaptation mechanisms against hurricanes. These authors found that the most effective mechanisms to cope with the negative effects of strong hurricanes are preparedness and formal financial instruments. They also found that informal insurance (financial help from friends and family, household resources, etc.) has no effect on offsetting the negative impact of hurricanes on businesses. They implement a bivariate probit estimation, controlling for bias that could arise from business demise. The evidence

[11] Geophysical Fluid Dynamic Laboratory. https://www.gfdl.noaa.gov/global-warming-and-hurricanes/ Visited on November 28, 2016

found in this paper and in the literature can help stimulate a discussion on the instruments that the public and private sector should create to help the population overcome negative shocks.

As mentioned by Hallagate et al. (2015), the informal population is badly equipped to bear with environmental shocks. These authors, and others like Boccanfuso and Savard (2011), show that workers in the informal sector are younger, with lower levels of educational attainment, and poorer than those in the formal sector. With this, understanding of the potential effects of climate shocks, access to insurance and financial instruments are low, making them more vulnerable and prone to fail in entrepreneurship endeavours. With this, falling into informality or remaining in it is not only an economic issue but a welfare issue because the productive system will decay and the sources of income become harder to find, as noted by Acevedo (2015) in the Colombian case. This analysis also follows findings by Carter et al. (2007) who show that natural disasters like droughts and hurricanes can push people to into poverty, since mechanisms to overcome the negative effects of such events are not available for this population.

The results add to the actual barriers to job formalization. As mentioned in FORLAC (2014), the most important barriers found in Jamaica (that probably exist in most developing economies) are "...low economic growth with low employment generation, tax incentives that give priority to capital intensive projects, sustained public spending adjustment policies, and low institutional capacity to promote and monitor compliance with labour standards or promote formality in employment." (FORLAC (2014)). With this, and few resources to confront natural disasters, informal labor in the economy could become stagnant and impossible to remove.

The results found are not definitive and need more research. For this study, a great effort to gather and harmonise data was implemented. However, computational constraints prevented deeper exploration. There are two main potential ways to expand this research: first, it is necessary to expand the state space since the only states studied were informal and formal, leaving out of analysis the unemployed and those who are out of the labour force. However, expansion of the state space requires more instrumental variables to identify all the equations that will be interacting in the multivariate probit and also more computational power since the simulation required for the estimation grows exponentially with the number of equations used in the model.

The second way to expand the research is the heterogeneous effect. It is extremely important to determine whether there are any differences in how industrial sectors are affected due to tropical storms in losing productive force and what switches workers make because of the storms.

Finally, from a macro perspective, Deryugina (2016) opens a new question regarding the fiscal instruments used to compensate for the negative effects of tropical storms. In this case, it would be interesting to study whether the implementation of free access to public health in Jamaica implies a reduction in the potential negative effects in health due to storms. On the other hand, it is interesting to understand how other fiscal instruments, like the national insurance scheme, reacts to tropical storms. Also, more household data from other Caribbean economies are needed to explore the external validity of these estimations.

2.5 Figures and Tables

Figure 2.1: Chain of events.

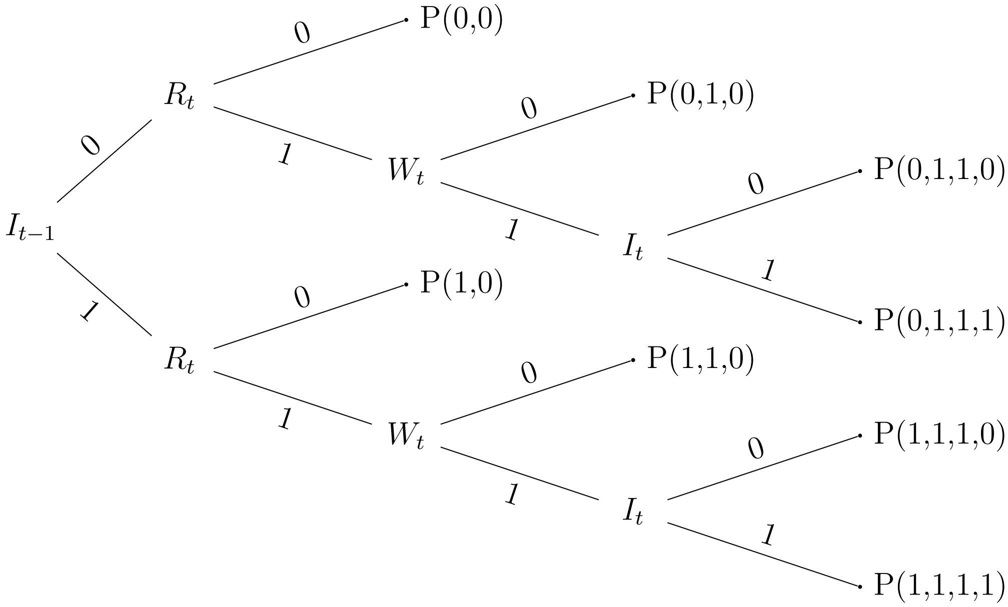


Figure 2.2: Set of storms used

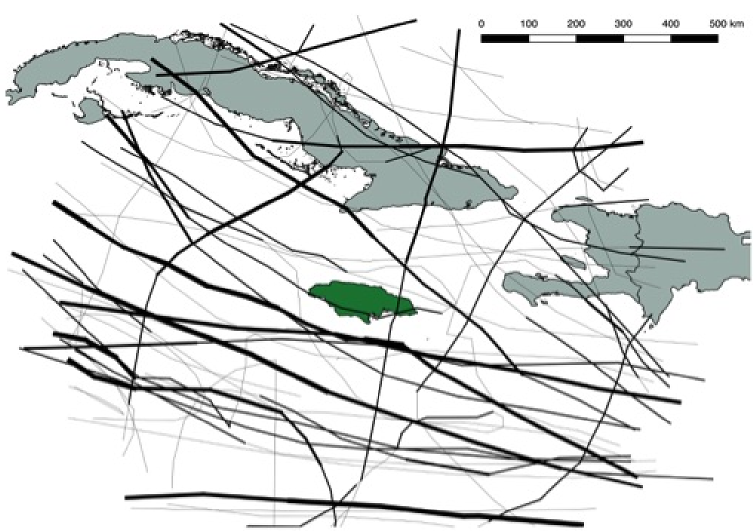


Figure 2.4: The destruction variable generated by hurricane Ivan in 2004

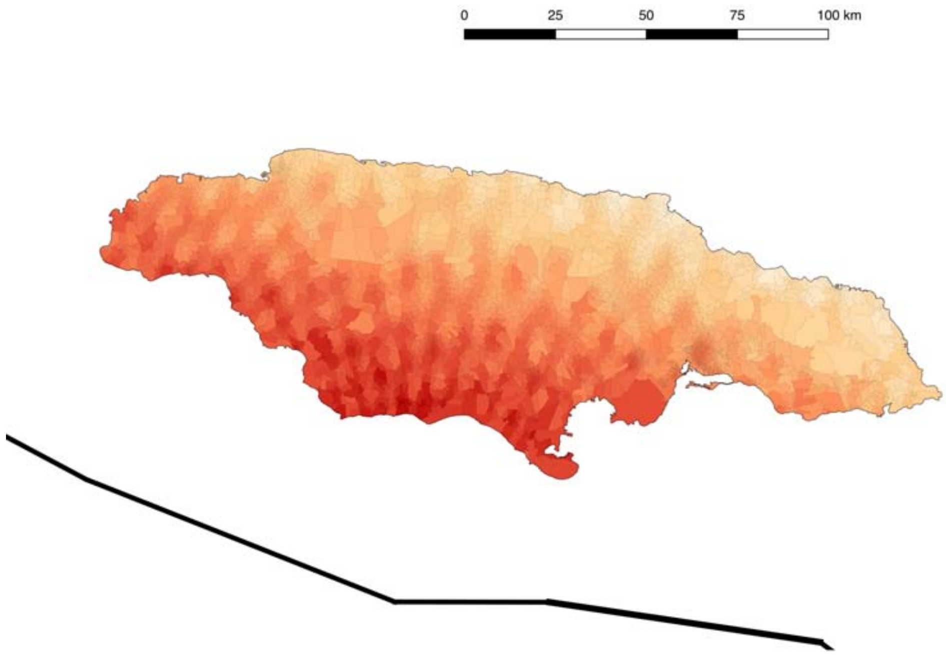


Figure 2.3: Wind field model structure based on Boose et al. (2004)

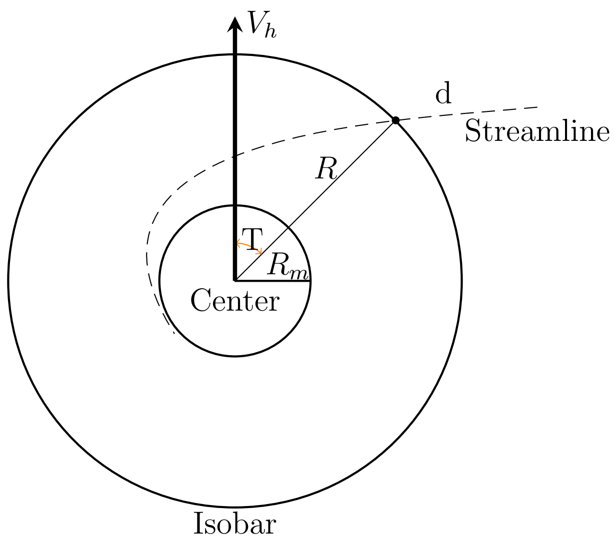
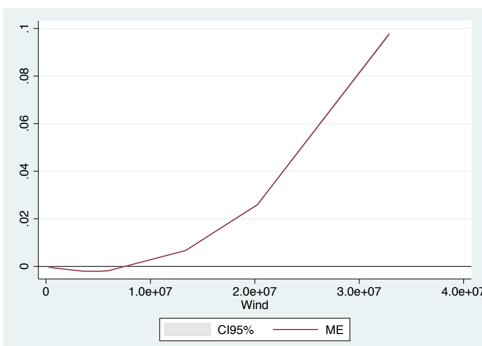


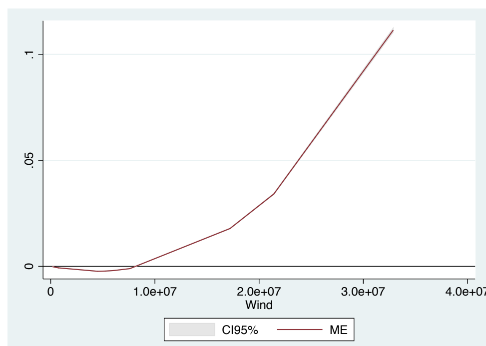
Figure 2.5: Sample of estimation: example

Quarter	Storm 1Q or 2Q before last interview (1/0)	Panels Surveyed in LFS							
		A	B	C	D	E	F	G	H
2005q1	0								
2005q2	0								
2005q3	0								
2005q4	1								
2006q1	1								
2006q2	0								
2006q3	0								

Figure 2.6: Marginal effects for informal to informal probability at different values of storms.



(a) Marginal effect of storms one quarter before t

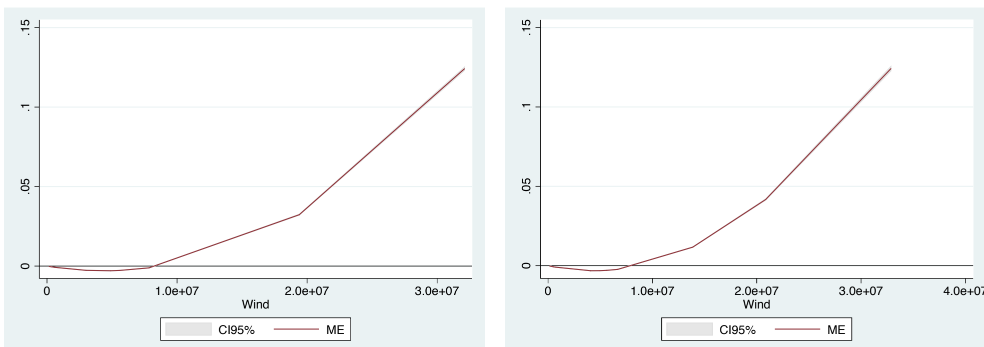


(b) Marginal effect of storms two quarters before t

Table 2.2: LFS Rotational panel

		A	B	C	D	E	F	G	H
Year t-1	January								
	April								
	July								
	October								
Year t	January								
	April								
	July								
	October								

Figure 2.7: Marginal effects for formal to informal probability at different values of storms.



(a) Marginal effect of storms one quarter before t

(b) Marginal effect of storms two quarters before t

Table 2.1: Likelihood contribution groups based on Cappellari and Jenkins (2008).

Group	Retention	Working	Informal	Interpretation
A	$R_{i,t} = 0$	No observed	Not observed	Panel attrtition
B	$R_{i,t} = 1$	$W_{i,t} = 0$	Not observed	Retained, unemployed
C	$R_{i,t} = 1$	$W_{i,t} = 1$	$I_{i,t} = 0$	Retained, formal employee
	$R_{i,t} = 1$	$W_{i,t} = 1$	$I_{i,t} = 1$	Retained, informal employee

Table 2.3: Empirical average informal job transition probabilities for Jamaican men.

Year $t-1$ State	Year t state (row %) Formal Group C	Informal Group C	Unemployed Group B	Attrition Group A
(a) Panel with $t-1$ and t information on informality (N=56839)				
Formal	91.3	8.7		
Informal	22	78		
All	69.8	30.2		
(b) Accounting for and missing information (N=60398)				
Formal	87.2	8.3	4.5	
Informal	20.1	71.1	8.8	
All	65.7	28.4	5.9	
(c) Accounting for and missing information (N=102572)				
Formal	52.5	5	2.7	39.7
Informal	11.3	39.9	5	43.8
All	38.7	16.7	3.5	41.1

Groups A, B, and C were defined in Table 2.1. The data used was the quarterly Labor Force Survey (with gaps) for between 2004 and 2014.

Table 2.4: Ignorability tests

Correlation of unobservables	Parameter	Estimate	SE
Retention, Informal at $t-1$	ρ_1	-0.018	0.044
Working at t, Informal at $t-1$	ρ_2	-0.17***	0.014
Working at t, Retention	ρ_3	-0.07	0.17
Informal in t, Informal at $t-1$	ρ_4	0.18	0.19
Informal in t, Retention	ρ_5	0.002	0.007
Informal in t, Working in t	ρ_6	-0.55***	0.18
Ignorability tests	χ^2	p-values	
Initial conditions $H_0 : \rho_1 = \rho_2 = \rho_4 = 0$	135.97	0.000	
Panel retention $H_0 : \rho_1 = \rho_3 = \rho_5 = 0$	0.31	0.9589	
Working $H_0 : \rho_2 = \rho_3 = \rho_6 = 0$	141.71	0.000	
Unobserved heterogeneity	145.34	0.000	
$H_0 : \rho_1 = \rho_2 = \rho_3 = \rho_4 = \rho_5 = \rho_6 = 0$			

Table 2.5: Transition probabilities for Jamaican men: Estimated parameters and Average individual marginal effect from multivariate probit.

Variable	$P(I_t = 1 \mid I_{t-1} = 1)$ ME (SE)	$P(I_t = 1 \mid I_{t-1} = 1)$ MVProbit Parameter (SE)	$P(I_t = 1 \mid I_{t-1} = 1)$ Univariate Probit (SE)	$P(I_t = 1 \mid F_{t-1} = 1)$ ME (SE)	$P(I_t = 1 \mid F_{t-1} = 1)$ MVProbit Parameter (SE)	$P(I_t = 1 \mid F_{t-1} = 1)$ Univariate Probit (SE)
Predicted probability	0.78			0.13		
Storm 1 Quarter before		-8.43e-09	-1.50e-08**		-1.21e-08*	-1.39e-08**
	-0.001***	(8.56e-09)	(6.89e-09)	-0.002***	(7.15e-09)	(6.82e-09)
Storm 1 Quarter before (sqr)	(1.91163E-06)	4.80e-16	8.43e-16***	(6.44685E-06)	5.85e-16*	6.50e-16**
		(4.03e-16)	(3.23e-16)		(3.34e-16)	(3.21e-16)
Storm 2 Quarter Before		-1.21e-08*	-2.18e-08***		-1.01e-10	-1.90e-09
	-0.0008***	(7.30e-09)	(6.02e-09)	-0.002***	(6.22e-09)	(5.87e-09)
Storm 2 Quarter Before (sqr)	(2.18521E-06)	6.74e-16**	1.08e-15***	(7.28392E-06)	1.36e-16	2.16e-16
		(3.40e-16)	(2.76e-16)		(2.82e-16)	(2.69e-16)

The four-variate probit estimation also uses dummies for year apart from the variables described in section 2.2.2. Estimated standard errors, reported in parentheses, are clustered at the individual level as described in the methodology section. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Chapter 3

Healthy to Work: The Impact of Free Public Healthcare on Health Status and Labor Supply in Jamaica[1]

Abstract

This study examines whether Jamaica's free public healthcare policy affected health status and labor supply of adult individuals. It compares outcomes of adults without health insurance versus their insured counterparts, before and after policy implementation. The study finds that the policy reduced both the likelihood of suffering illnesses with associated lost work days and the number of lost days due to illnesses by 28.6 percent and 34 percent, respectively. Consistent with the absence of "employment lock", no effects are found on employment at the extensive margin. However, consistent with a reduced number of days lost due to illnesses, there is a positive effect of 2.15 additional weekly labor hours. This is primarily a labor supply effect as the study shows that both reported and imputed hourly wages decreased by 0.15 and 0.06 log-points respectively. Back-of-the-envelope calculations suggest that the policy added a yearly average of US$PPP 26.6 million worth of net real production to the

[1] This paper is co-authored with Diether Beuermann. This is paper has been published at the Inter-American Development Bank as Working Paper. Click here to download: https://publications.iadb.org/handle/11319/7970

economy during the period 2008-12.

JEL classification: H51, I1, J22, O12, O54.

Key words: Free Public Healthcare, Health Status, Labor Supply, Jamaica.

3.1 Introduction

In April 2008, the Jamaican government passed a no-user-fee policy applicable to all public health facilities. This policy implied that Jamaicans no longer had to pay for healthcare services such as doctor's consultations, diagnostic services, hospital admissions, surgeries, medications, physiotherapy, ambulance, maternal services, and so forth. Prior to this policy, uninsured persons using public health facilities were required to pay out-of-pocket fees for these services. The rationale behind this policy was that user fees were regressive and prevented healthcare access to disadvantaged sectors of the population who could not afford the fees (Jamaican Ministry of Health, 2008). This type of policy is not idiosyncratic to Jamaica. As shown by Giedion et al. (2013), around thirty countries have implemented similar programs, and many others are considering doing so.

The policy, therefore, provided free universal public healthcare. One of the key motivations underlying implementation of the policy was that fees conveyed a negative impact on healthcare access resulting in deteriorating health outcomes and productivity losses. Therefore, it is relevant to evaluate whether the Jamaican policy influenced the health status of its direct beneficiaries (i.e., persons without health insurance). Furthermore, if increased healthcare access improved the average health of the benefited population, it could have originated positive effects on labor supply (Strauss and Thomas (1998)). Accordingly, the aim of this paper is estimating the causal effects of Jamaica's policy of providing free public healthcare on overall health status and labor market dynamics.

Related literature provided for the United States suggests the existence of a causal relation between health insurance and healthcare utilization (Anderson et al. (2012b);

Anderson et al. (2012a); Beuermann (2010); Card et al. (2009); Finkelstein (2007)). Similarly, Kondo and Shigeoka (2013) found that the universal health insurance implemented in Japan in 1961 had positive causal effects on hospital admissions, inpatient days, and outpatient visits. Bernal et al. (2014) showed that the provision of free health insurance among individuals out of the formal labor market in Peru had positive causal effects on the likelihood of visiting a doctor, receiving medication, receiving prenatal care, and being vaccinated. Knox (2016) showed that Mexico's Seguro Popular (SP) program -a health insurance scheme for informal workers- increased overall usage of public health centers and total medical visits. Gruber et al. (2014) found that Thailand's 2001 healthcare reform, which reduced copays to US$0.75, increased healthcare utilization, especially among the poor. Therefore, previously examined evidence consistently suggests a positive causal relation between the provision of health insurance and the utilization of health services. These results suggest that governments planning large expansions in public health insurance coverage would need to devote sufficient financial and human resources to cover the expected surge in healthcare demand.

Previous studies have also assessed the effects of health insurance on health status in different contexts. In the United States, Card et al. (2009) showed that health insurance coverage provided at age 65 reduced deaths among recipients of emergency services by 20 percent. Tanaka (2014) studied South Africa's experience where health user fees for children were abolished. The study found positive effects for early childhood development indicators measured by weight-for-age z-scores among children below six years old. Similarly, Gruber et al. (2014) found that prior to Thailand's 2001 healthcare reform; poorer provinces had significantly higher infant mortality rates than wealthier ones. After the reform, the authors found that this correlation evaporated to zero. Shigeoka (2014) studied the effects on mortality and expenditures of a reduction in patient-shared costs at age 70 in Japan. Findings suggest that there were little impacts on mortality and other health outcomes. Knox (2016) found that Mexico's SP program caused long-term (five years after program enactment) improvements in health measured by normal days lost due to illnesses-but only for women and girls under 10 years old. Therefore, evidence on the relation between health insurance and health status is somewhat mixed depending on the context and age group assessed.

Another strand of literature has studied labor market effects resulting from the provision of free healthcare, with a particular focus on informality. Mexico's SP program has attracted attention because it targeted informal workers. Therefore, several studies have tested whether SP altered the incentives of workers toward switching away from formality. Aterido et al. (2011) found that SP increased the share of informal workers by one percentage point. Similarly, Azuara and Marinescu (2013) found that SP increased the share of informality among the unskilled by around 0.9 percentage points. Bosch and Campos-Vasquez (2014), using detailed social security administrative records and the entire period of the program's rollout, showed that SP had a negative effect on the number of employers and employees formally registered in small- and medium-sized firms (up to 50 employees) equivalent to 4.6 percent and 4 percent, respectively. In Thailand, Wagstaff and Manochotphong (2012) found that universal health coverage encouraged employment among married women increasing their participation in the informal sector, and reduced formal sector employment among married men. In summary, the evidence suggests that targeting health coverage to the informal sector in environments with high informality can incentivize workers to leave the formal economy.

Discussion has recently emerged within the US environment related to the Patient Protection and Affordable Care Act (ACA). Prior to ACA, individuals primarily obtained health insurance coverage through their employers, as individually purchased plans were expensive and public health insurance was limited to specific segments of the population. Therefore, some argued that a significant share of the population sought employment purely to gain coverage (a phenomenon known as "employment lock"). However, as ACA made private insurance more affordable and expanded coverage of public health insurance, the policy could have reduced "employment lock", thereby reducing labor force participation. Consistent with the existence of "employment lock", initial findings provided by Garthwaite et al. (2014) exploiting an abrupt disenrollment of individuals insured by Medicaid (a means-tested publicly provided health insurance) in the state of Tennessee suggested a positive effect on employment. However, in contrast to previous findings, Leung and Mas (2016) did not find employment effects in response to the expansions of Medicaid resulting from ACA implementation.

We contribute to the international literature by presenting evidence on the effects of free public healthcare arising from a context not previously studied. Indeed, the Jamaican no-user-fee policy and context differs from previous studies in several aspects. First, the policy did not have any demographic targeting mechanism. This allows for study of the health effects on the economically active population (21-64 years old), which contrasts with previous studies focusing, due to policy design, either on children (Tanaka (2014)) or the elderly (Card et al. (2009); Shigeoka (2014)). Second, the policy did not include targeting mechanisms related to employment or formality status. As such, incentives to switch from the formal to the informal sector to benefit from the policy did not operate. Third, employer-sponsored health insurance in Jamaica is optional and limited. As a result, motivation to participate in the labor force is presumably unrelated to a pure motivation to access affordable health insurance or, in other words, "employment lock" is unlikely to exist. To the extent of our knowledge, this study is the first assessing the effects of free public healthcare on health outcomes and labor market dynamics among the economically active population in the absence of both incentives to become informal and "employment lock".

Disentangling causality between the policy and health or labor market outcomes is problematic. This problem exists because before and after comparisons would confound pre-existing trends with the program's effect. Therefore, to disentangle causality from secular trends, we used data from two household level surveys: the Jamaica Labor Force Survey and the Survey of Living Conditions. We stacked yearly waves of these surveys from 2002 until 2012 as a district level panel. Then we implemented a difference-in-differences strategy controlling for time invariant unobservable characteristics at the district level and exploiting two sources of variation. The first source is the timing of the policy enactment (i.e., before vs. after policy adoption), while the second is the cross-sectional individual level variation in the availability of health insurance (i.e., individuals without vs. individuals with access to formal health insurance).

Our main findings suggest a reduced likelihood of suffering illnesses associated with inability to carry out normal activities equivalent to two percentage points (or 28.6 percent with respect to the baseline mean). At the intensive margin, we find that the number of days where people were unable to perform normal activities due to illnesses suffered within the previous four weeks decreased by 0.17 days (equivalent to 34 percent with respect to the baseline mean). Therefore, there is evidence that the policy increased the general health of the population and, as suggested by Strauss and Thomas (1998), this could have translated into increased labor supply.

Consistent with the absence of "employment lock," we find no effects on the likelihood of employment at the extensive margin. We also find no effects on the likelihood of contributing to the social security system (a measure of labor formality). However, consistent with a reduced number of days lost due to illnesses, we find a positive effect of 2.15 additional weekly labor hours. We suggest that this effect at the intensive margin is primarily a labor supply effect as we show that both reported and imputed hourly wages decreased by 0.15 and 0.06 log-points respectively. In addition, we find that adults in the 40-64 age range (who were relatively disadvantaged at baseline regarding their health status) drive the positive health and labor supply estimated benefits. Back-of-the-envelope calculations suggest that the policy added a yearly average of US$PPP 26.6 million worth of net real production to the Jamaican economy during the period 2008-12.

The remainder of this paper is organized as follows. Section 2 gives a brief overview of the no-user-fee policy adopted in Jamaica. Section 3 describes the data. Section 4 presents the empirical strategy. We present our results in Section 5. Section 6 concludes.

3.2 The No-User-Fee Policy in Jamaica

Jamaica is an island country located in the Caribbean Sea 145 km south of Cuba and 191 km west of Hispaniola (Haiti and Dominican Republic). Administratively, the country is divided into 14 parishes. They are grouped into three historic counties, which have no administrative relevance. Every parish has a coast; none is landlocked. Jamaica is one of the largest economies in the Caribbean and qualifies as an upper-middle-income country. Its GDP is mainly driven by services (70 percent) related to the tourism industry. The main commodities produced in the country are alumina and bauxite, representing around 5 percent of GDP.

In April 2008, the government of Jamaica abolished all the user fees for facilities within the public health system, including hospitals, health centers, laboratories, diagnostic facilities, and pharmacies.[2] The elimination of fees also applied to medical services like registration, doctor's consultations, diagnostics, hospital admission, surgery, medications, physiotherapy, ambulance, and maternal care. Prior to this, individuals were required to pay out-of-pocket fees for these services. The main considerations underlying adoption of this policy included: (a) the fees were regressive and a major impediment to access to health; (b) the fees increased poverty because they reduced the disposable incomes of the poor and depleted their asset base; and (c) the fees had a negative effect on utilization resulting in deteriorating health outcomes, increasing morbidity and reduced life expectancy (Jamaican Ministry of Health, 2008).

According to the 2007 Jamaica Survey of Living Conditions, the second most important reason for not visiting a physician during illness episodes-which accounted for 17 percent of respondents-was that healthcare was not affordable (the first reason was that the illness was not serious enough, accounting for 40 percent of respondents).

[2] In May 2007, fees were abolished for children below 18 years old. Then in April 2008, fees were abolished for all users of the public health system. Since we will focus on persons between 21 and 64 years old, the relevant date in which these individuals were affected by the policy was 1 April 2008.

Moreover, this problem was more severe among households in the lowest quintile of per capita consumption, where 32 percent reported not visiting a physician while ill due to their inability to afford the associated fees. This figure drops to 15 percent among households in the second quintile of per capita consumption. For households in the third and fourth quintiles, the figure was 11 percent, and for the highest quintile, it was 4 percent. Therefore, the pre-policy evidence supports the regressive characteristic of health fees.

As such, the policy intended to "... improve access to healthcare for poor Jamaicans; reduce inequity in accessing health services; reorient the public health system to reflect a primary care focus; enhance staff efficiency by providing the right skill mix for service delivery; and find suitable financing and service delivery mechanisms" (Jamaican Ministry of Health, 2008).

Official statistics reveal that utilization patterns of the public health system saw significant shifts after policy adoption.[3] Indeed, average annual utilization between the years leading to the policy (2003-06) and the first four years of policy implementation (2008-11) showed significant increases in several types of healthcare services (Figure 3.1). The annual number of outpatient visits increased by 21 percent, emergency visits climbed by 58 percent, and hospital admissions grew by 8 percent. The number of laboratory tests performed jumped by 135 percent, while filled pharmacy prescriptions increased by 84 percent. X-Ray procedures showed a shift equivalent to 12 percent, but the bulk of the surge occurred in 2007, which was the year when fees were abolished for children under 18 years old. Finally, the number of surgeries (non showed in figure) seems more stable pattern with a shift of 5 percent before and after policy adoption.

Public expenditures in health jumped from a pre-policy (2002-06) yearly average of 2.42 percent of GDP to a post-policy yearly average (2008-12) of 3 percent of GDP. The extra funds were supposed to compensate for lost revenues from fees and satisfy the surge in patient load. However, the Medical Association of Jamaica (MAJ) argued that the additional public funds injected were insufficient to ensure the smooth

[3] Official statistics from the Jamaican Ministry of Health reported in Campbell (2013).

running of the health service. The MAJ suggested that the policy failed to address fundamental issues, such as upgrading primary care services, securing adequately trained and appropriately paid medical staff, and educating the public about the appropriate use of hospitals (De La Haya and Alexis (2012)). The authors report that inadequately staffed health facilities with respect to the increased demand has resulted in excessive waiting periods of up to 6-8 hours for non-emergencies.

The average real expenditure per medical service provided in public health facilities dropped by 19 percent between 2006 and 2009. Therefore, the increased demand outweighed the extra public funds invested in the health system after policy adoption. As such, it appears that the quality of public health services freely provided after policy adoption was not optimal; this is something to bear in mind when interpreting our results.

3.3 Methodology and Data

3.3.1 Data

We relied on two main sources of information. First, we used the Jamaica Labor Force Survey (LFS). The LFS is a quarterly survey representative at the parish and national levels. The survey collects information on individuals' employment status and earnings. Second, we used the Jamaica Survey of Living Conditions (SLC). The SLC is a nationally representative survey executed every year over a sub-sample of households interviewed in the second quarter LFS (labeled as the April LFS). The SLC contains information on individuals' self-reported health status, health insurance coverage, and sociodemographic characteristics.[4] See Appendix F for a detailed description of the LFS and the SLC designs.

For each year, we matched the April LFS and the SLC at the individual level to obtain a single database with individuals' information on both health and labor market indicators. We considered the repeated cross-sectional samples for years 2002,

[4]The April LFS execution period is between April and June. The SLC execution period regularly goes from June to November visiting a nationally representative subsample of the April LFS.

2004, 2006, 2007, 2008, 2009, 2010, and 2012 stacked as a district-level panel.[5] We did not use data for years 2003 and 2005 given that the health module was not included in the SLC. Year 2011 was a census year and the SLC was not executed. To avoid potential interactions with dependent health coverage and with health insurance coverage provided to pensioners (65+ years old) since year 2003, we restrict our sample to adults between 21 and 64 years old. Our overall sample comprises 35,434 individual-year observations.

A key piece of information that we will exploit refers to health insurance coverage. Individuals may have either private or government health insurance. Private insurance can be obtained individually or cooperatively through an organization. Government insurance is provided to public employees through collaborative arrangements with private insurance companies. Monthly health insurance premiums for public employees are 80 percent covered by the government and 20 percent covered by the employee. Both private and government sponsored health insurances offer equivalent benefits including hospitalization, outpatient care, surgical procedures, doctors' hospital visits, doctors' home visits, dental services, prescriptions, diagnostic services, and consultation fees. Both types of insurance cover insured people for services obtained in either private or public health facilities.

The share of people between 21 and 64 years old covered by any health insurance has been stable at around 17 percent over time (Figure 3.2). The great majority of them (15 percent) held private insurance; while only 2 percent held government-sponsored health insurance. Table 3.1 formally evidences that there were no significant differences in the share of insured persons before and after policy adoption. Therefore, it appears that, on average, insured persons did not drop their coverage as a response to the freely provided medical services available to all in public health facilities after policy adoption. This comes at relatively no surprise since the quality of the free public healthcare was not optimal as evidenced in the previous section. As such, uninsured persons (around 83 percent) were the group mainly benefited by the policy

[5] As of the writing of this paper, the SLC data for years 2013 onward was pending public release.

as they migrated from having no coverage at all to full accessibility to medical services (although not of optimal quality) at public health facilities without out-of-pocket expenditures. The latter will be central for our identification strategy.

Table 3.2 presents baseline sociodemographic characteristics pooling for years 2002 to 2007 differentiated by insurance coverage status. The average age is around 39 years-old being similar between uninsured and insured persons. Around 54 percent of individuals locate in the 21-39 age range, while 46 percent locate in the 40-64 range. The share of females stands at 51 percent and 58 percent for uninsured and insured respectively. Not surprisingly, uninsured persons are significantly less educated than insured counterparts averaging 9.55 vs. 11.62 years of education. Uninsured persons are significantly more likely to have incomplete secondary or lower, while insured counterparts are more likely to have tertiary education. Both uninsured and insured are equally likely to live with at least one minor (¡18 years old) at home (share of 41 percent). Uninsured are significantly more likely to be beneficiaries of the Jamaican conditional cash transfer program named Program of Advancement through Health and Education (PATH). Indeed, 26 percent of uninsured are PATH beneficiaries; while only 8 percent of insured are covered. Uninsured households have more minor members (1.56 vs. 1.01) and show larger household sizes (3.93 vs. 3.16).

Table 3.3 presents baseline levels for the outcomes of interest. Panel A shows self-reported health indicators asked with reference to the prior four weeks of the survey date. We observe that both uninsured and insured persons were equally likely to suffer illnesses with 11 percent reporting such occurrence. However, when taking into account the likelihood of suffering an illness associated with losing at least one day of normal activities, uninsured persons were significantly more likely to observe such episodes than insured counterparts (7 percent vs. 5 percent). Furthermore, when looking at the number of days in which persons reported that they were unable to carry out normal activities due to illnesses (i.e., activities of daily living lost-ADLs), we observe that uninsured persons almost double the level reported by insured counterparts (0.50 vs. 0.27 ADLs). Therefore, although equally likely to suffer illnesses, uninsured persons lost more productive days due to these occurrences than what insured counterparts did.

Panel B shows that uninsured were less likely to be employed when compared to insured counterparts (69 percent vs. 88 percent). Also uninsured were significantly less likely to work formally as only 50 percent of them contributed to the National Insurance Scheme (the Jamaican Social Security Agency or NIS), while 83 percent of insured did so. However, both groups were equally likely to have a secondary job and equivalent in terms of their overall average weekly working hours.[6] Nonetheless, the composition of labor supply was different as a higher fraction of uninsured reported to work less than 35 hours a week (12 percent vs. 3 percent), while insured counterparts showed a higher fraction of full time workers (97 percent vs. 88 percent). Reported hourly wage rates expressed in real 2014 US$PPP were significantly lower for uninsured, something not surprising as this group is significantly less educated on average. Given the relatively high nonresponse rates for wages, we calculated imputed wages for employed persons by regressing reported wages by survey year on a fifth-degree polynomial of age, an indicator function for gender, indicators for educational attainment; and all two-way interactions between age, gender, and the education variables. We then imputed wages using the model's predicted wages for all employed persons. In line with reported wages, imputed figures were also significantly lower for uninsured persons.

In summary, uninsured and insured populations were significantly different in their baseline levels of education, household composition, capacity to recover from illnesses, labor force participation, labor earnings, and labor formality. The policy, however, mainly affected the uninsured population as insured counterparts already had health coverage in the absence of the policy. Next, we explain how we exploit this fact toward isolating the causal effects of the policy on health status and labor market outcomes.

[6]Notice that whether individuals contributed to NIS, weekly working hours, and hourly wages were not collected in survey year 2002. Therefore, for these outcomes, we used data from 2004 to 2012.

3.3.2 Methodology

To estimate the causal effects of the policy, we stack the repeated cross-sectional yearly surveys as a district level panel to summarize the overall effect as the difference between mean outcomes before and after policy enactment. To do this, we exploit two sources of variation. The first is the timing of the policy, which became effective in April 2008. Therefore, survey rounds for years 2002-07 will serve as the pre-policy period; while survey rounds for 2008-12 will serve as the post-policy period. The second source is the cross-sectional individual level variation regarding health insurance coverage. Uninsured individuals constitute the group primarily affected by the policy. This is because health fees that otherwise would have been paid out-of-pocket by these individuals were eliminated. By contrast, insured individuals were not directly affected as they already enjoyed health coverage. In other words, insured individuals had medical coverage before and after the policy, but uninsured only accessed coverage after the policy. As such, we define uninsured individuals as the treatment group while insured individuals will serve as the control group.[7]

Formally, we estimate regression models of the following form:

$$Y_{idt} = \delta_d + \eta_t + \lambda \cdot T_{idt} + \beta \cdot T_{idt} \times Post_t + X'_{idt} \cdot \gamma + \epsilon_{idt} \tag{3.1}$$

where Y_{idt} is the outcome of interest for individual i, living in district d, observed at year t. δ_d is a district fixed effect. η_t is a year fixed effect. T_{idt} is an indicator that takes value of 1 if individual i is uninsured, and 0 otherwise. $Post_t$ is an indicator that takes the value of 1 from year 2008 onward, while 0 otherwise. X_{idt} is a vector of control variables including age, education, gender, household size, number of children in household, and an indicator variable for whether the household is PATH beneficiary. Finally, ϵ_{idt} is an error term that in all estimations we cluster at the district level to account for heteroskedasticity and serial correlation in disturbances among individuals residing in the same district.

[7] Alternatively, we also restrict the sample to uninsured and privately insured persons using the latter as the control group. Results from this sample were equivalent and are available upon request.

Some aspects of model 3.1 merit discussion. First, the district fixed effects control non-parametrically for any time-invariant unobservable characteristics at the district level. Second, the year fixed effects control non-parametrically for aggregate yearly shocks, for example from a particular year with more than usual demand in health services. Furthermore, the set of year fixed effects also control for secular trends in the outcomes of interest like aggregate rising health or labor supply that would have existed even in the absence of the policy. In this model, estimates of β provide a measure of the policy's average effect over the outcomes of interest. Specifically, it provides an estimate of the policy's impact in the years after its enactment, relative to the mean in the years prior to its adoption.

In the absence of representative individual level panel data, our main strategy relies on groups differentiated by their contemporaneous accessibility to health insurance reported in each yearly cross-section. Therefore, is important to test for the stability in terms of size and composition of these groups across time. As acknowledged before, it could be that the policy might have motivated individuals with prior access to health insurance to abandon it as the policy provided free public healthcare. Alternatively, uninsured individuals might have migrated to insured status to access private healthcare. If these potential behaviors were correlated with unobservables systematically associated with the outcomes of interest, then estimated impacts would be biased due to the endogenous migration between insurance coverage statuses. However, evidence provided in Table 1 suggests that this behavior is unlikely to be pervasive as the sizes of insured and uninsured populations have been stable over time, before and after policy adoption.

Nonetheless, even though groups are stable in terms of size, it could be possible that individuals might have changed insurance coverage status at the same rate in both directions. If these were the case, we should observe a break in the sociodemographic composition of uninsured relative to insured individuals, before and after policy adoption. Therefore, we test for this possibility by running model 3.1 using several individual sociodemographic characteristics as outcomes. In addition, to avoid potential problems due to multiple hypotheses testing, we compute a sociodemographic summary index that combines all individual characteristics into a single measure.[8] Table 3.4 shows that virtually all estimates of β (with the exception of a marginally significant estimated coefficient on age) are statistically indistinguishable from zero. Importantly, the sociodemographic index is both statistically and economically insignificant. This evidences that our treatment and control groups are not only stable over time in terms of size but also in terms of sociodemographic composition. The latter suggests that the possibility of individuals endogenously migrating between insurance coverage statuses after policy adoption is unlikely.

In this context, the key identification assumption is that, in the absence of the policy, both treatment and control groups would have shared parallel trends on the outcomes of interest. Although not entirely testable, this assumption can be partially tested by looking at the trends in the outcomes of interest during the pre-policy period. Indeed, a necessary condition for the identification assumption to hold is that the outcomes of interest between treated and control groups share parallel trends during the pre-treatment period. Accordingly, we formally test this through the estimation of the following event study model:

$$Y_{idt} = \delta_d + \eta_t + \lambda \cdot T_{idt} + \sum_{j=2002}^{2006} \beta_j \cdot \eta_j \cdot T_{idt} + \sum_{j=2008}^{2012} \beta_j \cdot \eta_j \cdot T_{idt} + X'_{idt} \cdot \gamma + \epsilon_{idt} \quad (3.2)$$

where all variables are defined as in model 3.1. However, the model introduces interactions between the treatment indicator and the year fixed effects being year 2007 the omitted interaction. Therefore, estimates of β_j corresponding to years 2002-

[8] We follow Katz, Kling, and Liebman (2007) by constructing a sociodemographic summary index Z, defined to be the equally weighted average of z scores of all characteristics listed in Table 4. The z scores were calculated by subtracting the control group (i.e., insured individuals) mean of each outcome and dividing by the control group standard deviation separately for each survey year. In that way, each component of the index has mean zero and standard deviation one for the control group. Because the absolute magnitude of the sociodemographic index is in units akin to standardised scores, the difference-in-differences estimates of Table 3.4 show where the mean of the treatment group (i.e., uninsured individuals) is in the distribution of the control group in terms of standard deviation units, before and after policy adoption.

06 provide formal measures of pre-policy differential trends between uninsured and insured. If the identification assumption holds, these estimates should be statistically indistinguishable from zero. Furthermore, estimates of β_j corresponding to years 2008-12 provide disaggregated estimated effects of the policy for each post-policy period relative to year 2007 (the omitted category).

Finally, we exploit the quarterly rotating panel scheme of the LFS to identify a small individual level panel subsample. In 2007, the LFS initiated a fresh rotating panel of households for the period 2007-09. Therefore, since the SLC covers a random sample of the April LFS, we identified individuals interviewed in both the April LFS and the SLC yearly from 2007 to 2009.[9] This resulted in a subsample of 684 individuals or 2,052 individual-year observations. We exploit this panel to test for the robustness of our results by running models 3.1 and 3.2 with individual fixed effects and classifying treatment status with the pre-treatment (i.e., 2007) insurance coverage status. If our main results are valid, we should expect similar point estimated effects while lower statistical power from this subsample.

3.4 Results

We begin by graphically inspecting health and employment trends differentiated between uninsured and insured individuals. Figure 3.3 visually shows that up to the year leading the policy adoption (i.e., year 2007) all main outcomes of interest shared raw common trends between uninsured and insured individuals. Outcomes related to the likelihood of being employed, contributing to the NIS, and having a secondary job remained sharing common trends between uninsured and insured after policy adoption. However, the number of ADLs lost due to illness shows a clear break in trends after policy adoption suggesting a continued declining pattern for uninsured with respect to insured individuals. For the number of weekly working hours, we also observe a relative increasing trend for uninsured when compared to insured following policy

[9] See Appendix 1 for a detailed description of the LFS rotating panel scheme.

adoption. Finally, while both uninsured and insured have declining trends in real wages, uninsured individuals present a relatively larger drop after policy adoption.

Therefore, this inspection shows that pre-policy raw trends look remarkably parallel for our main outcomes giving initial visual support to our identification strategy. Moreover, consistent with an absence of "employment lock," it does not seem to be strong effects on the extensive margin of employment. Similarly, it seems to be no effects on having a secondary job or in the likelihood of contributing to the NIS and thus being a formal employee. However, trends suggest the existence of a negative effect in terms of ADLs providing initial evidence that the policy helped uninsured individuals to recover quicker from illnesses. Likewise, there is a visual positive effect in terms of weekly working hours. Finally, consistent with a positive labor supply effect, there appears to be a negative impact in the hourly wage rate following policy adoption. Next, we formalize our visual analyses with the regression models discussed earlier.

3.4.1 Effects on Health Status

Panel A of Table 3.5 shows estimates of β from model 3.1 on health outcomes. In column 1, we estimated a restricted version where we replaced the year fixed effects with the $Post_t$ indicator and did not include sociodemographic controls. Column 2 shows results from the model including year fixed effects but without controlling for sociodemographic characteristics, while column 3 adds the sociodemographic controls. An initial observation is that our results remain stable across alternative specifications providing further support for the conditional exogeneity of the interaction between the uninsured indicator and the policy timing. This provides further support for the validity of our empirical strategy. Therefore, we will focus our discussion on the results from the preferred fully saturated model reported in column 3.

We first look at the likelihood of suffering any illness within the four weeks leading to the survey date. Although the point estimate is negative, it is statistically indistinguishable from zero. However, when looking at the likelihood of suffering any illness associated with losing at least one day of normal activities, we find that the policy caused a reduction of two percentage points. If we consider the baseline level for this indicator among uninsured equivalent to 7 percent, our results imply an economically significant reduction equivalent to 28.6 percent. This reduction is equivalent to the gap observed at baseline between uninsured and insured where the former were two percentage points more likely to experience illnesses associated with lost days. Therefore, the policy has been effective in fully closing the baseline gap with respect to this health indicator.

We now focus on the number of days in which persons reported that they were unable to carry out normal activities due to illnesses within the four weeks leading to the survey date (ADLs). Our results suggest a significant reduction equivalent to 0.17 days. The baseline level for uninsured was 0.50 days. Therefore, our estimates imply a reduction equivalent to 34 percent that is both statistically and economically significant. The baseline gap between uninsured and insured was 0.23 days favoring the latter. Thus, our estimated effect is equivalent to 74 percent of the baseline gap. As such, our results provide unambiguous evidence that the policy has significantly helped uninsured individuals to have quicker recovery periods from illnesses and losing a lower number of productive days. The magnitudes of our estimates are large suggesting that the policy has almost closed the baseline unfavorable gap on these health indicators between uninsured and insured individuals. In the next section, we explore whether these improvements occurred contemporaneously with changes in labor market dynamics.

3.4.2 Effects on Labor Market Dynamics

Panel B of Table 3.5 shows estimated effects on labor market indicators. We first assess the likelihood of being employed during the reference week of the LFS. Consistent with the absence of "employment lock", we found no effects on this indicator. Regarding employment formality, we look at the likelihood of contributing to the NIS. Consistent with the non-targeting design of the policy and an absence of incentives to switch from the formal to the informal sector, we found no effects. We also assess the extensive margin of having a secondary job and again we did not find significant effects. Therefore, the results on these indicators clearly suggest that the policy did not alter labor market dynamics at the extensive margin. Employment levels remained unchanged in terms of main and secondary occupations. Likewise, quality of employment in terms of formality (captured by the likelihood of contributing to NIS) remained unchanged.

However, when focusing on the intensive margin, we observe a positive and significant effect of 2.15 weekly hours of labor (equivalent to 4.96 percent with respect to the baseline level). Moreover, this effect is operating at the margin between working part and full time. Indeed, the likelihood of working less than 35 hours per week had a negative effect of three percentage points, while the likelihood of working full time (35 hours or more) increased by three percentage points. For the previous estimates to constitute mainly a labor supply effect there must not have been differential increases in labor demand for uninsured relative to insured individuals. To test this indirectly, we assess the effects of the policy on both reported and imputed hourly wages, which should decrease if there were a dominant labor supply effect.[10] Our estimates show that both reported and imputed wages had negative effects equivalent to 0.15 and 0.06 log-points respectively supporting the interpretation of our results mainly as to labor supply effects.

Our results, therefore, have shown that the policy not only benefited uninsured individuals by helping them to have quicker recoveries from illnesses, but also in that it caused them to supply more labor thereby creating more production for the economy. As such, we compare the costs associated with the policy with the extra marginal production to calculate the net benefit for the Jamaican economy. In terms of costs, during the five years leading to policy adoption (2002-06), the government

[10] All wages were deflated to 2014 Jamaican dollars using the official inflation rates; then the prevailing PPP conversion factor was applied to express wages in US$. To estimate the regression models, we took the natural logarithm of the real US$ PPP wage as dependent variable.

spent a yearly average of real US$PPP 546.06 million in the public health system.[11] By contrast, during the first five years of policy adoption, expenditures rose to a yearly average of real US$PPP 623.26 million. This implies that, on average, the policy has cost real US$PPP 77.2 million yearly between 2008 and 2012.

In terms of additional production, our estimates imply that employed uninsured individuals increased their labor supply, on average, by 2.15 weekly hours. Considering 48 working weeks during the year, this implies 103.2 extra yearly hours of labor supply for each benefited person. The 2011 population census counted 669,395 employed persons between 21 and 64 years old. The SLC reveals that between 2008 and 2012 an average of 77.04 percent of employed individuals between 21 and 64 years old were uninsured. Therefore, the size of the benefited population (i.e., employed uninsured individuals between 21 and 64 years old) comprises approximately 77.04 percent x 669,395 = 515,702 individuals. This implies that the policy increased aggregate labor supply by 103.2 x 515,702 = 53,220,446.4 hours each year between 2008 and 2012. Valuing each extra labor hour at the average real US$PPP minimum wage rate for this period (US$PPP 1.95 per hour), the estimated extra yearly production for the economy is US$PPP 103.8 million between 2008 and 2012. Considering the extra yearly cost in public healthcare of US$PPP 77.2 million, our estimates imply that the policy generated US$PPP 26.6 million of net yearly production for the Jamaican economy between years 2008 and 2012.

3.4.3 Robustness of the Effects

We now turn to assess the robustness of our results so that we can be certain of a causal interpretation. We first estimate regression model 3.2 to confirm that prior to policy enactment, both uninsured and insured individuals shared parallel trends with respect to the outcomes of interest. Table 3.6 shows that all of the estimates of β_j corresponding to years 2002-06 are statistically indistinguishable from zero. This evidence formally shows that the key identification assumption of parallel trends

[11] All monetary figures in this section are expressed in real 2014 US$PPP.

between treatment and control groups in the absence of the intervention holds for the pre-policy period. Of course, such assumption is not testable for the post-policy period. However, the evidence shows that one of the main necessary conditions to interpret our results as causal holds.

Having shown that one of the key identification assumptions hold for the pre-policy period, we now turn our focus to the estimates of β_j corresponding to the post-policy period. These estimates disentangle previously estimated average effects for the entire post-policy period into estimates of yearly effects. Therefore, these are useful to assess the timing of the effects for the different outcomes of interest. In terms of health outcomes, we observe that the likelihood of suffering an illness with at least one normal day lost observed a significant negative effect two years after policy adoption (column 2). When looking at the number of normal days lost due to illnesses (ADLs), we observe that the negative effects are present over the entire post-policy period. However, these are stronger two and three years after policy adoption (column 3).

Estimated yearly effects on labor market outcomes at the extensive margin in terms of employment, contributing to NIS, and having a secondary job are insignificant and bounce around zero for all periods (columns 4-6 and Figure 3.4). However, the number of weekly working hours observes positive and significant effects over the entire post-policy period (column 7 and Figure 4). Consistent with relatively stronger negative effects on ADLs, we also observe relatively stronger positive effects on weekly working hours two and three years after policy adoption (years 2009 and 2010). Finally, and confirming the interpretation of our results mainly as a labor supply effect, we observe consistent negative estimated coefficients on wages and imputed wages (columns 8-9 and Figure 4).

As a final robustness exercise, we estimate regression models 3.1 and 3.2 using a small subsample containing 684 individuals interviewed in 2007, 2008, and 2009 (i.e., 2,052 individual-year observations). This subsample has the advantage of observing the same individuals across time, which allows controlling for individual level time-invariant unobservable characteristics through the inclusion of individual fixed effects in the models. In addition, the data allow us to define treatment status according to individuals' pre-policy health coverage status while observing whether migration between coverage statuses occurred across time. However, given the small size of this subsample, we expect relatively lower statistical power with respect to the full sample.[12] Nonetheless, similarity between estimated effects using the full sample and the individual level panel subsample would provide further confidence on the reliability of our results.

Panel A of Table 3.7 shows estimated coefficients using the full subsample and classifying treatment status with the 2007 health insurance coverage information. An initial observation is that the vast majority (85.4 percent) maintained their 2007 health insurance coverage status entirely constant over time.[13] This provides further suggestive evidence that utilization of contemporaneous health insurance coverage information to define treatment status does not appear to introduce systemic biases. Indeed, estimated difference-in-differences effects show strong coincidence in terms of sign and magnitude when compared to the estimated impacts reported in Table 5. Statistical power, however, is lower as estimated standard errors are between two to five times larger. As a result, statistical significance is lost for the health outcomes. Furthermore, event study point estimates are also in line with the patterns found in Table 6. We observe health effects becoming stronger in 2009, while weekly working hours show significant effects for all post-policy periods with a relatively larger point estimate for 2009. Finally, consistent with a labor supply effect, estimates for both wages and imputed wages are consistently negative.

[12] Despite its small size, the individual level panel subsample shows great similarity in terms of insurance coverage rates when compared to the full sample. Indeed, in the full sample, uninsured and insured individual-year observations accounted for 82.83 percent and 17.17 percent respectively. While in the individual level panel subsample, the figures were 83.28 percent and 16.72 percent respectively.

[13] From the 684 individuals conforming the panel, 574 (83.92 percent) were uninsured in 2007 and constitute the treated group, while 110 (16.08 percent) were insured in 2007 and serve as the control group. 514 out of the 574 treated individuals maintained their uninsured status over time, while 70 out of the 110 control individuals maintained their insured status over time.

Finally, we estimated the models using a restricted individual panel including only persons who maintained their health insurance coverage status over time. This accounts for 85.4 percent of the individual panel subsample or 584 individuals (i.e., 1,752 individual-year observations). Panel B of Table 3.7 shows the estimated effects. Again, difference-in-differences point estimates for health outcomes, working hours, and wages coincide in sign and magnitude with full sample estimates shown in Table 5. Event study estimates consistently show relatively larger health and working hours' effects in 2009, and overall negative wage effects. Therefore, effects found with both versions of the individual level panel subsamples show equivalent effects as using our full sample. This provides increased confidence to interpret our main results as causal.

3.4.4 Heterogeneous Effects

A relevant distinction when assessing health effects is age. Relatively older individuals have a larger probability of falling ill. Therefore, assessing whether the policy impacted alternative age ranges differently can inform if the policy was most effective where initial health levels were lower or vice versa. Panel A of Table 3.8 shows results differentiated by age in two groups: individuals between 21-39 years old and individuals between 40-64 years old. Estimated health effects appear to be stronger for the 40-64 year-old group, with the difference between estimates on the likelihood of being ill with associated lost days being significant at the 10 percent level. Interestingly, is also true that the baseline health status was relatively worse for the 40-64 year-old group. Thus, results imply that the policy was relatively more effective in improving the health status of the relatively more disadvantaged group at baseline.

Consistent with previous labor market findings, we find no effects at the extensive margin on main employment, formality or secondary employment. However, and consistent with relatively larger gains in health for the 40-64 year-old group, we find a significant positive effect equivalent to 3.22 weekly hours of labor supply for this group. For the 21-39 year-old group we find no effect on labor supply. Appendix Table G1 further strengthens the robustness of these results by estimating regression model 3.2 for each age group separately showing that the identification assumptions hold for these subgroups. Therefore, our results point out toward stronger effects on both health and labor supply within the group of relatively more mature individuals who, at baseline, exhibited relatively more disadvantaged health status.

Another relevant dimension is gender. Panel B of Table 3.8 shows these results. The p-values related to tests for equality across gender-specific estimated effects suggest that both males and females equally benefited from the policy. Although point estimates differ, results point toward benefits in health status translated into positive effects in weekly hours of labor supply ranging between 1.63 and 2.55 hours. Appendix Table G2 further strengthens the robustness of these results by estimating regression model 3.2 for males and females separately showing that the identification assumptions hold for both gender groups.

Finally, a segment of the population that has attracted significant attention when evaluating the effects of public health insurance expansions refers to childless adults. This is because around 82 percent of those newly eligible for public health coverage under the Patient Protection and Affordable Care Act (ACA) in the US are adults living without children. In addition, childless adults have no dependent responsibilities at home and, as a result, might have lower employment attachment. Therefore, under the advent of freely available healthcare they might have higher incentives to leave their current jobs and engage in job searching for a better or more gratifying occupation.

Therefore, panel C of Table 3.8 shows estimated impacts by presence of minors at home. Interestingly, we find stronger health effects for childless adults when compared to adults living with at least one minor (<18 years old) at home. The effect on the likelihood of suffering an illness with lost days is negative and significant equivalent to three percentage points for childless adults vs. a zero effect for adults with minors. However, the baseline level for this outcome was also higher among childless adults (8 percent vs. 6 percent). Similarly, while the effects on ADLs are not significantly different between both groups, childless adults exhibit a larger point estimate equivalent to 0.26 lower ADLs lost because of the policy. Again, the baseline level of this indicator was larger for childless adults (0.57 vs. 0.4). Thus, relatively stronger health effects for childless adults have equated health indicators that favored adults living with minors before policy adoption. These results mirror heterogeneous effects by age and, indeed, childless adults exhibit a composition with a majority of individuals in the 40-64 age range (54.61 percent). While adults living with children include a majority in the 21-39 group (59.17 percent).

In terms of labor supply, consistent with previous results, we find no effects at the extensive margin in terms of primary employment and formality. This suggests that conjectures regarding probable differentiated incentives after the provision of free healthcare toward leaving current employment among childless adults are not present within the Jamaican context. We do find, however, a positive effect among childless adults regarding the likelihood of engaging in a secondary job. Therefore, if any, new job searches happen, but in addition to and not substituting, the primary employment. At the intensive margin, and consistent with stronger health improvements, we find that childless adults significantly increased their weekly labour supply by 3.19 hours. Appendix Table G3 further strengthens the robustness of these results by estimating regression model 3.2 for these groups showing that the identification assumptions hold.

3.5 Conclusion

In this study, we examine whether the introduction of universal free public healthcare in Jamaica in April 2008 affected health outcomes and labor supply of individuals between 21 and 64 years of age. To do this, we use a difference-in-differences strategy that compares health status and labor market outcomes between uninsured and insured individuals, before and after policy implementation. In terms of health status, we find that the likelihood of suffering illnesses associated with loss of normal days decreased by two percentage points (or 28.6 percent with respect to the baseline mean). At the intensive margin, we find that the number of days where people were unable to perform normal activities due to illnesses suffered within the previous four weeks decreased by 0.17 days (equivalent to 34 percent with respect to the baseline mean). Therefore, our estimates suggest that, on average, the policy increased the general health of the benefited population.

In terms of labor market outcomes, consistent with the absence of "employment lock," we find no effects on the likelihood of being at work. We also find no effects on the likelihood of contributing to the social security system (a measure of labor formality) or on the likelihood of having a secondary job. However, consistent with a reduced number of days lost due to illnesses, we find a positive effect of 2.15 additional weekly labor hours. We interpret the latter mainly as a labor supply effect given that both reported and imputed hourly wages had negative impacts equivalent to 0.15 and 0.06 log-points respectively. To give an estimate of the policy's benefit to the economy, we valued the extra hours of labor supply at the real average minimum wage rate for the period 2008-12 and subtracted the real extra expenditures in public health resulting from the policy. This exercise suggests that the policy added a yearly average of US$PPP 26.6 million worth of net real production to the Jamaican economy during the period 2008-12.

A variety of corroborating evidence supports the causal interpretation of our findings. The share of individuals with and without health insurance is stable over time before and after policy adoption. In addition, the trends in sociodemographic composition between uninsured and insured individuals are stable before and after policy adoption implying no shifting behavior in the composition of both groups resulting from the policy. Event study estimates show that no anticipatory effects were at work with respect to the outcomes of interest, supporting the validity of our empirical strategy. Individual level panel data, while relatively small, shows low incidence of shifting behavior between health insurance statuses over time. Moreover, estimated effects are similar in terms of sign and magnitude when compared to the full sample analyses.

Finally, heterogeneity of effects with respect to age and presence of minors at home confirm that hours of labor supply increase when health gains are relatively stronger. This suggests the presence of a causal chain in which benefited individuals recover quicker from illnesses and, as a result, are able to increase their working hours. Specifically, we show that health benefits and labor supply effects are concentrated among individuals between 40 and 64 years old. This segment was relatively disadvantaged in terms of health status at baseline and the policy narrowed this initial health gap, thereby increasing their labor supply by 3.22 weekly hours.

3.6 Tables and Figures

Figure 3.1: National health utilization trends

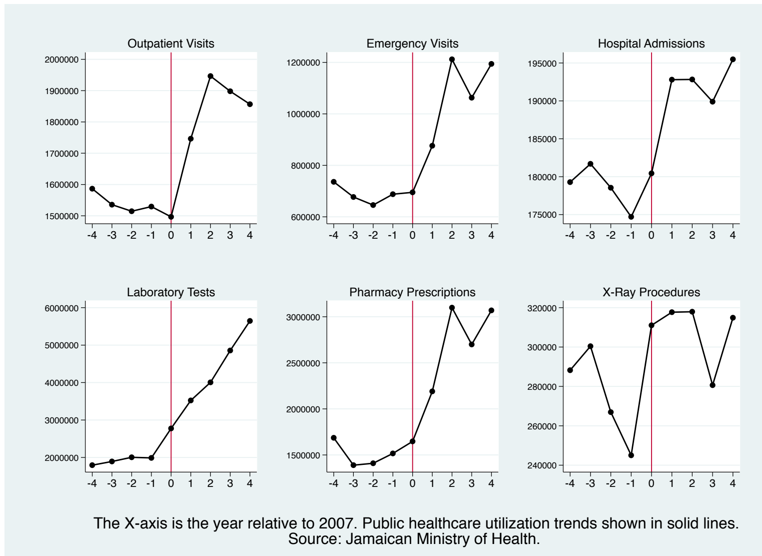


Figure 3.2: Health insurance coverage trends.

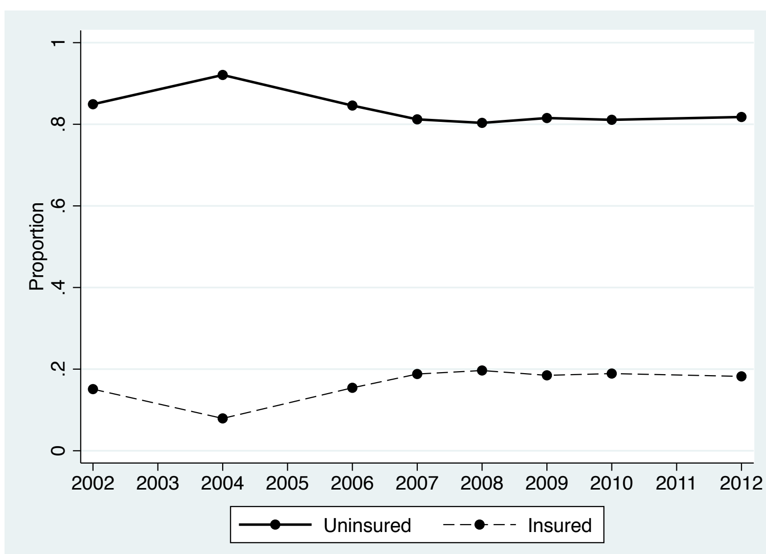


Figure 3.3: Health insurance coverage trends.

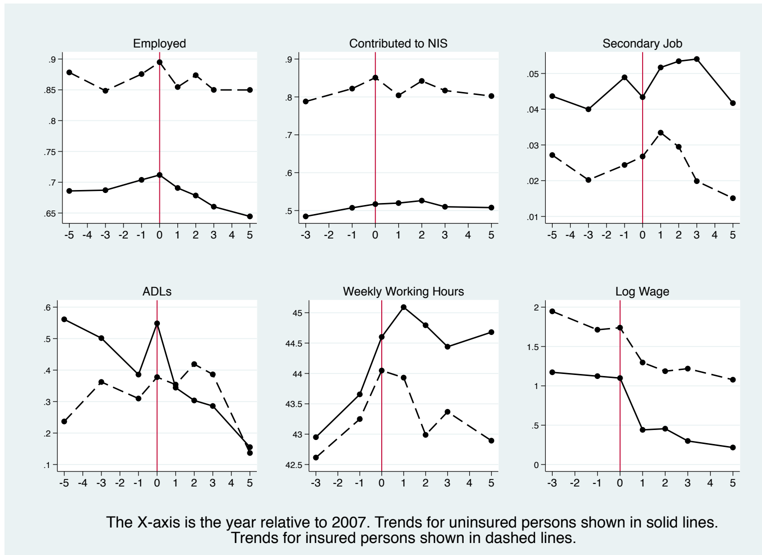


Figure 3.4: Event study estimates on labor market outcomes.

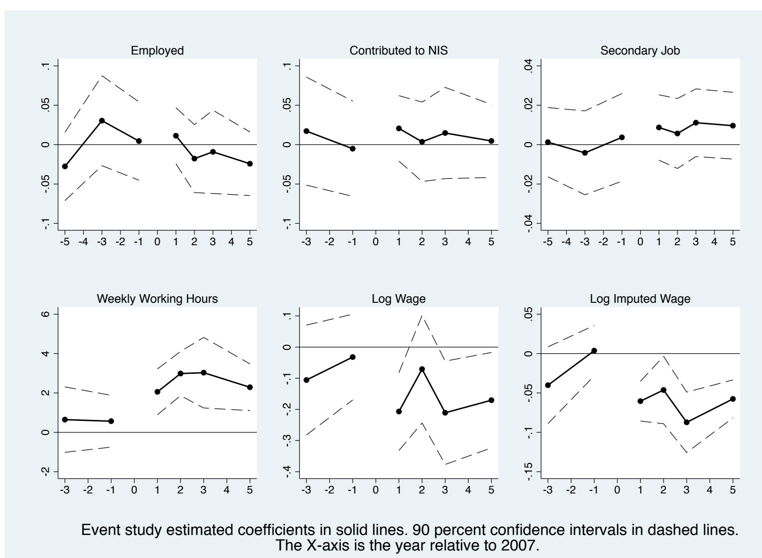


Table 3.1: Share Covered by Health Insurance Before and After Policy Adoption

Pre-policy comparison period:	**2007**	**2006-2007**	**2004-2007**	**2002-2007**
	(1)	**(2)**	**(3)**	**(4)**
Pre-policy mean	0.20	0.18	0.15	0.17
	A. Overall Post-Policy Difference			
Difference 2008-2012	0.01	0.01	0.01	0.02**
	(0.01)	(0.01)	(0.01)	(0.01)
	B. Yearly Post-Policy Differences			
Difference 2008	0.01	0.01	0.02	0.03**
	(0.01)	(0.01)	(0.01)	(0.01)
Difference 2009	0.01	0.01	0.01	0.01
	(0.01)	(0.01)	(0.01)	(0.01)
Difference 2010	0.01	0.01	0.01	0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Difference 2012	-0.00	-0.03	-0.01	0.02
	(0.03)	(0.04)	(0.03)	(0.02)
p-value for equality across rows	0.82	0.7	0.73	0.24

Notes: This table presents estimated differences in the proportion of adults 21-64 years old covered by health insurance before and after policy adoption. Estimated differences result from OLS regressions with district fixed-effects. Statistics are weighted by the inverse of the household level sampling probability to reflect survey design. Estimated standard errors, reported in parentheses, are clustered at the district level. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 3.2: Baseline Socio-Demographic Characteristics (2002 - 2007)

	Uninsured (1)	**Insured (2)**	**Difference (1) - (2) (3)**	**Observations (4)**
Age (in years)	39.08	39.61	-0.53	14,524
			(0.38)	
Share between 21 and 39	0.54	0.53	0.01	14,524
			(0.02)	
Share between 40 and 64	0.46	0.47	-0.01	14,524
			(0.02)	
Female	0.51	0.58	-0.07***	14,524
			(0.01)	
Years of education	9.55	11.62	-2.07***	14,524
			(0.09)	
Share with primary or lower	0.18	0.09	0.08***	14,524
			(0.01)	
Share with incomplete secondary	0.40	0.15	0.25***	14,524
			(0.01)	
Share with complete secondary	0.37	0.37	-0.00	14,524
			(0.02)	
Share with tertiary	0.05	0.38	-0.33***	14,524
			(0.02)	
Live with minors (<18)	0.41	0.41	-0.01	14,524
			(0.02)	
PATH beneficiary	0.26	0.08	0.18***	14,524
			(0.01)	
Number of minors in household (<18)	1.56	1.01	0.55***	8,524
			(0.05)	
Household size	3.93	3.16	0.77***	8,524
			(0.07)	

Notes: This table presents baseline mean socio-demographic characteristics (pooling years 2002-2007) for uninsured and insured persons in columns (1) and (2) respectively. Differences between uninsured and insured persons with their corresponding standard errors are shown in column (3). Statistics presented are weighted by the inverse of the household level sampling probability to reflect survey design. For the number of minors in household and household size, the number of observations refer to the number of households-year in the sample. Estimated standard errors, reported in parentheses, are clustered at the district level. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 3.3: Baseline Health and Labor Market Indicators (2002 - 2007)

	Uninsured (1)	**Insured (2)**	**Difference (1) - (2) (3)**	**Observations (4)**
			A. Health Indicators	
Suffered illness	0.11	0.11	0.00	14,458
			(0.01)	
Illness with lost days	0.07	0.05	0.02***	14,330
			(0.01)	
ADLs	0.50	0.27	0.23***	14,330
			(0.05)	
			B. Labor Market Indicators	
Employed	0.69	0.88	-0.19***	14,524
			(0.01)	
Contributed to NIS	0.50	0.83	-0.33***	7,940
			(0.01)	
Secondary job	0.04	0.03	0.01	14,524
			(0.01)	
Weekly working hours	43.33	43.55	-0.22	5,748
			(0.36)	
<35 hours per week	0.12	0.03	0.08***	5,748
			(0.01)	
35+ hours per week	0.88	0.97	-0.08***	5,748
			(0.01)	
Hourly wage (US$ PPP)	3.95	7.71	-3.75***	2,439
			(0.52)	
Imputed hourly wage (US$ PPP)	4.25	6.39	-2.14***	5,746
			(0.17)	

Notes: This table presents baseline mean indicators (pooling years 2002-2007) for uninsured and insured persons in columns (1) and (2) respectively. Differences between uninsured and insured persons with their corresponding standard errors are shown in column (3). Wages were deflated to 2014 Jamaican dollars using the official inflation rates; then the prevailing PPP conversion factor was applied to express figures in US$. To calculate the imputed wage, we regress reported wages by survey year on a fifth-degree polynomial of age, an indicator function for gender, an indicator function for incomplete secondary, complete secondary, at least some tertiary; and all two-way interactions between age, gender, and the education variables. We then impute wages using the predicted wages for all persons who reported being employed during the reference week. Cases in which imputed wages resulted in a negative number were left as missing. Statistics presented are weighted by the inverse of the household level sampling probability to reflect survey design. Estimated standard errors, reported in parentheses, are clustered at the district level. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 3.4: Socio-Demographic Composition Between Uninsured and Insured Persons Before and After Policy Adoption

	Estimated Changes		**Observations**
	(1)	**(2)**	**(3)**
Age (in years)	-0.80*	-0.84*	35,434
	(0.47)	(0.47)	
Female	-0.00	-0.00	35,434
	(0.02)	(0.02)	
Years of education	-0.06	-0.07	35,434
	(0.10)	(0.10)	
Live with minors (<18)	0.03	0.03	35,434
	(0.02)	(0.02)	
PATH beneficiary	-0.01	-0.01	35,434
	(0.01)	(0.01)	
Number of minors in household (<18)	-0.07	-0.06	20,744
	(0.07)	(0.07)	
Household size	-0.09	-0.09	20,744
	(0.10)	(0.10)	
Socio-demographic index (in standard deviations)	-0.03	-0.03	35,434
	(0.02)	(0.02)	
District fixed-effects	Yes	Yes	
Year fixed-effects	No	Yes	

Notes: This table presents estimated difference-in-differences changes in socio-demographic characteristics resulting from policy adoption. Estimated changes result from OLS regressions with district fixed-effects. Regressions are weighted by the inverse of the household level sampling probability to reflect survey design. For the number of minors in household and household size, the number of observations refer to the number of households-year in the sample. Estimated standard errors, reported in parentheses, are clustered at the district level. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 3.5: Health and Labor Market Effects

	Estimated Effects			Observations
	(1)	**(2)**	**(3)**	**(4)**
A. Health Indicators				
Suffered illness	-0.02*	-0.02	-0.02	35,368
	(0.01)	(0.01)	(0.01)	
Illness with lost days	-0.02**	-0.02**	-0.02**	35,209
	(0.01)	(0.01)	(0.01)	
ADLs	-0.19***	-0.18**	-0.17**	35,209
	(0.07)	(0.07)	(0.07)	
B. Labor Market Indicators				
Employed	0.01	0.01	0.00	35,434
	(0.02)	(0.02)	(0.02)	
Contributed to NIS	0.01	0.01	0.01	28,850
	(0.02)	(0.02)	(0.02)	
Secondary job	0.01	0.01	0.01	35,434
	(0.01)	(0.01)	(0.01)	
Weekly working hours	2.10***	2.02***	2.15***	20,423
	(0.43)	(0.44)	(0.44)	
<35 hours per week	-0.02*	-0.03**	-0.03**	20,423
	(0.01)	(0.01)	(0.01)	
35+ hours per week	0.02*	0.03**	0.03**	20,423
	(0.01)	(0.01)	(0.01)	
Log wage	-0.17***	-0.17***	-0.15***	7,505
	(0.05)	(0.05)	(0.05)	
Log imputed wage	-0.06**	-0.06**	-0.06***	20,397
	(0.02)	(0.02)	(0.01)	
District fixed-effects	Yes	Yes	Yes	
Year fixed-effects	No	Yes	Yes	
Socio-demographic controls	No	No	Yes	

Notes: This table presents estimated difference-in-differences effects resulting from policy adoption. Estimated effects result from OLS regressions with district fixed-effects. To calculate the imputed wage, we regress reported wages by survey year on a fifth-degree polynomial of age, an indicator function for gender, an indicator function for incomplete secondary, complete secondary, at least some tertiary; and all two-way interactions between age, gender, and the education variables. We then impute wages using the predicted wages for all persons who reported being employed during the reference week. Cases in which imputed wages resulted in a negative number were left as missing. Regressions are weighted by the inverse of the household level sampling probability to reflect survey design. Estimated standard errors, reported in parentheses, are clustered at the district level. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 3.6: Event Study Estimates

	Illness (1)	Illness with Lost Days (2)	ADLs (3)	Employed (4)	Contributed to NIS (5)	Secondary Job (6)	Working Hours (7)	Log Wage (8)	Log Imputed Wage (9)
Pre-policy trends									
Unisured x 2002	0.01	0.01	-0.07	-0.03		0.00			
	(0.02)	(0.02)	(0.14)	(0.03)		(0.01)			
Unisured x 2004	-0.04	-0.04	-0.44	0.03	0.02	-0.00	0.64	-0.11	-0.04
	(0.04)	(0.04)	(0.43)	(0.03)	(0.04)	(0.01)	(1.01)	(0.11)	(0.03)
Unisured x 2006	-0.03	-0.02	-0.24	0.00	-0.01	0.00	0.57	-0.03	0.00
	(0.03)	(0.02)	(0.17)	(0.03)	(0.04)	(0.01)	(0.80)	(0.08)	(0.02)
Post-policy effects									
Unisured x 2008	-0.02	-0.02	-0.23*	0.01	0.02	0.01	2.06***	-0.21***	-0.06***
	(0.02)	(0.02)	(0.13)	(0.02)	(0.03)	(0.01)	(0.71)	(0.08)	(0.02)
Unisured x 2009	-0.05*	-0.04**	-0.36**	-0.02	0.00	0.01	2.99***	-0.07	-0.05*
	(0.03)	(0.02)	(0.17)	(0.03)	(0.03)	(0.01)	(0.68)	(0.11)	(0.03)
Unisured x 2010	-0.02	-0.02	-0.41**	-0.01	0.01	0.01	3.03***	-0.21**	-0.09***
	(0.02)	(0.02)	(0.20)	(0.03)	(0.04)	(0.01)	(1.09)	(0.10)	(0.02)
Unisured x 2012	-0.01	-0.01	-0.25*	-0.02	0.00	0.01	2.29***	-0.17*	-0.06***
	(0.02)	(0.02)	(0.13)	(0.02)	(0.03)	(0.01)	(0.72)	(0.09)	(0.01)
Observations	35,368	35,209	35,209	35,434	28,850	35,434	20,423	7,505	20,397

Notes: This table presents estimated event study effects disentangling average post-policy effects into individual post-policy yearly effects and differential pre-policy trends. All coefficients are expressed with respect to year 2007 (the omitted interaction term). Estimated coefficients result from OLS regressions with district fixed-effects, socio-demographic controls, and year fixed-effects. Regressions are weighted by the inverse of the household level sampling probability to reflect survey design. Estimated standard errors, reported in parentheses, are clustered at the district level. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 3.7: Estimates with Individual Level Panel Data

	Illness	Illness with Lost Days	ADLs	Employed	Contributed to NIS	Secondary Job	Working Hours	Log Wage	Log Imputed Wage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
A. Full Panel: Treatment and Control Groups Defined by 2007 Insurance Coverage Status									
Difference-in-differences estimates									
Unisured x Post	-0.03	-0.02	-0.31	0.03	0.04	0.00	2.65**	-0.25*	-0.06
	(0.05)	(0.04)	(0.38)	(0.04)	(0.04)	(0.02)	(1.11)	(0.13)	(0.04)
Event study estimates									
Unisured x 2008	-0.01	0.02	-0.15	0.03	0.02	-0.01	2.35**	-0.25	-0.05
	(0.06)	(0.05)	(0.38)	(0.04)	(0.04)	(0.02)	(1.14)	(0.19)	(0.04)
Unisured x 2009	-0.05	-0.06	-0.47	0.03	0.06	0.01	2.95**	-0.25	-0.07
	(0.06)	(0.05)	(0.46)	(0.05)	(0.05)	(0.02)	(1.46)	(0.20)	(0.06)
Observations	2,052	2,047	2,047	2,052	2,052	2,052	1,508	624	1,508
B. Restricted Panel: Individuals with Constant Health Insurance Coverage Status over Time									
Difference-in-differences estimates									
Unisured x Post	-0.06	-0.04	-0.58	-0.02	-0.03	-0.02	3.19***	-0.22	-0.09*
	(0.05)	(0.04)	(0.41)	(0.04)	(0.04)	(0.02)	(1.20)	(0.15)	(0.05)
Event study estimates									
Unisured x 2008	-0.00	0.02	-0.26	-0.02	-0.05	-0.03	2.82**	-0.25	-0.06
	(0.06)	(0.06)	(0.37)	(0.04)	(0.03)	(0.03)	(1.23)	(0.21)	(0.04)
Unisured x 2009	-0.11**	-0.10**	-0.90	-0.01	-0.01	-0.00	3.55**	-0.20	-0.11*
	(0.05)	(0.04)	(0.58)	(0.04)	(0.05)	(0.02)	(1.48)	(0.21)	(0.07)
Observations	1,752	1,749	1,749	1,752	1,752	1,752	1,287	518	1,287

Notes: This table presents estimated difference-in-differences and event study effects using the individual level panel 2007-2009. Estimated coefficients are expressed with respect to year 2007 (the omitted interaction term) and result from OLS regressions with individual fixed-effects, socio-demographic controls, and year fixed-effects. Treatment and control groups are defined according to the 2007 health insurance coverage status. Estimated standard errors, reported in parentheses, are clustered at the district level. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table 3.8: Heterogenous Effects by Age, Gender, and Parental Status

	Illness (1)	Illness with Lost Days (2)	ADLs (3)	Employed (4)	Contributed to NIS (5)	Secondary Job (6)	Working Hours (7)
A. Heterogeneity by Age							
Estimated effects for ages 21-39	-0.00	-0.00	-0.13*	-0.01	0.00	-0.00	0.35
	(0.01)	(0.01)	(0.07)	(0.02)	(0.03)	(0.01)	(0.59)
Baseline mean for ages 21-39	0.07	0.04	0.28	0.67	0.48	0.03	43.72
Observations for ages 21-39	18,265	18,221	18,221	18,307	14,797	18,307	10,060
Estimated effects for ages 40-64	-0.04**	-0.04**	-0.29*	-0.01	0.02	0.01	3.22***
	(0.02)	(0.01)	(0.15)	(0.02)	(0.03)	(0.01)	(0.73)
Baseline mean for ages 40-64	0.17	0.10	0.77	0.70	0.53	0.05	42.85
Observations for ages 40-64	17,103	16,988	16,988	17,127	14,053	17,127	10,363
p-value of test for equality across groups	0.11	0.07	0.33	0.97	0.61	0.12	<0.01
B. Heterogeneity by Gender							
Estimated effects for males	-0.00	-0.02	-0.12	-0.01	0.00	0.01	2.55***
	(0.01)	(0.01)	(0.10)	(0.02)	(0.03)	(0.01)	(0.66)
Baseline mean for males	0.08	0.05	0.41	0.82	0.60	0.06	45.12
Observations for males	16,975	16,921	16,921	16,997	13,805	16,997	11,328
Estimated effects for females	-0.03*	-0.02	-0.22**	0.00	0.00	0.01	1.63***
	(0.02)	(0.01)	(0.11)	(0.02)	(0.03)	(0.01)	(0.63)
Baseline mean for females	0.15	0.09	0.59	0.56	0.40	0.02	40.79
Observations for females	18,393	18,288	18,288	18,437	15,045	18,437	9,095
p-value of test for equality across groups	0.17	0.91	0.5	0.74	0.96	0.94	0.31
C. Heterogeneity by Presence of Minors at Home							
Estimated effects for childless adults	-0.03	-0.03**	-0.26**	0.00	-0.00	0.01*	3.19***
	(0.02)	(0.01)	(0.12)	(0.02)	(0.03)	(0.01)	(0.62)
Baseline mean for childless adults	0.13	0.08	0.57	0.69	0.51	0.03	42.90
Observations for childless adults	19,251	19,145	19,145	19,286	15,389	19,286	10,998
Estimated effects for adults with minors	-0.01	0.00	-0.08	-0.01	0.02	-0.01	0.58
	(0.02)	(0.01)	(0.11)	(0.02)	(0.03)	(0.01)	(0.72)
Baseline mean for adults with minors	0.09	0.06	0.40	0.68	0.50	0.04	43.92
Observations for adults with minors	16,117	16,064	16,064	16,148	13,461	16,148	9,425
p-value of test for equality across groups	0.39	0.07	0.27	0.75	0.63	0.06	<0.01

Notes: This table presents estimated heterogeneous difference-in-differences effects by age, gender and presence of minors at home. Estimated coefficients result from OLS regressions with district fixed-effects, socio-demographic controls, and year fixed-effects. Regressions are weighted by the inverse of the household level sampling probability to reflect survey design. Estimated standard errors, reported in parentheses, are clustered at the district level. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Conclusions

Climate change will affect various welfare related outcomes. This dissertation presented three essays that exposes causal effect from environmental and policy shocks on children's health and adults' labor supply. In particular, contribution to the literature has been presented in terms of construction, mining, and analysis of new data and the implementation of sophisticated econometric and estimation methods that required to tackle programing and computationl constraints. The evidence seems to be important in term of the potential losses that society could confront in the potentialy adverse future climatic conditions, however, adpatation and reliance tools to negative shocks are at hand.

As shown, children and formal economy are negatively affected by the worsening conditions of natural shocks potentially related to climate change. On the one hand, the effects of tropical storms on early childhood development found were heterogeneous at region, gestation period, and early years of life levels when the Weight-for-Age outcome is analyzed. In coastal-rural areas, children are benefited from small storms in early gestation periods, however when small storms are combined with larger events like hurricane category 1, a negative effect rises. There is evidence that the relationship of causality between storms and health outcomes is not linear and in particular shows positive effects when storms are small. When this small events are combined with larger ones like hurricanes cat 1, the effect becomes negative, however, when the event is the largest registered, the effect is again positive.

On the other hand, it seems that the formal productive sector in small island economies is threatened by climatic shocks. When studied the effects of tropical storms on the transition probability to informal employment, I found that, regardless the initial state as formal or informal worker, that effect is positive. Due to the advantage on the storm's information provided by the dataset I created, I can test how the effect behaves as the accumulated power derived from tropical storms increases. The estimation results show that as the accumulated amount of destruction derived from the storms suffered during one to two quarters between observations, the probability of remain informal across periods vary between 1% to 11% depending on the amount of destruction received as consequence of the number of storms suffered in the period, in particular hurricanes. The proper probability for those men that start from a formal job varies between 1% to 12%.

However, exploring potential policy tools proposed by the literature to overcome negative climatic events, promising results are found. In terms of health status of Jamaica's economically active population, we find that the likelihood of suffering illnesses associated with loss of normal days decreased by two percentage points (or 28.6 percent with respect to the baseline mean). At the intensive margin, we find that the number of days where people were unable to perform normal activities due to illnesses suffered within the previous four weeks decreased by 0.17 days (equivalent to 34 percent with respect to the baseline mean). Therefore, our estimates suggest that, on average, the policy increased the general health of the benefited population. This evidence would indicate that this kind of policy interventions would reduce the risk associated to negative health shock that could be associated to environmental events.

The research and policy agenda on climate change is open and widening. The study of climate change has become an important field in development economics and a new challenge for public policy formulation, not only because the strong consequences for future generations due to environmental worsening conditions, but also because poor economies will be the more affected by natural negative shocks due to the large set of vulnerabilities, some of them exposed here. These vulnerabilities will imply challenges not only in the policy arena but in the academic one. Construction of insurance schemes against climatic events, adaptability measures like contingency policies, agricultural insurance, momentary boost in cash transfers schemes to overcome negative shocks and smooth consumption, and unemployment insurance schemes are some potential measures from the policy perspective that can be explored. The next steps in the advance of the present dissertation will be related to the exploration of new data and the expansion of the analysis at the sectoral and geographical level as well as the test of the external validity of the evidence found.

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Appendices

Appendix A

Jamaica Labour Force Survey and Jamaica Survey of Living Conditions Design - ECD

The Jamaica Labour Force Survey (LFS) is designed as a two-stage stratified random sample. The first stage includes a selection of Primary Sampling Units (PSUs), and the second stage a selection of dwellings. A PSU is an Enumeration District (ED) or a combination of EDs that is selected for a sample, usually containing a minimum of approximately 100 dwellings in the rural areas and a minimum of 150 dwellings for the urban communities. An ED is an independent geographic unit sharing common boundaries with contiguous EDs. After the random selection of PSUs, a listing operation of the dwellings located in each PSU is executed to define the master sample for the LFS. This master sample is revised every three to four years usually implying a new selection of PSUs, listing operation and revised selection of dwellings.

The LFS includes a rotating panel scheme as follows. Once the selected PSUs are listed, 32 dwellings are randomly selected from each PSU. These 32 dwellings are then divided into eight groups or panels of four dwellings each. Dwellings in panels 1 to 4 are interviewed in the first quarter LFS (16 dwellings per PSU each quarter). Dwellings in panels 3 to 6 are interviewed in the second quarter LFS. Dwellings in panels 5 to 8 are interviewed in the third quarter LFS. Dwellings in panels 1, 2, 7 and 8 are interviewed in the fourth quarter LFS. In the first quarter of the following year dwellings in panels 1 to 4 are interviewed again and the yearly cycle is repeated (Table A1). This rotating panel scheme with the same dwellings lasts until the master sample is revised usually every three to four years.

Table A1: Rotating Panel Scheme within each PSU

		Panel							
Year	LFS Quarter	1	2	3	4	5	6	7	8
t	January								
	April								
	July								
	October								
t+1	January								
	April								
	July								
	October								

Jamaica is administratively divided into 14 parishes. Each quarterly LFS is representative at the parish and the national level. The Survey of Living Conditions (SLC) usually covers a nationally representative subsample of the April LFS (covering approximately a third of the EDs sampled in the LFS). However, periodically every four or five years, the SLC covers the entire April LFS sample. This exercise is periodically conducted with the objective of producing consumption and poverty aggregates not only at the national level but also at the parish level with acceptable standard errors. Table A2 shows the number of EDs surveyed in the April LFS and SLC corresponding to the yearly periods used in our analyses. Within our study period, years 1996, 2002, 2008, and 2012 included large SLC samples covering the entirety of EDs surveyed in the April LFS.

Table A2: Surveyed EDs in the April LFS and SLC

Year	April LFS EDs	SLC EDs	% SLC Sample	No.Children 0 -5 years old in survey (comp. sample)	No.Children 0 -5 years old in survey (Coast-rural)
1993	n.a.	156	n.a.	838	81
1996	155	155	100	783	105
1997	n.a.	160	n.a.	771	166
1998	n.a.	478	n.a.	2,758	603
1999	n.a.	160	n.a.	651	127
2000	n.a.	161	n.a.	617	109
2002	522	522	100	2,271	465
2004	505	169	33	647	124
2006	507	170	34	527	79
2007	508	168	33	583	74
2008	612	612	100	1,696	275
2010	507	169	033	418	61
2012	508	508	100	1,551	300

Appendix B

Storms' set used in the analysis - ECD

Table B1: Set of storms

Year	**Storm**	**Max wind speed (Km/h)**	**Start date (near to Jamaica)**	**End date (near to Jamaica)**	**Saffir-Simpson Scale**
1987	UNNAMED	30	6-Sep-87	8-Sep-87	TD
1987	EMILY	110	20-Sep-87	26-Sep-87	TS
1987	FLOYD	65	9-Oct-87	13-Oct-87	TS
1987	UNNAMED	30	31-Oct-87	4-Nov-87	TD
1988	SIX:UNNAMED	30	20-Aug-88	24-Aug-88	TD
1988	CHRIS	45	21-Aug-88	30-Aug-88	TD
1988	GILBERT	160	8-Sep-88	19-Sep-88	SS 2
1988	KEITH	65	17-Nov-88	26-Nov-88	TS
1990	ARTHUR	60	22-Jul-90	27-Jul-90	TD
1990	MARCO	55	9-Oct-90	13-Oct-90	TD
1994	GORDON	75	8-Nov-94	21-Nov-94	TS
1995	ROXANNE	100	7-Oct-95	20-Oct-95	TS

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Table B1 – continued from previous page

Year	Storm	Max wind speed (Km/h)	Start date (near to Jamaica)	End date (near to Jamaica)	Saffir-Simpson Scale
1996	DOLLY	70	19-Aug-96	24-Aug-96	TS
1996	LILI	100	14-Oct-96	28-Oct-96	TS
1996	MARCO	65	13-Nov-96	26-Nov-96	TS
1998	GEORGES	135	15-Sep-98	1-Oct-98	SS 1
1998	MITCH	155	22-Oct-98	9-Nov-98	SS 2
1999	IRENE	95	12-Oct-99	19-Oct-99	TS
1999	LENNY	135	13-Nov-99	23-Nov-99	SS 1
2000	DEBBY	75	19-Aug-00	24-Aug-00	TS
2000	HELENE	60	15-Sep-00	25-Sep-00	TD
2001	CHANTAL	60	14-Aug-01	22-Aug-01	TD
2001	IRIS	125	4-Oct-01	9-Oct-01	SS 1
2002	ISIDORE	110	14-Sep-02	27-Sep-02	TS
2002	LILI	125	21-Sep-02	4-Oct-02	SS 1
2002	UNNAMED	30	14-Oct-02	16-Oct-02	TD
2003	CLAUDETTE	80	7-Jul-03	17-Jul-03	TS
2003	ODETTE	55	4-Dec-03	9-Dec-03	TD
2004	BONNIE	55	3-Aug-04	13-Aug-04	TD
2004	CHARLEY	130	9-Aug-04	15-Aug-04	SS 1
2004	IVAN	145	2-Sep-04	24-Sep-04	SS 1
2004	JEANNE	105	13-Sep-04	29-Sep-04	TS
2005	ALPHA	45	22-Oct-05	24-Oct-05	TD
2005	DENNIS	130	4-Jul-05	18-Jul-05	SS 1

Continued on next page

Table B1 – continued from previous page

Year	**Storm**	**Max wind speed (Km/h)**	**Start date (near to Jamaica)**	**End date (near to Jamaica)**	**Saffir-Simpson Scale**
2005	EMILY	140	11-Jul-05	21-Jul-05	SS 1
2005	GAMMA	45	14-Nov-05	21-Nov-05	TD
2005	WILMA	160	15-Oct-05	26-Oct-05	SS 2
2006	CHRIS	55	1-Aug-06	6-Aug-06	TD
2006	ERNESTO	75	24-Aug-06	4-Sep-06	TS
2007	DEAN	150	13-Aug-07	22-Aug-07	SS 1
2007	FELIX	150	31-Aug-07	6-Sep-07	SS 1
2007	NOEL	75	24-Oct-07	5-Nov-07	TS
2007	OLGA	50	10-Dec-07	16-Dec-07	TD
2008	FAY	60	15-Aug-08	28-Aug-08	TD
2008	GUSTAV	135	25-Aug-08	5-Sep-08	SS 1
2008	HANNA	75	28-Aug-08	8-Sep-08	TS
2008	IKE	125	1-Sep-08	15-Sep-08	SS 1
2008	PALOMA	125	5-Nov-08	14-Nov-08	SS 1
2010	ALEX	95	24-Jun-10	1-Jul-10	TS
2010	BONNIE	40	22-Jul-10	25-Jul-10	TD
2010	KARL	110	13-Sep-10	18-Sep-10	TS
2010	MATTHEW	50	23-Sep-10	26-Sep-10	TD
2010	NICOLE	40	28-Sep-10	30-Sep-10	TD
2010	RICHARD	85	19-Oct-10	26-Oct-10	TS
2010	TOMAS	85	29-Oct-10	10-Nov-10	TS
2011	EMILY	45	2-Aug-11	7-Aug-11	TD

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Table B1 – continued from previous page

Year	**Storm**	**Max wind speed (Km/h)**	**Start date (near to Jamaica)**	**End date (near to Jamaica)**	**Saffir-Simpson Scale**
2011	RINA	100	22-Oct-11	29-Oct-11	TS
2012	ERNESTO	75	1-Aug-12	10-Aug-12	TS
2012	HELENE	50	9-Aug-12	18-Aug-12	TD
2012	ISAAC	70	20-Aug-12	1-Sep-12	TS
2012	SANDY	100	21-Oct-12	31-Oct-12	TS
2013	DORIAN	50	31-Jul-13	31-Jul-13	TD
2014	HANNA:INVEST	35	25-Oct-14	26-Oct-14	TD

Appendix C

Robustness checks - ECD

Table C1: Storms' effects on controls - Coast-Rural

		Household head's education				Household's size					Household head and children's gender and age			
Destruction Received	Gestation period	Primary	Sec. incomplete	Secondary	Tertiary	Total hhs	hhs 0 to 5 years old	hhs 6 to 14 years old	hhs 15 to 25 years old	hhs 25 to 49 years old	Female household head	Age (household head)	Age (children)	Girl
Tropical storm (excld.Hurricanes)	1st Trimester	-0.13	0.1	0.02	0.01	-0.29	0.57	0.82	-0.19	-0.15	0.02	-0.23	-0.03	-0.08
		(0.1)	(0.16)	(0.14)	(0.06)	(0.63)	(0.48)	(0.73)	(0.56)	(0.44)	(0.17)	(4.52)	(0.19)	(0.14)
	2nd Trimester	-0.004	0.002	0.002	-0.0001	-0.1*	-0.08	-0.11	-0.01	-0.06	-0.003	-0.25	0.004	0.001
		(0.008)	(0.01)	(0.01)	(0.01)	(0.06)	(0.06)	(0.09)	(0.07)	(0.04)	(0.01)	(0.42)	(0.02)	(0.01)
	3rd Trimester	-0.04	0.1	-0.04	-0.02	0.42	0.4	0.54	0.22	0.05	0.11*	-3.02	-0.04	-0.01
		(0.04)	(0.06)	(0.05)	(0.03)	(0.34)	(0.26)	(0.34)	(0.29)	(0.24)	(0.06)	(2.07)	(0.11)	(0.07)
	Observations	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	2569	2569
At most one hurricane	1st Trimester	-0.32*	0.3	0.01	0.01	-0.66	0.24	1.26	-0.58	-0.48	-0.07	0.11	-2	-0.3
		(0.17)	(0.26)	(0.22)	(0.1)	(1.06)	(0.83)	(1.29)	(0.92)	(0.75)	(0.24)	(0.28)	(8.09)	(0.36)
	2nd Trimester	-0.114	0.059	0.048	0.0072	-1.44	-1.34	-1.79	-0.02	-0.9*	-0.009	-0.05	-3.13	0.079
		(0.11)	(0.16)	(0.15)	(0.06)	(0.91)	(0.96)	(1.28)	(0.9)	(0.5)	(0.15)	(0.16)	(5.69)	(0.23)
	3rd Trimester	-0.08	0.24	-0.15	-0.01	1.15	1.11	0.97	0.89	0.08	0.01	0.33*	-3.97	-0.06
		(0.13)	(0.17)	(0.14)	(0.07)	(0.99)	(0.71)	(0.94)	(0.84)	(0.7)	(0.21)	(0.18)	(5.76)	(0.35)
	Observations	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	2569	2569
Two or more hurricanes	1st Trimester	-0.41**	0.44	0.08	-0.11	0.79	-0.91	1.19	-0.36	-0.47	-0.34	-0.23	0.02	-0.71
		(0.19)	(0.35)	(0.32)	(0.13)	(1.71)	(1.15)	(1.77)	(1.04)	(0.93)	(0.38)	(0.34)	(12.9)	(0.64)
	2nd Trimester	-0.4**	0.374	0.002	0.0195	0.3	-1.35	-2.2	1.33	-0.85	-0.185	0.07	2.409	0.189
		(0.16)	(0.27)	(0.23)	(0.14)	(1.64)	(1.26)	(1.71)	(1.14)	(0.85)	(0.32)	(0.31)	(9.46)	(0.57)
	3rd Trimester	-0.1	0.22	-0.16	0.04	0.08	0.6	-0.85	1.12	-0.25	-0.02	0.34	2.8	-0.3
		(0.13)	(0.19)	(0.18)	(0.07)	(1.22)	(0.75)	(1.21)	(0.82)	(0.73)	(0.34)	(0.22)	(7.28)	(0.45)
	Observations	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	*2569*	2569	2569

Notes: This table presents the test on covariates using the specification described in equation 1.3. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. The selected sample is children living in rural-coast area and the estimated standard errors, reported in parentheses, are clustered at the district level. Values for simulation on tropical storm were 215500 for Q1, 211261 for Q2, 224532 for Q3 and 256540 gestation corresponding from destruction due to the impact of non hurricane storms in the period. Values for simulation on one hurricane were 5.4 mill. for Q1 and Q2, 4.9 mill. for Q3 and 5.3mill. Values for simulation on on more than one hurricane were 21 mill. for Q1 and Q2 and 19.5 mill. for Q3 and gestation corresponding from average destruction due to the impact of two or more hurricanes in the same period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table C2: Adaptability measured as change in house outer walls' quality

Destruction Received	Outcome variable: Dummy of good quality walls.						
Average tropical storm (excld. Hurricanes)	12 month Storm	0.000	0.022	0.025	0.015	0.011	0.011
		(0.042)	(0.042)	(0.042)	(0.042)	(0.043)	(0.043)
At most one hurricane	12 month Storm	0.000	0.011	0.013	0.008	0.005	0.006
		(0.021)	(0.021)	(0.021)	(0.021)	(0.022)	(0.022)
Two or more hurricanes	12 month Storm	0.066	0.109	0.126	0.116	0.101	0.098
		(0.125)	(0.140)	(0.145)	(0.143)	(0.144)	(0.146)
	Education controls	No	Yes	Yes	Yes	Yes	Yes
	Head's age	No	No	Yes	Yes	Yes	Yes
Controls	household's size controls	No	No	No	Yes	Yes	Yes
	Children's age	No	No	No	No	Yes	Yes
	Children and heand's female dummy	No	No	No	No	No	Yes
Observations		*2401*	*2292*	*2292*	*2292*	*2292*	*2292*

Notes: This table presents the test on covariates using the specification described in equation 1.3. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. The selected sample is children living in rural-coast area and the estimated standard errors, reported in parentheses, are clustered at the district level. Values for simulation on tropical storm were 215500 for Q1, 211261 for Q2, 224532 for Q3 and 256540 gestation corresponding from destruction due to the impact of non hurricane storms in the period. Values for simulation on one hurricane were 5.4 mill. for Q1 and Q2, 4.9 mill. for Q3 and 5.3mill. Values for simulation on on more than one hurricane were 21 mill. for Q1 and Q2 and 19.5 mill. for Q3 and gestation corresponding from average destruction due to the impact of two or more hurricanes in the same period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table C3: Placebo test average destruction due to average tropical storm

		Assuming storms hit two years after measurement					Assuming storms hit three years after measurement				
Sample	Gestation period	Birth weight	Low birth weight	ZWH	ZWA	ZHA	Birth weight	Low birth weight	ZWH	ZWA	ZHA
Coast rural sample	1st Trimester	-0.008	-0.001	-0.009	0.03	0.003	0.001	-0.002	-0.005	-0.02	-0.01
		(0.03)	(0.02)	(0.07)	(0.06)	(0.08)	(0.008)	(0.003)	(0.01)	(0.02)	(0.02)
	2nd Trimester	0.0097	-0.03***	-0.001	0.0004	0.02	0.003	-0.01	-0.33*	-0.18	0.11
		(0.01)	(0.009)	(0.03)	(0.03)	(0.03)	(0.07)	(0.02)	(0.18)	(0.14)	(0.13)
	3rd Trimester	-0.02	0.01	-0.09	-0.15	0.04	0.18*	0.01	-0.07	-0.19	0.08
		(0.11)	(0.06)	(0.11)	(0.12)	(0.18)	(0.1)	(0.07)	(0.28)	(0.26)	(0.36)
	Observations	*1764*	*1764*	*2052*	*2186*	*2159*	*1764*	*1764*	*2052*	*2186*	*2159*
Complete sample	1st Trimester	0.008	-0.007	0.008	0.006	0.01	0.002	-0.004	-0.01	-0.01	0.005
		(0.01)	(0.006)	(0.03)	(0.03)	(0.03)	(0.004)	(0.003)	(0.01)	(0.01)	(0.01)
	2nd Trimester	-0.003	-0.003	-0.009	-0.01	0.003	-0.03	0.02*	-0.16***	-0.13**	-0.08
		(0.007)	(0.004)	(0.01)	(0.01)	(0.02)	(0.02)	(0.012)	(0.05)	(0.05)	(0.06)
	3rd Trimester	-0.00001	-0.0004	0.01	0.01	-0.001	0.01	-0.02	0.07	0.09	0.06
		(0.004)	(0.002)	(0.01)	(0.02)	(0.02)	(0.05)	(0.03)	(0.13)	(0.13)	(0.15)
	Observations	*9963*	*9963*	*11255*	*11991*	*11864*	*9963*	*9963*	*11255*	*11991*	*11864*

Notes: This table presents the results for the placebo test using the equation 1.3. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Controls included are: household head's education and age and a dummy for female head, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, and child's age and gender (dummy for female). The selected sample is children living in rural-coast area and the estimated standard errors, reported in parentheses, are clustered at the district level. Values for simulation were 21 mill. for Q1 and Q2 and 19.5 mill. for Q3 and gestation corresponding from median destruction due to the impact of two or more hurricanes in the same period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table C4: Placebo test average destruction due to at most one hurricane

Sample	Gestation period	Assuming storms hit two years after measurement					Assuming storms hit three years after measurement				
		Birth weight	Low birth weight	ZWH	ZWA	ZHA	Birth weight	Low birth weight	ZWH	ZWA	ZHA
Coast rural sample	1st Trimester	-0.34	-0.02	-0.26	-0.45	-0.76	-0.06	-0.05	-0.15	-0.85	-0.42
		(0.31)	(0.2)	(0.66)	(0.59)	(0.82)	(0.34)	(0.13)	(0.58)	(0.93)	(1.02)
	2nd Trimester	0.1	-0.32***	-0.02	-0.04	0.15	0.03	-0.03	-1.56**	-0.98	0.38
		(0.16)	(0.11)	(0.36)	(0.31)	(0.43)	(0.29)	(0.1)	(0.77)	(0.64)	(0.59)
	3rd Trimester	-0.05	0.02	-0.26	-0.44	0.1	0.02*	0.0002	-0.01	-0.01	0.02
		(0.31)	(0.18)	(0.31)	(0.35)	(0.53)	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)
	Observations	*1764*	*1764*	*2052*	*2186*	*2159*	*1764*	*1764*	*2052*	*2186*	*2159*
Complete sample	1st Trimester	-0.03	-0.05	-0.01	-0.02	0.003	0.02	-0.13	-0.43	-0.4	0.1
		(0.1)	(0.06)	(0.22)	(0.24)	(0.31)	(0.14)	(0.09)	(0.36)	(0.36)	(0.49)
	2nd Trimester	-0.03	-0.03	-0.07	-0.09	0.02	-0.09	0.07	-0.66***	-0.51**	-0.25
		(0.07)	(0.04)	(0.15)	(0.14)	(0.18)	(0.08)	(0.05)	(0.21)	(0.22)	(0.23)
	3rd Trimester	-0.001	-0.003	0.09	0.04	-0.01	0.001	-0.001	0.001	0.002	0.002
		(0.03)	(0.02)	(0.11)	(0.13)	(0.13)	(0.001)	(0.001)	(0.003)	(0.003)	(0.004)
	Observations	*9963*	*9963*	*11255*	*11991*	*11864*	*9963*	*9963*	*11255*	*11991*	*11864*

Notes: This table presents the results for the placebo test using the equation 1.3. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Controls included are: household head's education and age and a dummy for female head, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, and child's age and gender (dummy for female). The selected sample is children living in rural-coast area and the estimated standard errors, reported in parentheses, are clustered at the district level. Values for simulation were 5.4 mill. for Q1 and Q2, 4.9 mill. for Q3 and 5.3mill. gestation corresponding from median destruction due to the impact of at most one hurricanes in the period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table C5: Placebo test average destruction due to two or more hurricanes

Sample	Gestation period	Assuming storms hit two years after measurement					Assuming storms hit three years after measurement				
		Birth weight	Low birth weight	ZWH	ZWA	ZHA	Birth weight	Low birth weight	ZWH	ZWA	ZHA
Coast rural sample	1st Trimester	-0.48	-0.26	-0.52	-1.12	-1.24	-0.63*	0.2	0.46	0.3	-0.39
		(0.48)	(0.26)	(0.71)	(0.74)	(1.01)	(0.37)	(0.19)	(0.69)	(1.03)	(1.2)
	2nd Trimester	0.09	-0.26***	-0.02	-0.02	0.14	-0.01	-0.05	-1.09*	-1.89**	-0.78
		(0.13)	(0.09)	(0.29)	(0.26)	(0.35)	(0.42)	(0.2)	(0.65)	(0.83)	(1)
	3rd Trimester	-0.05	0.02	-0.26	-0.44	0.1	0.02*	0.0002	-0.01	-0.01	0.02
		(0.31)	(0.18)	(0.31)	(0.35)	(0.53)	(0.01)	(0.01)	(0.02)	(0.02)	(0.03)
	Observations	*1764*	*1764*	*2052*	*2186*	*2159*	*1764*	*1764*	*2052*	*2186*	*2159*
Complete sample	1st Trimester	-0.1	-0.05	-0.07	0.02	-0.09	-0.2	-0.04	0.29	-0.07	-0.25
		(0.16)	(0.09)	(0.35)	(0.39)	(0.52)	(0.14)	(0.08)	(0.29)	(0.35)	(0.42)
	2nd Trimester	-0.03	-0.02	-0.06	-0.08	0.02	0.03	-0.08	-0.81**	-0.67	0.13
		(0.05)	(0.03)	(0.12)	(0.11)	(0.14)	(0.15)	(0.11)	(0.41)	(0.44)	(0.57)
	3rd Trimester	-0.001	-0.003	0.09	0.04	-0.01	0.001	-0.001	0.001	0.002	0.002
		(0.03)	(0.02)	(0.11)	(0.13)	(0.13)	(0.001)	(0.001)	(0.003)	(0.003)	(0.004)
	Observations	*9963*	*9963*	*11255*	*11991*	*11864*	*9963*	*9963*	*11255*	*11991*	*11864*

Notes: This table presents the results for the placebo test using the equation 1.3. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Controls included are: household head's education and age and a dummy for female head, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, and child's age and geneder (dummy for female). The selected sample is children living in rural-coast area and the estimated standard errors, reported in parentheses, are clustered at the district level. Values for simulation were 21 mill. for Q1 and Q2 and 19.5 mill. for Q3 and gestation corresponding from median destruction due to the impact of two or more hurricanes in the same period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table C6: Bound analysis. Using tropical storms only (excluding hurricanes)

Gestation period	Birth weight			Low birth weight			ZWH			ZWA			ZHA		
	10th percentile	P.E	90th percentile	Best	P.E	Worst	10th percentile	P.E	90th percentile	10th percentile	P.E	90th percentile	10th percentile	P.E	90th percentile
1st Trimester	-0.07	-0.17	-0.14	0.13	0.21	0.11	0.31	0.3	0.24	0.44	0.46	0.36	0.43	0.35	0.25
	(0.18)	(0.18)	(0.17)	(0.15)	(0.15)	(0.17)	(0.46)	(0.46)	(0.46)	(0.38)	(0.38)	(0.39)	(0.29)	(0.29)	(0.29)
2nd Trimester	0.04**	0.04**	0.04**	-0.01	-0.01	-0.01	0.07	0.08*	0.07	0.05	0.07*	0.04	-0.003	0.003	0.002
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)
3rd Trimester	-0.07	-0.05	-0.06	-0.03	-0.04	0.05	0.29	0.34*	0.35	0.25	0.34*	0.31*	-0.06	-0.07	0.02
	(0.1)	(0.11)	(0.11)	(0.06)	(0.07)	(0.08)	(0.21)	(0.21)	(0.21)	(0.18)	(0.19)	(0.19)	(0.22)	(0.22)	(0.2)
Observations	*1771*	*1764*	*1771*	*1771*	*1764*	*1771*	*2055*	*2052*	*2055*	*2190*	*2186*	*2190*	*2165*	*2159*	*2165*
Clusters	*329*	*329*	*329*	*329*	*329*	*329*	*335*	*335*	*335*	*338*	*338*	*338*	*337*	*337*	*337*
	25th percentile	P.E	75th percentile				25th percentile	P.E	75th percentile	25th percentile	P.E	75th percentile	25th percentile	P.E	75th percentile
1st Trimester	-0.08	-0.17	-0.12				0.29	0.3	0.26	0.43	0.46	0.41	0.35	0.35	0.3
	(0.18)	(0.18)	(0.17)				(0.46)	(0.46)	(0.46)	(0.38)	(0.38)	(0.38)	(0.29)	(0.29)	(0.29)
2nd Trimester	0.04**	0.04**	0.04**				0.07	0.08*	0.07	0.05	0.07*	0.05	-0.003	0.003	0.001
	(0.02)	(0.02)	(0.02)				(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)	(0.04)
3rd Trimester	-0.07	-0.05	-0.06				0.3	0.34*	0.33	0.26	0.34*	0.28	-0.03	-0.07	-0.003
	(0.1)	(0.11)	(0.1)				(0.21)	(0.21)	(0.21)	(0.19)	(0.19)	(0.18)	(0.2)	(0.22)	(0.2)
Observations	*1771*	*1764*	*1771*				*2055*	*2052*	*2055*	*2190*	*2186*	*2190*	*2165*	*2159*	*2165*
Clusters	*329*	*329*	*329*				*335*	*335*	*335*	*338*	*338*	*338*	*337*	*337*	*337*

Notes: This table presents the results for the bounds test. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Controls included are: household head's education and age and a dummy for female head, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, and child's age and gender (dummy for female). The selected sample is children living in rural-coast area and the estimated standard errors, reported in parentheses, are clustered at the district level. Values for simulation were 215500 for Q1, 211261 for Q2, and 224532 for Q3 corresponding from average destruction due to the impact of non hurricane storms in the period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table C7: Bound analysis. Using destruction due to at most one hurricane

Gestation period	Birth weight			Low birth weight			ZWH			ZWA			ZHA		
	10th percentile	P.E	90th percentile	Best	P.E	Worst	10th percentile	P.E	90th percentile	10th percentile	P.E	90th percentile	10th percentile	P.E	90th percentile
1st Trimester	-0.29	-0.45	-0.39	0.22	0.39	0.24	0.51	0.71	0.55	0.39	0.5	0.33	0.05	0.14	0.06
	(0.32)	(0.34)	(0.3)	(0.27)	(0.27)	(0.3)	(0.79)	(0.84)	(0.78)	(0.63)	(0.68)	(0.65)	(0.52)	(0.55)	(0.54)
2nd Trimester	0.39*	0.35*	0.34*	-0.07	-0.08	-0.005	0.91	1.13*	0.93	0.71	0.98*	0.53	0.044	0.145	0.111
	(0.22)	(0.21)	(0.21)	(0.15)	(0.15)	(0.18)	(0.59)	(0.58)	(0.61)	(0.57)	(0.57)	(0.67)	(0.55)	(0.57)	(0.57)
3rd Trimester	-0.36	-0.27	-0.26	0.07	0.06	0.32	-0.16	-0.01	0.04	-0.37	-0.26	-0.15	-0.58	-0.51	-0.32
	(0.29)	(0.3)	(0.29)	(0.16)	(0.17)	(0.22)	(0.61)	(0.6)	(0.59)	(0.54)	(0.56)	(0.55)	(0.64)	(0.63)	(0.61)
Observations	*1771*	*1764*	*1771*	*1771*	*1764*	*1771*	*2055*	*2052*	*2055*	*2190*	*2186*	*2190*	*2165*	*2159*	*2165*
Clusters	*329*	*329*	*329*	*329*	*329*	*329*	*335*	*335*	*335*	*338*	*338*	*338*	*337*	*337*	*337*
	25th percentile	P.E	75th percentile				25th percentile	P.E	75th percentile	25th percentile	P.E	75th percentile	25th percentile	P.E	75th percentile
1st Trimester	-0.3	-0.45	-0.4				0.53	0.71	0.54	0.4	0.5	0.39	0.11	0.14	0.04
	(0.32)	(0.34)	(0.3)				(0.79)	(0.84)	(0.79)	(0.64)	(0.68)	(0.64)	(0.53)	(0.55)	(0.53)
2nd Trimester	0.36*	0.35*	0.34				0.92	1.13*	0.93	0.69	0.98*	0.73	0.064	0.145	0.101
	(0.21)	(0.21)	(0.21)				(0.59)	(0.58)	(0.61)	(0.58)	(0.57)	(0.58)	(0.55)	(0.57)	(0.56)
3rd Trimester	-0.34	-0.27	-0.28				-0.12	-0.01	-0.02	-0.34	-0.26	-0.28	-0.48	-0.51	-0.384
	(0.29)	(0.3)	(0.29)				(0.6)	(0.6)	(0.59)	(0.54)	(0.56)	(0.53)	(0.62)	(0.63)	(0.61)
Observations	*1771*	*1764*	*1771*				*2055*	*2052*	*2055*	*2190*	*2186*	*2190*	*2165*	*2159*	*2165*
Clusters	*329*	*329*	*329*				*335*	*335*	*335*	*338*	*338*	*338*	*337*	*337*	*337*

Notes: This table presents the results for the bounds test. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Controls included are: household head's education and age and a dummy for female head, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, and child's age and gender (dummy for female). The selected sample is children living in rural-coast area and the standard errors are clustered at the district level. Values for simulation were 5.4 mill. for Q1 and Q2, and 4.9 mill. for Q3 corresponding from average destruction due to the impact of at least one hurricanes in the period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table C8: Bound analysis. Using destruction due to two or more hurricanes

Gestation period	Birth weight			Low birth weight			ZWH			ZWA			ZHA		
	10th percentile	P.E	90th percentile	Best	P.E	Worst	10th percentile	P.E	90th percentile	10th percentile	P.E	90th percentile	10th percentile	P.E	90th percentile
1st Trimester	-0.29	-0.45	-0.39	0.22	0.39	0.24	0.51	0.71	0.55	0.39	0.5	0.33	0.05	0.14	0.06
	(0.32)	(0.34)	(0.3)	(0.27)	(0.27)	(0.3)	(0.79)	(0.84)	(0.78)	(0.63)	(0.68)	(0.65)	(0.52)	(0.55)	(0.54)
2nd Trimester	0.39*	0.35*	0.34*	-0.07	-0.08	-0.005	0.91	1.13*	0.93	0.71	0.98*	0.53	0.044	0.145	0.111
	(0.22)	(0.21)	(0.21)	(0.15)	(0.15)	(0.18)	(0.59)	(0.58)	(0.61)	(0.57)	(0.57)	(0.67)	(0.55)	(0.57)	(0.57)
3rd Trimester	-0.36	-0.27	-0.26	0.07	0.06	0.32	-0.16	-0.01	0.04	-0.37	-0.26	-0.15	-0.58	-0.51	-0.32
	(0.29)	(0.3)	(0.29)	(0.16)	(0.17)	(0.22)	(0.61)	(0.6)	(0.59)	(0.54)	(0.56)	(0.55)	(0.64)	(0.63)	(0.61)
Observations	*1771*	*1764*	*1771*	*1771*	*1764*	*1771*	*2055*	*2052*	*2055*	*2190*	*2186*	*2190*	*2165*	*2159*	*2165*
Clusters	*329*	*329*	*329*	*329*	*329*	*329*	*335*	*335*	*335*	*338*	*338*	*338*	*337*	*337*	*337*
	25th percentile	P.E	75th percentile				25th percentile	P.E	75th percentile	25th percentile	P.E	75th percentile	25th percentile	P.E	75th percentile
1st Trimester	-0.3	-0.45	-0.4				0.53	0.71	0.54	0.4	0.5	0.39	0.11	0.14	0.04
	(0.32)	(0.34)	(0.3)				(0.79)	(0.84)	(0.79)	(0.64)	(0.68)	(0.64)	(0.53)	(0.55)	(0.53)
2nd Trimester	0.36*	0.35*	0.34				0.92	1.13*	0.93	0.69	0.98*	0.73	0.064	0.145	0.101
	(0.21)	(0.21)	(0.21)				(0.59)	(0.58)	(0.61)	(0.58)	(0.57)	(0.58)	(0.55)	(0.57)	(0.56)
3rd Trimester	-0.34	-0.27	-0.28				-0.12	-0.01	-0.02	-0.34	-0.26	-0.28	-0.48	-0.51	-0.384
	(0.29)	(0.3)	(0.29)				(0.6)	(0.6)	(0.59)	(0.54)	(0.56)	(0.53)	(0.62)	(0.63)	(0.61)
Observations	*1771*	*1764*	*1771*				*2055*	*2052*	*2055*	*2190*	*2186*	*2190*	*2165*	*2159*	*2165*
Clusters	*329*	*329*	*329*				*335*	*335*	*335*	*338*	*338*	*338*	*337*	*337*	*337*

Notes: This table presents the results for the bounds test. All the regressions include controls and fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Controls included are: household head's education and age and a dummy for female head, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, and child's age and gender (dummy for female). The selected sample is children living in rural-coast area and the standard errors are clustered at the district level. Values for simulation were 5.4 mill. for Q1 and Q2, and 4.9 mill. for Q3 corresponding from average destruction due to the impact of at two more hurricanes in the period. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Appendix D

Tropical storms' set (Informality)

Table D1: Set of storms

Year	Storm	Max wind speed (Km/h)	Start date (near to Jamaica)	End date (near to Jamaica)	Saffir-Simpson Scale
2004	BONNIE	55	3-Aug-04	13-Aug-04	T D
2004	CHARLEY	130	9-Aug-04	15-Aug-04	SS 1
2004	IVAN	145	2-Sep-04	24-Sep-04	SS 1
2004	JEANNE	105	13-Sep-04	29-Sep-04	T S
2005	ALPHA	45	22-Oct-05	24-Oct-05	T D
2005	DENNIS	130	4-Jul-05	18-Jul-05	SS 1
2005	EMILY	140	11-Jul-05	21-Jul-05	SS 1
2005	GAMMA	45	14-Nov-05	21-Nov-05	T D
2005	WILMA	160	15-Oct-05	26-Oct-05	SS 2
2006	CHRIS	55	1-Aug-06	6-Aug-06	T D
2006	ERNESTO	75	24-Aug-06	4-Sep-06	T S
2007	DEAN	150	13-Aug-07	22-Aug-07	SS 1
2007	FELIX	150	31-Aug-07	6-Sep-07	SS 1

Continued on next page

Table D1 – continued from previous page

Year	**Storm**	**Max wind speed (Km/h)**	**Start date (near to Jamaica)**	**End date (near to Jamaica)**	**Saffir-Simpson Scale**
2007	NOEL	75	24-Oct-07	5-Nov-07	T S
2007	OLGA	50	10-Dec-07	16-Dec-07	T D
2008	FAY	60	15-Aug-08	28-Aug-08	T D
2008	GUSTAV	135	25-Aug-08	5-Sep-08	SS 1
2008	HANNA	75	28-Aug-08	8-Sep-08	T S
2008	IKE	125	1-Sep-08	15-Sep-08	SS 1
2008	PALOMA	125	5-Nov-08	14-Nov-08	SS 1
2010	ALEX	95	24-Jun-10	1-Jul-10	T S
2010	BONNIE	40	22-Jul-10	25-Jul-10	T D
2010	KARL	110	13-Sep-10	18-Sep-10	T S
2010	MATTHEW	50	23-Sep-10	26-Sep-10	T D
2010	NICOLE	40	28-Sep-10	30-Sep-10	T D
2010	RICHARD	85	19-Oct-10	26-Oct-10	T S
2010	TOMAS	85	29-Oct-10	10-Nov-10	T S
2011	EMILY	45	2-Aug-11	7-Aug-11	T D
2011	RINA	100	22-Oct-11	29-Oct-11	T S
2012	ERNESTO	75	1-Aug-12	10-Aug-12	T S
2012	HELENE	50	9-Aug-12	18-Aug-12	T D
2012	ISAAC	70	20-Aug-12	1-Sep-12	T S
2012	SANDY	100	21-Oct-12	31-Oct-12	T S
2013	DORIAN	50	31-Jul-13	31-Jul-13	TD
2014	HANNA	35	25-Oct-14	26-Oct-14	TD

Appendix E

Estimation results (Informality)

Table E1: Estimation results

	$P(Informal_{t-1})$		$P(Retention)$		$P(working_t)$		$P(Informal_t \mid Informal_{t-1})$		$P(Informal_t \mid Formal_{t-1})$	
Variable	Coef	SE	Coef	SE	Coef	SE	Coef	SE	Coef	SE
T_1Q_as1_t							-8.43E-09	8.56E-09	-1.21E-08	7.15E-09
T_1Q_as2_t							4.80E-16	4.03E-16	5.85E-16	3.34E-16
T_2Q_as1_t							-1.21E-08	7.30E-09	-1.01E-10	6.22E-09
T_2Q_as2_t							6.74E-16	3.40E-16	1.36E-16	2.82E-16
T_1Q_as1_t_1	-7.77E-09	4.70E-09	2.37E-08	2.10E-08	-1.36E-08	6.11E-09				
T_1Q_as2_t_1	4.06E-16	2.21E-16	-9.41E-16	1.01E-15	8.61E-16	2.94E-16				
T_2Q_as1_t_1	-7.30E-09	5.41E-09	-6.65E-09	1.89E-08	-1.32E-08	6.75E-09				
T_2Q_as2_t_1	3.42E-16	2.42E-16	-3.52E-17	8.46E-16	4.77E-16	3.07E-16				
rural_t_1	0.44	0.02	-0.11	0.08	-0.07	0.03	0.35	0.06	0.21	0.05
professionals_t_1	0.05	0.06					0.10	0.09	0.05	0.06
h_week_t_1	0.01	0.00					0.01	0.00	0.01	0.00
age_t_1	-0.01	0.01	-0.03	0.02	0.06	0.01	0.00	0.01	-0.03	0.01
age2_t_1	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
no_qualif	1.24	0.16	0.17	0.33	-0.40	0.14	1.08	0.31	0.82	0.19
other_qualif	0.44	0.17	0.09	0.36	-0.20	0.15	0.64	0.29	0.29	0.18
o_level	0.31	0.16	0.13	0.34	-0.09	0.15	0.56	0.28	0.17	0.17
other_high_deg	-0.20	0.17	0.11	0.34	0.33	0.15	0.08	0.30	-0.13	0.18
Working in the last 5 years	0.15	0.02								
dist_kingston			0.0026	0.00086						
dist_avg_altitude_t_1					0.00010	0.000075				
_cons	-1.81	0.20	3.45	0.57	0.47	0.19	0.82	0.61	-1.63	0.30

All the regressions include fixed effects for birth year-month, survey year, district, and district-birth year-specific linear time trend. Following Duflo (2003), The included controls are: household head's education (dummies for primary, secondary incomplete, secondary complete, tertiary), and age, household size, number of individuals in household of age 0-5, 6-14, 15-24, 25-49, a dummy that accounts for complete vaccination at the measurement age (num. of doses of dtp and ovp, measles and bcg at born), and the child's age in months. Error in parenthesis are robust clustered at the district level.

Appendix F

Jamaica Labor Force Survey and Jamaica Survey of Living Conditions Design (Health Fees)

The Jamaica Labor Force Survey (LFS) is designed as a two-stage stratified random sample. The first stage includes a selection of Primary Sampling Units (PSUs), and the second stage a selection of dwellings. A PSU is an Enumeration District (ED) or a combination of EDs that is selected for a sample, usually containing a minimum of approximately 100 dwellings in the rural areas and a minimum of 150 dwellings for the urban communities. An ED is an independent geographic unit sharing common boundaries with contiguous EDs. After the random selection of PSUs, a listing operation of the dwellings located in each PSU is executed to define the master sample for the LFS. This master sample is revised every three to four years usually implying a new selection of PSUs, listing operation and revised selection of dwellings.

The LFS includes a rotating panel scheme as follows. Once the selected PSUs are listed, 32 dwellings are randomly selected from each PSU. These 32 dwellings are then divided into eight groups or panels of four dwellings each. Dwellings in panels 1 to 4 are interviewed in the first quarter LFS (16 dwellings per PSU each quarter). Dwellings in panels 3 to 6 are interviewed in the second quarter LFS. Dwellings in panels 5 to 8 are interviewed in the third quarter LFS. Dwellings in panels 1, 2, 7 and 8 are interviewed in the fourth quarter LFS. In the first quarter of the following year dwellings in panels 1 to 4 are interviewed again and the yearly cycle is repeated (Table F1). This rotating panel scheme with the same dwellings lasts until the master sample is revised usually every three to four years.

Table F1: LFS Rotational panel

		Panel							
Year	LFS Quarter	1	2	3	4	5	6	7	8
t	January								
	April								
	July								
	October								
t+1	January								
	April								
	July								
	October								

Jamaica is administratively divided into 14 parishes. Each quarterly LFS is representative at the parish and the national level. The Survey of Living Conditions (SLC) usually covers a nationally representative subsample of the April LFS (covering approximately a third of the EDs sampled in the LFS). However, periodically every four or five years, the SLC covers the entire April LFS sample. This exercise is periodically conducted with the objective of producing consumption and poverty aggregates not only at the national level but also at the parish level with acceptable standard errors. Table F2 shows the number of EDs surveyed in the April LFS and SLC corresponding to the yearly periods used in our analyses. Within our study period, years 2002, 2008, and 2012 included large SLC samples covering the entirety of EDs surveyed in the April LFS.

Table F2: Surveyed EDs in the April LFS and SLC

Year	April LFS EDs	SLC EDs	% SLC Sample
2002	522	522	1.00
2004	505	169	0.33
2006	507	170	0.34
2007	508	168	0.33
2008	612	612	1.00
2009	508	169	0.33
2010	507	169	0.33
2012	508	508	1.00

The LFS rotating panel wave that we exploit to identify the individual level panel sub-sample covers from the first quarter of 2007 until the last quarter of 2009. Therefore, we first identified dwellings covered in both the April LFS and the SLC each year from 2007 to 2009. After this step, we identified the same individuals who were covered by both surveys yearly between 2007 and 2009. This exercise resulted in a subsample of 684 individuals (or 2,052 individual-year observations) between 21 and 64 years old.

Appendix G

Estimation results (Health Fees)

Table G1: Event Study Estimates by Age

	Illness	Illness with Days Lost	ADLs	Employed	Contributed to NIS	Secondary Job	Working Hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. 21 - 39 years old							
Pre-policy trends							
Unisured x 2002	0.06*	0.04*	0.10	-0.02		-0.03**	
	(0.03)	(0.02)	(0.10)	(0.04)		(0.01)	
Unisured x 2004	0.00	0.02	0.13	0.03	-0.01	-0.01	2.11
	(0.04)	(0.03)	(0.14)	(0.05)	(0.05)	(0.01)	(1.34)
Unisured x 2006	-0.00	0.00	-0.07	0.02	0.01	-0.01	0.50
	(0.04)	(0.03)	(0.14)	(0.04)	(0.05)	(0.01)	(1.01)
Post-policy effects							
Unisured x 2008	0.03	0.02	-0.09	-0.00	-0.01	-0.02**	0.51
	(0.03)	(0.02)	(0.10)	(0.03)	(0.04)	(0.01)	(0.86)
Unisured x 2009	0.01	0.01	-0.13	0.00	-0.00	-0.02	0.96
	(0.03)	(0.03)	(0.16)	(0.03)	(0.04)	(0.02)	(0.93)
Unisured x 2010	0.01	0.00	-0.05	-0.01	0.02	-0.01	0.89
	(0.03)	(0.03)	(0.12)	(0.04)	(0.05)	(0.01)	(1.11)
Unisured x 2012	0.05*	0.03	-0.06	-0.03	0.02	-0.02**	1.36
	(0.03)	(0.02)	(0.10)	(0.04)	(0.04)	(0.01)	(0.93)
Observations	18,265	18,221	18,221	18,307	14,797	18,307	10,060
Panel B. 40 - 64 years old							
Pre-policy trends							
Unisured x 2002	-0.06	-0.02	-0.13	0.02		0.04*	
	(0.04)	(0.03)	(0.29)	(0.04)		(0.02)	
Unisured x 2004	-0.09	-0.09	-0.92	0.06	0.05	-0.01	-0.23
	(0.07)	(0.07)	(0.87)	(0.05)	(0.07)	(0.03)	(1.84)
Unisured x 2006	-0.05	-0.06	-0.44	0.03	-0.02	0.02	1.29
	(0.04)	(0.03)	(0.29)	(0.05)	(0.05)	(0.03)	(1.36)
Post-policy effects							
Unisured x 2008	-0.07**	-0.05*	-0.37	0.02	0.05	0.04**	2.78**
	(0.03)	(0.03)	(0.26)	(0.03)	(0.03)	(0.02)	(1.28)
Unisured x 2009	-0.12***	-0.10***	-0.63**	-0.01	0.03	0.03*	4.51***
	(0.04)	(0.03)	(0.32)	(0.03)	(0.04)	(0.02)	(1.20)
Unisured x 2010	-0.06	-0.06*	-0.77**	-0.00	0.01	0.03	5.32***
	(0.04)	(0.03)	(0.36)	(0.04)	(0.04)	(0.02)	(1.82)
Unisured x 2012	-0.08**	-0.06**	-0.42*	0.01	0.00	0.04*	2.87***
	(0.03)	(0.03)	(0.25)	(0.03)	(0.04)	(0.02)	(1.08)
Observations	17,103	16,988	16,988	17,127	14,053	17,127	10,363

Notes: This table presents estimated event study effects disentangling average post-policy effects into individual post-policy yearly effects and differential pre-policy trends by age groups. All coefficients are expressed with respect to year 2007. Estimated coefficients result from OLS regressions with district fixed-effects, socio-demographic controls, and year fixed-effects. Regressions are weighted by the inverse of the household level sampling probability to reflect survey design. Estimated standard errors, reported in parentheses, are clustered at the district level. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table G2: Event Study Estimates by Gender

	Illness	Illness with Days Lost	ADLs	Employed	Contributed to NIS	Secondary Job	Working Hours
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Panel A. 21 - 39 years old							
Pre-policy trends							
Unisured x 2002	0.00	0.00	-0.16	-0.03		0.00	
	(0.03)	(0.02)	(0.22)	(0.03)		(0.02)	
Unisured x 2004	-0.03	0.01	0.04	0.01	-0.03	-0.02	1.09
	(0.04)	(0.03)	(0.23)	(0.04)	(0.06)	(0.03)	(1.43)
Unisured x 2006	0.01	-0.02	-0.25	0.05	0.03	0.03	1.45
	(0.03)	(0.03)	(0.25)	(0.04)	(0.05)	(0.03)	(1.28)
Post-policy effects							
Unisured x 2008	-0.01	-0.02	-0.29	-0.01	0.02	0.01	1.81
	(0.03)	(0.02)	(0.23)	(0.03)	(0.03)	(0.02)	(1.21)
Unisured x 2009	-0.01	-0.04	-0.32	-0.04	-0.01	-0.00	3.49***
	(0.03)	(0.03)	(0.27)	(0.03)	(0.04)	(0.02)	(1.08)
Unisured x 2010	-0.00	-0.02	-0.49	-0.00	-0.00	0.01	4.76**
	(0.03)	(0.03)	(0.33)	(0.04)	(0.04)	(0.02)	(2.05)
Unisured x 2012	0.02	-0.00	-0.04	-0.02	0.02	0.02	3.67***
	(0.03)	(0.02)	(0.21)	(0.03)	(0.04)	(0.02)	(1.02)
Observations	16,975	16,921	16,921	16,997	13,805	16,997	11,328
Panel B. 40 - 64 years old							
Pre-policy trends							
Unisured x 2002	0.00	0.00	0.00	-0.02		0.01	
	(0.03)	(0.03)	(0.25)	(0.04)		(0.01)	
Unisured x 2004	-0.06	-0.06	-0.64	0.05	0.04	0.01	-0.41
	(0.06)	(0.05)	(0.58)	(0.05)	(0.06)	(0.01)	(1.41)
Unisured x 2006	-0.05	-0.02	-0.22	-0.00	-0.02	-0.00	0.18
	(0.04)	(0.03)	(0.26)	(0.05)	(0.05)	(0.01)	(1.11)
Post-policy effects							
Unisured x 2008	-0.04	-0.02	-0.21	0.02	0.01	0.01	2.19**
	(0.03)	(0.03)	(0.22)	(0.03)	(0.04)	(0.01)	(0.85)
Unisured x 2009	-0.07*	-0.04	-0.41	-0.00	0.00	0.01	2.07**
	(0.04)	(0.03)	(0.28)	(0.04)	(0.04)	(0.01)	(0.97)
Unisured x 2010	-0.05	-0.02	-0.34	-0.01	0.02	0.01	1.60
	(0.04)	(0.03)	(0.30)	(0.04)	(0.04)	(0.01)	(1.10)
Unisured x 2012	-0.03	-0.02	-0.38	-0.03	-0.02	0.01	0.71
	(0.03)	(0.03)	(0.24)	(0.04)	(0.04)	(0.01)	(0.98)
Observations	18,393	18,288	18,288	18,437	15,045	18,437	9,095

Notes: This table presents estimated event study effects disentangling average post-policy effects into individual post-policy yearly effects and differential pre-policy trends by gender. All coefficients are expressed with respect to year 2007. Estimated coefficients result from OLS regressions with district fixed-effects, socio-demographic controls, and year fixed-effects. Regressions are weighted by the inverse of the household level sampling probability to reflect survey design. Estimated standard errors, reported in parentheses, are clustered at the district level. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.

Table G3: Event Study Estimates by Presence of Minors at Home

	Illness (1)	Illness with Days Lost (2)	ADLs (3)	Employed (4)	Contributed to NIS (5)	Secondary Job (6)	Working Hours (7)
				Panel A. 21 - 39 years old			
Pre-policy trends							
Unisured x 2002	0.00	0.00	0.04	-0.04		0.02	
	(0.03)	(0.02)	(0.22)	(0.04)		(0.02)	
Unisured x 2004	-0.07	-0.05	-0.79	0.06	0.07	0.00	-0.59
	(0.06)	(0.06)	(0.72)	(0.05)	(0.06)	(0.02)	(1.53)
Unisured x 2006	-0.03	-0.01	-0.04	0.01	0.04	0.01	0.77
	(0.04)	(0.03)	(0.24)	(0.04)	(0.05)	(0.02)	(1.04)
Post-policy effects							
Unisured x 2008	-0.04	-0.03	-0.31	0.01	0.04	0.03*	2.56**
	(0.03)	(0.02)	(0.20)	(0.03)	(0.03)	(0.01)	(1.24)
Unisured x 2009	-0.07*	-0.07**	-0.36	-0.01	0.02	0.00	4.33***
	(0.04)	(0.03)	(0.26)	(0.04)	(0.05)	(0.02)	(0.97)
Unisured x 2010	-0.04	-0.04	-0.52	-0.03	-0.00	0.02	4.75**
	(0.04)	(0.03)	(0.32)	(0.05)	(0.05)	(0.02)	(1.86)
Unisured x 2012	-0.01	-0.02	-0.19	-0.01	0.02	0.02	2.92***
	(0.03)	(0.02)	(0.20)	(0.04)	(0.04)	(0.02)	(0.96)
Observations	19,251	19,145	19,145	19,286	15,389	19,286	10,998
				Panel B. 40 - 64 years old			
Pre-policy trends							
Unisured x 2002	-0.00	-0.00	-0.26	-0.01		-0.01	
	(0.04)	(0.03)	(0.23)	(0.04)		(0.02)	
Unisured x 2004	0.01	-0.01	0.06	0.03	-0.02	0.01	2.73*
	(0.05)	(0.04)	(0.32)	(0.06)	(0.06)	(0.02)	(1.41)
Unisured x 2006	-0.02	-0.04	-0.46	0.01	-0.02	0.01	0.69
	(0.04)	(0.04)	(0.30)	(0.05)	(0.06)	(0.02)	(1.32)
Post-policy effects							
Unisured x 2008	-0.01	0.00	-0.19	0.01	0.01	-0.01	1.36
	(0.03)	(0.03)	(0.22)	(0.03)	(0.04)	(0.01)	(0.87)
Unisured x 2009	-0.02	-0.02	-0.39	-0.00	0.01	-0.00	1.70*
	(0.03)	(0.03)	(0.26)	(0.04)	(0.04)	(0.02)	(1.00)
Unisured x 2010	-0.01	-0.00	-0.28	0.00	0.03	-0.01	1.03
	(0.03)	(0.03)	(0.23)	(0.04)	(0.05)	(0.02)	(1.27)
Unisured x 2012	-0.01	-0.01	-0.33	-0.03	-0.00	-0.01	0.95
	(0.03)	(0.03)	(0.22)	(0.04)	(0.04)	(0.01)	(1.04)
Observations	16,117	16,064	16,064	16,148	13,461	16,148	9,425

Notes: This table presents estimated event study effects disentangling average post-policy effects into individual post-policy yearly effects and differential pre-policy trends by presence of minors at home. All coefficients are expressed with respect to year 2007. Estimated coefficients result from OLS regressions with district fixed-effects, socio-demographic controls, and year fixed-effects. Regressions are weighted by the inverse of the household level sampling probability to reflect survey design. Estimated standard errors, reported in parentheses, are clustered at the district level. Significance at the one, five and ten percent levels is indicated by ***, ** and * respectively.