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Healthy to Work

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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 economy during the period 2008–12.

JEL classification: H51, I1, J22, O12, O54.
Keywords: Jamaica, Free Public Healthcare, Health Status, Labor Supply.

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, Alfonso, and Díaz (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, Dobkin, and Gross, 2012; 2014; Beuermann, 2010; Card, Dobkin and Maestas, 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, Carpio and Klein (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, Hendren and Townsend (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, Hallward-Driemeier and Pages-Serra (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-Vázquez (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 Manachotphong (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.

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.[1] 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). 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.[2] 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 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

[1] 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.

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

the year when fees were abolished for children under 18 years old. Finally, the number of surgeries showed a 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 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 Haye 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. The 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.[3] See Appendix 1 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, 2004, 2006, 2007, 2008,

[3] 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.

2009, 2010, and 2012 stacked as a district-level panel.[4] 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 2). The great majority of them (15 percent) held private insurance; while only 2 percent held government-sponsored health insurance. Table 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 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 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

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

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 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.[5] 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

[5] 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.

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.

4. Empirical Strategy

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.[6]

Formally, we estimate regression models of the following form:

$$Y_{idt} = \delta_d + \eta_t + \lambda \cdot T_{idt} + \beta \cdot T_{idt} \cdot Post_t + X'_{idt}\gamma + \varepsilon_{idt} \tag{1}$$

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 the value of 1 if

[6] 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.

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.

Some aspects of model (1) merit discussion. First, the district fixed effects control nonparametrically for any time-invariant unobservable characteristics at the district level. Second, the year fixed effects control nonparametrically 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 (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.[7] Table 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:

$$Y_{idt} = \delta_d + \eta_t + \lambda \cdot T_{idt} + \sum_{j=2002}^{2006} \beta_j \cdot \eta_j \cdot T_{idt} + \sum_{j=2008}^{2012} \beta_j \cdot \eta_j \cdot T_{idt} + X'_{idt}\gamma + \varepsilon_{idt} \tag{2}$$

where all variables are defined as in (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 2002–06 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

[7] 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 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.

households for the period 2007–09.[8] 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. 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 (1) and (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.

5. Results

We begin by graphically inspecting health and employment trends differentiated between uninsured and insured individuals. Figure 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 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.

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

5.1. Effects on Health Status

Panel A of Table 5 shows estimates of β from model (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.

5.2. Effects on Labor Market Dynamics

Panel B of Table 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.[9] 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 spent a yearly average of real US$PPP 546.06 million in the public health system.[10] By contrast, during the first five years of policy adoption,

[9] 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.
[10] All monetary figures in this section are expressed in real 2014 US$PPP.

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.

5.3. 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 (2) to confirm that prior to policy enactment, both uninsured and insured individuals shared parallel trends with respect to the outcomes of interest. Table 6 shows that all of the estimates of βj corresponding to years 2002–06 are statistically indistinguishable from zero. This evidence formally shows that the key identification assumption of parallel trends 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 βj 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 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 (1) and (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.[11] 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 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.[12] 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

[11] 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.

[12] 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.

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.

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 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.

5.4. 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 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 1 further strengthens the robustness of these results by estimating regression model (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 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 2 further strengthens the robustness of these results by estimating regression model (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 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 3 further strengthens the robustness of these results by estimating regression model (2) for these groups showing that the identification assumptions hold.

6. 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.

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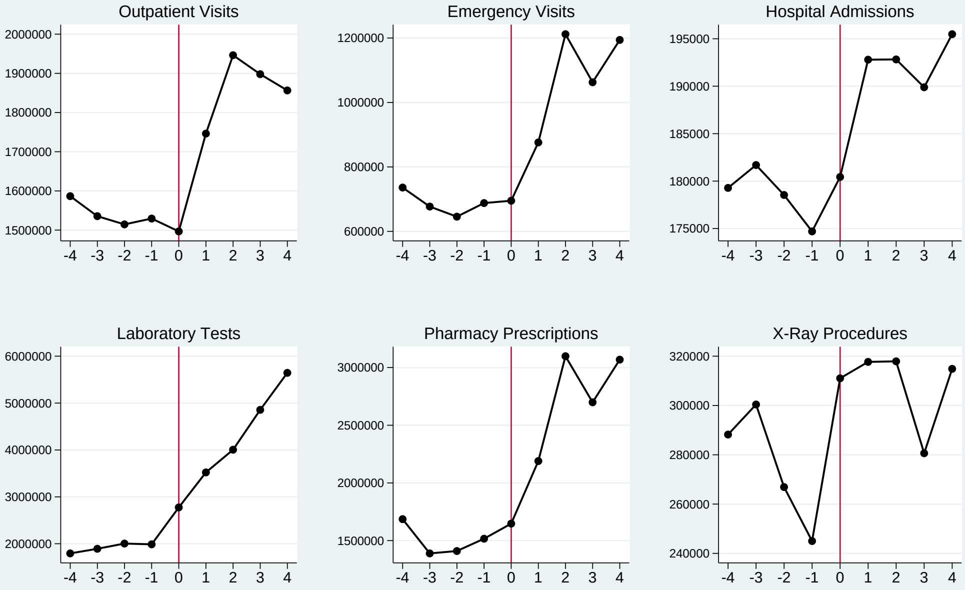
Figure 1: National Public Healthcare Utilization Trends



The X-axis is the year relative to 2007. Public healthcare utilization trends shown in solid lines.
Source: Jamaican Ministry of Health.

Figure 2: Health Insurance Coverage Trends

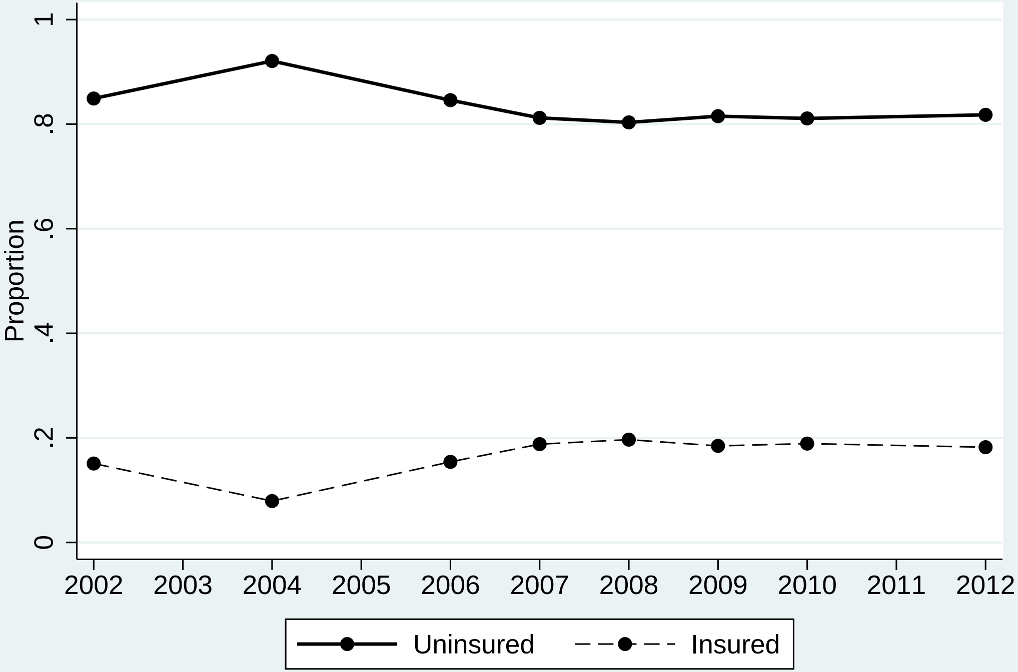
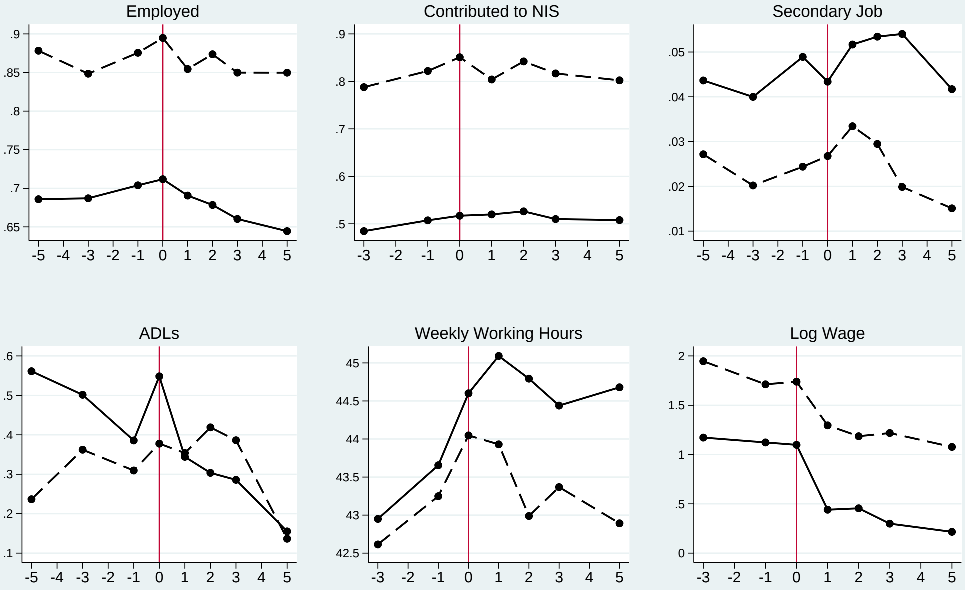
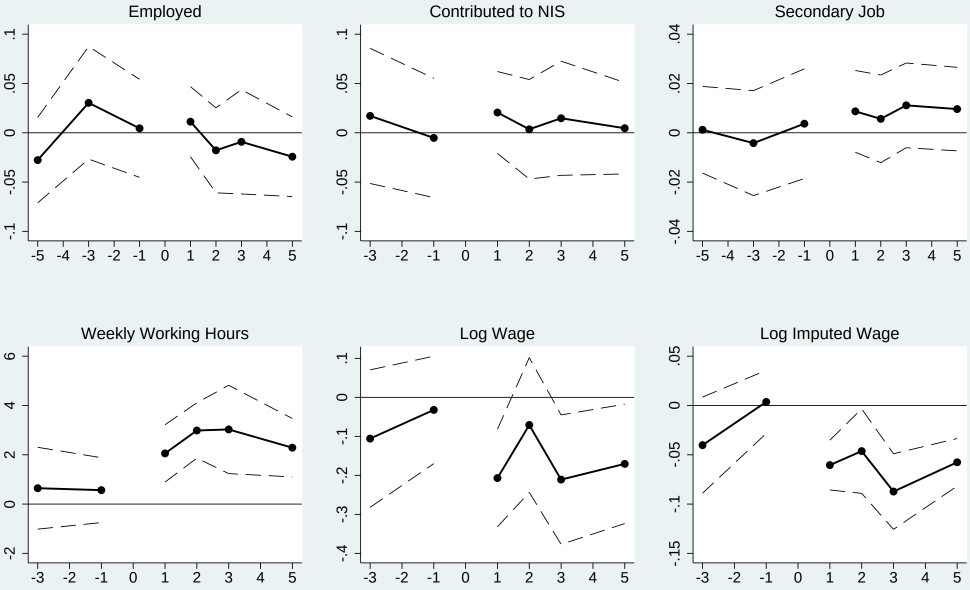


Figure 3: Trends for Main Outcomes, Uninsured vs. Insured



The X-axis is the year relative to 2007. Trends for uninsured persons shown in solid lines.
Trends for insured persons shown in dashed lines.

Figure 4: Event Study Estimates on Labor Market Outcomes



Event study estimated coefficients in solid lines. 90 percent confidence intervals in dashed lines.
The X-axis is the year relative to 2007.

Table 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 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: 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 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 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 6: Event Study Estimates

	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)
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 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 8: Heterogenous Effects by Age, Gender, and Parental Status

	Illness	Illness with Lost Days	ADLs	Employed	Contributed to NIS	Secondary Job	Working Hours
	(1)	(2)	(3)	(4)	(5)	(6)	(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.

Appendix 1: Jamaica Labor Force Survey and Jamaica Survey of Living Conditions Design

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 A1). This rotating panel scheme with the same dwellings lasts until the master sample is revised usually every three to four years.

Table A1: LFS 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 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
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 Table 1: Event Study Estimates by Age

	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	0.06*	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.

Appendix Table 2: 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. Males			
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. Females			
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.

Appendix Table 3: 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. Childless Adults			
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. Adults Living with Minors			
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.