Good Policy Gone Bad? Health Insurance, Premium Changes and Labour Supply in Rwanda

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Abstract

This paper is the first empirical analysis to assess the effect of health insurance premium changes on labour markets in the context of a developing country. In 2011, the government of Rwanda implemented a health insurance premium policy change that increased insurance premiums for non-poor individuals by 200% while providing poor households with waivers. We apply difference-in-differences with matching on national representative cross-sectional data to estimate the effect of health insurance enrolment and premium changes on labour supply. The most surprising result is that both premium increases and waivers reduce labour supply. We reconcile this finding by providing a theoretical model that allows for endogenous household responses due to changes in the health insurance premium via income thresholds. Our model implies that our findings are likely driven by manipulation of the community-based targeting method, highlighting the necessity to reassess the efficacy of related targeting methods in low-income countries.

Keywords: Health Insurance, Premium Changes, Labour Supply, Rwanda
JEL Codes: I13, I15, I18, J01

1 Introduction

The relationship between health, health insurance and labour markets is central to the study of human capital (Currie & Madrian, 1999), as well as for questions related to poverty reduction and social welfare. Poor health negatively affects labour market participation (Weil, 2007) and vice versa; healthier individuals have the ability to work longer, miss fewer days off from work and are subsequently more likely to be socioeconomically better off (Gruber & Madrian, 2004).

In this regard, the provision of health insurance has several crucial functions. First, it provides individuals financial protection in the case of inability to work. Second, access to health insurance covers healthcare-related costs of sickness through channels that do not infringe on household budgets. It, thus, enables savings and the ability to allocate more resources (including time) to more economically productive ventures (Currie & Madrian, 1999; Gruber, 2000; Gruber & Yelowitz, 1999). This further implies that individuals with access to health insurance tend to have higher labour market participation. However, while these effects of health insurance provision on labour market outcomes are broadly documented in high-income countries, the implications for low-income countries are less well understood. Moreover, much less is known about labour market responses to changes in the cost of health insurance (premiums) in low-income countries.

This paper provides the first empirical analysis to assess the effects of health insurance enrolment and changes in health insurance premiums on labour markets in the context of a developing country. In doing so, we provide novel insights on the effects of (i) health insurance provision and (ii) premium changes. We find that acquiring health insurance decreases labour supply. We further show that individuals receiving premium waivers, as well as those individuals subject to premium increases, both respond by reducing total labour supply by almost equivalent margins. This result is mainly driven by wage-related non-agricultural hours. Viewed through the lens of our theoretical model, these changes are likely driven by different incentives related to the income effect of premium changes and manipulation of the community-based targeting method.

Empirically, we study a policy change in Rwanda. Rwanda is particularly suitable for this exercise as it is one of the few countries in Africa that have a national health insurance programme of substantial coverage for about 81% of the population (Chemouni, 2018). However, a substantial proportion remains uninsured, enabling us to conduct a with and without analysis over time. We further utilise a 2011 policy change that sought to raise more resources for the scheme as well as enable more equitable cross-subsidisation. This was implemented by providing premium waivers to households categorised as poor and imposing a premiums increase (200 - 700% increase) on households that were classified as non-poor. We then use the Integrated Households Living Conditions Surveys (EICV) and combine a difference-in-differences strategy with weighting to estimate causal effects.

We develop a theoretical model that builds on bunching theories (Saez, 2010; Shi, 2016), which allows for endogenous household responses due to changes in the health insurance premium via income thresholds. The choice of income (socioeconomic status) thresholds is motivated by the fact that many developing countries implement a community-based poverty ranking to identify beneficiaries for a wide range of social programmes, including health insurance schemes (Ezeanya, 2015). Importantly, the eligibility categorisation process makes it even more profound that negative incentives to work can suffice due to the benefits of a 'poor' categorisation. Being categorised as poor not only provides individuals with a health insurance premium waiver but also makes them eligible for a wide range of social protection interventions such as financial services, public works programmes and scholarships. This kind of behavioural response regarding the individual's welfare status to benefit from social programmes is analogous to income manipulation in the *Affirmative Care* subsidies interventions in the United States (Shi, 2016). Similarly, our theoretical implications relate to income tax bunching along the intensive margin of income tax thresholds, when individuals are faced with a risk of paying higher taxes (Saez, 2010).

Our estimations show that acquiring health insurance reduced total hours worked by about 4%. For non-agricultural wage activities, labour supply reduced by 27% of the baseline mean. Regarding premium changes, we find that premium increases led to a 13% reduction in non-agricultural hours and 34% for non-agricultural wage activities. Similarly, Premium waivers have a large and significant effect, where we observe a 7% and a 22% reduction in non-agricultural and (non-agricultural) wage activities respectively. These results, thus, have significant implications for our understanding of how individuals shift labour supply in a context where health insurance premiums can pose a significant cost for households. Moreover, our theory helps us to reconcile what appears to be intriguing in our findings -

why individuals reduce labour supply both for premium increases and waivers. Our analysis therefore highlights that a revisiting of the community-based targeting is worthwhile in addressing labour market participation in a developing country context.

Related Literature. The relationship between health insurance and labour markets has been widely studied. However, previous studies are predominantly concerned with analysing high-income countries, with mixed findings.[1] Reviewing the literature, two major reasons for this imbalance in the distribution of empirical work become apparent. First, the arrangement of health insurance markets in the United States compared to other (both high and low income) countries (Ellis, Chen, & Luscombe, 2014; Feng & Zhao, 2018) provides rich variations for empirical analysis. More specifically, the United States do not have universal health insurance and differences in state policies, as well as employer-provided insurance, provide multiple variations of interest to empirical studies. On the contrary, many other high-income countries have universal insurance programmes with progressive income-based premiums or tax-based health services. In these countries, there is limited variation in prices and policies that could potentially affect individuals or employers.[2]

Second, many developing countries generally do not have substantial health insurance programmes that might enable larger-scale studies on labour market outcomes.[3] While some Latin American and several Asian middle-income countries have undergone health reforms, introducing universal health coverage policies in the last two decades, literature on how it has affected labour markets is still scarce. A recent systematic review of health insurance and labour markets found that 75 percent of the studies were from the United States and not study from a low-income country was included (Le, Groot, Tomini, & Tomini, 2019a). Nonetheless, we highlight the small body of work available.

[1] Most empirical investigations relate to the United States (Colman, Dave, & Lenhart, 2019).

[2] Overall, evidence from US studies suggests that increases in health insurance premiums can negatively affect labour market outcomes by increasing the risk of job loss, especially when employers are mandated to provide insurance to near-minimum wage workers (Baicker & Levy, 2008). Attaining health insurance for previously uninsured individuals can reduce the probability of employment (Baicker & Chandra, 2005, 2006; Ciccarelli, 2020; Dague, 2014; Peng, Guo, & Meyerhoefer, 2020), lead more individuals into part-time employment (Baicker & Chandra, 2006), negatively affect the number of hours worked (Baicker & Chandra, 2005, 2006), and overall wages might decline (Baicker & Chandra, 2006; Blewett, Graven, Ziegenfuss, & Davern, 2009; Heim, Lurie, & Simon, 2018; Kolstad & Kowalski, 2016; Qin & Chernew, 2014).

[3] The majority of insurance programmes in low and middle-income countries are social health insurance programmes, often targeting government civil servants who are a small proportion of the population. It is, therefore, common that national surveys do not even attempt to assess health insurance enrolment.

A first observation is that following on insurance reforms in China and Taiwan from the late 1990s to mid 2000s, studies from developing countries are mainly from these regions. These studies are equally inclusive, with divergent results and therefore provide more space for further research. (Si, 2021) found that overall, adoption of health insurance had not significant effects on labour force participation though there was evidence of more employment mobility. However, Shen, Parker, Brown, and Fang (2017) found a positive effect on hours spent in both farm and off-farm activities, especially for male workers. But Luo and Escalante (2020) observed that rural residents reduced the probability of working in the agricultural sector and increase in working hours for those who remained in the sector increased only marginally significant. Focusing on women, Liao (2011) found that health insurance introduction led to 30-40 percent reduction labour force participation especially those who were married and had younger children. Assessing the effect of a staggered roll-out of universal health coverage in Thailand, Wagstaff and Moreno-Serra (2009) found an overall increase in employment especially for married women particularly in the informal sector, but a decrease in formal employment among married men. Le, Groot, Tomini, and Tomini (2019b) found that accessing insurance reduced hours worked by up to six while the probability of employment was also reduced by 2.8 percent. Two studies in Pakistan study the effect of health insurance on child labour (Frölich & Landmann, 2018; Landmann & Frölich, 2015). In both studies, the authors find that extension of health insurance reduced child labour, in form of hours worked as a child and working in hazardous jobs.

Elsewhere in Latin America, in a policy change that extended health coverage to dependent children of private sector workers in Uruguay, Bergolo and Cruces (2014) found that the extending social insurance led to a 5 percent increase in pre-reform employment levels though small firms (that were required to pay employee premiums) increase under-reporting of employee salaries by 25 percent. del Valle (2021) has also recently shown that the expansion of health insurance among the informal sector in Mexico led to an increase in labour supply, especially by enabling previously unemployed women to join the informal sector hence increasing self-employment rates. Finally, Molina-Vera (2021) assesses the employer insurance reform in Ecuador in 2009-2010 and found that extending employer health insurance coverage to families increased the probability of entering formal employment, especially individuals with children. The only study in Africa is Garcia-Mandico, Reichert, and Strupat (2021) conducted in Ghana, who exploited a staggered roll-out of the

Ghana National Health Insurance Scheme and applied a regression discontinuity strategy to study insurance enrolment effects. They found a significant negative effect on the labour supply of healthy adults and a negative trend in earnings.

Our work not only contributes to these few studies but is also the first, to the best of our knowledge, to assess the effect premium policy changes have had on predominantly rural labour markets in a low-income country.

The rest of the paper is organised as follows. Section 2 outlines the evolution of health insurance in Rwanda and elaborates on the 2011 premium policy change. Section 3 presents the theoretical framework and identification. Section 4 describes the data, and in Section 5 we provide the results, followed by a discussion in Section 6. Section 7 concludes.

2 INSTITUTIONAL CONTEXT

2.1 Health insurance in Rwanda

Rwanda is one of the few countries in Africa with a national health insurance programme of substantial coverage. In 1998, Rwanda piloted the Community-Based Health Insurance (CBHI) programme in three districts.[4] Initial results encouraged a government-backed scaling up such that by 2004, 1.7 million individuals (21% of the population) were enrolled (Diop & Butera, 2005), and 85% of the population was enrolled by 2008 (Lu et al., 2012). As of 2016, 74 per cent of the population was covered (NISR, 2018b). Health insurance roll-out leveraged not only the existing culture of mutual support but also a strong political will (Chemouni, 2018) and donor support through premium subsidisation and start-up investments (Kalk, Groos, Karasi, & Girrbach, 2010; Logie, Rowson, & Ndagije, 2008).

Despite such growth, three issues remained of concern. First, the structure of the premium was flat such that both poor and rich households paid the same (Schmidt, Mayindo, & Kalk, 2006). This particular premium structure limited equity and, even more so, disincentivised a large proportion of poor households from enrolling in the scheme. Co-payments of 10% of medical bills remained prohibitive for many poor individuals (Kalk et al., 2010). Secondly, even with low premiums, the funding base was too small to enable the provision of a wide range of essential services. Due to the above two reasons, financial

[4] Throughout this paper, we simultaneously use the terms community-based health insurance and health insurance to imply the Rwandan community-based health insurance programme

sustainability of the scheme would be unrealistic (Chemouni, 2018). Maintaining a standard premium for all citizens likely excluded the poor; limited resources did not provide for a wider set of necessary health services, and donor resources would not be continuously guaranteed. The government, therefore, passed laws compelling mandatory enrolment (Government of Rwanda, 2016). Nonetheless, these mandatory enrolment laws also failed to raise the resources needed.

2.2 *Ubudehe* and the 2011 premiums policy change

In July 2011, to incentivise poor households' enrolment and promote equity through cross-subsidisation, the Government of Rwanda passed a new premiums policy (MoH, 2012; Vogel, 2011). As opposed to flat premiums for all individuals levied from when the insurance programme was initiated to 2010, the new policy reform introduced a progressive premium structure allocating higher premiums to non-poor households and providing waivers to poor households. But the classification of poor and non-poor was not based on means-tested methods. Instead, it was based on a community-based poverty ranking process known as *Ubudehe*. *Ubudehe* is a traditional Rwandan cultural practice of helping each other. Essentially, communities identified the most vulnerable among them and supported them in times of need (Rutikanga, 2019). While the practice is said to have existed in local communities for a long time, the post-1994 era has seen a formalisation of the process, government uptake and inclusion in policy-making. The current form of *Ubudehe* is in form of a participatory community poverty ranking process in which households use a pre-defined methodology to rank households in categories according to their poverty levels. Accordingly, the categorisation process generates a ranking of households ranging from the poorest to the richest. The first major nationwide categorisation process was conducted in 2001 and subsequent processes have been repeated every 2-3 years. In earlier years (2000-2010), the process produced six categories corresponding to Category 1= households in destitution (abject poor); Category 2 - the very poor; Category 3 - the poor; Category 4 - resourceful poor; Category 4, Category 5- the food rich and Category 6 - the money rich (Sabates-Wheeler, Yates, Wylde, & Gatsinzi, 2015). A 2013/14 methodology change reduced the categories to four, ranging from category 1 (poorest) to 4 the richest (Dushimimana, 2019; MINALOC, 2016). It is important to note that these poverty classifications are used for a wide range of government social protection programmes such as cash transfers (Ezeanya-

Esiobu, 2017; Habimana, Haughton, Nkurunziza, & Haughton, 2021; Nirere, 2022). Moreover, the government further encourages other development organisations to use the targeting method for their programming. Hence most development interventions in Rwanda largely adopt this method. The process has been heralded as one of the key ' homegrown' initiatives integrated into Rwanda's development policies (Hasselskog, 2018; Hasselskog & Schierenbeck, 2015).

Based on this categorisation process, the policy change had two implications for health insurance premium structures: (1) a premium increase for households identified as non-poor households and (2) a premium waiver for households identified as poor. Table 1 below shows these implications.

[Table 1 about here.]

Essentially, the policy change created two treatments - the premium waiver treatment (treatment 1) and the premium waiver treatment (treatment 2). Individuals who remained uninsured compose the control group. The implications of these premium changes on households' welfare are not trivial. With a mean household size of the bottom two quintiles equal to 4.1 persons and a pre-reform consumption poverty line of Rwanda Francs 118,000 per year (NISR, 2012), post-reform premiums increased from 3.5% of the national poverty line to 10.4%. For a majority of households below the poverty line, such premiums would constitute a large proportion of their income. If a household would be classified as poor in the *Ubudehe* categories, it would be receiving an indirect transfer (unspent incomes) of the same margin.

3 METHODOLOGY

3.1 Theoretical framework

We first outline a microeconomic model that is the basis for the analysis of the effects of changes in the health insurance premiums on labour supply (and wages). The model specification is similar to the one developed by Shi (2016), who extend Saez (2010) to allow for endogenous household responses due to changes in the health insurance premium. While Shi (2016) considers a health insurance subsidy, we take both a subsidy and a penalty into consideration.

Consider a population of individuals i that are heterogeneous with respect to their ability a_i, which is independently distributed and drawn from a distribution $G_a(a)$. An individual's utility function $v_i(I_i, z_i)$ consists of Income I_i and the labour effort z_i. The latter may relate to the hours worked or any other labour supply related effort an individual has to exert. Following Saez (2010), an individual's utility function is represented by a quasi-linear and an iso-elastic utility function of the form

$$v_i(I_i, z_i) = I_i - \frac{a_i}{1 + 1/\epsilon}\left(\frac{z_i}{a_i}\right)^{1+1/\epsilon} \tag{1}$$

where ϵ is the labour supply elasticity, which is constant due to the iso-elasticity assumption. Given the individual's ability a_i, he chooses labour supply z_i to maximise utility u_i. The latter term, hence, captures the disutility from labour, which depends on both hours worked z_i and ability a_i. The equation implies that individuals face a trade-off between income and leisure since more hours worked (z_i) increase income, while implying a negative utility from exerting effort.

The individual's income is constituted by two sources: wage income W_i earned through labour supply [5] and a transfer T_i, which represents a subsidy or penalty related to the health insurance premium. Income is then given by

$$I_i = W_i + \iota T_i \tag{2}$$

where $\iota \in (0, 1)$ is an indicator function for whether an individual has enrolled in the health insurance scheme. The expected wage income is assumed to be proportional to labour with wage rate w:

$$W_i = wz_i + e_i \tag{3}$$

where e_i is an idiosyncratic shock to the wage and is independently distributed and drawn from a distribution $G_e(e)$.

[5] Note that in a subsistence economy, wage income can also be represented by the value of subsistence production.

A subsidy related to the health insurance premium s_i is granted to the individual if his wage income W_i lies below an income threshold I^*. This case captures the poorest households that would be classified in Category 1 according to the *Ubudehe* categorisation ranking. If the individual's wage income lies above the income threshold, he will receive a penalty of t_i to his health insurance premium. Here, we only consider one insurance category for the richer individuals. This simplifies the presentation without affecting the substance of the policy reform. The insurance-related transfer can then be written as

$$T_i \equiv \begin{cases} s_i & \text{if} \quad \iota = 1 \quad \text{and} \quad W_i \leq I^* \\ -t_i & \text{if} \quad \iota = 1 \quad \text{and} \quad W_i > I^* \\ 0 & \text{if} \quad \iota = 0. \end{cases} \tag{4}$$

Hence, an individual enrolled in the health insurance scheme faces two possible outcomes. We denote the probability of falling below the income threshold I^* as $P(W \leq I^*)$. The expected utility under enrolment can then be written as

$$Eu_i(z_i \,|\, \iota = 1) = wz_i + e_i + [P(W \leq I^*)s_i + (1 - P(W \leq I^*))t_i] - \frac{a_i}{1 + 1/\epsilon}\left(\frac{z_i}{a_i}\right)^{1+1/\epsilon} \tag{5}$$

where $wz_i + e_i$ is the expected wage income, the last term is the disutility from labour, and the term in the middle captures the expected utility from receiving a transfer.[6] The equation further implies that the individual chooses labour supply before learning about the shock realisation e_i and, hence, cannot know with certainty whether the insurance penalty or subsidy applies.

Equation (5) highlights how a health insurance policy can affect an individual's labour supply decision, given that he had been enrolled in the health insurance scheme pre-reform. Since pre-reform individuals neither received a penalty nor a waver, i.e. $s_i = t_i = 0$, the FOC of the expected utility with respect to labour supply is given by

$$w = \left(\frac{z_i}{a_i}\right)^{1/\epsilon}. \tag{6}$$

[6] An individual not covered under the health insurance scheme receives his wage income (3) with certainty.

From this equation, and assuming that the wage rate is constant $w = 1$, it follows that labour supply pre-reform is equal to the ability of the individual, i.e. $z_i = a_i$, and can be interpreted as potential earnings in the absence of transfers.

Post-reform, the FOC of the expected utility with respect to labour supply is given by

$$w = \left(\frac{z_i}{a_i}\right)^{1/\epsilon} - \left[\frac{\partial P(W \leq I^*)}{\partial z_i} s_i + \left(1 - \frac{\partial P(W \leq I^*)}{\partial z_i}\right) t_i\right] \tag{7}$$

which highlights that an individual will take into consideration the effect his labour supply decision has on income and thus on the probability of falling below the income threshold I^*. In other words, in order to remain below the income threshold, an individual may choose to lower his labour supply z_i. With reference to the original framework by Saez (2010), this is an important mechanism that ought to be considered in the context of the health insurance policy reform. More specifically, the tax evasion model by Saez (2010) is motivated by the empirical evidence of bunching around the threshold of the first income tax bracket where tax liability starts. The effect of changes in the health insurance premium along the intensive margin may come along with a similar behavioural response due to the targeting mechanism employed by the government. Hence, our analysis intends to capture this possibility.

Furthermore, we note that in our model, individuals are assumed to understand all consequences of labour supply and insurance choices. The behavioural health economics literature provides evidence that individuals may make mistakes when choosing/evaluating health insurances (see e.g. Chandra, Handel, and Schwartzstein (2019)). In Section 5, we further discuss the implications regarding consumer-choice mistakes in health-care utilisation.

3.2 Identification and empirical strategy

In Section 2.2, we reveal that the policy change introduced two premium-related changes, namely (1) a premium increase to individuals categorised as non-poor and (2) premium waivers to individuals categorised as poor. The poverty categorisation process is based on the *Ubudehe* community-based poverty assessment and categorisation process. To streamline the targeting of the poor in an expanding social protection portfolio, the government sought to expand the scope and use of the socioeconomic profiling of households called the *Ubudehe* categories. In 2008, using the results of the 2007/08 categorisation process, the government set up a national database comprising all households in the country with their respective poverty category. From here, it would be easy, cost-effective and efficient to target and monitor graduation. However, while the government of Rwanda sought an efficient way of managing the target process of their social protection programmes, the community-based targeting methodology used was and would prove to be flawed, effectively creating a natural experiment of an almost random or at least non-systematic allocation of treatments to individuals.[7]. We leverage this near-randomness to assess the effect of each of the two policy change dimensions on labour supply.

There are several reasons why the *Ubudehe* classification became problematic. First, many individuals in the communities realised that there would be extensive benefits from being classified as poor. This was because the process transformed from a poverty mapping tool to more of a resource allocation too (Gaynor, 2014). To some extent, the perfect practice of classification exercises relies on the naivety of households in providing correct information and abstracts away from individual incentives and manipulation. But as studies have found, this is rarely the case (Hasselskog, 2018; Williams, Nzahabwanayo, Lavers, & Ndushabandi, 2020).

Secondly, local government and lower administrative officers continued to express dissatisfaction with the methodology provided to guide communities in the ranking exercises. On the one hand, local leaders suggested that each region, district, and village were distinct for a blanket methodology to be useful (Williams et al., 2020). On the other hand, it has been recorded that due to poverty-reduction performance targets set for local governments, local government officers continued to experience pressure to meet set targets. Qualitative research has therefore shown that many local government officials were likely to actively mistargeted by allocating *Ubudehe*-related cash transfers or financial inclusion services to non-poor households which had the highest potential for showing positive returns and graduation from poverty (Hasselskog, 2018).

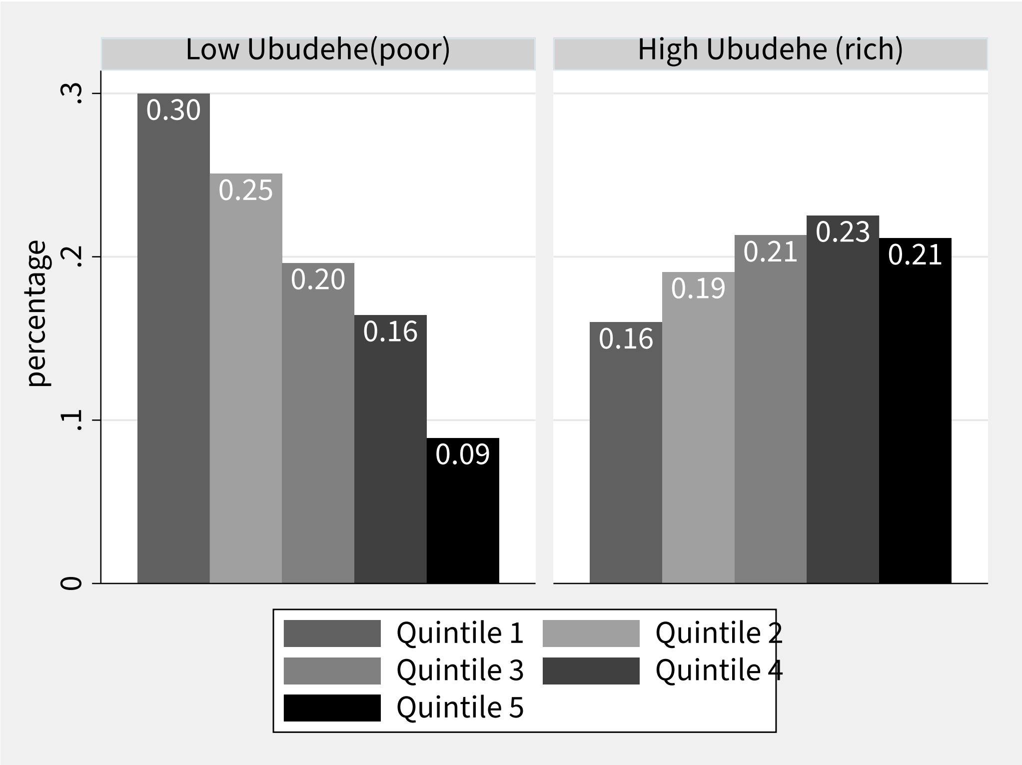
The above reasons provided the grounds that targeting errors were inherent though not random. Sabates-Wheeler et al. (2015) and Kidd and Kabare (2019) showed that the correlation between consumption poverty and *Ubudehe* categories was very low, suggesting mistargeted related exclusion errors of more than 60%. Consequently, many

[7]Extended work on the negative repercussions of community-based targeting has been discussed by many researchers including Alatas, Banerjee, Hanna, Olken, and Tobias (2012); Houssou, Asante-Addo, Andam, and Ragasa (2019); Premand and Schnitzer (2020); Schnitzer (2019); Stoeffler, Fontshi, and Lungela (2020) and Hillebrecht, Klonner, A Pacere, and Souares (2020) among others

poor households would have experienced a large shock on their household budgets by being categorised as non-poor while rich households categorised as poor would have experienced large returns in saved incomes (would be premiums) and other services.

To test the presence of this mistargeting in our data, we descriptively show how a large proportion of poor individuals as per the Ubudehe categorisation were instead in the highest consumption quintiles. Figure 1 shows a classification of poverty by Ubudehe and means-tested consumption. We observe that more than a quarter of high consumption households (25.7 %) were classified as poor. On the right side of Figure 1 we show that 33.8% of households classified in higher Ubudehe categories were instead of a very low consumption level.

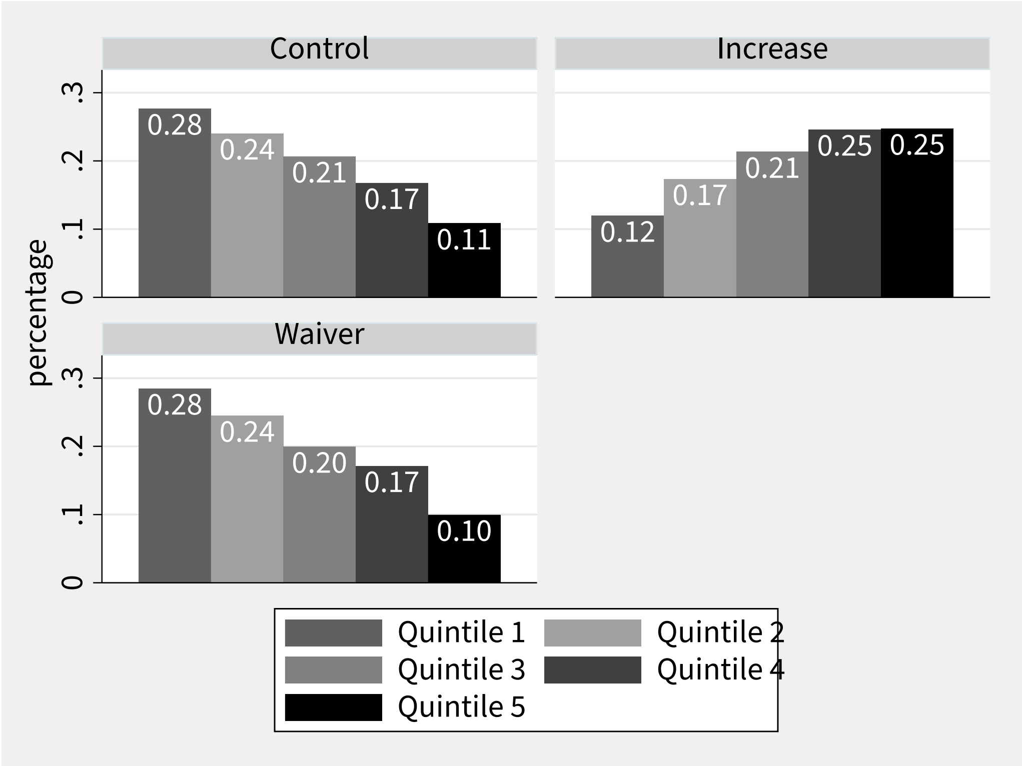
Figure 1: Consumption quintiles over Ubudehe categories



Observing the distribution according to the policy change (treatments), we observe a similar pattern. About 29.3% of households which experienced a premium increase were of the poorest quintiles. On the other hand, about 27% of households classified as poor and hence, receiving premium waiver were of the highest consumption quintiles.

While targeting imperfections would have proven challenging and unfavourable for policy making, they form a key part of our identification for causal estimations. Hypothetically, targeting imperfections provides a quasi-random scenario of treatment

Figure 2: Consumption quintiles over treatment groups



allocation, for which effects observed should point more to a targeting manipulation process similar to Shi (2016)'s income threshold incentives in the affirmative care act. These targeting imperfections are a source of endogeneity concerns.

3.3 Data

We use data from the Integrated Households Living Conditions Survey (EICV). The EICV surveys are nationally representative, cross-sectional surveys that have been collected by the National Institute of Statistics of Rwanda since 2001. The surveys are used by the government of Rwanda as the main poverty monitoring surveys and therefore cover a wide range of topics. For the purposes of this study, we use the 2010/11, 2013/14 and 2016/17 rounds, using the 2010 pre-reform survey as the baseline and the two follow-up surveys as the short-term and medium-term assessment. We exclude the 2001 and 2005 survey rounds for two reasons. First, the government of Rwanda had not started applying the *Ubudehe* targeting method across social protection programmes. For this reason, data for *Ubudehe* can only be traced to the surveys until 2008/09 - the rankings used in our baseline. Secondly, only the 2010/11 survey is consistent with the later rounds in measuring key outcomes, including labour force participation and labour supply.

The sampling of the EICV surveys is based on the National Population and Housing Census (NPHC) sampling frames. The 2010/11 survey was based on the 2002 NPHC sampling frame while the later surveys were based on the 2012 NPHC sampling frame. The surveys employed a stratified multi-stage sampling criteria, stratifying enumeration areas by rural and urban. Enumeration areas were further identified by sector, cell and village codes [8]. In 27 of the 30 districts (non-Kigali city), 30 enumeration areas were selected and 12 households were interviewed in each cluster. Kigali City is composed of 3 districts. In each of the three districts, 30 sample enumeration areas were selected, and 9 households were selected from a cluster. In each of the survey rounds, data collection took 12 months (October to October) in order to account for seasonality in consumption, agricultural activities and overall income. In all the survey rounds, response rates above 95% were registered, with the 2012/14 and 2016/17 reaching 99% and 100% respectively. In each of the rounds, about 14,000 households were surveyed, capturing between 45,000 and 46,000 individuals per survey. Our key outcomes are based on the jobs and employment module that records employable activities and that an individual was involved in in the last 7 days.

We take several steps to construct our sample. Our sample is limited to individuals, who are expected to be in active labour markets. Therefore, we drop individuals who were in full-time education in the last 12 months and those, above 64 or below 14 years. To avoid a possible measurement error on the upper tail (in the form of extreme outliers), we further winsorise reported weekly hours in each measured category to a maximum of 120 hours. In all the survey rounds, a small proportion of households do not know their *Ubudehe* status, which determines the treatment. Overall, this treatment variable was missing in 4.6% of the households who were also excluded from analysis[9].

[8] Sectors are equivalent to sub-district administrative areas while Cells and Villages are lower-level administrative areas created after the 2006 re-demarcation of with-in-country administrative units.

[9] Checking whether there were any systematic differences between households whose treatment variable was missing and those whose it was missing, we found that generally richer households had missing Ubudehe category. Individuals in these households were more likely to be (i) from quintile 5 than the lower quintiles, (ii) from households with smaller sizes, (iii) have fewer livestock units, (iv) fewer land parcels and (v) were more likely from urban areas. Furthermore, they were also younger and more likely to have internet

A key issue to note is that the 2010 survey did not contain the *Ubudehe* status variable, a variable that identified treatment status pre-reform. Previously, Sabates-Wheeler et al. (2015) conducted a name-by-village matching exercise that linked the 2010 survey with the national *Ubudehe* database as of 2010. Seventy-six per cent of the 2010 survey was perfectly matched, thus creating our complete pre-reform data.[10] Our final sample, shown in Table 2, therefore, comprises 76,309 individuals from 35,508 households.

[Table 2 about here.]

We use a large set of observed individual and household characteristics in our analysis. Among the individual characteristics, we include gender (1 if male), age, presented in six bins: less or equal to 20 years, 21-30, 31-40, 41-50, 51-60 and 61 or more years, education level, ownership of a savings account and membership in community social support group - tontine. Next, we include household-level controls. These include total household size and the total number of workers in the household, dummies for household loans in the last 12 months, access to the internet, access to information (radio or television) and access to electricity. We then include the number of agricultural land parcels a household owned and total livestock units. We are cognizant of the fact that this particular targeting method is/was used by the government in targeting other social protection programmes. This may imply that our results could be driven by other programmes other than health insurance changes. To mitigate this threat, we controlled for other social protection programmes in the flagship Vision 2020 Umurenge Programme (VUP) which cash transfers, public works programmes and financial inclusion instruments. The combined participation in these programmes was 14% in 2010, 12.2% in 2013 and 11.4% in 2016. Health insurance is therefore by far the largest programmme that uses this targeting method and, therefore, the one that is most likely to drive the behavioural changes we observe in our analysis. We compute tropical livestock units using the International Livestock Research Institute classification of tropical livestock units (Njuki et al., 2011). Using the household food and non-food expenditure data, the EICV datasets contain consumption quintiles, developed in a consistent manner using the same basket of goods and services across all three rounds (Fatima & Yoshida, 2018). Our estimations further include time to key services - markets and hospitals - as well as whether the household was in a rural or urban location. To account for seasonality and possible

[10] Sabates-Wheeler et al. (2015)'s matching exercise was commissioned by the National Institute of Statistics of Rwanda and its sharing was approved accordingly.

regional/district variation in labour markets, we include months and districts dummies in all our analysis. Given that the data was collected across the year, the distribution of observations by months was almost equivalent. Observing summary estimations of the differences between the treatments and the control confirms these concerns. Table A1 in the Appendix shows the sample descriptive statistics of individuals and household characteristics by treatment status.

The outcomes of interest are (i) total hours worked in the last seven days. We then separate total time into (ii) hours allocated to agricultural farm activities, and (iii) hours allocated to non-agricultural activities. A component of non-agriculture time allocation is allocated to (iv) wage employment activities. Table 3 shows the descriptive statistics of the outcomes categorised by insurance status. We find that in all the years under assessment, individuals in insurance were significantly different from those not in insurance in the labour allocation. Insured individuals in 2010 worked overall 0.77 hours (46 minutes) per week more than those not in insurance. Insured individuals also worked 1.3 hours more in non-agricultural activities but also worked about 1.36 hours less in wage-related activities. These differences persist in both significance and magnitude in the short and medium terms. In 2016/17, the difference in total hours worked was 1.3 hours. The difference in non-agricultural hours increased to 2 hours while the difference in wage employment hours more than doubled from 1.36 hours to 2.81 hours per week.

Table 4 shows the evolving differences across individuals exposed to different policy change effects. The first part of the table (2010/2011) is similar to Table 3 since the baseline represents a period when the policy change was not yet implemented. In the short term, we notice that people who receive a waiver work generally less than those who either got a premium increase or are not insured. Individuals with a premium waiver work about 2.3 hours less than those who are not insured (control) and about 4.4 hours less than those who got a premium increase. Regarding non-agricultural hours, individuals who have a waiver worked about 4.8 hours less than those with an increase and about 2.8 hours less than control. These results provide the first indication of the income effect of the premium waiver. However, individuals who received an increase in premiums worked less in wage activities than either waiver-receiving or control individuals. Similar to receiving insurance, these differences increase in both significance and magnitude in the medium term. In the medium term, individuals who got a premium increased worked overall 5 hours more than those who got a waiver and were also working 2.2 hours more than control individuals. Those who received a waiver worked 2.8 hours less than control individuals. Looking at the non-agricultural hours, the differences were almost similar in magnitude. Those receiving a waiver worked 2.5 hours and 5.3 hours less than control and premium increase respectively. Individuals with a premium increase worked 2.8 non-agricultural hours more than control. The trend in wage employment hours allocation was similar to short-term trends. Individuals with a premium increase generally worked fewer wage hours, working 2.7 hours fewer than control and about 0.6 hours fewer than those with a premium waiver. Individuals with a premium waiver work 2.1 hours fewer than control.

[Table 3 about here.]

[Table 4 about here.]

3.4 Empirical estimation

We estimate the extensive and intensive margin of the policy change on labour supply, i.e. (1) the effect of health insurance enrolment and (2) the effect of insurance premium changes. The analysis addresses the short and medium terms via a multi-valued difference-in-differences strategy (Cattaneo, 2010; Uysal, 2015). A threat to the estimation of causal effects is the non-random nature of selecting into each of the treatments. As we show earlier, targeting imperfections imply that we notice significant differences between each of the two treatments and the control. To remove these endogeneity concerns, we adopt a weighting estimator combined with the difference-in-differences in the form of a doubly-robust estimator over repeated cross-sections (Blundell & Costa Dias, 2009; Cattaneo, 2010; Fredriksson & de Oliveira, 2019; Ryan, Burgess Jr, & Dimick, 2015; Sant'Anna & Zhao, 2020). Our empirical assessment takes two dimensions. First, we are interested in studying the effect of insurance enrolment on labour supply. To estimate this, first, we use a logistic regression model to determine the probability of enrolling in health insurance. We then construct the inverse probability weights of the first-stage estimated probabilities and use them in a second-stage difference-in-difference regression. The key outcome regression is then given as:

$$Y_{ijtkm} = \tau^{ipw\_ps} + \beta_1 Insurance_{ijtkm} + \beta_2 Post_{it} + \beta_3 Post_{it} * Insurance_{ijktm} + \vec{X}_{ijt}\beta_4 + \vec{X_{ijt}} * Post_{it}\beta_5 + \mu_k + \varsigma_m + \varepsilon_{ijtkm}. \tag{8}$$

Where, Y_ijktm is the labour supply outcome for individual i in household j in district k, month m and t time t. $Insurance_{ijktm}$ is a dummy variable for having health insurance or not and $Post_{it}$ is a time indicator which takes the values 0 for 2010 (as the baseline), 1 for 2013 and 2 for 2016 as the short and medium terms respectively. $\vec{X}_{it}$ stands for individual and household controls as in Table A1 in the Online Appendix. To account for variation across space and time in labour supply, we include μ_k, representing district fixed effects, and ς_m are month fixed effects that capture unobserved determinants of labour allocation that are constant in a district and month of a year, respectively. ε_{ijktm} is the standard error term. Standard errors are clustered at the year by district level to account for possible exposure to local negative or positive shocks that might affect labour market conditions. In this regression, the main coefficient of interest is β_3 which is the coefficient for the interaction of time dummies and being insured. The propensity score of enrolment in insurance is computed from a first stage logistic regression model and τ^{ipw_ps} shows the computed inverse probability weight of the propensity score as per Hirano and Imbens (2001, 2004), which is included in the second stage OLS regressions. Since we have repeated cross-sectional data, an additional concern is mean reversion brought by group compositional changes. It is plausible that in each individual round of data, covariates can have differential effects on treatment selection as each new sample is drawn. Mean reversion biases estimates. To alleviate this concern, we adopt a trends-absorption strategy by including interactions of all controls with the time dummies. This strategy is efficient in accounting for any influence of covariates on treatment adoption (La Ferrara & Milazzo, 2017; Molina-Vera, 2021). The vector $\vec{X_{ijt}} * Post_{it}$ shows these controls. It is important to note that with cross-sectional data, we do not observe changes in specific units but rather groups of individuals with comparable conditions.

Secondly, we are interested in estimating the effect of premium changes applying to insured individuals on labour supply. In the case of insurance enrolment, participation and non-participation take only two values, as is the convention in many policy evaluations. However, to study the effect of insurance policy change in the current set-up, our treatment assignment takes multiple values. In our case, three values are observed, corresponding to premium increase, premium waiver and uninsured/control. This is the case of multivalued treatment effects (Cattaneo, 2010). In this case, instead of constructing the weights from a propensity score emanating from a binary treatment variable (as in Model 8, we construct weights from generalised propensity scores emanating from a multinomial logistic regression.[11]. The basic multinomial treatment model is then defined as

$$T_{ijtkm} = \vec{X}_{ijt}\beta_1 + \beta_2 Post_{it} + \vec{X_{ijt}} * Post_{it}\beta_3 + \mu_k + \varsigma_m + \varepsilon_{ijtkm}. \tag{9}$$

where the treatment variable is given as:

$$T_{ijkm} \equiv \begin{cases} 0 = \text{control} \\ 1 = \text{premium increase} \\ 2 = \text{premium waiver.} \end{cases} \tag{10}$$

In all the models, we use a uniform set of controls. Therefore, $Post_{it}$, $\vec{X}_{it}$ and $\vec{X_{ijt}} * Post_{it}$ are as defined in Model 8 above. The second stage outcome equation is a difference-in-differences model given as:

$$\begin{aligned} Y_{ijtkm} = \tau^{ipw\_gps} + \beta_1 Increase_{ijtkm} + \beta_2 Waiver_{ijtkm} + \beta_3 Post_{it} + \\ \beta_4 Post_{it} * Increase_{ijktm} + \beta_5 Post_{it} * Waiver_{ijtkm} + \\ + \vec{X}_{ijt}\beta_6 + \vec{X_{ijt}} * Post_{it}\beta_7 + \mu_k + \varsigma_m + \varepsilon_{ijtkm}. \end{aligned} \tag{11}$$

The outcomes in Model 11 are as defined in Model 8. Y_{ijtkm} is the labour supplied (hours in the past 7 days) of individual i in household j in time t in district k and month m. $Increase_{it}$ is an indicator for premium increase which is equivalent to 1 if an individual was in a higher *Ubudehe* category and also enrolled in CBHI and thus received a premium increase after 2012 and 0 otherwise. $Waiver_{ijktm}$ is an indicator for waiver treatment that takes the value of 1 if an individual was categorised as poor and was enrolled in CBHI and this received a premium waiver after 2012 and 0 otherwise. $Post_{it}$ are time dummies for 2013 (short term)

[11] Cattaneo (2010) suggests a multinomial logistic regression but an ordered logit model as in Uysal (2015) also works in an almost identical fashion

and 2016 (medium term). The term τ^{ipw_gps} accounts for the inverse probability weight from the generalised propensity score. The coefficients of interest are β_4 and β_5 which are the interaction terms of the premium increase and premium waiver treatments respectively.

4 RESULTS

4.1 Effect of health insurance

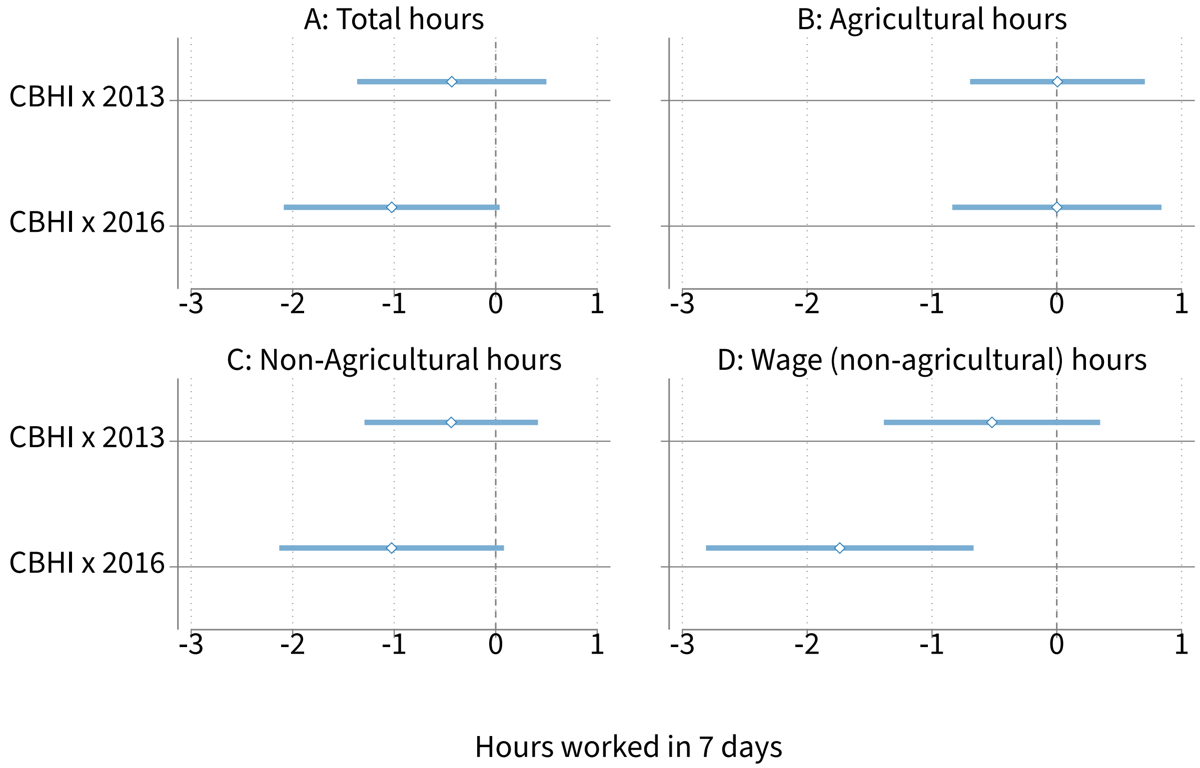
First, we look at the effect of health insurance on labour supply. The main outcomes are total hours worked on all activities in the last 7 days, hours worked in agricultural activities, hours worked in non-agricultural activities and hours worked on wage activities.[12] We implement the regression Model 8 including the full set of controls as shown in Table A1 in the Online Appendix, including month and district fixed effects. The results are presented in Figure 3, where The different sub-figures correspond to the specific specifications for each outcome variable.[13] We show in Figure A1 and Table A2 in the Online Appendix that the matching efficiency is achieved, evidenced by the density graphs of before and after weighting in each period.

We observe that between 2010 and 2013, acquiring health insurance did not change individual labour allocation by statistically significant margins. However, a medium-term assessment (2010 -2016) shows that individuals changed labour allocation by statistically significant margins. Overall time allocated to work in the previous seven days was reduced by about 1 hour, significant at 10% (Figure 3:A). Individuals reduced the non-agricultural labour supply by slightly over one hour, significant at 10% (Figure 3:C). A larger reduction of 1.74 hours was observed in the time allocated to non-agricultural wage activities (Figure 3:D). In terms of baseline mean, total time was reduced by 3.9% of the overall baseline mean. For wage activity hours, labour supply was reduced by 26.8% of the baseline mean and 23.7% of the baseline mean for control individuals.

[12] Note that hours worked on wage activities are a sub-set of non-agricultural activities.

[13] Table A3 in the Online Appendix provides the tabular format of these results.

Figure 3: Effect of insurance enrolment on labour supply



4.2 Sub-group effects

We are interested in two dimensions of heterogeneities; the gender and the location dimensions. To assess these heterogeneities, we implement Model 8 separately for the different sub-samples (i.e men and women separately for the gender assessment and rural and urban separately for the location assessment.

4.2.1 Effects across men and women

Table 5 shows results of the gender assessment. The key finding is that most of the effects accrue to men except for medium-term reductions in wage labour supply. First, we find that while overall total time reduced by just under one hour (Figure 4:A), men reduced the time by 1.8 hours in the medium term, increasing in both magnitude and significance. We do not observe a statistically significant effect in women regarding overall time worked.

[Table 5 about here.]

Secondly, we observe an increasingly negative effect on men in both the short and medium terms regarding non-agricultural time allocation. We find that men reduce non-agricultural time by about 1.13 hours in the short term and about 2.3 hours in the medium term while there is no statistically significant effect among women. Finally, regarding wage hours, we observe a trend similar to that of non-agricultural hours time allocation. Men reduce time to wage activities by 1.6 hours in the short term and increase to 2.2 hours in the medium term. Similarly, women show a statistically significant reduction of 1.3 hours of wage activities in the medium-term. These findings indeed lend weight to the income effect hypothesis, that gaining insurance likely reduced out-of-pocket health spending in general and in Rwanda specifically (Woldemichael, Gurara, & Shimeles, 2019). The would-be health expenditures are in turn not spent, hence increasing household welfare. Households are more likely to respond to this positive income shock by reducing the labour supply and possibly taking more leisure.

4.2.2 Effects across rural and urban regions

Rural and urban inhabitants were likely to have varying labour allocations given that the concentrations of their employment might be different (for instance, own farm agricultural employment versus wage or non-agricultural informal sector employment). Table 6 shows these differences.

[Table 6 about here.]

Results show that the effects are, by and large, concentrated in rural areas. In the short-term, there are no statistically significant effects observed apart from wage hours reduction in rural regions by about 40 minutes (0.667 hours), significant at a 10% level. In the medium term, individuals in rural areas reduce overall time by 1.13 hours, non-agricultural time by just over 1 hour and wage employment time by 2 hours individuals. We do not observe any effects accruing to urban residents in each of the four models.

4.3 Effects of the policy change on premiums

Figure 4 below presents the average treatment effects of the premium policy change on labour supply. The different sub-figures in 4, correspond to the different specifications for each outcome variable. Table A5 in the Online Appendix provides the complete set of results in tabular format. Furthermore, Figure A2 in the Online Appendix shows the matching efficiency across the two treatments and the control group in all periods. We find that both premium waivers and premium increases had a negative effect on labour supply, especially in the medium term (2010-2016). Starting with the effect of premium increases, we observe that the total labour supply in weekly hours was reduced by 1.1 hours, significant at 10% (Figure 4:A). Non-agricultural hours reduced by over 1.3 hours per week (Figure 4: C) and non-agricultural wage hours reduced by 2.2 hours (Figure 4:D). These were substantial reductions in labour allocation corresponding to 4.2% on baseline total hours, 12.7% of baseline non-agricultural hours and 33.6% of baseline (non-agricultural) wage hours.

Figure 4: Effects of premium changes on labour allocation

The effect of Premium waivers on labour supply is shown to have an even greater effect. Individuals who received premium waivers after the policy change reduced total labour supply by over 1.8 hours , significant at 5% level (Figure 4:A). Non-agricultural labour supply reduced by 2 hours (Figure 4:C) and non-agricultural wage labour supply reduced by close to 1.5 hours (Figure 4:D). Viewing these through the lens of baseline changes, overall labour supply reduced by 7%, non-agricultural labour reduced by 19% and wage labour in non-agricultural activities reduced by 22.4%.

4.4 Sub-group effects

4.4.1 Effects across men and women

We assess separately the effect of premiums changes on women and men by implementing Model 11 for men and women separately and show these results in Table 7 below. First, looking at the effect of premium increases, we find that men reduced overall work hours by a magnitude and significance level higher than women. Premium increases led to a reduction in the total labour supply of men by close to 1.7 hours in a week. Men's reduction in non-agricultural hours was even higher, at 2.5 hours in a week and wage hours reduced by close to 2.6 hours. These reductions were equivalent to 5.5%, 16.1% and 25.4% of baseline means. All these results accrue only in the medium-term.

[Table 7 about here.]

The effects of premium waivers were present, however, only in the medium term. We found that premium waivers generally had a larger effect on labour supply than premium increases. Overall total labour supply was reduced by 2.6 hours for males and was not significant for females. Agricultural time was reduced by slightly over 4.2 hours. Agricultural time was reduced by 1.2 hours for female individuals. These effects were equivalent to 8.5 per cent of total time worked for men at baseline and 27% of the time men allocated to non-agricultural activities in 2010 (baseline). Agricultural time reduction for women was equivalent to 7.3% of their baseline agricultural labour supply.

4.4.2 Effects across rural and urban regions

We then look at the effects across rural and urban regions in Table 8. The findings suggest that almost all observed effects happened in rural regions. The reduction in total hours of 1.3 hours was observed in rural regions, equivalent to about 5.3% of their baseline mean rural labour supply. We observed a reduction in non-agricultural labour supply in rural areas of 1.3 hours (5.1% of baseline mean). Regarding wage labour, we observe an increasing magnitude and significance level of wage labour between the short and medium terms. In the short term, rural labour supply for wage activities was reduced by 52 minutes (0.86 hours) and reduced by a further 2.4 hours in the medium term, equivalent to about 13.7% of the baseline mean.

[Table 8 about here.]

The effects of premium waivers were also more pronounced in rural than urban regions. Overall labour supply was reduced by 1.7 hours in rural regions (about 6.7% of baseline mean). Non-agricultural labour supply was reduced by a larger magnitude of 26.7% (2.15 hours) and wage labour reduced by an even larger magnitude of 40%, i.e. by 2 hours. These effects were observed in the medium-term.

4.5 Robustness

We perform robustness checks of the policy change results by testing fake treatment to control for further endogeneity concerns. This is akin to placebo tests which are widely used in applied studies (Gertler, Martinez, Premand, Rawlings, & Vermeersch, 2016; Sant'Anna & Zhao, 2020), including del Valle (2021), who assessed the effect of public health insurance on labour markets in Mexico. In this placebo regression, we assign treatment to other health insurance programmes which were not affected by the premium policy change. The other health insurance programmes include the military health insurance programme (MMI), RAMA insurance, employer insurance and any other insurance programme. Eighty-four per cent of these other insurance programmes are RAMA and MMI, which are public service insurance programmes that insure civil servants and military personnel respectively. In essence, the fake treatment is composed mainly of other government health insurance programmes that were not affected by the policy change.

To be confident in causal claims, we expect that either there is no statistically significant effect of the fake treatment or if a significant result is observed, it should be in the opposite direction of our point estimates. If a significant result is observed in a similar direction as our point estimates, we cannot rule out that the effects observed are caused by anything else other than the policy change. Results of our placebo tests are shown in Table A5 in the Online Appendix. We confirm no statistically significant effect of the fake treatment on labour supply in both the short and medium terms, underlining our main results as being driven by the policy change only.

5 DISCUSSION

5.1 Interpretation of empirical results

Using cross-sectional nationally representative household survey data from Rwanda, we apply difference-in-differences combined with matching to estimate the causal effects of health insurance enrolment and premium changes. We find evidence of an income effect after health insurance enrolment, with individuals reducing total labour supply and especially labour supply to wage activities. Our findings here contribute to the sparse evidence on health insurance and labour markets in low-income countries. Our results are in line with Garcia-Mandico et al. (2021), who find that acquisition of health insurance was associated with lower labour force participation in Ghana. In their study, Garcia-Mandico et al. (2021) suggest that such reductions in labour supply were evidence of the crowding-out effect of health insurance by reducing the propensity of household members extensively supplying their labour to cover health costs. Complementing their interpretations, our findings suggest that this is an income effect scenario in line with Nyman's Income Hypothesis (Nyman, 2001, 2008). The basic premise of the income effect hypothesis is that individuals derive more welfare benefits from health insurance than not being insured. Due to these welfare benefits, individuals might then change their socioeconomic behaviour by, for instance working less, taking on more leisure or even undertaking more risk behaviours in the form of moral hazard. This interpretation is also supported by two other studies assessing health insurance acquisition and labour supply in the context of child labour (Frölich & Landmann, 2018; Landmann & Frölich, 2015). In these studies, the authors find that after the acquisition of health insurance (though coupled with micro-finance services), households were likely to reduce child labour.

Next, we assess the effect of premium changes on labour supply. The 2011 premiums policy change provides a unique opportunity to conduct the first study assessing the effect of premium changes on labour markets in a low-income country. The policy change further utilised a plausibly flawed targeting methodology to select individuals who would either pay higher premiums or benefit from premium waivers. We find that individuals who received a premium increase were more likely to work less and most likely reduce labour hours allocated to wage activities. However, we also find some supportive - though weakly significant - evidence of waivers on reducing labour, mainly agricultural hours. While these findings appear intriguing at first, we propose a theoretical model that enables a realistic interpretation. Our model builds on the tax bunching literature (Saez, 2010) and Shi (2016)'s extension to health insurance.

Our theoretical model suggests that, on the one hand, given then easily manipulable eligibility criteria, individuals reduce their labour supply to qualify for premium waivers. This behavioural response is due to the targeting mechanism employed by the government in selecting households that receive a waiver. As in Shi (2016), we assumed that the process of community-based targeting regarding premium waivers and penalties was known and manipulable by individuals. This is not unique to our study. Indeed, many empirical studies assessing the efficacy of other community-targeting interventions in low-income countries suggest that this assumption is valid and the implications are empirically important (see e.g. Alatas et al. (2012); Hillebrecht et al. (2020); Houssou et al. (2019); Premand and Schnitzer (2020); Schnitzer (2019); Stoeffler et al. (2020)). In Rwanda, the Ubudehe community-based targeting process has been used by the government to identify beneficiaries for a wide range of social programmes, including school scholarships, cash transfers and public work programmes as well as health insurance waivers and step-wise premiums (Ezeanya, 2015). This behavioural response is therefore incentivised by not only the individual's welfare gain through health insurance premium waivers but also that the individual's household would stand a chance of benefiting from a range of other social protection and poverty-targeted programmes.

Though the Ubudehe process was heralded as a major 'home-grown' initiative (Ezeanya, 2015; Rutikanga, 2019), this well-intended policy ended with unintended negative results. Targeting poverty-reduction programmes using the *Ubudehe* categorisation started at the beginning of the 2010s. This also happens to be the decade where Rwanda's poverty reduction and growth in key sectors such as agriculture stalled (World Bank, 2020).

Our analysis here speaks directly to policy-making in Rwanda. Since the start of using this categorisation process to identify beneficiaries of various social programmes in 2011, the process has generated increasing debate elucidating on its weaknesses in capturing the actual poor (Hasselskog, 2018; Sabates-Wheeler et al., 2015). The categorisation process continues to undergo methodological updating with the hope of increasing its precision in correctly identifying the poor (Bizimungu, 2020; Dushimimana, 2019; MINALOC, 2016; Ntirenganya, 2020). But even methodological adjustments might not have been able to correct the negative effects. In 2020, the government of Rwanda announced an end to the use of the *Ubudehe* categories in the targeting of beneficiaries of all social programmes, including health insurance waivers and premiums, partly based on the knowledge policy makers have known that substantial manipulation was present (Bishumba, 2021; Mutanganshuro, 2020; Ntirenganya, 2020).

Our findings also reveal important heterogeneities in the effect of the policy change on different genders (men and women) and locations. Overall, men were more likely to reduce the labour supply than women. A recent study in Mexico del Valle (2021) found that acquiring health insurance reduced the likelihood of unemployment among women. Our study showed positive coefficients on non-agricultural labour allocation for women. However, these were not statistically significant. Nevertheless, acquisition of insurance or relaxing the burdens of insurance subscription for the poorest, especially among women, could spur more economic activities through greater engagement in non-agricultural and plausibly higher-return activities.

Lastly, we discuss some limitations of our analysis. One limitation lies in the nature of the data available. Cross-sectional data do not allow us to assess actual transitions of the same individuals before and after the policy change. This means that we are only able to assess the changes in individuals of similar characteristics and not the same individual. While this analysis would have strongly benefited from panel data, it was not available. We, thus, encourage researchers with access to the Rwandan panel data (NISR, 2018a) to conduct new and additional assessments on this policy change for more precise effects. Furthermore, in our theoretical model individuals are assumed to understand all consequences of labour supply and insurance choices perfectly. However, given that it is not easy to assess the the costs and benefits of insurance and to forecast the need for health care, individuals may make mistakes when choosing health insurances. The behavioural health-economics literature provides evidence on consumer mistakes in health-care utilisation and treatment choices (Chandra et al., 2019; Handel, 2013). The interaction between choice difficulties or biases and the aforementioned behavioural considerations with regard to labour supply, is likely to amplify the issues arising due to mistargeting.

6 Conclusion

This paper studies the effects of health insurance enrolment and premium changes on labour supply in Rwanda between 2010 and 2017. Rwanda provides a rare case of a low-income country with high coverage of health insurance. The 2011 premium policy change provides a unique opportunity to evaluate the effects on labour supply, relevant to other low-income countries.

We estimate effects through matched-difference-in-differences estimations by separating individuals who were exposed to higher insurance premiums and those who received premium waivers. We compare these individuals with those who remained uninsured throughout the period. A plausibly flawed targeting method provides us with a quasi-random scenario for which conditioning on observable characteristics of individuals provides causal estimates. We find that acquiring health insurance leads individuals to work significantly less. This result is possibly driven by the income effect of health insurance acquisition. We further find that an increase in premiums induced a decrease in labour supply especially non-agricultural wage-related labour supply for men and rural-based individuals. This reduction in labour supply was more likely driven by manipulation behaviour given the possible benefits of individuals being categorised as poor in a community-based targeting process. Premium waivers generally led individuals, especially men, to reduce labour supply. This finding is likely driven by the income effect of health insurance, as individuals' insurance costs were relaxed and hence retained unspent incomes for higher welfare.

Our theoretical model validates these postulations associated with income and manipulation effects. Our study is of important policy relevance as it reflects the government of Rwanda's increasing realisation of flawed targeting and the need to continuously revise targeting methodologies. The policy relevance of this study does not only inform low-income countries but relates also to high-income countries (see e.g. Shi (2016)). This study, thus, highlights the necessity for revisiting community-based targeting methods especially in addressing the unintended effects emanating from possible manipulation.

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Table 1: *Ubudehe* categories & CBHI premiums

Ubudehe	Premium group	Pre-reform premiums (RwF)	Post-reform premiums (RwF)	Post-reform Change
Poor (cat 1)	One	1,000	**0**	**Waiver**
Non-poor(cat 2 &3)	Two	1,000	3,000	**200 %**
Non-poor (cat 4)	Three	1,000	7,000	**600 %**

Source: (MINALOC, 2016) and (Kwibuka, 2017)

Table 2: Study sample

	2010	2013	2016
Comparison (Not insured)	7,557 (31.62%)	7,560 (28.88%)	6,769 (28.68%)
Treatment 1 (Premium Increase)	11,337 (47.44%)	13,322 (52.66%)	16,911 (62.38%)
Treatment 2 (Premium Waiver)	5,006 (20.95%)	4,818 (17.46%)	3,429 (12.65%)
Total Individuals	23,900	25,300	27,109
Households	11,090	11,754	12,664

Table 3: Differences between insured and non-insured individuals

	2010/2011			2013/2014			2016/17		
	(1)	(2)	t-test	(3)	(4)	t-test	(5)	(6)	t-test
Variable	No Insurance Mean/SE	Insurance Mean/SE	Difference (1)-(2)	No Insurance Mean/SE	Insurance Mean/SE	Difference (3)-(4)	No Insurance Mean/SE	Insurance Mean/SE	Difference (5)-(6)
Total Hours	26.745	27.510	-0.765***	28.060	29.034	-0.975***	28.203	29.504	-1.301***
	[0.871]	[0.724]		[0.634]	[0.660]		[0.735]	[0.925]	
Agric Hours	16.846	16.291	0.555	17.256	17.409	-0.153	17.158	16.451	0.706*
	[0.578]	[0.629]		[0.694]	[0.643]		[0.793]	[0.875]	
Non-Agri Hours	9.899	11.219	-1.320***	10.803	11.625	-0.822***	11.046	13.053	-2.007***
	[1.306]	[1.206]		[1.045]	[0.934]		[1.290]	[1.634]	
Wage Hours	7.658	6.302	1.356***	12.287	9.968	2.319***	14.657	11.848	2.809***
	[0.949]	[0.775]		[0.663]	[0.571]		[0.817]	[0.999]	
N	7234	15822		7226	17021		6387	19255	
Clusters	30	30		30	30		30	30	
F-stat			31.481***			42.187***			26.899***

The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. Standard errors are clustered at year by district. Covariates include months and districts dummies. Significance levels correspond with *** $p<0.01$ for 1 %, ** $p<0.05$ for 5% and * $p<0.1$ for 10%.

Table 4: Differences by policy change treatment status

	Baseline (2010/2011)			Short term (2013/2014)						Medium term (2016/2016)					
	(1) No Insurance Mean/SE	(2) Insured Mean/SE	t-test Diff (1)-(2)	(3) No Insurance Mean/SE	(4) Increase Mean/SE	(5) Waiver Mean/SE	t-test Diff (3)-(4)	t-test Diff (3)-(5)	t-test Diff (4)-(5)	(6) No Insurance Mean/SE	(7) Increase Mean/SE	(8) Waiver Mean/SE	t-test Diff (6)-(7)	t-test Diff (6)-(8)	t-test Diff (7)-(8)
Total Hours	25.604	26.645	-1.041***	26.820	28.950	24.564	-2.130***	2.256***	4.386***	26.612	28.775	23.766	-2.163***	2.846***	5.009***
	(0.754)	(0.605)		(0.585)	(0.569)	(0.764)				(0.629)	(0.735)	(0.555)			
Agric Hours	16.126	15.771	0.355	16.494	16.602	17.008	-0.108	-0.514	-0.406	16.189	15.528	15.801	0.662	0.388**	-0.274*
	(0.608)	(0.671)		(0.691)	(0.744)	(0.606)				(0.825)	(1.029)	(0.620)			
Non Agric Hours	9.478	10.874	-1.396***	10.326	12.348	7.555	-2.022***	2.770***	4.792***	10.422	13.247	7.964	-2.825***	2.458**	5.282***
	(1.216)	(1.124)		(0.982)	(0.964)	(0.635)				(1.170)	(1.596)	(0.726)			
Wage Hours	7.331	6.104	1.226***	11.744	9.327	10.278	2.417***	1.467***	-0.950**	13.830	11.116	11.708	2.714***	2.122**	-0.592***
	(0.883)	(0.724)		(0.621)	(0.597)	(0.526)				(0.726)	(0.979)	(0.481)			
N	7557	16343		7560	13322	4418				6769	16911	3429			
Clusters	30	30		30	30	30				30	30	30			
F-stat			36.703***				38.726***	12.322***	45.945***				26.875***	5.962***	19.825***

The value displayed for t-tests are the differences in the means across the groups. The value displayed for F-tests are the F-statistics. Standard errors are clustered at year by district. Covariates include months and districts dummies. Significance levels correspond with *** $p<0.01$ for 1 %, ** $p<0.05$ for 5% and * $p<0.1$ for 10%.

Table 5: Average treatment effect of health insurance enrolment by gender

	Total Hours		Agric Hours		Non-Agric Hours		Wage Hours	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Men	Women	Men	Women	Men	Women	Men	Women
Insurance x 2013	-0.591	-0.343	0.538	-0.509	-1.129*	0.166	-1.156*	-0.064
	(0.601)	(0.579)	(0.490)	(0.453)	(0.659)	(0.526)	(0.690)	(0.505)
Insurance x 2016	-1.789**	-0.291	0.500	-0.677	-2.289**	0.387	-2.171**	-1.272**
	(0.796)	(0.605)	(0.551)	(0.453)	(0.931)	(0.583)	(0.935)	(0.543)
Observations	34,498	41,628	34,498	41,628	34,498	41,628	34,498	41,628
R-squared	0.120	0.109	0.124	0.204	0.187	0.254	0.119	0.168

All models include the full set of controls shown in Table A1 in the Online Appendix, plus month and district fixed effects. Standard errors (in parenthesis) are clustered at year by district. Significance levels correspond with *** $p<0.01$ for 1 %, ** $p<0.05$ for 5% and * $p<0.1$ for 10%.

Table 6: Average treatment effect of health insurance enrolment by location

	Total Hours		Agric Hours		Non-Agric Hours		Wage Hours	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Urban	Rural	Urban	Rural	Urban	Rural	Urban	Rural
Insurance x 2013	-1.552	-0.433	-0.484	0.106	-0.400	-0.539	-1.126	-0.667*
	(1.776)	(0.425)	(0.802)	(0.407)	(0.424)	(0.379)	(1.795)	(0.392)
Insurance x 2016	-0.342	-1.130**	0.333	-0.120	-0.923	-1.011*	-0.607	-1.996***
	(2.293)	(0.526)	(0.718)	(0.485)	(0.578)	(0.571)	(1.681)	(0.621)
Observations	10,146	65,980	10,146	65,980	76,126	65,980	10,146	65,980
R-squared	0.169	0.115	0.261	0.092	0.242	0.144	0.199	0.130

All models include the full set of controls shown in Table A1 in the Online Appendix, plus month and district fixed effects. Standard errors (in parenthesis) are clustered at year by district. Significance levels correspond with *** $p<0.01$ for 1 %, ** $p<0.05$ for 5% and * $p<0.1$ for 10%.

Table 7: Effect of premium changes (ATE) on men and women

	Total Hours		Agric Hours		Non-Agric Hours		Wage Hours	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Male	Female	Male	Female	Male	Female	Male	Female
Increase x 2013	-0.282	-0.281	0.667	-0.189	-0.949	-0.092	-1.283	-0.494
	(0.732)	(0.622)	(0.564)	(0.465)	(0.752)	(0.560)	(0.830)	(0.498)
Increase x 2016	-1.680*	-0.474	0.848	-0.457	-2.528**	-0.017	-2.551**	-1.788***
	(0.872)	(0.635)	(0.629)	(0.448)	(1.010)	(0.624)	(1.004)	(0.549)
Waiver x 2013	-1.030	-0.580	-0.056	-0.353	-0.974	-0.227	-1.102	1.074
	(1.217)	(0.909)	(0.723)	(0.681)	(1.183)	(0.755)	(1.076)	(0.721)
Waiver x 2016	-2.568*	-1.074	1.673	-1.237*	-4.241***	0.163	-1.857	-0.998
	(1.342)	(0.973)	(1.102)	(0.719)	(1.420)	(0.800)	(1.465)	(0.767)
Observations	34,569	41,740	34,569	41,740	34,569	41,740	34,569	41,740
R-squared	0.117	0.109	0.122	0.208	0.184	0.240	0.112	0.151
Baseline	30.355	23.023	14.641	16.896	15.713	6.127	10.027	3.610
Baseline (control)	28.424	23.012	14.787	17.357	13.637	5.655	10.725	4.211

All models include the full set of controls shown in Table A1 in the Online Appendix, plus month and district fixed effects. Standard errors (in parenthesis) are clustered at year by district. Significance levels correspond with *** $p<0.01$ for 1 %, ** $p<0.05$ for 5% and * $p<0.1$ for 10%.

Table 8: Effect of premium changes (ATE) in rural and urban regions

	Total Hours		Agric Hours		Non-Agric Hours		Wage Hours	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
VARIABLES	Urban	Rural	Urban	Rural	Urban	Rural	Urban	Rural
Increase x 2013	-1.329	-0.264	-0.096	0.257	-1.233	-0.521	-1.333	-0.865*
	(1.823)	(0.452)	(0.932)	(0.426)	(1.889)	(0.408)	(1.953)	(0.453)
Increase x 2016	0.557	-1.310**	0.994	0.035	-0.437	-1.344**	-0.617	-2.372***
	(2.244)	(0.575)	(0.796)	(0.511)	(2.348)	(0.632)	(1.812)	(0.669)
Waiver x 2013	-1.315	-0.866	-1.136	-0.012	-0.179	-0.854	1.282	-0.303
	(2.878)	(0.716)	(1.402)	(0.536)	(2.765)	(0.697)	(1.864)	(0.565)
Waiver x 2016	-2.256	-1.698**	-1.802	0.450	-0.454	-2.149**	1.641	-1.970**
	(2.580)	(0.851)	(1.333)	(0.771)	(2.599)	(0.894)	(2.592)	(0.884)
Observations	10,162	66,147	10,162	66,147	10,162	66,147	10,162	66,147
R-squared	0.171	0.118	0.277	0.092	0.252	0.146	0.192	0.127
Baseline	34.137	25.163	7.584	17.107	26.557	8.057	17.263	4.905
Baseline (control)	33.993	24.389	7.910	17.316	26.083	7.073	19.046	5.634

All models include the full set of controls shown in Table A1 in the Online Appendix, plus month and district fixed effects. Standard errors (in parenthesis) are clustered at year by district. Significance levels correspond with *** $p<0.01$ for 1 %, ** $p<0.05$ for 5% and * $p<0.1$ for 10%.