ECONSTOR

Make Your Publications Visible.

A Service of



Leibniz-Informationszentrum Wirtschaft Leibniz Information Centre for Economics

Rukundo, Emmanuel Nshakira; Schroeder, Sarah; Hisarciklilar, Mehtap; McKay, Andrew D.

Working Paper Health insurance premium changes and labor supply: Evidence from a low-income country

Ruhr Economic Papers, No. 1103

Provided in Cooperation with: RWI – Leibniz-Institut für Wirtschaftsforschung, Essen

Suggested Citation: Rukundo, Emmanuel Nshakira; Schroeder, Sarah; Hisarciklilar, Mehtap; McKay, Andrew D. (2024) : Health insurance premium changes and labor supply: Evidence from a low-income country, Ruhr Economic Papers, No. 1103, ISBN 978-3-96973-281-6, RWI - Leibniz-Institut für Wirtschaftsforschung, Essen, https://doi.org/10.4419/96973281

This Version is available at: https://hdl.handle.net/10419/308800

Standard-Nutzungsbedingungen:

Die Dokumente auf EconStor dürfen zu eigenen wissenschaftlichen Zwecken und zum Privatgebrauch gespeichert und kopiert werden.

Sie dürfen die Dokumente nicht für öffentliche oder kommerzielle Zwecke vervielfältigen, öffentlich ausstellen, öffentlich zugänglich machen, vertreiben oder anderweitig nutzen.

Sofern die Verfasser die Dokumente unter Open-Content-Lizenzen (insbesondere CC-Lizenzen) zur Verfügung gestellt haben sollten, gelten abweichend von diesen Nutzungsbedingungen die in der dort genannten Lizenz gewährten Nutzungsrechte.

Terms of use:

Documents in EconStor may be saved and copied for your personal and scholarly purposes.

You are not to copy documents for public or commercial purposes, to exhibit the documents publicly, to make them publicly available on the internet, or to distribute or otherwise use the documents in public.

If the documents have been made available under an Open Content Licence (especially Creative Commons Licences), you may exercise further usage rights as specified in the indicated licence.



WWW.ECONSTOR.EU



RUHR ECONOMIC PAPERS

Emmanuel Nshakira-Rukundo Sarah Schroeder Mehtap Hisarciklilar Andy McKay

> Health Insurance Premium Changes and Labor Supply: Evidence from a Low-Income Country

> > **CVI** #1103

Imprint

Ruhr Economic Papers

Published by

RWI – Leibniz-Institut für Wirtschaftsforschung Hohenzollernstr. 1-3, 45128 Essen, Germany Ruhr-Universität Bochum (RUB), Department of Economics Universitätsstr. 150, 44801 Bochum, Germany Technische Universität Dortmund, Department of Economic and Social Sciences Vogelpothsweg 87, 44227 Dortmund, Germany Universität Duisburg-Essen, Department of Economics Universitätsstr. 12, 45117 Essen, Germany

Editors

Prof. Dr. Thomas K. Bauer RUB, Department of Economics, Empirical Economics Phone: +49 (0) 234/3 22 83 41, e-mail: thomas.bauer@rub.de Prof. Dr. Wolfgang Leininger Technische Universität Dortmund, Department of Economic and Social Sciences Economics - Microeconomics Phone: +49 (0) 231/7 55-3297, e-mail: W.Leininger@tu-dortmund.de Prof. Dr. Volker Clausen University of Duisburg-Essen, Department of Economics International Economics Phone: +49 (0) 201/1 83-3655, e-mail: vclausen@vwl.uni-due.de Prof. Dr. Ronald Bachmann, Prof. Dr. Almut Balleer, Prof. Dr. Manuel Frondel, Prof. Dr. Ansgar Wübker RWI, Phone: +49 (0) 201/81 49-213, e-mail: presse@rwi-essen.de

Editorial Office

Sabine Weiler

RWI, Phone: +49 (0) 201/81 49-213, e-mail: sabine.weiler@rwi-essen.de

Ruhr Economic Papers #1103

Responsible Editor: Manuel Frondel

All rights reserved. Essen, Germany, 2024

ISSN 1864-4872 (online) - ISBN 978-3-96973-281-6

The working papers published in the series constitute work in progress circulated to stimulate discussion and critical comments. Views expressed represent exclusively the authors' own opinions and do not necessarily reflect those of the editors.

Ruhr Economic Papers #1103

Emmanuel Nshakira-Rukundo, Sarah Schroeder, Mehtap Hisarciklilar, and Andy McKay

Health Insurance Premium Changes and Labor Supply: Evidence from a Low-Income Country



Bibliografische Informationen der Deutschen Nationalbibliothek

The Deutsche Nationalbibliothek lists this publication in the Deutsche Nationalbibliografie; detailed bibliographic data are available on the Internet at http://dnb.dnb.de

RWI is funded by the Federal Government and the federal state of North Rhine-Westphalia.

https://dx.doi.org/10.4419/96973281 ISSN 1864-4872 (online) ISBN 978-3-96973-281-6 Emmanuel Nshakira-Rukundo, Sarah Schroeder, Mehtap Hisarciklilar, and Andy McKay*

Health Insurance Premium Changes and Labor Supply: Evidence from a Low-Income Country

Abstract

We study the effect of a health insurance premium policy change on labor supply. Using a matching with difference-in-differences strategy on pooled nationwide cross-sectional and panel data we find that both premium waivers and premium increases led to a reduction in labor supply by almost similar margins. We also show that the policy change reduced the probability of wage employment and increased domestic labor supply, pointing to potential income effect for waivers and potential manipulation effects for premium increases. Our results are robust to various specifications and raise concerns for the unintended effects of popular but likely inefficient community-based welfare targeting methods.

JEL-Codes: I13, I15, I18, J01

Keywords: Health insurance; premium changes; labor supply; Rwanda

September 2024

^{*} Emmanuel Nshakira-Rukundo, RWI, University of Bonn, and Apata Insights; Sarah Schroeder, Aarhus University; Mehtap Hisarciklilar, Coventry University; Andy McKay, University of Sussex. – All correspondence to: Emmanuel Nshakira-Rukundo, RWI, Hohenzollernstraße 1–3, 45128 Essen, Germany, e-mail: erukundo@rwi-essen.de

1 INTRODUCTION

The relationship between health, health insurance and labor markets is central to the study of human capital (Currie & Madrian, 1999), as well as for poverty reduction and social welfare. Poor health negatively affects labor market participation (Weil, 2007) and vice versa; healthier individuals have the ability to work longer, miss fewer days at work, and 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, in the form of protection from healthcare expenses. Secondly, because access to health insurance covers healthcare-related costs of sickness without infringing on household budgets (Wagstaff, Eozenou, & Smitz, 2020; Wagstaff et al., 2018), it enables savings from unspent budgets and the ability to allocate more resources (including time) to more economically productive ventures (Currie & Madrian, 1999; Gruber, 2000; Gruber & Yelowitz, 1999). This may lead individuals with access to health insurance to have higher labor market participation. Both sets of reasons, alluding to financial protection and to savings and budget protection, are more likely to imply that health insurance will be associated with increased income in what is classically called the "income effect" (Nyman, 2001, 2008). However, while the effects of health insurance on labor markets are broadly documented in high-income countries, there is very limited evidence on this for low-income countries. Moreover, very little is known about labor market responses to changes in the cost of health insurance (premiums) in low-income settings.

This study primarily investigates the latter question while focusing on Rwanda. Rwanda is particularly suitable for this exercise as it is one of the few countries in Africa that have a national health insurance program of substantial coverage estimated at about 81% of the population (Chemouni, 2018). In 2011, the government introduced a progressive premiums policy in a bid to raise more resources for the program, as well as to enable more equitable cross-subsidisation between the non-poor and poor individuals. This was implemented by providing premium waivers to households categorized as poor and imposing premium increases ranigng from 200 to 700% on households classified as non-poor. However, the potentially flawed community-based ranking process used to identify the well-off and the poor implied that allocation of these policy change dimensions was highly error-prone and the outcome on who received which policy change dimension was nearly as good as random.

Utilising a differences-in-differences strategy combined with matching, we study the effect of this policy change on labor supply and find that the policy change had a substantial negative effect on labour supply across both policy change variations. Premium increases led to a 24% reduction in non-agricultural labor supply and a 21% reduction in wage activities, each relative to the baseline means. Similarly, premium waivers had a large, negative and significant effect on non-agricultural activities (35% reduction) and wage activities (24.5% reduction) relative to baseline means. These results, thus, have significant implications for our understanding how individuals shift labor supply in a context where health insurance premiums can pose a significant cost for households.

Our interpretation of these results relies on a theoretical model that reconciles what appears to be intriguing in our findings - why individuals reduce labor supply when faced with either the disadvantages of premium increases or the advantages of premium waivers. We build on bunching theories (Saez, 2010; Shi, 2016), which allow us to assume endogenous household responses to changes in the health insurance premium via welfare (income) thresholds. The choice of welfare (income) 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 programs, including health insurance schemes (Ezeanya, 2015). Importantly, the eligibility categorization process creates negative incentives to supply labour due to the substantial benefits of a 'poor' categorization. Being categorized 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 programs and education scholarships (Ezeanya, 2015). This kind of behavioural response regarding the individual's welfare status to benefit from social programs 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 analysis therefore highlights that a revisiting of the community-based targeting is worthwhile in addressing labor market participation in a developing country context.

Related Literature. Very little known about the effect of health insurance premiums on labor markers in low-income countries (Le, Groot, Tomini, & Tomini, 2019a). Many low income countries do not (yet) have substantial levels of health insurance penetration to aid assessments of its impact at broadly representative levels. Rwanda, however, provides a unique

case for which out study is possible. We, therefore, contribute to a very small group of studies in low and middle income countries that attempt to interrogate this issue. From China, Shen, Parker, Brown, and Fang (2017) found that gaining health insurance increased labor market participation especially for men and Luo and Escalante (2020) found that insured individuals in rural regions were more likely to move out of the agricultural sector. Liao (2011) found large reductions in labor force participation especially for women. Other negative effects are observed in Vietnam (Le, Groot, Tomini, & Tomini, 2019b) and Ghana (Garcia-Mandico, Reichert, & Strupat, 2021). However, studies from Uruguay (Bergolo & Cruces, 2014), Mexico (del Valle, 2021) and Ecuador (Molina-Vera, 2021) all find significant positive effects on respective health insurance introductions on labor supply, mainly by reducing potential exit from labor markets for informal wage earners. Landmann and Frölich (2015) and Frölich and Landmann (2018) studied how the extension of health insurance to low-income informal sector households in Pakistan affected child labor and observed significant reductions including reducing hours spent in hazardous activities. And yet, all these studies assess the introduction of health insurance and not the effects of (more exogenous) changes in health insurance premiums. We believe that this is most likely the first study in from a low-income country that assesses the effect of premium changes on labor supply.

Secondly, we contribute to literature on social welfare programs and reforms (Chan & Moffitt, 2018; Moffitt, 2004), especially programs in developing countries or those use community-based targeting methods (vis-a-vis proxy means tests) and those that exploit thresholds (Alatas, Banerjee, Hanna, Olken, & Tobias, 2012; Banerjee, Hanna, Olken, & Lisker, 2024; Hanna & Karlan, 2017). In one dimension, we include for instance tax rebate programs (Saez, 2010) and health insurance programs that change premium structure (for instance providing subsidies) at a given threshold (Einav, Finkelstein, & Schrimpf, 2017; Shi, 2016). These are in the broadly-defined bunching literature (Kleven, 2016; Saez, 2010). In another dimension, we specifically contribute to welfare programs such as cash transfers in low-income countries that employ community-based targeting. A significant body of research has shown the inferiority of community-based targeting in reaching the most at need (Alatas et al., 2012; Hillebrecht, Klonner, A Pacere, & Souares, 2020; Houssou, Asante-Addo, Andam, & Ragasa, 2019; Premand & Schnitzer, 2020; Schnitzer, 2019; Stoeffler, Fontshi, & Lungela, 2020). This paper therefore extends the assessment of these types of interventions based on

thresholds derived from community rankings. Our results, therefore, have policy implications for low income countries investing in social welfare programs (not limited to health insurance) and using or intending to use community-based targeting methods in program distribution.

The rest of the paper is organised as follows. Section 2 outlines the institutional context of the paper elaborating on Rwandan health insurance and the 2011 premium policy change. Section 3 presents the methodology, identification and empirical specifications, Section 4 provided the results and Sections 5 and 6 provide the discussion and conclusion respectively.

2 INSTITUTIONAL CONTEXT

2.1 Health Insurance in Rwanda

Rwanda is one of the few countries in Africa with a national health insurance program of substantial coverage. In 1998, Rwanda piloted the Community-Based Health Insurance (CBHI) program in three districts.¹ Health insurance was then nationally adopted by 2005, with coverage expanding from the three pilot districts to the whole country (Diop & Ba, 2010; Diop & Butera, 2005; Schneider, 2005). Leveraging the existing culture of mutual support and a strong political will (Chemouni, 2018), including allocating donor support to the program, (Kalk, Groos, Karasi, & Girrbach, 2010; Logie, Rowson, & Ndagije, 2008) contributed to fast-paced enrolment. By 2010, over 85% of the population were reported to have been enrolled (Lu et al., 2012). This level of insurance participation remains unique in Africa. The scheme is available to anyone in the informal sector. Full membership requires a premium payment for each family member more than 6 months old. At the point of care, individuals are also required to pay a co-payment 200 Rwanda Francs and 10% of the cost of services. The health insurance provides coverage for all primary and tertiary care services sought at public health facilities in the country.

Despite such reported growth in enrolment, the program struggled with some key concerns. First, the structure of the premium was initially the same across all income groups (Schmidt, Mayindo, & Kalk, 2006). This meant that challenges of equity were soon observed. For instance, insured people had a five times more likelihood of utilising health services than non-insured people (Schneider & Hanson, 2006). However, in a post-conflict country where

¹Throughout this paper, we simultaneously use the terms community-based health insurance and health insurance to imply the Rwandan community-based health insurance program

at the time more than half the population lived below a dollar a day, even such premiums and additional co-payments were prohibitive for a large section of poor households (Kalk et al., 2010). Without the guarantee of donor resources and with a large section of poor households economically prohibited from enrolling, financial sustainability of the program was under question. One avenue was to make the program compulsory and transit from its original voluntary nature (Chemouni, 2018; Ridde et al., 2018). In 2007 and later in 2015, the Government of Rwanda passed laws mandating compulsory health insurance enrolment and attracting punishments for discouraging enrolment (Government of Rwanda, 2016). More recently, in 2019, additional statutory orders were passed on strategies to increase resources for the insurance program (Government of Rwanda, 2020). It is not clear whether mandatory laws are strictly enacted (Koch et al., 2022). However, what is clear is that a large section of the population remains uninsured. In Supplementary Table A1, we show that at baseline, while rates of enrolment were 84% among the richest, they were only about 52% among the poorest.

2.2 The 2011 Premiums Policy Change and *Ubudehe* Poverty Classifications

In July 2011, to incentivize poor households' enrolment and to promote equity through crosssubsidisation, the Government of Rwanda passed a new premiums policy (MoH, 2012; Vogel, 2011). As opposed to uniform premiums, a new premiums structure was announced to commence in July 2011, characterised by progressive payments across a welfare continuum. On the one hand premium waivers were made to the poorest households and on the other, insurance premiums for non-poor households increased by 200% for the averagely nonpoor and 600% for those classified as well-off (Kalisa, Musange, Collins, Saya, & Kunda, 2016; MoH, 2012; Vogel, 2011)². Table 1 below illustrates the policy change. Co-payment levels were retained; however, for the poor households, co-payments would also be waived. Classification of households into poor or non-poor categories was based on a community-based poverty ranking process known as *Ubudehe* classification.

 $^{^{2}}$ In general, only a very small portion of households in out sample (less than 0.5% were classified as well-off), we therefore drop these from the analysis since they are essentially outliers. However, their inclusion does not change the results in any noticeable way).

	Dramium	Pre-reform	Post-reform	Post reform	
Ubudehe	group	premiums	premiums	abanga	
		(RwF)	(RwF)	change	
Poor (Cat1)	1	1000	0	Waiver	
Non-poor (Cat 2 & 3)	2	1000	3000	200%	
Non-poor (Cat 4)	3	1000	7000	600%	

Table 1: Ubudehe categories & CBHI premiums

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

Ubudehe is a traditional Rwandan cultural practice of individuals 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 historically existed in local communities, the post-1994 era saw its formalisation and inclusion into the government's policy-making process. The current form of *Ubudehe* is a participatory community poverty ranking process in which communities use a pre-defined methodology to rank households from the poorest to the richest. The first major nationwide categorization 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 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).

Over the last decade, these poverty classifications have been used for a wide range of government social protection programs such as cash transfers and school scholarships for poor households (Ezeanya-Esiobu, 2017; Habimana, Haughton, Nkurunziza, & Haughton, 2021; Nirere, 2022). Moreover, the government further encouraged other development organisations to use the targeting method for their programming. The process has been heralded as one of the key 'homegrown' initiatives integrated into Rwanda's development policies (Hasselskog, 2018; Hasselskog & Schierenbeck, 2015; Rutikanga, 2019). A key point for our identification is that

the *Ubudehe* ranking process captures a lot more transitory differences in economic well being because every couple of years, the process is repeated and households can go up or down the ranking. In a small qualitative sample of about 100 individuals, Gaynor (2016) observed that 62% of the respondents had moved a category up and only 2% of the respondents had moved a category down. However, as we see in Section 3.1, these movements might not have clearly reflected households' welfare as the process itself did not cleanly identify the actual poor.

The transfers and penalties as a result of the policy change are not negligible. With a mean household size in the bottom two quintiles of 4.1 persons and a pre-reform consumption poverty line of 118,000 Rwanda Francs, per year (NISR, 2012), post-reform premiums increased from 3.5% of the national poverty line to 10.4%. For those households affected by this who are 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 a similar margin.

3 METHODOLOGY

3.1 Identification and Empirical Estimations

As already noted, the two premium-related changes were based on the *Ubudehe* communitybased poverty assessment and associated categorization. To streamline the targeting of the poor in while expanding social protection portfolio, the government sought to expand the scope and use of the *Ubudehe* categories for the socioeconomic profiling of households. In 2008, using the results of the 2007/08 categorization process, a national database was set up comprising all households in the country with their respective poverty category. Our identification is based on a community-based targeting method, which we argue that its failure to correctly identify the poor and the non-poor generated quasi-random variations on who recieves a given policy change dimension.

In several instances, community-based targeting is easy to implement, cost-effective, inclusive and can also be popular (Hanna & Karlan, 2017; Hillebrecht et al., 2020). However, as recent studies have also shown, it tends to be ineffective in capturing the actual poor when compared to means-tested methods (Alatas et al., 2012; Hillebrecht et al., 2020; Houssou et al., 2019; Premand & Schnitzer, 2020; Schnitzer, 2019; Stoeffler et al., 2020). The targeting errors in community-based targeting might emanate from several sources including elite capture,

failure of community members to correctly adhere to the targeting guidelines and spatial differences in the conceptualisation of poverty among others (Alatas et al., 2012; Hillebrecht et al., 2020; Schnitzer, 2019).

In the Rwanda situation, two main issues emerged that would have made community-based targeting an ineffective method. The first one is the potential of community manipulation. Accordingly, in the early days of the *Ubudehe* classification, the process was only used for identifying the poor. However, in order to better support the poor once they had been identified, the results of the ranking process would be used to allocate an extensive list of welfare programmes including financial inclusion, employment, school scholarships and health insurance (Ezeanya, 2015; Williams, Nzahabwanayo, Lavers, & Ndushabandi, 2020). Gaynor (2016) further reveals that that outcomes of the the *Ubudehe* has some tax implications. It, therefore, would become economically lucrative to be classified as poor because the process had transformed from a poverty mapping tool to more of a resource allocation tool (Gaynor, 2016). With the knowledge that a given classification had implications on levied premiums (Hasselskog, 2018) as well as additional social transfer programs, even better-off households had an incentive to mis-report (Nkurunziza, 2023b).

Another possible source of potential exogeneity is administrative performance contracts referred to as *imihigo*. Under the *imihigo* performance contracts, local administrators committed to achieving set development targets in various sectors (Chemouni, 2014). Setting and achieving the targets would not only bring local administrators prestige, but also additional financing through performance-based mandates. Qualitative research has therefore found that the process was complicated, often sprinkled with some data inaccuracies as administrative officers needed to show positive outcomes related to their commitments (Ansoms, Marijnen, Cioffo, & Murison, 2017; Hasselskog, 2016). However, one key inefficiency was how local administrators either set priorities for the targets or selected beneficiaries. In healthrelated performance-based financing, researchers observed that hospitals and local health administrators prioritised the "easy to reach" sub-population (Lannes, Meessen, Soucat, & Basinga, 2016) or indicators of "least effort" (Basinga et al., 2011), inadvertently increasing health inequities. In other social protection transfers, Hasselskog (2018) shows that local administrators allocated more resources to those "able to use them'' rather that the poor who would otherwise be deserving of them. This strategic mis-allocation was partly due to measurement inconsistencies between community-based ranking and administrative records.

In our data for 2013/14 and 2016/16 in which both the community-generated ranking and the administrative office ranking were both recorded, we observed that about 18.5% of all records had different rankings. Suffice to say, the Rwandan media have consistently reported on these issues in the last decade often calling for more honesty in administration and community ranking processes (Mbabazi, 2019; Nkurunziza, 2023b; Rugira, 2014; The East African, 2015).

The scenario above builds the foundation for our causal identification. We posit that in a landscape where there are targeting errors on all sides, the outcome is quasi-random or as good as random. Empirical assessments by Sabates-Wheeler et al. (2015) and Kidd and Athias (2019) both show that correlations between consumption measures of welfare and *ubudehe* ranking were very low. Assessing targeting effectiveness of the Vision 2020 Umurenge Program (VUP), which also uses the *ubudehe* rankings, Kidd and Athias (2019) found exclusion errors of 97 %, finding it the worst ranked program among 38 programs in 23 countries. Consequently, many poor households would have experienced a large shock to their household budgets by being categorized as non-poor, while rich households categorized as poor would have experienced large returns in saved incomes (would be premiums) and other services. For poor households that would have been correctly categorized and hence receive a premium waiver, they would have received a substantial positive transfer in form of waived costs of insurance and hence a household budget relaxation.

Our exploration of the data we use attests to this randomness and exogeneity in targeting. Like Sabates-Wheeler et al. (2015), we correlate policy change dimensions and consumption quintiles. Figure 1 shows how classification of poverty status by *Ubudehe* compares with means-tested consumption. About 24% of the high consumption households were actually classified as poor. On the right side of Figure 1 we show that 35% of households classified in higher *Ubudehe* categories instead had a very low consumption level, in other words, poor.

Observing the distribution according to the policy change (treatments), we observe a similar pattern. About 30% of households who experienced a premium increase were from the poorest quintiles. On the other hand, about 26% of households classified as poor and hence, receiving premium waivers were from 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 estimation. These targeting imperfections provide a quasi-random scenario of treatment allocation in which respondents





Figure 2: Consumption quintiles over treatment groups



capacity to self-select into a treatment/ policy change arm is negated by administrative decisions. They are therefore left with ex-post behaviour changes emanating from the policy change itself and not any other pre-policy change characteristic.

3.2 Data

We use data from the Integrated Households Living Conditions Surveys (EICV)³. The EICV surveys are nationally representative, cross-sectional surveys collected by the National Institute of Statistics of Rwanda since 1999. The surveys are used by the Government of Rwanda as the main welfare and poverty monitoring surveys and therefore, cover a wide range of topics. The sampling of the EICV surveys is based on the National Population and Housing Census (NPHC) sampling frames. The surveys employed a multi-stage sampling criteria, stratifying enumeration areas by rural and urban location. Enumeration areas were further identified by sector, cell and village codes⁴. 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 these, 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, about 900 enumeration areas are included in each survey round. In all the survey rounds, response rates above 95% were registered, with the 2013/14 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.

We use the 2010/11, 2013/14 and 2016/17 rounds, exploiting the 2010 pre-reform survey as the baseline and the two follow-up surveys as the short-term and medium-term observations. We do not use the 2001 and 2005 survey rounds for two reasons. First, the government of Rwanda had not yet started applying the *Ubudehe* community based poverty ranking process in targeting social protection programs. Secondly, only the 2010/11 survey is consistent with the later rounds in measuring key outcomes, including those relating to labor.

We take several steps to construct our sample. First, we limit the sample to individuals who are expected to be active in labor markets. Therefore, we exclude individuals who were in full-time education in the last 12 months, as well as those above 64 or below 14 years, who are not technically expected to be in active labor markets. Secondly, to avoid a possible measurement error on the upper tail (in the form of extreme outliers), we further truncate reported weekly hours in each measured category to a maximum of 120 hours.

³EICV represents the original name for the survey in French: Enquete Integrale sur Les Conditions de Vie des menages)

⁴Sectors are equivalent to sub-district administrative areas while cells and villages are lower-level administrative areas created after the 2006 re-demarcation of within-country administrative units.

Each survey round also provides information on households' *Ubudehe* status, which determines the treatment. This variable was directly collected only in the 2013/14 and 2016/17 survey rounds. For the baseline *Ubudehe* category variable, we rely on the matching process and data from Sabates-Wheeler et al. (2015). In their matching process, Sabates-Wheeler et al. (2015) accessed the names and all other identifying information from the 2010 survey and the 2008 national *Ubudehe* database. In their matching algorithm using Stata's user-written command, RECLINK⁵ Sabates-Wheeler et al. (2015) achieved a perfect matching of 76% of the 2010 survey, thus creating our complete pre-reform data. Including the non-perfect matching on baseline *Ubudehe* category, including those who selected "I don't know". Across the overall sample, 12.6% of the households had this missing variable, of which 2010 baseline had 24% missing, 2013/14 has 4.4% missing and the 2016/17 survey had 3.6% missing. We exclude these from the main analysis, though report this as a sensitivity check.⁶ Our final sample, shown in Table 2, therefore, comprises 71,104 individuals from 32,941 households.

	2010/11	2013/14	2016/17
Premium Increase	9,805 (47.38%)	12,572 (51.89%)	16,270 (62.14%)
Premium Waiver	4,409 (21.31%)	4,418 (18.23%)	3,429 (13.1%)
Control	6,479 (31.31%)	7,239 (29.88%)	6,483 (24.76%)
Total	20,693	24,229	26,182

Table 2: Study sample

Finally, we also have access to a small panel dataset of two waves between 2010 (baseline) and 2013/4 (short term). The panel dataset emanates from a small component of households from the 2010 survey who were followed up in 2013/14 and 2016/17 surveys, covering 1998 households in 2010, 2423 households and 2427 households in 2013/14 and 2016/17 respectively (NISR, 2018b). However, due to data accessibility limitations, we were only able to acquire the baseline and first follow-up. In addition, we are able to match about 1300 households and 5180 individuals from the baseline across the two rounds. The panel data

⁵RECLINK allows records across different datasets with no perfect identifying key exists through probabilistic fuzzy matching of a bigram string comparator across defined matching variables

⁶For the sesitivity check we implement a missing-a t-random imputation and impute these missing data. Results including these missing *Ubudehe* category variable are included in supplementary materials.

regressions therefore provide a robustness for our short term results. The main purpose of the panel data is to therefore assess the extent to which it aligns with the short term pooled cross-sectional results.

3.3 Empirical Estimations

The two dimensions of the policy change were (1) premium increases for the non-poor people and (2) premium waivers for the poor people. Moreover, as we show in Table A1, a substantial proportion of people remain uninsured. These provide an appropriate control group. While we start with a premise that the two exposures and the comparison groups are more or less the same, we find that there were several characteristics in which these groups were different from each other, as shown in Table A9. To remove this potential bias in our causal analysis, we adopt a matching with difference-in-differences strategy in which the weighting estimator helps in attaining efficient comparability between the various policy change dimensions based on the observed characteristics (Blundell & Costa Dias, 2009; Cattaneo, 2010; Fredriksson & de Oliveira, 2019; Ryan, Burgess Jr, & Dimick, 2015; P. H. Sant'Anna & Zhao, 2020). We thus implement a two-step regression in which the first step is to create a weighting estimators and the second stage is the outcome regression. Since the policy change had two dimensions, in the first stage, we use a multinomial logistic regression to estimate a generalised propensity score (Cattaneo, 2010; Uysal, 2015). The basic multinomial treatment model is then defined as

$$T_{ijtkm} = \vec{X}_{ijt}\beta_1 + \beta_2 Post_{it} + \vec{X}_{ijt} * Post_{it}\beta_3 + \mu_k + \varsigma_m + \varepsilon_{ijtkm}.$$
(1)

where the treatment variable (policy change dimensions) is given as:

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

 $Post_{it}$, represents time dummies for the short (2013) and medium term (2016), \vec{X}_{it} stands for individual and household controls as shown Table A1 in the Online Appendix. To account for variation across space and time in labor supply, we include μ_k , representing district fixed

effects, and ς_m are month fixed effects that capture unobserved determinants of labor 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 labor market conditions. The vector $\vec{X_{ijt}} *$ $Post_{it}$ interacts the controls and the time dummies to absorb the differences emanting from trends. From the above model, we predict generalised propensity score weights τ^{gps_weight} which will then be plugged into the second-stage outcome regression. Supplementary Table A9 and Figure A3 show that weighting be the generalised propensity score, the differences between policy change dimensions and the comparison group are removed.

The second stage outcome equation is a difference-in-differences model given as:

$$Y_{ijtkm} = \tau^{gps_weight} + \beta_1 Increase_{ijtkm} + \beta_2 Waiver_{ijtkm} + \beta_3 Post_{it} + \beta_4 Post_{it} * Increase_{ijktm} + \beta_5 Post_{it} * Waiver_{ijtkm} + \vec{X}_{ijt}\beta_6 + \vec{X}_{ijt} * Post_{it}\beta_7 + \mu_k + \varsigma_m + \varepsilon_{ijtkm}.$$
(3)

The main outcomes that are presented by Y_{ijtkm} are (1) labor supply in number of hours worked in the past 7 days for individual *i* in household *j* in time *t* in district *k* and month *m*, (2) hours worked in agriculture, (3) hours worked in non-agricultural activities and (4) hours worked in wage employment. *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 categorized as poor and was enrolled in CBHI and so received a premium waiver after 2012 and 0 otherwise. *Post_{it}* are time dummies for 2013 (short term) and 2016 (medium term). The term τ^{gps_weight} is the generalised probability score weight from the first stage multinomial regression (Equation 1) of the policy change dimension assigned to an individual. The coefficients of interest are β_4 and β_5 which are the interaction terms of the premium increase and premium waiver treatments respectively.

There are a number of potential threats to our identification of causal effects. The first is that there is a potential change in group composition, especially if the policy aimed at encouraging the poor households to enrol in CBHI succeeded, implying that the control group (those not affected by the policy change) would become fewer by the year. The change in group composition might imply that part of the observed effect might be emanating from the changes in the sample rather than the policy change itself especially as some of the sample units react to the treatment. In our case, this seems plausible given that poor individuals are more likely to acquire health insurance partly due the policy that incentivizes health insurance for this group and not necessarily because of health insurance itself. It is plausible that in each individual round of data, covariates can have differential effects on treatment selection as each new sample is drawn, causing regression to the mean (Daw & Hatfield, 2018), which biases estimates. To alleviate this concern, we adopt a trends-absorption strategy represented by the term $\vec{X_{ijt}} * Post_{it}$. This strategy, previously used by La Ferrara and Milazzo (2017) and Molina-Vera (2021), relies on including interactions of all controls with the time dummies, hence making sure that any potential effect that controls would have had on both insurance enrolment and *Ubudehe* classification is accounted for.

3.4 Sample Description

We use a large set of observed individual and household characteristics in our analysis. Among the individual characteristics, we include gender, 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. We do not expect moral hazard in individuals' behaviour and decision to enrol within a year because individuals enrol for a whole year and there is no much changes in month-on-month insurance status. However, allocation of time to employment can be affected by health status. We therefore include a control for health status in the last 14 days prior to the survey. 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 owns and total livestock units (using the International Livestock Research Institute classification (Njuki et al., 2011)).

We are cognizant of the fact that this particular targeting method is/was used by the government in targeting other social protection programs. This may imply that our results could be driven by other programs other than health insurance changes. To mitigate this threat, we controlled for other social protection programs in the flagship Vision 2020 Umurenge program (VUP), which included cash transfers, public works programs and financial inclusion instruments. The combined participation in these programs 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.

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. We use travel time as the indicator for access to services. We also include a rural-urban dummy that indicates a household's location. To account for seasonality and possible regional/district variation in labor markets, we include month and district 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.

Supplementary Tables A3 and A9 show the summary statistics of our sample by both health insurance adoption and health insurance premium changes. The pairwise regressions then include weights from the first stage treatment prediction model. Supplementary Table A3 shows the mean differences between individuals enrolled in health insurance and those not. We notice that in all but six of the 29 controls included in the model the balancing is not perfect. Looking at the graphic view of balancing efficiency in Supplementary Figure A1, we observe that balance efficiency is achieved with the strong overlap across matched compared to unmatched samples. In Table A9, we show the within-wave balancing of treatment and control groups. Column 4 of Table A9 compares premium increase with control and shows that of all the 29 base controls included in the model, only 5 are not perfectly balanced. In Column 5 of the table, we show the balance of premium waiver with control. In this comparison, 9/29 controls are not perfectly balanced. Though the balancing efficiency is imperfect in a small proportion of the controls, the material differences are very small. Indeed, a graphic view of balance kernel density graphs in Supplementary Figure A3 shows a strong overlap across the two treatment groups and the control, implying sufficiently good matching and controlling for observed differences between the treatments and control groups.

Outcomes 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. The outcomes of interest are (i) total hours worked in the last seven days, which we then separate into (ii) hours allocated to agricultural 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 evolving differences across individuals exposed to different policy change effects. In the short term, we notice that people who receive a waiver work generally less than those who either had a premium increase or were not insured. Individuals with a premium waiver worked about 2.3 hours less than the non insured (control) and about 4.4 hours less than those with a premium increase. Regarding non-agricultural hours, individuals with a waiver worked about 4.8 hours less than those with an increase and about 2.8 hours less than the control. These results provide the first indication of the possible 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 with a premium increase worked overall 5 hours more than those with 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.

	Baseli	ine (2010/20)11)	Short term			(2013/2014)			Medium term (2016/2017)					
	(1)	(2)	t-test	(3)	(4)	(5)	t-test	t-test	t-test	(6)	(7)	(8)	t-test	t-test	t-test
	No Insurance	Insured	Diff	No Insurance	Increase	Waiver	Diff	Diff	Diff	No Insurance	Increase	Waiver	Diff	Diff	Diff
	Mean/SE	Mean/SE	(1)-(2)	Mean/SE	Mean/SE	Mean/SE	(3)-(4)	(3)-(5)	(4)-(5)	Mean/SE	Mean/SE	Mean/SE	(6)-(7)	(6)-(8)	(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***

Table 3: Descriptive differences by policy change across time

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%.

4 RESULTS

4.1 Effects of the Premium Policy Change

Table 4 presents the average treatment effects of the premium policy changes. The results show that in the short term, neither premium waivers nor premium increases had a significant effect on labor supply in any the four categories. But in the medium term, we observed that both premium waivers and premium increases had a negative and statistically significant effect on labor supply in some dimensions. Premium increases had a statistically significant negative effect only on hours of wage labor, which fell by around two hours, equivalent to about 21% of the baseline control mean.

	(1)	(2)	(3)	(4)
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
Premium increase x 2013	0.140	0.419	-1.490	-1.124
	(0.526)	(0.448)	(0.938)	(0.819)
Premium increase x 2016	-0.538	-0.260	-2.549***	-2.523***
	(0.615)	(0.516)	(0.921)	(0.855)
Premium waiver x 2013	-0.215	0.115	-0.531	-0.311
	(0.717)	(0.599)	(1.508)	(0.905)
Premium waiver x 2016	-2.018**	-0.077	-3.653***	-3.003***
	(0.890)	(0.793)	(1.323)	(1.060)
Constant	24.268***	19.967***	15.325***	13.607***
	(1.568)	(1.140)	(2.560)	(1.881)
Observations	67,925	61,052	41,626	40,390
R-squared	0.144	0.069	0.222	0.234
Baseline Mean	28.03	16.32	11.42	10.67
Baseline mean (control)	27.64	16.85	10.49	12.25
Increase – % mean (short)	0.00500	0.0257	-0.131	-0.105
Increase – % mean (medium)	-0.0195	-0.0154	-0.243	-0.206
Waiver - % mean(short)	-0.00767	0.00702	-0.0465	-0.0291
Waiver – % mean (medium)	-0.0730	-0.00456	-0.348	-0.245

Table 4: Main results showing the effect of premiums change on labour supply

All models include the full set of controls shown in Supplementary Tables A3 and A9. Standard errors in parentheses clustered at Sector level. Significance levels corresponding to *** p<0.01 for 1% ** p<0.05 for 5% and * p<0.1 for 10%

Turning to the effect of premium waivers, we observe large and significant effects in the medium term across the board except for agricultural labor supply. Starting with total labor supply, we find that premium waivers led to the reduction of total labor supply by over 2 hours a week, equivalent to about 7.3%. The reductions in non-agricultural and wage labor supply are larger in significance and magnitude. We observed that premium waivers led to a reduction in non-agricultural labor supply by 3.7 hours, close to 35% of the baseline control mean. Reductions in wage labor supply was also more than 3 hours a week, and about 24.5% of baseline mean for the control group.

4.1.1 Sub-Group Effects

Effects by Gender We assess separately the effect of premiums changes on women and men by implementing regression 3 for men and women separately and show these results in Supplementary Table A10. First, looking at the effect of premium increases, we observe that to some extent, premium increases have a similar effect on men and women but the magnitudes are larger for men than for women. We observe that in the short term, premium increases significantly reduce non-agricultural labor supply for men by close to 2 hours, but for women the effect is smaller (-0.72) and insignificant. However, in the medium term, premium increases significantly reduce non-agricultural labor supply for women by 1.66 hours a week. For men, the effects are bigger, close to 2.9 hours per week. A 1.6 hours reduction in non-agricultural labor supply for women is about 22.2% of baseline non-agricultural labor supply and for men, the reduction was about 16% of baseline time. Men therefore reduced non-agricultural labor supply by smaller margins than women in the medium term. Regarding wage employment, for both men and women, premium increases reduce labor supply by 2.5 hours for men and 2.3 hours for women. Given than women's baseline weekly wage supply was only about 6.8 hours this reduction was equivalent to about 32.6% whereas for men, the reduction was about 14.8 % of baseline mean. We therefore observe that for wage labor supply, even when the labor supply reductions are close in absolute values between men and women, women reduce labor supply by more than twice as much as men in the medium term.

For premium waivers, we do not observe a very clear pattern of difference in the effects between men and women. However, we observe that the significant total labor supply reductions are observed for men but not women. While the main results presented in Table 4 show that overall labor supply reduced by about 2.1 hours a week, men reduced total labor supply by over 3.5 hours a week. By their baseline average of about 33 hours a week, men reduced labor supply by about 10.7% due to premium waivers. However, we do not find statistically significant medium term results for men in other classifications studied. This could imply that men reduce small amounts in various activities, but the sum of these adds up to a statistically significant margins of total labor supplied. Regarding women, we find that premium waivers reduced wage employment activities by statistically significant margins compared to other categories. Women reduce wage employment labor supply by about 2.4 hours in a week. This is significant at a 10 % level and the magnitudes are big, at 35% of the baseline mean and 31% of the baseline mean of the control group.

It is therefore very clear that for premium waivers, men tend to be the largest beneficiaries of the potential income effect that causes a reduction in time use. Premium waivers imply that as the would-be costs for insurance are waived, individuals gain this income by not spending it. This increases disposable income and incentivizes individuals to supply less labor.

Effects Across Rural and Urban Localities Supplementary Table A11 shows the results separated by location where we look at rural and urban areas separately. In rural areas, we observe consistent reductions in labor supply across the board as an effect of both premium increases and waivers. Premium increases reduced total labor supply by about one hour in the medium term. For non-agricultural and wage employment activities, premium increases reduced labor supplied in both the short and medium terms. The labor supply reductions for non agricultural activities increase from 1.6 hours in the short tern to about 3.3 hours in the medium term. Comparing to pre-policy change levels for rural dwellers, the reductions were equivalent to 12.4% in the short term and 25.7% in the medium term. For wage employment activities, the reduction in labor supplied also increases from about 1.6 hours in the short term to about 3.3 hours in the medium term. These wage employment labor supply reductions were very significant in magnitude, amounting to 16.4% in the short term and 34.8% in the medium term. The reductions due to premium waivers were even larger though only significant in the medium term. Non-agricultural labor supply experienced a reductions of 3.4 hours in a week, equivalent to about 26.4%. Wage labor supply reduced by over 4 hours in a week, or about 43% in the medium term.

In urban regions, there is some strongly suggestive evidence of the different implications of premium waivers and increases regarding agricultural labor supply. Regarding premium increases, we observe a strong shift to agricultural activities in the medium term (though this effect is not significant in the short term). In the medium term, individuals who received premium increases allocated more labor to agricultural activities, increasing by close to 3.9 hours in a week. Compared to pre-policy agricultural labor supply, this increase was about 49.4% and it was 44%. compared to the control group at baseline. However, premium waivers in urban areas had the exact opposite effect. Individuals who received waivers moved out of agriculture almost completely. We observed that receiving premium waivers led to a reduction in agricultural labor supply in urban areas by 117% compared to baseline. On the one hand, it is plausible that individuals in urban areas generally move out of the agricultural sector faster than in rural areas. However, these results might also point to the potential substitution effect. One scenario could be that individuals do not fully move out of agriculture but that they might provide much less agricultural labor if the income effect is greater than the substitution effect. Later we will show that premium waivers led to significant reductions in agricultural paid labor by 5.8% overall, but these reductions were concentrated in urban areas where premium waiver led to reduction in agricultural paid labor by 15% in the medium term while having insignificant effects in the rural areas.

Effects by Poverty Levels Finally, we explore the effect of the policy change by poverty status by running Equation 3 for poor and non-poor individuals separately, obtained through, dividing the samples based on poverty status (poor versus non-poor) as provided by the NISR. In Table 5 we show results in columns 1 to 4 for poor individuals and 5 to 8 for non-poor individuals. There are some slight differences. For instance, for non-agricultural hours, poor people reduce labor supply by about 2.9 hours in a week. The coefficient for non-poor individuals is negative though not significant. This implies that poor individuals are likely to respond to premium increases more than non-poor people regarding their non-agriculture labor supply decisions. However, regarding wage labor supply, we observe that the non-poor respond more than the poor. Non-poor individuals reduce wage labor supply by about 2.5 hours a week. The coefficient for poor people is significant at 10% level, but also smaller in absolute size (about 1.6 hours) and magnitude (about 16.2 %).

The effect of premium waivers is also somewhat mixed. For poor individuals, premium waivers have a negative effect across all measures though this is significant only for agricultural and wage activities (though both are only significant at the 10% level. For non-poor individuals, we observe a significant effect only for total labor supply but not for other measures.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	
			Poor		Non-poor				
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours	Total Hours	Agric Hours	Non Agric Hours	Wage Hours	
Premium increase x 2013	-0.376	0.201	-1.696	-1.203	0.635	0.609	-1.134	-0.806	
	(0.630)	(0.635)	(1.073)	(0.816)	(0.707)	(0.492)	(1.069)	(1.045)	
Premium increase x 2016	-0.935	-0.634	-2.892***	-1.627*	0.341	0.154	-1.792	-2.510**	
	(0.687)	(0.594)	(0.924)	(0.856)	(0.840)	(0.726)	(1.089)	(1.110)	
Premiun waiver x 2013	-1.393	-0.799	-1.074	-1.983	-0.370	-1.362	1.396	0.794	
	(0.984)	(0.771)	(1.817)	(1.404)	(1.166)	(1.004)	(2.119)	(1.652)	
Premium waiver x 2016	-1.713	-1.708*	-1.926	-2.408*	-2.621*	-0.742	-2.753	-2.823	
	(1.058)	(0.869)	(1.539)	(1.432)	(1.353)	(1.374)	(1.783)	(1.762)	
Constant	19.870***	19.949***	5.933**	6.015***	23.551***	22.091***	12.987***	12.976***	
	(1.332)	(1.285)	(2.901)	(1.646)	(1.824)	(1.634)	(2.979)	(2.196)	
Observations	29,732	28,551	16,311	21,003	47,509	40,022	30,716	25,185	
R-squared	0.077	0.070	0.120	0.119	0.164	0.074	0.243	0.286	

Table 5: Effect of premium changes between poor and non-poor individuals

 $Standard\ errors\ in\ parentheses.\ Significance\ levels\ correspond\ with\ *\ p<0.10\ for\ 10\%,\ **\ p<0.05\ for\ 5\%\ and\ ***\ p<0.01\ for\ 1\%,\ **\ p<0.01\ for\ 1\ for\ 1\ for\ 1\ for\ 1\ for\$

Quantile Regressions We now implement quantile regressions to assess the effect of the policy change along a data-driven continuum of welfare. We show these results in Supplementary Table A12. Starting with premium increases and total labor supply, we observe that by and large, premium increases do not influence total labor supply. Across all quantiles, there is a negative coefficient but it is only significant in the 70th percentile, where individuals reduce total labor supply by close to 1.3 hours a week as a result of premium increases. However, quantile regressions show changes in labor composition too as the effects are statistically significant in non-agricultural and wage labor supply.

The effect of premium increases on non-agricultural labor supply is concentrated among individuals in the middle of the welfare distribution and attenuates to insignificance by the 80th percentile. Short term labor supply reduction in non-agricultural activities increase from about 1.2 hours in the 30th percentile to about 2.4 hours in a week in the 50th percentile. In the medium term, non-agricultural labor supply reductions are more profound. Significant reductions were observed from the 30th until the 70th percentile, peaking in the 40th where

individuals reduce labor supply by about 3.6 hours a week. Premium waivers also significantly shift non-agricultural labor supply. Effects are observed between the 30th percentile (reduction of 2.9 hours) and the 70th percentile (3.3 hours' reduction). Effects of premium waivers peak in the 60th percentile where individuals reduce time worked by 4.1 hours a week. However, we observe an intriguing result in richest 90th percentile for premium waivers. At the very top of the distribution, it seems that premium waivers make people supply more non-agricultural labor. This could be a result of a very small number of observations at the very top which cause such an anomaly. But another interpretation might be that indeed, among the richest, who erroneously receive premium waivers, they do increase labor supply - potentially through self employment, consistent with studies elsewhere (Holtz-Eakin, Penrod, & Rosen, 1996; Lee, 2019).

Finally, we look at the effects on wage labor supply. Overall, the trajectory of wage labor supply is similar to that of non-agricultural labor supply. Both premium increases and premium waivers have an effect on wage labor supply. In the short term, significant effects emanating from premium increases are observed from the 40^{th} percentile with about 1 hour reduction over a week to the 60^{th} percentile where about 1.5 hours reductions was observed. In the medium term, the effects are larger in both magnitude and significance, from about 2.4 hours a week in the 40^{th} percentile all-through to 2.5 hours in the 90^{th} percentile. labor supply reductions peak in the 60^{th} percentile with about 3.5 hours reduction. The effect of premium waivers was significant between the 40^{th} and 60^{th} percentiles averaging between 3.2 - 3.5 hours per week.

4.2 Robustness and Sensitivity

4.2.1 Placebo/ Fake Treatments

To ascertain that our results are not happening by chance or simply trends overtime, it is important to conduct placebo tests (Eggers, Tunon, & Dafoe, 2023; Gertler, Martinez, Premand, Rawlings, & Vermeersch, 2016; P. H. Sant'Anna & Zhao, 2020). Placebo tests are increasingly applied in in empirical observational studies, including del Valle (2021), who assessed the effect of public health insurance on labor markets in Mexico. We conduct two placebo tests both in which we use a group that was not technically exposed to the policy change or health insurance and thus was not expected to change labor supply as a result of health insurance adoption or

premium policy changes. There should be no effect of the placebo treatment on the outcomes of interest, or if at all there is, it should not be in the same direction as as the observed treatment effects.

The first placebo treatment are individuals who had other types of insurance that were not affected by the policy change and therefore should not have had to react to the changed in premiums. There are three other types of insurance in Rwanda. The Rwandaise d'Assurance Maladie (RAMA) is an insurance program for the civil service implemented by the Rwanda Social Security Board (RSSB), the Military Medical Insurance (MMI) scheme provides insurance services to members of the military, police and other security services. In addition, other employer-based insurance programs are available. Combined, these insurance programs covered around 5.5 % of the general public in 2019, compared to about 77% covered by the CBHI (NISR, MOH, & ICF, 2021). Given that these programs were out of the scope of the law governing CBHI in Rwanda, the premiums policy change did not affect them. Moreover, these programs tend to cover the richest households (NISR et al., 2021) who most likely have different labor supply trajectories than poor households. We therefore expect ex ante that these households are more likely to have much larger reductions in agricultural hours since they are less likely to be employed in the sector but more likely to have higher non-agricultural and wage labor supply. In case of effects of the same magnitude as the ones we observe in our primary analysis or of the same direction, then our identification strategy is threatened. However, for agricultural labor supply, the lower (and significant) the placebo regressions are, the more likely our identification strategy is threatened and very large placebo estimates underline our exante expectation that individuals covered by these insurance scheme are less likely to be in agriculture and more likely to be moving out of agriculture sector. Supplementary Table A15 shows the results of this placebo regressions. The results indicate mull results in all the expected outcomes. For agriculture labor supply, the results indicate a large effect of other insurance scheme on agricultural labor supply. This is as expected. Notice that in our main regressions in Table 4, we do not observe a significant effect of insurance and of premium changes on labor supply. We can therefore comfortably suggest that our results are by and large driven by changes in insurance and changes in insurance premiums and not other unobserved changes in labor markets.

The second placebo treatment follows Abadie, Diamond, and Hainmueller (2010) to implement permutation tests in the form of synthetic controls for the treatment groups. The methods involves resampling from the non-exposed population and reassign those most likely to be affected by the treatment had they had the treatment as the fake treatment. The setup is as follows. As we show above, our treatments are conditional on having health insurance. Individuals who might be of a higher socioeconomic status but do not have insurance are therefore not exposed to the policy change and therefore are not expected to change their labor supply as a result of insurance premium changes. We therefore create another fake treatment by assigning higher socioeconomic status individuals (*Ubudehe* status of 3 and above) as a treatment and lower socioeconomic status individuals (*Ubudehe* status 1 and 2) as control. The expectation is that if there would be any effect, it should be in the opposite direction because higher socioeconomic status individuals most likely supply more labor than lower socioeconomic status individuals most likely supply more labor than lower socioeconomic status individuals most likely supply more labor than lower socioeconomic status individuals most likely supply more labor than lower socioeconomic status individuals most likely supply more labor than lower socioeconomic status individuals most likely supply more labor than lower socioeconomic status individuals most likely supply more labor than lower socioeconomic status individuals most likely supply more labor than lower socioeconomic status individuals most likely supply more labor than lower socioeconomic status individuals most likely supply more labor than lower socioeconomic status individuals most likely supply more labor than lower socioeconomic status individuals. Results presented in Supplementary Table A16 indicate null effects across all the measures of labor supply.

Finally, we can assume within the higher socioeconomic status (SES) households and the lower SES households, insurance enrolment might happen as in our sample. For households that would be insured in the lower SES households, they would receive a premium waiver while those that would be insured in the high SES households, they would receive a premium increase. Here, we just mimic our sample but only with the control households. We can therefore create synthetic controls following the Abadie et al. (2010) strategy and implement regression 3 to assess if there would be any effects my policy change dimension. As in the previous placebo specifications, the expectation is of null results. Results shown in Supplementary Table A17 show that indeed, there are no significant effects from the placebo. We can reject the hypothesis that effects observed are not related to the policy change or are simply trends over time in labor supply.

4.2.2 Including Imputed Treatment

As mentioned in the data description section, a substantial number of observations did not have an *Ubudehe* category. This was more especially at baseline in which about 24% were not matched by Sabates-Wheeler et al. (2015). In our main regressions, we exclude these observations and use only those which the key variable was observed. There could therefore be some concerns that the there are some systematic differences between the missing and observed

Ubudehe units and these differences might bias the results. Indeed, 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) with fewer livestock units, (iv) with fewer land parcels and (v) were more likely from urban areas. Furthermore, they were also younger and more likely to have internet. To test whether our results were sensitive to this missingness, we implemented a missing-at-random multiple imputation and use an ordered logit model to predict missingness. We then use the imputed data to re-run our key models to ascertain that our results are not sensitive. We show these results in Supplementary Table A18 and observe that the results remain largely unchanged and if anything, show even more significant effects regarding total labor supply and larger magnitudes for non-agricultural labor supply. We are therefore confident that our results are not sensitive to differential missingness in the main variable that determines the treatment allocation.

4.2.3 Analysis with Time-invariant controls

There are two concerns of our identification of casual estimates using repeated cross-sectional data. The first is the potential of compositional changes in groups (P. H. C. Sant'Anna & Xu, 2023) which can lead to regression to the mean where controls included in the model also respond to the treatment in other unobserved dimensions (Daw & Hatfield, 2018; Roth, Sant'Anna, Bilinski, & Poe, 2023). Previous studies have dealt with concerns of compositional changes in a number of ways including using multinomial generalised propensity scores (Hong, 2013; P. H. C. Sant'Anna & Xu, 2023) or inverse probability weighting (Stuart et al., 2014), as we have done here. However, we implement two additional strategies for dealing with this concern. First, we include trend absorption terms by including interactions of control variables and the time dummies as additional controls (La Ferrara & Milazzo, 2017). This helps that any differential changes in treatment emanating from how controls respond to the treatment are absorbed in the time trends. The second concern is the potential collider bias, where a number of controls included in the model might also be influenced by the treatment-determining variable, the Ubudehe ranking. This is likely to lead to instability in the treatment effects hence biasing the causal estimate. A strategy commonly used in the literature is sensitivity analysis such as the framework proposed by Oster (2019) and Diegert, Masten, and Poirier (2023). However, these sensitivity analysis methods are unsuitable for multi-value multi-period treatment designs such as ours. However, one strategy that is suitable for a case like outs and achieves both sensitivity and robustness goals is conducting parsimonious regressions without and controls and then in a hierarchical manner, introduce controls and observe the amount of change on the R^2 . This is a kin to implementing only time invariant controls as proposed by Zeldow and Hatfield (2021).

	(1)	(2)	(3)	(4)
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
Premium increase x 2013	0.149	0.159	-0.510	-1.247
	(0.539)	(0.449)	(0.963)	(0.945)
Premium increase x 2016	-0.225	-0.029	-2.783***	-2.445**
	(0.572)	(0.431)	(0.947)	(0.937)
Premium waiver x 2013	-1.116	-0.004	-1.946	-0.661
	(0.678)	(0.555)	(1.253)	(0.840)
Premium waiver x 2016	-1.332*	-0.810	-1.773*	-0.691
	(0.698)	(0.565)	(1.030)	(0.974)
Constant	19.899***	19.952***	9.543***	3.662***
	(0.571)	(0.499)	(1.170)	(0.781)
Observations	67,966	61,091	41,664	40,421
R-squared	0.078	0.038	0.119	0.134

 Table 6: Using only Time-invariant controls

Time-invariant controls include age cohort dummies, rural/urba location, month and district fixed effects. Robust standard errors in parentheses. Significance levels corresponding to *** p<0.01 for 1% ** p<0.05 for 5% and * p<0.1 for 10%

In our case, the time-invariant controls are the age cohorts, rural/urban location dummy, month and district fixed effects. Limiting our controls to only these, and implementing regression model (4) above, we show results in Table 6. The results reveal that almost all the effects observed in our preferred model of the full set of controls hold in the time-invariant model. The only major difference is the effect of premium waivers on wage labor supply which is no longer significant in the time-invariant model. To a large extent, compositional changes

therefore do not seem to bias our results. In addition, we find that our results are not sensitive to potential confounding controls or collider bias. We do not conduct the formal sensitivity checks by Oster (2019) but instead observe the changes in the R^2 with the addition of controls. In all the four main regressions, the increase in R^2 emanating from additional controls (δ) is between 75% and 87%.

4.2.4 Panel Data Analysis

The EICV dataset has a small panel component, where we can identify panel units in 2010 and 2013 surveys of which around 2600 individuals are observed in two waves. We use these data to replicate the short term results in the main analysis. Our intention of assessing if the direction of effects and significance levels are the same, i.e null findings, as in the main results. We thus run the following panel fixed effects model.

$$Y_{ijtkm} = \tau^{gps} + \beta_1 Increase_{ijtkm} + \beta_2 Waiver_{ijtkm} + \beta_3 Post_{it} +$$

$$\vec{X}_{ijt}\beta_6 + Increase_{ijtkm} * Post_{it}\beta_7 + Waiver_{ijtkm} * Post_{it}\beta_8 + \mu_k + \varsigma_m + \varepsilon_{ijtkm}.$$
(4)

The main difference between these panel models and the pooled cross-sectional models is that the weights used (τ^{gps}) are only from the baseline since units are the same in both rounds. In addition, while the base control variables are the same as in the pooled cross-sections, we do not include trend absorption terms since the units also do not change. The main results shown in Table 7 indicate that as in the pooled cross-sectional analysis, there were no significant overall short term effects of the policy change on labour supply. Furthermore, classifying by gender, we do not find key differences between pooled cross-sectional analysis and panel analysis further underlining the consistence of our estimates.

4.3 Potential Pathways

In this sub-section, we postulate a number of ways in which the effect of premium changes could affect labor supply. We conduct regressions similar to Equation 3 with several secondary outcomes.

	(1)	(2)	(3)	(4)
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
Premium Increase x 2013	1.125	1.017	1.253	-3.529
	(1.511)	(1.419)	(2.064)	(2.402)
Premium waiver x 2013	2.124	0.305	0.914	-1.068
	(1.664)	(1.329)	(2.385)	(2.319)
Constant	13.515**	10.868*	-1.446	11.202
	(6.011)	(6.340)	(16.001)	(12.053)
	• • • •	• • • • •		
Number of observations	3,892	3,648	1,730	2,036
Baseline control mean	25.66	17.90	20.32	12.75

Table 7: Panel data results of effect of premium changes

All regressions include the full set of controls. Robust standard errors in parentheses. Significance levels corresponding to *** p<0.01 for 1% ** p<0.05 for 5% and * p<0.1 for 10%

Increasing Domestic Time Use and Potential Leisure: An essential inquiry arises: if individuals decrease their overall labor supply, where do they allocate their potential hours of work? They are likely to increase time in domestic work or leisure; here we can investigate time dedicated to domestic activities. To test these hypotheses, we conduct regressions of policy change treatments on the number of hours devoted to domestic activities, the probability of self-employment, and the likelihood of engaging in unpaid care work.

Regrettably, there were some inconsistencies in the available data pertaining to domestic work. Typically, domestic work encompasses activities such as searching for cooking fuel (firewood or making charcoal), tending to livestock, fetching water, cooking, shopping for household items at the market, and performing other household chores including childcare. All these activities were consistently reported for the baseline and short term, but not for the medium term. In the medium term, only the time spent on searching for cooking fuels, tending to livestock, and cooking was included. Consequently, we establish two variables: a short-term outcome variable for domestic labor supply and a medium-term outcome variable that employs cooking, livestock care, and fuel searching as proxies for domestic labor supply.

We present these findings in Supplementary Table A19, illustrating that indeed, individuals tend to allocate more time to domestic, mostly unpaid, activities. In the initial three models of Supplementary Table A19, we outline the short-term assessment of total domestic time for the entire sample (Model 1), men (Model 2), and women (Model 3), respectively.
Overall, we observe that individuals exposed to premium increases spend an additional 1.2 hours, approximately 5.7%, engaged in domestic activities compared to the control group. Examining this pattern by gender, we note that men respond to both premium increases and premium waivers. Men subjected to premium increases devote roughly 1.3 hours more to domestic activities, constituting approximately 12.5% of their pre-policy change levels. Moreover, men subject to premium waivers allocate even more time to domestic tasks, spending approximately 1.8 additional hours, or around 16.3% more time in domestic activities. As for women, significant effects are solely observed for premium increases, with an increase of approximately 1.3 hours per week in domestic activities.

Models 4, 5, and 6 in Supplementary Table A19 present the outcomes of domestic time use evaluated based on only three of the six questions. Remarkably, the observed trends remain consistent across these results. In the medium term, premium increases continue to drive a positive increase in domestic labor supply for both men and women, while premium waivers have a more pronounced effect on men than on women. The key implication of these results is that premium increases divert individuals from productive work towards unpaid household labor.

Affecting the Probability of Employment: Secondly, we explore whether obtaining insurance or being impacted by the diverse facets of the policy change influences employment. If the income effect dominates, we anticipate individuals work fewer hours, consequently decreasing their likelihood of employment. Similarly, if a manipulation effect is at play, individuals would also decrease their labor supply, initially by reducing their probability of employment. The results presented in Supplementary Table A20 depict the impact of insurance acquisition on employment across different dimensions. Overall, we do not discern any significant effects of insurance acquisition on employment.

The results presented in Supplementary Table A21 examine the impact of the policy change. In Panel 1, we observe the effects of waivers and increases across various mediator measures of employment for the entire sample, while Panel 2 and Panel 3 depict these effects separately for men and women, respectively. Overall, premium waivers decrease the probability of wage employment and the likelihood of supplying agricultural labor for wages by 4% and 6%, respectively. When disaggregated by gender, we find that premium waivers lead to a 4% reduction in the probability of employment for both men and women. Additionally, for men, there is a 6.4% decrease in the likelihood of being employed in a wage job and a similar decrease of about 6% in supplying agricultural labor. Even in the short term, premium waivers result in a 5.6% reduction in securing a wage job. While we do not observe the same effects for women, overall, premium waivers also reduce the probability of supplying agricultural wage labor by approximately 5.2%.

Demand for Additional Employment: If individuals are either benefiting from an income gain/ income effect due to health insurance and the related financial protection and income effect (Nyman, 2008), they are more likely to reduce their demand for additional employment (in either additional jobs or additional hours of work). Two rounds of our data allow us to test for this. Results in Supplementary Table A22 show these results. Panel 1 of the table shows results of the effect of health insurance adoption. Acquiring health insurance reduced the probability of desiring an additional job by about 2.6% with men reducing their desire of an additional job by a larger proportion (3%) compared to women (2.4%). Panel 2 shows the effect of the policy change. We observe that indeed, in the short term, there is evidence of reduced probability of seeking an additional job. This was mainly driven by individuals exposed to premium increases for whom the probability of desiring an additional job was lower by 3.3%. For these individuals, the decline is more or less similar across men and women. For premium waivers, we did not observe an overall effect on desiring additional employment. However, this was because almost all the effect was driven by men. For men, receiving premium waivers reduced the probability of desiring an additional job by 5%.

5 INTERPRETATION OF RESULTS

5.1 Conceptual Framework

To interpret the effects of changes in health insurance premiums on labor supply (and wages), we establish a microeconomic model rooted in the work of Saez (2010) and Shi (2016). This model accounts for endogenous household responses to shifts in health insurance premiums, considering both subsidies and penalties.⁷

⁷While 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 labor effort z_i . The latter may relate to the hours worked or any other labor 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

$$v_i(I_i, z_i) = I_i - \frac{a_i}{1 + 1/\epsilon} \left(\frac{z_i}{a_i}\right)^{1 + 1/\epsilon}$$
(5)

where ϵ is the labor supply elasticity, which is constant due to the iso-elasticity assumption. Given the individual's ability a_i , he chooses labor supply z_i to maximise utility u_i . The latter term, hence, captures the disutility from labor, 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 labor supply ⁸ and a transfer T_i , which represents a subsidy or penalty related to the health insurance premium. Income is then given by

$$I_i = W_i + \iota T_i \tag{6}$$

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 labor with wage rate w:

$$W_i = wz_i + e_i \tag{7}$$

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

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* categorization ranking. If

⁸Note that in a subsistence economy, wage income can also be represented by the value of subsistence production.

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

$$T_{i} \equiv \begin{cases} s_{i} & \text{if } \iota = 1 \quad \text{and} \quad W_{i} \leq I^{*} \\ -t_{i} & \text{if } \iota = 1 \quad \text{and} \quad W_{i} > I^{*} \\ 0 & \text{if } \iota = 0. \end{cases}$$

$$\tag{8}$$

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

$$Eu_i(z_i \mid \iota = 1) = wz_i + e_i + [P(W \le I^*)s_i + (1 - P(W \le I^*))t_i] - \frac{a_i}{1 + 1/\epsilon} \left(\frac{z_i}{a_i}\right)^{1 + 1/\epsilon}$$
(9)

where $wz_i + e_i$ is the expected wage income, the last term is the disutility from labor, and the term in the middle captures the expected utility from receiving a transfer.⁹ The equation further implies that the individual chooses labor supply before learning about the shock realisation e_i and, hence, cannot know with certainty whether the insurance penalty or subsidy applies.

Equation (9) highlights how a health insurance policy can affect an individual's labor 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 labor supply is given by

$$w = \left(\frac{z_i}{a_i}\right)^{1/\epsilon}.$$
(10)

From this equation, and assuming that the wage rate is constant w = 1, it follows that labor 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.

⁹An individual not covered under the health insurance scheme receives his wage income (7) with certainty.

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

$$w = \left(\frac{z_i}{a_i}\right)^{1/\epsilon} - \left[\frac{\partial P(W \le I^*)}{\partial z_i}s_i + \left(1 - \frac{\partial P(W \le I^*)}{\partial z_i}\right)t_i\right]$$
(11)

which highlights that an individual will take into consideration the effect his labor 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 labor 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.

In order to see this intuition holding in our framework, consider an individual with ability a_i , whose known labor income wz_i is close to the income threshold I^* (see equation (7)). Since he does not know the shock realisation e_i yet, the individual may fall below or above the threshold I^* . Denoting the first derivative of $P(W \le I^*)$ with respect to labor effort z_i as ϕ we can show that the policy change will lead to a decrease in labor supply under the following condition

$$\frac{\phi}{1-\phi} > \frac{t}{s} \tag{12}$$

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

5.2 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. Individuals reduced labor supply in non-agricultural and wage activities. Our findings here are consistent with Garcia-Mandico et al. (2021) in Ghana. The key contribution of this paper however is on premium reforms and labor supply. The plausibly flawed targeting method accompanied by community and administrative stakes to under-report or over-report welfare implied that the process of targeting was quasi-random and plausibly exogenous. We observe that both premium increases and premium waivers reduced labor supply in substantial magnitudes.

Our findings here can be interpreted in two dimensions. The first one is that using the income effect hypothesis, providing waivers to some households was a form of transfers. Moreover, gaining waivers also meant waived co-payments of health costs hence increasing the magnitude of the transfer substantially. Our theoretical model in Section 5.2 therefore makes consistent predictions that such an income effect might reduce the propensity to supply labor. We thus observe that waivers were associated with lower probability of having a wage job or supply agricultural labor, more prominently for men. The second direction of our analysis is on the substitution effect for individuals who receive premium increases. We prefer referring to this as 'manipulation effect'. Manipulation happens when individuals under-report their income or any other welfare measure in order to stay or become eligible for the waivers or subsidies. Our results are consistent with recent research from high-income countries which shows this kind of manipulation around thresholds (Heim, Hunter, Isen, Lurie, & Ramnath, 2021; Shi, 2016).

Our theoretical model suggests that this kind of manipulation is a behavioural response is due to the targeting mechanism employed 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 therefore manipulable by individuals. This is not unique to our study. Indeed, many empirical studies assessing the efficacy of other communitytargeting 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 the case of cash transfers). Given than the same targeting process is used to identify beneficiaries for a wide range of social programs, including school scholarships, cash transfers and public work programs as well as health insurance waivers and step-wise premiums (Ezeanya, 2015), it is likely that such manipulation is widespread. In our regressions, we control for the largest of these other social protection programs. We are therefore confident that our estimates are driven by the health insurance policy reform and not other programs.

Potentially, what gives even more policy weight to our results is recent changes by the government in the measurement and use of *Ubudehe* classification. First, in 2020, the government updated the methodology again, moving from a four-tiered welfare scale to a 5-tiered scale in an effort to better ascertain the poor Ntirenganya (2020). However, question still remain about the comprehensiveness of the assessment with about 40% of households reporting missing the assessment (Mutanganshuro, 2020). Second, in a bold move, the government scrapped using the classification altogether (Nkurunziza, 2023b). In the place of *Ubudehe* categorization, the government proposed a new social registry system (Nkurunziza, 2023a, 2024). Researchers and observers will be looking forward to assessing how the new system works and the impacts it will create. For this and other questions, the research interest remains potent.

Some limitations Lastly, we discuss one limitation of our analysis, which 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. To overcome this limitation, we exploited the small panel dataset that follows the same individuals. The analysis here agrees with the pooled cross-sectional results and confirms the findings. However, the results of the panel should be taken with some caution. While the panel baseline included about 2000 households, (NISR, 2018a), we were only able to correctly match about 1300 households and about 2600 individuals to a follow-up. Panel deprecation is therefore an issue we could not overcome. Secondly, the panel was only present for the short term and not the medium term. We were therefore unable to replicate the whole pooled cross-sectional analysis in the panel form.

6 CONCLUSION

This paper studies the effects of health insurance enrolment and premium changes on labor 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 labor 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 while comparing both with those who were uninsured at a given point in time. 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 labor supply especially non-agricultural and wage-related labor supply for men and rural-based individuals. This reduction in labor supply was more likely driven by manipulation behaviour as individuals likely adjust their reporting of welfare levels to gain eligibility for premium waivers. Premium waivers generally led individuals, especially men, to reduce labor 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. Additional savings from insurance co-payments likely relax household budgets even more.

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 that might use thresholds for welfare programs.

Acknowledgements. We are grateful to Katrine Løken and three anonymous reviewers from the *Review* of Economics Studies, Daniela Scur, Trinh Pham, Elizabeth Bageant, Christopher Barrett, Matthias Rieger, Lee Crawfurd, Sylvan Herskowitz, Kibrom Abay, Pamela Jakiela, Manuela Angelucci, Magdalena Bennett, Raissa Fabregas, Taryn Dinkelman, Zoe McLaren, Owen O'Donnell, Kati Krähnert, participants at the Nordic Development Economics Conference 2021, European Health Economics PhD Conference and 2022 EuHEA Seminar Series 2022, EEA Meeting 2023, RES Conference 2023 and German Health Economics Association

Conference 2024. We thank the National Institute of Statistics of Rwanda (NISR) for providing the data and specifically to Serge Mugabo for his support in data clarifications. We are very grateful to Emily Wylde for providing the 2010 Ubudehe database matched data. All errors remain exclusively of the authors.

Disclosure Statement: This project was funded through a University of Bonn Argelander Early Career Research Grant to Emmanuel Nshakira-Rukundo. Emmanuel also benefited from the IFPRI-Cornell University STAARS mentorship program. All the other co-authors have nothing to disclose.

Online Appendix for: HEALTH INSURANCE PREMIUM CHANGES AND LABOR SUPPLY: EVIDENCE FROM A LOW INCOME COUNTRY

Supplementary Tables and Figures

	2005	2010	2013	2016
Quintile 1	30.00	52.90	55.91	60.60
Quintile 2	33.60	61.40	63.22	66.87
Quintile 3	43.00	69.30	67.44	73.04
Quintile 4	46.30	74.50	73.00	77.07
Quintile 5	37.80	84.50	68.23	70.24
All CBHI	38.14	68.52	65.56	69.56
Other Insurance	5.30	3.30	4.01	4.62
Uninsured	56.6	28.18	30.43	25.82

Table A1: Insurance participation levels across socioeconomic quintiles (2005-2016)

Source: (NISR, 2006, 2012, 2015a, 2018b)

Table A2: Characteristics of different	Ubudehe categories	over the study peri	iod
----------------------------------------	--------------------	---------------------	-----

	2008 and 2013 Ubudehe classification		2015 Ubudehe classification
1	Destitute. Need to beg to survive. Have no land or livestock.		
1	Lack adequate shelter, clothing and food.	1	Families that do not own a house and can hardly afford basic needs.
2	Physically capable of working on land owned by others,		
2	although they themselves have either no land, or very small landholdings, and no livestock.		
3	Have some land and housing; live on their own labour and produce,	2	Those who have a dwelling of their own or are able to rent one but rarely get full-time jobs
5	though they have no savings; they can eat, even if the food is not very nutritious.	2	Those who have a dwelning of their own of are able to tent one but fatery get fun-thine jobs
	They have small ruminants and their children go to primary school:		Those who have a job and farmers who go beyond subsistence farming to produce
4	have a few animals and petty income to satisfy a few other needs	3	a surplus which can be sold. The latter also includes those with small and medium
	nuve a few animals and perty meenie to satisfy a few other needs		enterprises who can provide employment to a larger number of people.
	Have larger landholdings with fertile soil and enough to eat;		
5	have livestock, often have paid jobs and can access health care;		Those who own large-scale businesses individuals working with international
5	employ others on their own farms and at times have access to	4	organisations and industries as well as public servants
	paid employment; have some savings		organisations and industries, as wen as public servants
	Have land and livestock and often have salaried jobs;		
6	have good housing, often own a vehicle, and have enough		
	money to lend and to get credit from the bank; many migrate to urban centres		

Effect of Health Insurance Enrolment

Whereas our main focus of the this paper is understanding the effect of health insurance premium policy change, it is important to highlight thatthe policy change worked only through first acquiring health insurance. Since we have only pooled cross-sectional data, we are not able to observe transitions in states of health insurance enrolment and only observe health insurance status at a point in time. It is therefore useful to also assess the effect of acquiring health insurance in itself. In this case, this part of the study is more in line with existing studies that assessed only the effect of health insurance enrolment on labour markets del Valle (2021); Frölich and Landmann (2018); Garcia-Mandico et al. (2021); Landmann and Frölich (2015); Le et al. (2019b); Liao (2011); Luo and Escalante (2020); Molina-Vera (2021).

First, we use a logistic regression to determine the probability of enrolling in health insurance. We then construct the inverse probability weights of the first-stage estimated probabilities and then plug them the second-stage difference-in-difference weighted regression. The main outcome regression is as follows.

$$Y_{ijtkm} = \tau^{ipw_ps} + \beta_1 Insurance_{ijtkm} + \beta_2 Post_{it} + \beta_3 Post_{it} * Insurance_{ijktm} + \vec{X}_{ijt}\beta_4 + \vec{X}_{ijt} * Post_{it}\beta_5 + \mu_k + \varsigma_m + \varepsilon_{ijtkm}.$$
(13)

Where, $Y_{ij}ktm$ is the labor 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} represents two time dummy variables for 2013 (short term effect) and 2016 (medium term), taking the value 0 in the baseline (2010). \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 labor supply, we include μ_k , representing district fixed effects, and ς_m are month fixed effects that capture unobserved determinants of labor 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 labor 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 vector $\vec{X_{ijt}} * Post_{it}$ is the interaction of all controls with time dummies to absorb and differences in time trends. Supplementary Table A3 and Figure A1 indicate the matching efficiency is attained.

Variable	Mean/(SE)	Mean/(SE)	Mean difference
Sex (1= male)	0.455	0.454	0.002
	(0.004)	(0.002)	
15-25 years	0.288	0.280	0.008**
-	(0.003)	(0.002)	
26 - 35	0.266	0.272	-0.007*
	(0.003)	(0.002)	
36-45	0.195	0.199	-0.004
	(0.003)	(0.002)	
46-55	0.147	0.147	-0.000
	(0.003)	(0.002)	0.000
>=56 years	0 104	0.102	0.003
2-50 years	(0.003)	(0.001)	0.005
Primary school	0.600	0.701	0.003
Timary school	(0,004)	(0.002)	-0.005
Married	(0.004)	(0.002)	0.012***
Married	0.393	(0.008)	-0.013
a :	(0.004)	(0.002)	0.000
Savings account	0.179	0.181	-0.002
	(0.003)	(0.002)	0.005
Tontine membership	0.261	0.266	-0.005
	(0.003)	(0.002)	
Poor health (14 day-recall)	0.177	0.182	-0.005*
	(0.003)	(0.002)	
Taken a loan	0.713	0.714	-0.001
	(0.004)	(0.002)	
Internet connection	0.091	0.088	0.003
	(0.003)	(0.001)	
Radio, TV or computer	0.608	0.615	-0.008*
, <u>i</u>	(0.004)	(0.002)	
Household size	5.285	5.269	0.015
	(0.017)	(0.009)	
Has electricity	0.637	0.634	0.003
	(0.004)	(0.002)	
Number of agricultural parcels	2.845	2.893	-0.049*
rumber of agricultural pareets	(0.023)	(0.011)	0.049
Total livestock units	(0.023)	0.600	0.013
Total Investoek units	(0.023)	(0.009)	0.015
Even encolled in VUD are shown	(0.010)	(0.004)	0.002
Ever enrolled in vor program	(0.002)	(0.125)	0.002
	(0.003)	(0.001)	0.002
Poorest (Q1)	0.195	0.192	0.003
	(0.003)	(0.002)	0.000
Poor (Q2)	0.205	0.203	0.002
	(0.003)	(0.002)	
Average (Q3)	0.207	0.207	-0.000
	(0.003)	(0.002)	
Rich (Q4)	0.207	0.209	-0.001
	(0.003)	(0.002)	
Richest (Q5)	0.185	0.189	-0.004
	(0.004)	(0.002)	
Time to hospital in minutes	167.545	167.566	-0.021
	(0.801)	(0.477)	
Time to market in minutes	54.576	54.579	-0.003
	(0.280)	(0.178)	
Ubudehe miss	0.120	0.121	-0.002
e eudene_mbs	(0.002)	(0,001)	5.002
poor housing	0.896	0.807	-0.001
poor_nousing	(0.002)	(0.001)	-0.001
Urban anymorphism area (2002)	(0.002)	0.120	0.003
Utban enumeration area (2002)	0.142	0.139	0.005
N	(0.003)	(0.001)	00070
IN	23569	57409	80978

Table A3: Covariate balancing for health insurance (CBHI) enrollment

 $\label{eq:standard} \hline Standard errors in parentheses clustered at Sector level. Significance levels corresponding to *** p<0.01 for 1% ** p<0.05 for 5% and * p<0.1 for 10% \\ \hline$



Figure A1: Matching efficiency between control (no CBHI) and treatment (CBHI) groups across survey rounds

Probability of insurance

The results are presented in Figure A2, where the different sub-figures correspond to the specific specifications for each outcome variable. Table A3 is the tabular format of these results. We show in Figure A1 in the Online Appendix that the matching efficiency is achieved, evidenced by the density graphs of before and after weighting in each period.



Figure A2: Effect of insurance enrolment on labor supply

Starting with a short term assessment, we observe that between 2010 and 2013, acquiring health insurance did not change individual labor allocation by statistically significant margins. Though there are negative coefficients for non-agricultural and wage labor supply, these are insignificant. But in the medium-term assessment (2010 -2016), there was a statistically significant negative association between health insurance and labor supply to non-agricultural and wage activities. Regarding non-agricultural activities, we observed that acquiring health insurance led to a reduction in non-agricultural labor supply by more than 2.2 hours per week. For wage activities, we also observe that labor supply reduced by 2.1 hours per week. In the medium term, non-agricultural labor supply reduced by 19% of the baseline mean and 21% of the control mean. For wage activities, it reduced by 20% in of the baseline mean and about 17% of the control mean.

Sub-Group Effects of Health Insurance Enrolment

We consider three levels of heterogeneity: gender, location (rural or urban residents) and levels of poverty. We thus implement regression 13 separately for the different sub-groups of interest.

	(1)	(2)	(3)	(4)
VARIABLES	Total Hours	Agric Hours	Non-Agric Hours	Wage Hours
CBHI x 2013	0.044	0.137	-1.173	-0.729
	(0.483)	(0.379)	(0.814)	(0.649)
CBHI x 2016	-0.435	-0.228	-2.209***	-2.129***
	(0.566)	(0.456)	(0.807)	(0.725)
Constant	26.473***	18.584***	18.904***	15.893***
	(1.194)	(0.935)	(1.937)	(1.449)
Observations	76,884	68,242	46,825	46,095
R-squared	0.157	0.069	0.239	0.270
Mean of dep	27.77	16.31	11.37	10.73
Mean of dep (control)	27.46	16.85	10.49	12.26
% Baseline Mean (short term)	0.002	0.008	-0.103	-0.068
% Baseline mean (medium)	-0.016	-0.0140	-0.194	-0.199
% Control mean (short term)	0.002	0.008	-0.112	-0.059
% Control mean (medium)	-0.016	-0.014	-0.211	-0.174

Table A4: Main results showing the effect of insurance enrolment on labour supply

All models include the full set of controls shown in Supplementary Tables A3 and A9. Standard errors in parentheses clustered at Sector level. Significance levels corresponding to *** p<0.01 for 1% ** p<0.05 for 5% and * p<0.1 for 10%

Effects by Gender Supplementary Table A5 shows results of the gender assessment. The key finding is that most of the effects are observed for men. Though the results shown in Figure A2 above indicate insignificant results in the short term, when the sample is separated between men and women, we observe that men reduced wage labour supply by about 1.5 hours in a week while we do not observe any effects for women. The reduction was equivalent to about 9.3% of wage time for men.

In the medium term, we observe that in both non-agricultural activities and wage activities, labour supply reduced significantly for men. For non-agricultural activities, non-agricultural activities reduced by about 1.5 hours a week, about 8.7% of total non-agricultural labour supply for men. For wage activities, the labour supply reductions increased further to more than 2.5 hours per week. This labour supply reduction in wage activities was equivalent to about 15.8% of total wage time reduction compared to baseline. For women, we did not observe a lot of significant labour supply reduction except for wage activities in the medium term. For this, we found that women reduced wage activities labour supply by slightly over 1.1 hours in a week, equivalent to about 17% of women's labour supply to wage activities.

	(1)	(2)	(3) Men	(4)	(5)	(9)	(7) Vomen	(8)
VARIABLES	Total Hours	Agri Hours	Non Agric Hours	Wage Hours	Total Hours	Agri Hours	Non Agric Hours	Wage Hours
CBHI x 2013	-0.219	0.732	-0.357	-1.492**	0.181	-0.205	0.249	0.607
	(0.610)	(0.503)	(0.506)	(0.712)	(0.575)	(0.456)	(0.561)	(0.537)
CBHI x 2016	-1.138	0.512	-1.476**	-2.532**	0.209	-0.339	-0.784	-1.118*
	(0.819)	(0.569)	(0.630)	(0.993)	(0.589)	(0.496)	(0.659)	(0.573)
Constant	31.827^{***}	16.465^{***}	10.403^{***}	19.705^{***}	24.317^{***}	14.495***	9.063***	8.246***
	(1.759)	(1.158)	(1.388)	(1.685)	(1.241)	(1.069)	(1.254)	(1.134)
Observations	34,758	34,758	76,884	34,758	42,126	42,126	42,126	42,126
R-squared	0.135	0.123	0.318	0.137	0.141	0.174	0.333	0.217
All models incl parenthesis) are	lude the full se clustered at ye	t of controls sh ear by district.	lown in Table A1 in Significance levels o	the Online App correspond wit	pendix, plus m h *** p<0.01	onth and distri for 1 %, ** p<	ct fixed effects. Star 0.05 for 5% and $*_{\rm I}$	ndard errors (in ><0.1 for 10%.

Table A5: Effect of health insurance enrolment by gender

Effects Across Rural and Urban Localities Rural and urban inhabitants are likely to have varying labour allocations given that the concentrations of their employment is likely different from urban residents. We use the rural – urban categorisation to divide the sample into urban and rural residents and implement regression 13 accordingly. Supplementary Table A6 shows these differences.

Results show that the effects are fully concentrated in rural areas. In both the short and medium terms, we did not observe any significant effects in among urban residents. If anything there may be a tendency to increase labour supply, though this is never significant. In rural areas, we observe significant effects only in the medium term and non-significant effects in the short term. Total labour supply reduced in the medium term by just under 1 hour, significant at a 10% level. This, in the context of baseline labour supply was equivalent to about a reduction in labour supply of about 3.8%. Rural non-agricultural time reduced by slightly over 2 hours, equivalent to about 24.8% of total rural non-agricultural time at baseline or about 27.7% of rural non-agricultural labour for the uninsured individuals. In the same direction as non-agricultural labour allocation reductions, rural residents also reduced wage labour supply substantially in the medium term. Wage labour supply reduced by about 2.4 hours per week. Given than rural wage labour supply starts from a low value (about 10.7 hours per week), a reduction of 2.4 hours is of great magnitude. This was equivalent to 27.9% of baseline wage labour supply and for the control group at baseline, this was about 24% reduction.

Effects by Poverty Levels To assess the effects by poverty status, we were interested in understanding the differential impacts across the income poverty continuum. For this we recategorize the National Institute of Statistics of Rwanda's three poverty categories, namely; extremely poor, poor and non-poor (NISR, 2012, 2015b, 2018b). into a binary indicator that shows poor households (either poor or extremely poor) or non-poor. Running the same regression 13 by these sub samples, we observe that to a large extent, the results are mainly driven by the poor sub-sample. In Table A7, we find that for non-poor households in the medium term, wage labour supply reduced by 1.75 hours. This was about 14.7% of baseline wage labour supply or 16.4% of baseline wage labour supply for non-poor households. For poor households, we observed agricultural labour supply non-agricultural labour supply and wage labour supply all declined in the medium term. For non-agricultural hours, labour supply reduced by close to 1.4 hours while wage labour supply reduced by close to 1.5 hours. We also observe that for poor people, there was a reduction in agricultural labour supply by about 0.86 hours in a week, significant at a 10% level. These results are however to be taken with some caution as we later show in the placebo results do not hold.

Quantile Regressions As we mention above, the poverty measures used are NISR computations from each cross-section. One alternative for this is implementing quantile regressions that can show distributional effects across the continuum of within data welfare.

	(1)	(2)	(3)	(4)	(5)	(9)	(7)	(8)
		1	Jrban			, ,	Rural	
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
CBHI v 2013	1 138	0 737		0 160	975 0	0.731	0.501	0 773
	001.1	701.0	0.444	001.0	070-0-	1.62.0		0.110-
	(2.179)	(0.856)	(2.164)	(1.894)	(0.423)	(0.379)	(0.411)	(0.463)
CBHI x 2016	3.193	1.174	2.132	1.526	-0.993*	0.038	-2.027***	-2.416***
	(2.328)	(0.843)	(2.269)	(1.477)	(0.551)	(0.467)	(0.625)	(0.674)
Constant	27.198^{***}	14.023^{***}	16.764^{***}	11.522^{***}	24.803^{***}	16.658^{***}	7.297***	10.022^{***}
	(3.159)	(3.092)	(3.663)	(3.856)	(1.298)	(0.937)	(1.262)	(1.126)
Observations	9,987	9,987	9,987	9,987	66,897	66,897	66,897	66,897
R-squared	0.203	0.223	0.304	0.266	0.114	0.079	0.249	0.145
All models incl parenthesis) are	ude the full se clustered at ye	t of controls sho ear by district. S	own in Table A1 in Jignificance levels c	the Online Apl orrespond with	pendix, plus m 1 *** p<0.01 f	onth and distric or 1 %, ** p<0	ct fixed effects. Star 0.05 for 5% and * p<	idard errors (in <0.1 for 10%.

Table A6: Effect of health insurance enrolment by location

	(1)	(2) N	(3) 00-000r	(4)	(5)	(9)	(7) Poor	(8)
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
CBHI x 2013	0.573	0.495	0.022	0.010	-0.631	-0.062	-0.706	-0.788
	(0.635)	(0.420)	(0.728)	(0.701)	(0.566)	(0.537)	(0.482)	(0.563)
CBHI x 2016	0.081	0.756	-1.170	-1.753**	-0.831	-0.861*	-1.387**	-1.469**
	(0.742)	(0.592)	(0.773)	(0.781)	(0.616)	(0.491)	(0.643)	(0.661)
Constant	25.460***	16.778^{***}	9.125***	9.818^{***}	17.842^{***}	18.578 * * *	-0.844	5.239***
	(1.493)	(1.035)	(1.762)	(1.154)	(1.238)	(1.099)	(0.964)	(0.995)
Observations	47,160	47,160	47,160	47,160	29,724	29,724	29,724	29,724
R-squared	0.169	0.174	0.326	0.248	0.069	0.085	0.231	0.112
Standard errors correspond with	s are clustered a start p<0.01 f	at year by distrior 1% , ** p<0	ct. Controls include 1.05 for 5% and * p<	the full list in <0.1 for 10%.	Table A3 and 1	nonths and dis	tricts dummies. Sigr	nificance levels
-	-	•	•					

statu
poverty
by
of health insurance
Effect
A7:
Table

This then allow us to observe if there are different effects across the various segments of the distribution. We implement quantile regressions using the improved and fast process by Chernozhukov, Fernández-Val, and Melly (2022) and present the results in Supplementary Table A8. The results indicate that by and large, within total labour supply, agricultural labour supply, there is no distributional effects across the welfare continuum. We did not observe any significant effects in any welfare quantile. However, regarding non-agricultural and wage employment labour supply, we observe clear distributional effects with individuals in the middle of the distribution having large labour supply reductions compared to those on the edges. Regarding non-agricultural labour supply, we observed medium term reductions from the 30th to around the 80th percentile, with a peak observed between the 40th and 50th percentiles. Regarding wage labour supply, we observe significant effects of health insurance acquisition start trickling in in the 60th percentile where individuals reduced labour supply by about 2.2 hours. Labour supply reduction for wage activities peak in the 80th percentile where individuals reduced labour supply by close to 3.3 hours in a week.

The results agree, to a large extent, with the ones in Table A7 where poverty is determined from outside the data and imposed; this showed larger reductions in hours devoted to wage labour among non poor households. The quantile results show that, by and large, households starting from the 60th percentile on-wards respond to wage time reductions more than poorer households. These findings indeed lend weight to the income effect hypothesis, that gaining insurance likely reduced out-of-pocket health spending (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.

Additional Tables: Effect of Premium Changes

		(1)	(2)	(3)	(4)
Percentile		Total Hours	Agric Hours	Non-Agric Hours	Wage Hours
10 th	CBHI x 2013	0.0302	3.07e-12	3.30e-13	2.37e-11
10		(0.445)	(0.0932)	(0.0385)	(0.0291)
	CBHI x 2016	-0.181	-8.36e-12	3.96e-13	-3.45e-12
		(0.490)	(0.0903)	(0.164)	(0.0419)
20 th	CBHI x 2013	-0.720	0.0997	9.00e-13	7.70e-13
20		(0.607)	(0.209)	(0.105)	(0.0992)
	CBHI x 2016	-0.863	0.111	3.16e-12	-9.52e-13
		(0.717)	(0.202)	(0.212)	(0.141)
30 th	CBHI x 2013	-0.448	0.225	-0.0232	-4.57e-14
50		(0.572)	(0.308)	(0.0956)	(0.0761)
	CBHI x 2016	-0.607	0.0914	-1.368**	-2.73e-14
		(0.608)	(0.352)	(0.529)	(0.0864)
40 th	CBHI x 2013	-0.349	0.226	-0.0526	-4.00e-13
40		(0.474)	(0.422)	(0.129)	(0.0738)
	CBHI x 2016	-0.898	0.109	-2.505***	-1.03e-12
		(0.589)	(0.452)	(0.732)	(0.107)
50 th	CBHI x 2013	-0.0565	0.559	-0.0358	-4.10e-11
50		(0.496)	(0.446)	(0.121)	(0.0839)
	CBHI x 2016	-0.497	0.266	-2.366***	-1.78e-10
		(0.559)	(0.497)	(0.833)	(0.136)
60 th	CBHI x 2013	-0.122	0.109	-0.0387	-0.661**
00		(0.540)	(0.475)	(0.104)	(0.289)
	CBHI x 2016	-1.028	-0.185	-1.927***	-2.156***
		(0.621)	(0.465)	(0.688)	(0.613)
70 th	CBHI x 2013	0.175	-0.159	-0.324*	-0.671
70		(0.553)	(0.538)	(0.182)	(0.472)
	CBHI x 2016	-0.751	-0.154	-1.811***	-2.896***
		(0.599)	(0.612)	(0.573)	(0.787)
80 th	CBHI x 2013	0.0502	-0.187	-0.453	-0.951
80		(0.552)	(0.561)	(0.482)	(0.742)
	CBHI x 2016	-0.637	-0.406	-1.539**	-3.254***
		(0.630)	(0.676)	(0.714)	(0.867)
ooth	CBHI x 2013	1.011	-0.222	0.170	-0.205
90		(0.662)	(0.751)	(0.808)	(0.872)
	CBHI x 2016	0.284	-0.624	0.0950	-2.472**
		(0.767)	(0.858)	(0.822)	(1.033)
N	77241	77241	77241	77241	

Table A8: Quantile regressions for effect of health insurance

Quantile regressions are estimated using the user written command *qrprocess*. All models include the full set of controls shown in Table A3 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%.

	(1)	(2)	(3)	(4)	(5)
	Control	Premium	Premium		
	Control	Increase	Waiver		
	Mean /(SF)	Mean/(SF)	Mean/(SE)	Mean	Mean
	Mean (GE)	Mean/(BE)	Wiedli/(SE)	difference (2-1)	difference (3-1)
Sex (1=Male)	0.457	0.453	0.448	-0.004	-0.009
	(0.004)	(0.003)	(0.008)		
14–25 years	0.288	0.281	0.274	-0.007*	-0.014*
	(0.003)	(0.002)	(0.007)		
26-35 years	0.266	0.273	0.275	0.006	0.009
	(0.003)	(0.002)	(0.008)		
36 – 45 years	0.194	0.198	0.183	0.003	-0.012*
	(0.003)	(0.002)	(0.005)		
46 – 55 years	0.148	0.146	0.160	-0.002	0.012
	(0.003)	(0.002)	(0.007)		
>=56 years	0.104	0.103	0.108	-0.001	0.004
	(0.003)	(0.002)	(0.005)		
Primary school	0.698	0.702	0.686	0.004	-0.012
	(0.004)	(0.002)	(0.008)		
Married	0.596	0.609	0.611	0.013**	0.015*
	(0.004)	(0.003)	(0.008)		
Savings account	0.179	0.180	0.171	0.001	-0.008
	(0.003)	(0.002)	(0.006)		
Tontine	0.260	0.265	0.255	0.005	-0.005
	(0.004)	(0.002)	(0.007)		
Health problems	0.177	0.186	0.207	0.009**	0.030***
	(0.003)	(0.002)	(0.008)		
Taken a loan	0.712	0.716	0.721	0.004	0.009
	(0.004)	(0.002)	(0.007)		
Internet connection	0.094	0.089	0.101	-0.005	0.006
	(0.003)	(0.001)	(0.008)		
Radio, TV or computer	0.612	0.615	0.587	0.004	-0.025**
	(0.004)	(0.003)	(0.008)		
Households size	5.315	5.264	5.306	-0.051**	-0.009
	(0.019)	(0.011)	(0.066)		
Electricity	0.639	0.635	0.620	-0.003	-0.018*
	(0.004)	(0.003)	(0.008)		
Number of land parcels	2.877	2.899	3.057	0.022	0.180
	(0.029)	(0.012)	(0.109)		
Total livestock units	0.648	0.618	0.659	-0.030**	0.011
	(0.012)	(0.004)	(0.030)		
VUP program	0.121	0.126	0.133	0.005	0.012**
	(0.002)	(0.002)	(0.004)		
Poorest (Q1)	0.193	0.190	0.195	-0.003	0.003
	(0.003)	(0.003)	(0.005)		
Poor (Q2)	0.204	0.203	0.198	-0.001	-0.006
	(0.003)	(0.002)	(0.005)		
Average (Q3)	0.206	0.209	0.200	0.002	-0.006
	(0.003)	(0.002)	(0.006)		
Rich (Q4)	0.207	0.208	0.209	0.001	0.001
	(0.003)	(0.002)	(0.007)		
Richest (Q5)	0.190	0.190	0.198	0.001	0.008
	(0.004)	(0.002)	(0.010)		
Minutes to hospital	167.273	167.587	169.219	0.314	1.946
	(0.831)	(0.599)	(1.678)		
Minutes to market	54.452	54.547	55.199	0.095	0.747
	(0.289)	(0.224)	(0.693)		
Ubudehe missing	0.125	0.123	0.064	-0.002	-0.062***
	(0.002)	(0.002)	(0.008)		
Poor housing	0.897	0.898	0.875	0.001	-0.022***
	(0.002)	(0.002)	(0.007)		
Urban/rural 2002	0.146	0.140	0.137	-0.006	-0.009
	(0.003)	(0.002)	(0.008)		
N	23569	45098	12311	68667	35880

Table A9: Covariate balancing for control and treatment groups

 $\label{eq:standard} \hline Standard errors in parentheses clustered at Sector level. Significance levels corresponding to *** p<0.01 for 1% ** p<0.05 for 5% and * p<0.1 for 10% \\$



Figure A3: Matching efficiency between control (no CBHI) and treatment groups across survey rounds

Probability of insurance

	(1)	(2)	(3) Men	(4)	(5)	(9)	(7) Vomen	(8)
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
Premium increase x 2013	-0.124	0.588	-1.966*	-1.444	0.217	0.074	-0.719	-0.792
Premium increase x 2016	(0.720) -0.984	(0.020) -0.174	(1.072) -2.878**	(1.039) -2.499**	(86C.U) -0.046	(21C.0) -0.426	(1.124) -1.650*	(0./10) -2.246***
	(0.919)	(0.718)	(1.175)	(1.176)	(0.597)	(0.494)	(0.957)	(0.710)
Fremium waiver X 2013	-1./95 (1.262)	-2.498	1.001 (1.883)	-1.848 (1.730)	0.907)	-0.342 (0.781)	-0.829 (1.577)	(1.010)
Premium waiver x 2016	-3.530**	-1.762	-2.984	-2.686	-1.421	-1.138	-1.394	-2.442*
	(1.560)	(1.356)	(2.025)	(2.157)	(1.183)	(0.950)	(1.638)	(1.393)
Constant	30.336^{***}	26.344^{***}	23.577***	23.919***	25.096^{***}	18.843^{***}	21.540^{***}	15.630^{***}
	(2.003)	(1.841)	(3.110)	(2.507)	(1.488)	(0.716)	(2.344)	(2.031)
Observations	34,900	29,239	25,656	24,078	42,341	39,334	21,371	22,110
R-squared	0.138	0.075	0.195	0.168	0.121	0.055	0.252	0.288
All models include the full are clustered at year by dist	set of controls rict. Significan	shown in Table ce levels corres	e A1 in the Online A pond with *** p<0	Appendix, plus .01 for 1 %, **	month and dist p<0.05 for 5%	rict fixed effec 6 and * p<0.1	ts. Standard errors (for 10%.	in parenthesis)

Table A10: Effect of premium changes on men and women

	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)
		1	Jrban				Rural	
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
Premium increase x 2013	0.755	1.878	-0.850	0.568	-0.242	0.198	-1.578*	-1.554**
	(2.214)	(1.694)	(2.612)	(2.443)	(0.462)	(0.430)	(0.832)	(0.735)
Premium increase x 2016	3.826	3.883 * *	1.562	2.787	-1.096^{*}	-0.506	-3.266***	-3.304***
	(2.500)	(1.667)	(2.647)	(2.339)	(0.583)	(0.512)	(0.883)	(0.820)
Premium waiver x 2013	-1.631	-2.533	-0.508	3.388	-0.912	-0.525	0.242	-1.494
	(3.288)	(2.709)	(3.638)	(2.879)	(0.724)	(0.621)	(1.553)	(1.128)
Premium waiver x 2016	-2.768	-9.208***	0.365	1.417	-2.219**	-0.330	-3.372**	-4.051^{***}
	(3.388)	(2.659)	(3.835)	(3.029)	(0.851)	(0.851)	(1.400)	(1.303)
Constant	23.237***	16.258^{***}	15.502^{***}	15.183 **	24.125***	20.556***	14.585^{***}	12.688^{***}
	(4.487)	(4.637)	(4.865)	(7.070)	(1.421)	(1.286)	(2.342)	(2.138)
Observations	10,022	4,879	8,396	6,303	67,219	63,694	38,631	39,885
R-squared	0.222	0.186	0.253	0.329	0.126	0.066	0.184	0.166
All models include the full are clustered at year by dist	set of controls rict. Significan	shown in Table ce levels corres	Al in the Online A pond with *** p<0	Appendix, plus .01 for 1 %, **	month and dist p<0.05 for 59	rict fixed effec δ and * p<0.1	ts. Standard errors (for 10%.	in parenthesis)

Table A11: Effect of premium changes in rural and urban localities

		(1)	(2)	(3)	(4)
Percentile		Total Hours	Agric Hours	Non-Agric Hours	Wage Hours
	Premium increase x 2013	0.259	1.34e-12	-5.58e-11	3.38e-14
10	D	(0.517)	(0.164)	(0.250)	(0.119)
	Premium increase x 2010	(0.571)	(0.179)	-1.09e-11	-1.40e-15
	Premium waiver x 2013	-1 071*	-1 11e-12	-5 41e-11	-1 13e-13
	Tiennum warver x 2015	(0.591)	(0.294)	(0.448)	(0.235)
	Premium waiver x 2016	-1.055	-3.78e-13	-1.25e-10	-1.42e-13
		(0.687)	(0.304)	(0.536)	(0.256)
	Premium increase x 2013	-0.360	0.252	-0.239	-2.41e-12
20 th		(0.676)	(0.371)	(0.307)	(0.277)
20	Premium increase x 2016	-0.645	0.116	-0.795	-6.97e-12
		(0.824)	(0.445)	(0.609)	(0.333)
	Premium waiver x 2013	-1.261	-0.0859	-0.0754	-8.30e-13
	Bromium weiver v 2016	(1.009) 2.547*	(0.755)	(0.527)	(0.432)
	Fieliliulli walvel x 2010	-2.347	-0.115	-1.425	-7.796-12
	Premium increase x 2013	-0.307	0.0596	-1 225**	-1 54e-11
	Trennum mereuse x 2015	(0.625)	(0.507)	(0.531)	(0.300)
30 ^m	Premium increase x 2016	-0.639	-0.341	-2.246***	-4.46e-11
		(0.747)	(0.570)	(0.761)	(0.387)
	Premium waiver x 2013	-1.462	-0.337	-0.328	4.97e-12
		(1.013)	(0.780)	(0.662)	(0.548)
	Premium waiver x 2016	-3.182**	-1.271	-2.855***	-4.99e-11
		(1.307)	(0.908)	(1.076)	(0.633)
	Premium increase x 2013	-0.406	0.207	-1.880***	-0.997**
40^{th}	Dramium in analyse v 2016	(0.599)	(0.612)	(0.698)	(0.488)
	Premium increase x 2016	-1.059	-0.187	-3.38/***	-2.3/5***
	Premium waiver x 2013	-1 588*	-0.516	0.452	-0.824
	Tiennum warver x 2015	(0.895)	(0.986)	(0.942)	(1.020)
	Premium waiver x 2016	-3.243**	-0.858	-2.645*	-3.178**
		(1.308)	(1.090)	(1.334)	(1.249)
	Premium increase x 2013	0.273	0.673	-2.372***	-1.526**
50 th		(0.563)	(0.585)	(0.893)	(0.643)
50	Premium increase x 2016	-0.521	0.143	-3.174***	-2.525***
	D · · · 2012	(0.617)	(0.594)	(0.882)	(0.764)
	Premium waiver x 2013	-1.415	-0./18	-0.517	-0.554
	Premium waiver x 2016	-3.051**	-0.981	(1.491)	-3 377**
	r teinium warver x 2010	(1.178)	(1.038)	(1.592)	(1.318)
	Premium increase x 2013	0.307	0.529	-1.392	-1.458*
COth		(0.559)	(0.555)	(1.266)	(0.858)
60	Premium increase x 2016	-0.822	-0.522	-2.806**	-3.503***
		(0.635)	(0.609)	(1.145)	(0.870)
	Premium waiver x 2013	-0.405	-0.696	-1.080	-0.563
	D · · · 0017	(0.996)	(1.009)	(2.093)	(1.263)
	Premium waiver x 2016	-2.352*	-1.512	-4.068**	-3.452**
	Bramium ingrange x 2012	(1.2/1)	(1.255)	(1.961)	(1.528)
	Fielinum merease x 2013	(0.598)	(0.594)	-0.048	-0.800
70 th	Premium increase x 2016	-1.278**	-0.528	-2.466**	-2.972***
		(0.612)	(0.717)	(1.191)	(0.809)
	Premium waiver x 2013	-0.548	-0.935	-0.277	-0.727
		(1.033)	(1.015)	(1.812)	(1.572)
	Premium waiver x 2016	-2.852**	-1.546	-3.341*	-2.974
		(1.242)	(1.352)	(1.741)	(1.856)
	Premium increase x 2013	0.466	0.706	0.0262	-1.602
80 th	Bromium inorocco y 2016	(0.665)	(0.624)	(1.341)	(1.014)
	Premium increase x 2010	-0.578	-0.557	-1.555	-3.410****
	Premium waiver x 2013	1.031	-1 197	2 699	-0.681
	- 101110111 WULLOI A 2013	(1.015)	(1.060)	(2.169)	(1.524)
	Premium waiver x 2016	-0.896	-1.690	-0.513	-1.887
		(1.168)	(1.356)	(1.630)	(1.523)
	Premium increase x 2013	0.761	0.369	1.550	-2.076
90 th		(0.672)	(0.656)	(1.476)	(1.353)
20	Premium increase x 2016	-0.0787	-0.288	0.231	-2.493*
	D · · ·	(0.861)	(0.741)	(1.348)	(1.486)
	Premium waiver x 2013	2.041	-1.681	8.945***	1.648
	Premium waiver v 2016	(1.293)	(1.139)	(5.105)	(1.972)
	i ionnum walvel x 2010	(1 427)	(1.323)	(2,696)	(2.013)
N		77241	68573	47027	46188

Table A12: Quantile regressions for the effect of the insurance policy change

Standard errors in parentheses. Significance levels correspond with * p<0.10 for 10%, ** p<0.05 for 5% and *** p<0.01 for 1%

			Male			ц	temale	
	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
Premium increase x 2013	-1.033	-0.273	0.031	-6.272	2.634	1.629	2.264	-0.346
	(2.078)	(1.788)	(3.196)	(3.781)	(1.707)	(1.543)	(2.305)	(2.028)
Premium waiver x 2013	3.420	-0.633	2.817	-0.289	0.166	-0.192	-3.239	-3.156
	(2.497)	(1.718)	(3.594)	(3.635)	(1.873)	(1.661)	(3.197)	(2.166)
Constant	8.413	23.239***	28.642^{**}	-6.899	2.226	7.891	5.733	-1.810
	(12.232)	(6.812)	(12.969)	(14.702)	(8.067)	(7.619)	(11.946)	(11.576)
Observations	1,668	1,496	1,034	1,076	2,208	2,138	692	952
Baseline control mean	29.09	16.61	22.75	17.26	22.99	18.72	16.19	7.972
Robust standard errors in p	barentheses. Sig	gnificance level	s corresponding to *	*** p<0.01 for	1% ** p<0.05	5 for 5% and $*_{\rm J}$	p<0.1 for 10%	

by gender
f effects
results of
nel data
ole A13: Pai
Tai

	(1)	(2)	(3)	(4)
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
Premium Increase x 2013	0.759	0.149	2.396	-3.347*
	(1.590)	(1.154)	(2.032)	(1.920)
Premium waiver x 2013	3.076**	1.713	0.986	-0.619
	(1.499)	(1.376)	(2.041)	(1.913)
Constant	16.871***	8.991*	-9.212	3.076
	(5.493)	(4.471)	(12.473)	(13.050)
Number of observations	4,914	4,560	2,214	2,612
Baseline control mean	25.98	17.86	21.08	13.78

Table A14: Panel data results of effect of insurance enrolment and premium changes - including imputed *Ubudehe*

Robust standard errors in parentheses. Significance levels corresponding to *** $p{<}0.01$ for 1% ** $p{<}0.05$ for 5% and * $p{<}0.1$ for 10%

Table A15:	Placebo	1:	Comparing	other	insurance	programmes	(not	affected)	with	control
group										

	(1)	$\langle 0 \rangle$	$\langle 2 \rangle$	(1)
	(1)	(2)	(3)	(4)
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
Other insurance x 2013	-3.411	-2.473	1.073	-0.846
	(2.685)	(1.961)	(3.738)	(3.070)
Other insurance x 2016	-0.887	-7.255***	-4.691	-2.737
	(2.858)	(2.162)	(3.531)	(3.553)
Constant	22.898***	10.461***	8.854	13.996*
	(8.041)	(3.761)	(13.680)	(7.574)
Observations	19,093	15,819	12,636	13,361
R-squared	0.309	0.310	0.370	0.440

All models include the full set of controls shown in Supplementary Tables A3 and A9. Standard errors in parentheses clustered at Sector level. Significance levels corresponding to *** p<0.01 for 1% ** p<0.05 for 5% and * p<0.1 for 10%

	(1)	(2)	(3)	(4)
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
Placebo x 2013	1.203	0.341	2.375	2.700**
	(0.848)	(0.755)	(1.579)	(1.141)
Placebo x 2016	-0.373	-0.353	0.529	1.533
	(0.807)	(0.839)	(1.144)	(0.942)
Constant	25.947***	16.484***	18.651***	18.101***
	(2.018)	(2.478)	(3.679)	(2.289)
Observations	21,383	19,373	12,705	15,111
R-squared	0.136	0.076	0.207	0.227

Table A16: Placebo 2: Abadie et al. (2010) synthetic control treatments from resampling the control group: effect of insurance adoption

All models include the full set of controls shown in Supplementary Tables A3 and A9. Standard errors in parentheses clustered at Sector level. Significance levels corresponding to *** p<0.01 for 1% ** p<0.05 for 5% and * p<0.1 for 10%

1	Table	A17:	Placebo	3:	Abadie	et al.	(2010)	3-dimensiona	l synthetic	controls	mimicking
	premi	um inc	crease am	ong	high SE	S, pre	mium w	aivers among l	ow SES an	d control	

	(1)	(2)	(3)	(4)
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
Fake Premium waiver x 2013	0.947	1.800**	-1.297	-1.202
	(1.136)	(0.834)	(2.404)	(1.102)
Fake Premium waiver x 2016	0.505	0.747	-0.653	-0.795
	(0.784)	(0.702)	(1.094)	(0.959)
Fake Premium increase x 2013	0.282	0.407	0.763	-0.394
	(0.720)	(0.578)	(1.195)	(0.987)
Fake Premium increase x 2016	-0.215	-0.108	0.191	0.112
	(0.939)	(0.638)	(1.333)	(1.167)
Constant	27.886***	16.892***	22.649***	18.179***
	(1.530)	(1.340)	(2.205)	(2.081)
Observations	22,385	20,046	13,421	15,755
R-squared	0.154	0.069	0.237	0.250

All models include the full set of controls shown in Supplementary Tables A3 and A9. Standard errors (in parenthesis) are clustered at year-district-village level. Significance levels correspond with *** p<0.01 for 1 %, ** p<0.05 for 5% and * p<0.1 for 10%.

	(1)	(2)	(3)	(4)
VARIABLES	Total Hours	Agric Hours	Non Agric Hours	Wage Hours
Premium Increase x 2013	-0.143	0.512	-0.721	-1.224
	(0.551)	(0.438)	(0.964)	(0.791)
Premium Increase x 2016	-1.056*	0.070	-2.291*	-3.133***
	(0.630)	(0.543)	(1.253)	(0.865)
Premium Waiver x 2013	-1.877*	-0.668	-1.009	0.023
	(0.953)	(0.613)	(1.995)	(1.038)
Premium Waiver x 2016	-2.818**	-0.164	-5.396**	-1.795
	(1.080)	(1.047)	(2.124)	(1.321)
Constant	28.095***	20.685***	21.509***	8.714***
	(1.012)	(0.697)	(2.197)	(0.797)
Observations	80,978	61,154	37,786	46,188
R-squared	0.032	0.044	0.119	0.135

Table A18: Main results including observations with missing Ubudehe category

All models include the full set of controls shown in Supplementary Tables A3 and A9. Robust standard errors in parentheses. Significance levels corresponding to *** p<0.01 for 1% ** p<0.05 for 5% and * p<0.1 for 10%

	(1)	(2)	(3)	(4)	(5)	(6)
	Short te	rm full dome	stic time	Short & me	dium term pr	oxy domestic time
VARIABLES	All	Men	Women	All	Men	Women
Premium increase x 2013	1.217**	1.336***	1.324*	0.363**	0.486***	0.377
	(0.466)	(0.467)	(0.724)	(0.165)	(0.171)	(0.298)
Premium increase x 2016	()		(,	0.338**	0.374**	0.409*
				(0.132)	(0.151)	(0.240)
Premium waiver x 2013	0.700	1.738***	0.170	-0.070	0.679***	-0.576
	(0.643)	(0.576)	(0.908)	(0.271)	(0.208)	(0.379)
Premium waiver x 2016				0.094	0.356**	0.025
				(0.236)	(0.179)	(0.309)
Constant	30.457***	15.362***	31.230***	16.687***	7.131***	15.221***
	(1.301)	(1.671)	(1.754)	(0.567)	(0.530)	(0.900)
Observations	44,922	20,197	24,725	71,061	32,062	38,999
R-squared	0.330	0.115	0.151	0.532	0.127	0.507
Baseline Mean	21.16	10.67	29.85	8.998	2.290	14.56
Baseline mean (control)	20.23	10.70	29.03	8.818	2.659	14.50
Increase – % mean (short)	0.0575	0.125	0.0443	0.0403	0.212	0.0259
Waiver – % mean(short)	0.0331	0.163	0.00569	-0.00776	0.296	-0.0395
Increase – % mean (medium)				0.0383	0.141	0.0282
Waiver – % mean (medium)				0.0107	0.134	0.00171

Table A19: Effect of the policy change on domestic labour supply

Results in the first model show the short term assessment using the full domestic labour supply elicitation. Model 2 shows both the short and medium term assessment of domestic labour supply but using total time spent on cooking, searching for cooking fuel and tending to livestock as a proxy for domestic labour supply. Standard errors in parentheses clustered at Sector level. Significance levels corresponding to *** p<0.01 for 1% ** p<0.05 for 5% and * p<0.1 for 10%

(1) (2) (3) (4) (5) VARIABLES Working Agric job Non agric job Wage job Agric labo Panel 1: Overall CBHI x 2013 -0.006 0.006 -0.002 0.012 0.018 (0.007) (0.007) (0.011) (0.011) (0.011) CBHI x 2016 -0.009 0.010 -0.003 -0.013 -0.010 Constant 0.863*** 0.860*** 0.260*** 0.652*** 0.553** (0.021) (0.018) (0.027) (0.030) (0.032) Observations 78,438 77,241 77,241 77,241 80,978 R-squared 0.038 0.418 0.310 0.172 0.211 Observations 78,438 77,241 77,241 77,241 80,978 R-squared 0.038 0.418 0.310 0.172 0.211 CBHI x 2013 -0.006 0.015 0.004 -0.006 0.023 (0.011) (0.013										
VARIABLES Working Agric job Non agric job Wage job Agric labol Panel 1: Overall CBHI x 2013 -0.006 0.006 -0.002 0.012 0.018 (0.007) (0.007) (0.011) (0.011) (0.011) (0.011) CBHI x 2016 -0.009 0.010 -0.003 -0.013 -0.012 Constant 0.863*** 0.860*** 0.260*** 0.652*** 0.553** (0.021) (0.018) (0.027) (0.030) (0.032) Observations 78,438 77,241 77,241 77,241 80,978 R-squared 0.038 0.418 0.310 0.172 0.211 Panel 2: Men CBHI x 2016 -0.006 0.015 0.004 -0.006 0.018) (0.010) (0.013) (0.010) (0.018) (0.018) CBHI x 2016 -0.003 0.764*** 0.443*** 0.790*** 0.495** (0.020) (0.025) (0.0		(1)	(2)	(3)	(4)	(5)				
$\begin{tabular}{ c c c c c c c c c c c c c c c c c c c$	VARIABLES	Working	Agric job	Non agric job	Wage job	Agric labou				
$\begin{tabular}{ c c c c c c c c c c c c c c c c c c c$										
$\begin{array}{c c c c c c c c c c c c c c c c c c c $	Panel 1: Overall									
$\begin{array}{c ccccccccccccccccccccccccccccccccccc$	CBHI x 2013	-0.006	0.006	-0.002	0.012	0.018				
$\begin{array}{c ccccccccccccccccccccccccccccccccccc$		(0.007)	(0.007)	(0.011)	(0.011)	(0.011)				
$\begin{array}{cccccccccccccccccccccccccccccccccccc$	CBHI x 2016	-0.009	0.010	-0.003	-0.013	-0.010				
$\begin{array}{cccccccccccccccccccccccccccccccccccc$		(0.009)	(0.008)	(0.009)	(0.012)	(0.012)				
$\begin{array}{c ccccccccccccccccccccccccccccccccccc$	Constant	0.863***	0.860***	0.260***	0.652***	0.553***				
Observations R-squared 78,438 0.038 77,241 0.418 77,241 0.310 77,241 0.172 80,978 0.211 Panel 2: Men CBHI x 2013 -0.006 0.015 0.004 -0.006 0.023 CBHI x 2016 -0.006 0.013 0.009 -0.011 -0.006 0.023 CBHI x 2016 -0.013 0.020 0.009 -0.011 -0.001 Constant 0.898*** 0.764*** 0.443*** 0.790*** 0.495*** Constant 0.898*** 0.764*** 0.443*** 0.790*** 0.495*** O.020 (0.020) (0.025) (0.034) (0.039) (0.040) O Panel 3: Women CBHI x 2016 -0.007 -0.002 -0.014 -0.020 <th <="" colspan="4" td=""><td></td><td>(0.021)</td><td>(0.018)</td><td>(0.027)</td><td>(0.030)</td><td>(0.032)</td></th>	<td></td> <td>(0.021)</td> <td>(0.018)</td> <td>(0.027)</td> <td>(0.030)</td> <td>(0.032)</td>					(0.021)	(0.018)	(0.027)	(0.030)	(0.032)
Observations 78,438 77,241 77,241 77,241 80,978 R-squared 0.038 0.418 0.310 0.172 0.211 Panel 2: Men CBHI x 2013 -0.006 0.015 0.004 -0.006 0.023 (0.010) (0.010) (0.016) (0.017) (0.016) CBHI x 2016 -0.013 0.020 0.009 -0.011 -0.001 (0.011) (0.013) (0.010) (0.018) (0.018) Constant $0.898***$ $0.764***$ $0.443***$ $0.790***$ $0.495**$ (0.020) (0.025) (0.034) (0.039) (0.040) Observations $35,493$ $34,900$ $34,900$ $34,900$ $36,698$ R-squared 0.036 0.375 0.235 0.118 0.179 CBHI x 2013 -0.007 -0.002 -0.004 $0.027*$ 0.013 CBHI x 2016 -0.009 -0.003 -0.012 -0.014										
R-squared 0.038 0.418 0.310 0.172 0.211 Panel 2: Men CBHI x 2013 -0.006 0.015 0.004 -0.006 0.023 (0.010) (0.010) (0.016) (0.017) (0.016) CBHI x 2016 -0.013 0.020 0.009 -0.011 -0.001 (0.011) (0.013) (0.010) (0.018) (0.018) Constant 0.898^{***} 0.764^{***} 0.443^{***} 0.790^{***} 0.495^{***} (0.020) (0.025) (0.034) (0.039) (0.040) Observations $35,493$ $34,900$ $34,900$ $34,900$ $34,900$ $36,698$ R-squared 0.036 0.375 0.235 0.118 0.179 Panel 3: Women CBHI x 2013 -0.007 -0.002 -0.004 0.027^* 0.013 (0.007) (0.008) (0.014) (0.016) (0.014) CBHI x 2016 -0	Observations	78,438	77,241	77,241	77,241	80,978				
$\begin{array}{c c c c c c c c c c c c c c c c c c c $	R-squared	0.038	0.418	0.310	0.172	0.211				
Panel 2: Men CBHI x 2013 -0.006 0.015 0.004 -0.006 0.023 (0.010) (0.013) 0.009 -0.011 -0.001 CBHI x 2016 -0.013 0.020 0.009 -0.011 -0.001 -0.001 -0.001 -0.001 -0.001 -0.001 -0.001 -0.001 -0.001 -0.001 -0.001 -0.001 -0.018 -0.018 -0.044 -0.002 -0.013 -0.013 -0.027* -0.013 -0.027* -0.013 -0.027* -0.013 -0.027* -0.013 -0.027* -0.013 -0.027* -0.013 -0.020 -0.014 -0.020 -0.014 -0.020 <th colsp<="" td=""><td></td><td></td><td></td><td></td><td></td><td></td></th>	<td></td> <td></td> <td></td> <td></td> <td></td> <td></td>									
CBHI x 2013 -0.006 0.015 0.004 -0.006 0.023 (0.010) (0.010) (0.016) (0.017) (0.016) CBHI x 2016 -0.013 0.020 0.009 -0.011 -0.001 (0.011) (0.013) (0.010) (0.018) (0.018) Constant 0.898^{***} 0.764^{***} 0.443^{***} 0.790^{***} 0.495^{**} (0.020) (0.025) (0.034) (0.039) (0.040) Observations $35,493$ $34,900$ $34,900$ $34,900$ $36,698$ R-squared 0.036 0.375 0.235 0.118 0.179 Panel 3: Women CBHI x 2013 -0.007 -0.002 -0.004 0.027^* 0.013 (0.007) (0.008) (0.014) (0.016) (0.014) CBHI x 2016 -0.009 -0.003 -0.012 -0.014 -0.020 (0.010) (0.008) (0.013) (0.013) (0.013) Constant 0.801^{***} 0.886^{***} 0.276^{***} <td></td> <td></td> <td>Pane</td> <td>1 2: Men</td> <td></td> <td></td>			Pane	1 2: Men						
$\begin{array}{c ccccccccccccccccccccccccccccccccccc$	CBHI x 2013	-0.006	0.015	0.004	-0.006	0.023				
$\begin{array}{c ccccccccccccccccccccccccccccccccccc$		(0.010)	(0.010)	(0.016)	(0.017)	(0.016)				
Constant (0.011) $0.898***$ (0.020) (0.013) (0.025) (0.010) (0.034) (0.018) (0.039) (0.018) (0.040) Observations R-squared $35,493$ 0.036 $34,900$ 0.375 $34,900$ 0.235 $34,900$ 0.118 $34,900$ $0.039)$ $36,698$ 0.118 CBHI x 2013 -0.007 (0.007) -0.002 (0.008) -0.004 (0.014) $0.027*$ (0.016) 0.013 (0.014) CBHI x 2016 -0.007 	CBHI x 2016	-0.013	0.020	0.009	-0.011	-0.001				
Constant 0.898^{***} 0.764^{***} 0.443^{***} 0.790^{***} 0.495^{***} (0.020) (0.025) (0.034) (0.039) (0.040) Observations $35,493$ $34,900$ $34,900$ $34,900$ $36,698$ R-squared 0.036 0.375 0.235 0.118 0.179 Panel 3: WomenCBHI x 2013-0.007-0.002-0.004 0.027^{*} 0.013 (0.007) (0.008) (0.014) (0.016) (0.014) CBHI x 2016 -0.009 -0.003 -0.012 -0.014 -0.020 (0.010) (0.008) (0.013) (0.013) (0.013) Constant 0.801^{***} 0.886^{***} 0.276^{***} 0.637^{***} 0.510^{***} Observations $42,945$ $42,341$ $42,341$ $42,341$ $44,280$ P sequerad 0.057 0.478 0.222 0.101 0.247		(0.011)	(0.013)	(0.010)	(0.018)	(0.018)				
(0.020) (0.025) (0.034) (0.039) (0.040) Observations 35,493 34,900 34,900 34,900 36,698 R-squared 0.036 0.375 0.235 0.118 0.179 Panel 3: Women CBHI x 2013 -0.007 -0.002 -0.004 0.027* 0.013 (0.007) (0.008) (0.014) (0.016) (0.014) CBHI x 2016 -0.009 -0.003 -0.012 -0.014 -0.020 (0.010) (0.008) (0.013) (0.013) (0.013) Constant 0.801*** 0.886*** 0.276*** 0.637*** 0.510** (0.029) (0.020) (0.036) (0.037) (0.034) Observations 42,945 42,341 42,341 42,341 44,280 R sequered 0.057 0.478 0.222 0.101	Constant	0.898***	0.764***	0.443***	0.790***	0.495***				
Observations R-squared $35,493$ 0.036 $34,900$ 0.375 $34,900$ 0.235 $34,900$ 0.118 $36,698$ 0.179 Panel 3: WomenCBHI x 2013 -0.007 0.007 -0.002 0.008 -0.004 0.014 $0.027*$ 0.016 0.013 0.016 CBHI x 2016 -0.007 -0.009 -0.003 -0.012 -0.014 -0.014 -0.020 0.013 CBHI x 2016 -0.009 -0.009 -0.003 -0.012 -0.014 -0.014 -0.020 0.013 Constant 0.801^{***} 0.801^{***} 0.886^{***} 0.276^{***} 0.637^{***} 		(0.020)	(0.025)	(0.034)	(0.039)	(0.040)				
Observations $35,493$ $34,900$ $34,900$ $34,900$ $34,900$ $36,698$ R-squared 0.036 0.375 0.235 0.118 0.179 Panel 3: Women CBHI x 2013 -0.007 -0.002 -0.004 $0.027*$ 0.013 CBHI x 2016 -0.007 -0.002 -0.014 0.013 CBHI x 2016 -0.009 -0.003 -0.012 -0.014 -0.020 (0.010) (0.008) (0.013) (0.013) (0.013) (0.013) (0.013) (0.013) (0.037) (0.034) Observations $42,945$ $42,341$ $42,341$ $44,280$ 0.2247										
R-squared 0.036 0.375 0.235 0.118 0.179 Panel 3: Women CBHI x 2013 -0.007 -0.002 -0.004 $0.027*$ 0.013 (0.007) (0.008) (0.014) (0.016) (0.014) CBHI x 2016 -0.009 -0.003 -0.012 -0.014 -0.020 (0.010) (0.008) (0.013) (0.013) (0.013) Constant 0.801^{***} 0.886^{***} 0.276^{***} 0.637^{***} 0.510^{***} Observations $42,945$ $42,341$ $42,341$ $42,341$ $44,280$ P sequerad 0.057 0.478 0.222 0.101 0.247	Observations	35,493	34,900	34,900	34,900	36,698				
Panel 3: WomenCBHI x 2013 -0.007 -0.002 -0.004 $0.027*$ 0.013 (0.007)(0.008)(0.014)(0.016)(0.014)CBHI x 2016 -0.009 -0.003 -0.012 -0.014 -0.020 (0.010)(0.008)(0.013)(0.013)(0.013)Constant 0.801^{***} 0.886^{***} 0.276^{***} 0.637^{***} 0.510^{***} Observations $42,945$ $42,341$ $42,341$ $42,341$ $44,280$ Descuered 0.057 0.478 0.222 0.101 0.247	R-squared	0.036	0.375	0.235	0.118	0.179				
Take 5. WomenCBHI x 2013 -0.007 -0.002 -0.004 $0.027*$ 0.013 (0.007)(0.008)(0.014)(0.016)(0.013)CBHI x 2016 -0.009 -0.003 -0.012 -0.014 -0.020 (0.010)(0.008)(0.013)(0.013)(0.013)Constant 0.801^{***} 0.276^{***} 0.637^{***} 0.510^{**} (0.029)(0.020)(0.036)(0.037)(0.034)Observations $42,945$ $42,341$ $42,341$ $42,341$ $44,280$ P compared 0.057 0.478 0.222 0.101 0.2477	Donal 2: Warran									
CBHI X 2013 -0.007 -0.002 -0.004 0.027^* 0.013 (0.007) (0.008) (0.014) (0.016) (0.014) CBHI X 2016 -0.009 -0.003 -0.012 -0.014 -0.020 (0.010) (0.008) (0.013) (0.013) (0.013) Constant 0.801^{***} 0.886^{***} 0.276^{***} 0.637^{***} 0.510^{***} (0.029) (0.020) (0.036) (0.037) (0.034) Observations $42,945$ $42,341$ $42,341$ $42,341$ $44,280$ P sequerad 0.057 0.478 0.222 0.101 0.247	CD111 - 2012	0.007	0.000	0.004	0.027*	0.012				
$\begin{array}{cccccccccccccccccccccccccccccccccccc$	СВНІ х 2013	-0.00/	-0.002	-0.004	0.02/*	0.013				
CBHI X 2010 -0.009 -0.003 -0.012 -0.014 -0.020 (0.010) (0.008) (0.013) (0.013) (0.013) Constant 0.801^{***} 0.886^{***} 0.276^{***} 0.637^{***} 0.510^{***} (0.029) (0.020) (0.036) (0.037) (0.034) Observations $42,945$ $42,341$ $42,341$ $42,341$ $44,280$ R sequered 0.057 0.478 0.222 0.101 0.247	CD111 2016	(0.007)	(0.008)	(0.014)	(0.016)	(0.014)				
(0.010) (0.008) (0.013) (0.013) (0.013) Constant 0.801^{***} 0.886^{***} 0.276^{***} 0.637^{***} 0.510^{**} (0.029) (0.020) (0.036) (0.037) (0.034) Observations $42,945$ $42,341$ $42,341$ $42,341$ $44,280$ P sequered 0.057 0.478 0.222 0.101 0.247	CBHI x 2016	-0.009	-0.003	-0.012	-0.014	-0.020				
Constant 0.801^{***} 0.886^{***} $0.2/6^{***}$ $0.63/^{***}$ 0.510^{***} (0.029) (0.020) (0.036) (0.037) (0.034) Observations $42,945$ $42,341$ $42,341$ $42,341$ $44,280$ R sequered 0.057 0.478 0.222 0.101 0.247	Constant	(U.UIU)	(0.008)	(0.013)	(0.013)	(0.013)				
(0.029) (0.020) (0.036) (0.037) (0.034) Observations $42,945 42,341 42,341 42,341 44,280$ R sequered $0.057 0.478 0.222 0.101 0.247$	Constant	0.801***	0.020	$0.2/6^{***}$	$0.03/^{***}$	0.024				
Observations 42,945 42,341 42,341 42,341 44,280 R sequered 0.057 0.478 0.222 0.101 0.247		(0.029)	(0.020)	(0.036)	(0.037)	(0.034)				
D constrained 0.057 0.479 0.222 0.101 0.247	Observations	42 945	42 341	42 341	42 341	44 280				
K-SUBJEO UUN/ U4/A UN// UNSI U $1/4/$	R-squared	0.057	0.478	0322	0.101	0 247				

Standard errors in parentheses clustered at Sector level. Significance levels corresponding to *** $p{<}0.01$ for 1% ** $p{<}0.05$ for 5% and * $p{<}0.1$ for 10%

	(1)	(2)	(3)	(4)	(5)
VARIABLES	Working	Agric job	Non agric job	Wage job	Agric labour
	Panel 1: Overall				
Premium increase x 2013	-0.006	0.009	-0.004	0.009	0.017
	(0.008)	(0.008)	(0.011)	(0.012)	(0.012)
Premium increase x 2016	-0.007	0.016	-0.005	-0.007	-0.000
	(0.010)	(0.009)	(0.011)	(0.014)	(0.014)
Premium waiver x 2013	-0.014	-0.017	0.000	-0.009	-0.007
	(0.012)	(0.012)	(0.020)	(0.021)	(0.021)
Premium waiver x 2016	-0.019	-0.005	0.021	-0.039*	-0.058***
	(0.014)	(0.014)	(0.018)	(0.020)	(0.021)
Constant	0.871***	0.877***	0.202***	0.609***	0.540***
	(0.023)	(0.019)	(0.042)	(0.029)	(0.036)
Observations	78 438	77 241	77 241	77 241	80 978
R-squared	0.041	0.383	0.306	0.179	0.214
	01011	01000	0.000	01175	0.211
	Panel 2: Men				
Premium increase x 2013	-0.007	0.016	0.004	-0.010	0.029*
	(0.011)	(0.012)	(0.016)	(0.018)	(0.016)
Premium increase x 2016	-0.007	0.029*	0.011	-0.012	0.004
	(0.012)	(0.015)	(0.010)	(0.019)	(0.018)
Premium waiver x 2013	-0.022	-0.013	0.007	-0.056**	-0.024
D : : 2016	(0.014)	(0.017)	(0.025)	(0.026)	(0.027)
Premium waiver x 2016	-0.040**	0.010	0.035*	-0.064**	-0.060*
Constant	(0.01/)	(0.023)	(0.020)	(0.029)	(0.030)
Constant	$0.8/8^{***}$	0.814^{***}	0.308^{***}	0.795^{***}	0.53/***
	(0.028)	(0.034)	(0.052)	(0.051)	(0.057)
Observations	35,493	34,900	34,900	34,900	36,698
R-squared	0.043	0.339	0.243	0.129	0.183
	Panel 3: Women				
Premium increase x 2013	-0.006	0.001	-0.006	0.024	0.007
	(0.010)	(0.008)	(0.015)	(0.016)	(0.015)
Premium increase x 2016	-0.008	0.002	-0.017	-0.004	-0.007
	(0.012)	(0.008)	(0.016)	(0.014)	(0.016)
Premium waiver x 2013	-0.008	-0.021	-0.002	0.033	0.010
	(0.017)	(0.013)	(0.025)	(0.026)	(0.024)
Premium waiver x 2016	-0.007	-0.023*	0.019	-0.013	-0.052**
	(0.020)	(0.013)	(0.022)	(0.022)	(0.022)
Constant	0.822***	0.877***	0.245***	0.566***	0.459***
	(0.028)	(0.024)	(0.046)	(0.038)	(0.037)
Observations	42 0 45	40.241	40.241	40.241	44 290
Observations	42,945	42,341	42,341	42,341	44,280
K-squared	0.062	0.446	0.320	0.198	0.251

Table A21: Effect of policy change on the probability of various employment dimensions

Standard errors in parentheses. Significance levels correspond with * p<0.10 for 10%, ** p<0.05 for 5% and *** p<0.01 for 1%

	(1)	(2)	(3)
VARIABLES	All	Men	Women
Premium increase x 2013	-0.033**	-0.035**	-0.034**
	(0.013)	(0.017)	(0.015)
Premium waiver x 2013	-0.017	-0.051**	0.012
	(0.019)	(0.025)	(0.023)
Constant	0.186***	0.272***	0.189***
	(0.063)	(0.075)	(0.070)
Observations	53,912	24,386	29,526
R-squared	0.140	0.133	0.148

Table A22: Demand for additional work

Standard errors in parentheses clustered at Sector level. Significance levels corresponding to *** $p{<}0.01$ for 1% ** $p{<}0.05$ for 5% and * $p{<}0.1$ for 10%

Table A23: Agriculture sector indicators over the period

	Survey Round		
	2010	2013	2016
Number of land parcels owned	3.39	3.24	2.62
Number of crops cultivated	3.61	3.66	3.29
Number of households	11,890	10,476	10,405

Data from the three rounds of the household survey

References

- Abadie, A., Diamond, A., & Hainmueller, J. (2010). Synthetic control methods for comparative case studies: Estimating the effect of California's Tobacco control program. *Journal of the American Statistical Association*, *105*(490), 493–505.
- Alatas, V., Banerjee, A., Hanna, R., Olken, B. A., & Tobias, J. (2012). Targeting the poor: Evidence from a field experiment in Indonesia. *American Economic Review*, 102(4), 1206–1240.
- Ansoms, A., Marijnen, E., Cioffo, G., & Murison, J. (2017). Statistics versus livelihoods: questioning Rwanda's pathway out of poverty. *Review of African Political Economy*, 44(151), 47–65.
- Banerjee, A., Hanna, R., Olken, B. A., & Lisker, D. S. (2024). Social Protection in the Developing World. Unpublished Working Paper. Retrieved from https://economics.mit.edu/sites/default/files/2023-08/ Social{_}Protection{_}paper{_}manuscript.pdf
- Basinga, P., Gertler, P. J., Binagwaho, A., Soucat, A. L. B., Sturdy, J., & Vermeersch, C. M. J. (2011). Eff ect on maternal and child health services in Rwanda of payment to primary health-care providers for performance : an impact evaluation. *The Lancet*, 377(9775), 1421–1428.
- Bergolo, M., & Cruces, G. (2014). Work and tax evasion incentive effects of social insurance programs. Evidence from an employment-based benefit extension. *Journal of Public Economics*, 117, 211–228.
- Blundell, R., & Costa Dias, M. (2009). Alternative approaches to evaluation in empirical microeconomics. *Journal of Human Resources*, 44(3), 565–641.
- Cattaneo, M. D. (2010). Efficient semiparametric estimation of multi-valued treatment effects under ignorability. *Journal of Econometrics*, *155*(2), 138–154.
- Chan, M. K., & Moffitt, R. (2018). Welfare Reform and the Labor Market. *Annual Review of Economics*, *10*, 347–381.
- Chandra, A., Handel, B., & Schwartzstein, J. (2019). Behavioral economics and health-care markets. In *Handbook of behavioral economics* (Vol. 2, pp. 459–502). Elsevier B.V.
- Chemouni, B. (2014). Explaining the design of the Rwandan decentralization: elite vulnerability and the territorial repartition of power. *Journal of Eastern African Studies*, 8(2), 246–262.
- Chemouni, B. (2018). The political path to universal health coverage : Power , ideas and community-based health insurance in Rwanda. *World Development*, *106*, 87–98.
- Chernozhukov, V., Fernández-Val, I., & Melly, B. (2022). Fast algorithms for the quantile regression process. *Empirical Economics*, 62(1), 7–33.
- Currie, J., & Madrian, B. C. (1999). Health, health insurance and the labor market. In O. Ashenfelter & D. Card (Eds.), *Handbook of labor economics* (pp. 3309–3416).
- Daw, J. R., & Hatfield, L. A. (2018). Matching and Regression to the Mean in Difference-in-Differences Analysis. *Health Services Research*, 53(6), 4138–4156.
- del Valle, A. (2021). The effects of public health insurance in labor markets with informal jobs: Evidence from Mexico. *Journal of Health Economics*, 77, 102454.
- Diegert, P., Masten, M. A., & Poirier, A. (2023). Assessing Omitted Variable Bias when the Controls are Endogenous. Retrieved from http://arxiv.org/abs/2206 .02303
- Diop, F. P., & Ba, A. (2010). *Mutual Health Insurance*, *Scaling-Up and the Expansion of Health Insurance in Africa* (Tech. Rep.). Bethesda; Maryland: Abt Associates.

- Diop, F. P., & Butera, J. D. (2005). *Community-Based Health Insurance in Rwanda* (No. November). Washington DC.
- Dushimimana, M.-A. (2019, feb). *Ubudehe categories to be reviewed*. Kigali. Retrieved from https://www.newtimes.co.rw/news/ubudehe-categories-be -reviewed
- Eggers, A., Tunon, G., & Dafoe, A. (2023). Placebo Tests for Causal Inference. *American Journal of Political Science*, 00(0), 1–16.
- Einav, L., Finkelstein, A., & Schrimpf, P. (2017). Bunching at the kink: Implications for spending responses to health insurance contracts. *Journal of Public Economics*, 146, 27–40.
- Ezeanya, C. (2015). Home-grown and grassroots-based strategies for determining inequality towards policy action Rwanda's Ubudehe approach in perspective (Vol. 2015 / 008; Tech. Rep.). Helsinki: UN-WIDER.
- Ezeanya-Esiobu, C. (2017). The Rise of Homegrown Ideas and Grassroots Voices : New Directions in Social Policy in Rwanda (Tech. Rep. No. 6). Paris: UNRISD.
- Fatima, F., & Yoshida, N. (2018). Revisiting the Poverty Trend in Rwanda: 2010/11 to 2013/14 (No. September). Washington DC.
- Fredriksson, A., & de Oliveira, G. M. (2019). Impact evaluation using Difference-in-Differences. *RAUSP Management Journal*, 54(4), 519–532.
- Frölich, M., & Landmann, A. (2018). Effects of Insurance on Child Labour: Ex-Ante and Ex-Post Behavioural Changes. *Journal of Development Studies*, 54(6), 1002–1018.
- Garcia-Mandico, S., Reichert, A., & Strupat, C. (2021). The social value of health insurance results from Ghana. *Journal of Public Economics*, 194, 104314.
- Gaynor, N. (2016). Beneath the veneer: decentralisation and post-conflict reconstruction in Rwanda. *Third World Thematics: A TWQ Journal*, 1(6), 779–798.
- Gertler, P. G., Martinez, S., Premand, P., Rawlings, L. B., & Vermeersch, C. M. J. (2016). *Impact Evaluation in Practice* (Second ed.). Washington DC: The World Bank.
- Government of Rwanda. (2016). *Governing the Organisation, Functioning and Management* of Health Insurance Schemes in Rwanda. Government of Rwanda.
- Government of Rwanda. (2020). Government of Rwanda. The Prime Minister's order related to the community-based health insurance scheme subsidies (No. February). Government of Rwanda. Retrieved from https://gazettes.africa/archive/rw/2020/ rw-government-gazette-dated-2020-10-01-no-Special.pdf
- Gruber, J. (2000). Health insurance and the labor market. In *Handbook of health economics* (Vol. 1, pp. 645–706). Elsevier Science B.V.
- Gruber, J., & Madrian, B. C. (2004). Health Insurance, Labor Supply, and Job Mobility: A Critical Review of the Literature. *Health Policy and the Uninsured*, Washington, D.C.: Urban Institute Press.
- Gruber, J., & Yelowitz, A. (1999). Public health insurance and private savings. *Journal of Political Economy*, *107*(6), 1249–1274.
- Habimana, D., Haughton, J., Nkurunziza, J., & Haughton, D. M. A. (2021). Measuring the impact of unconditional cash transfers on consumption and poverty in Rwanda. World Development Perspectives, 23(November 2020), 100341.
- Hanna, R., & Karlan, D. (2017). Designing Social Protection Programs. In A. V. Banerjee & E. Duflo (Eds.), *Handbook of economic field experiments* (Vol. 2, pp. 515–553). Elsevier Ltd. Retrieved from http://dx.doi.org/10.1016/bs.hefe.2016 .07.002
- Hasselskog, M. (2016). Participation or What? Local Experiences and Perceptions of Household Performance Contracting in Rwanda. *Forum for Development Studies*, 43(2), 177–199.
- Hasselskog, M. (2018). Rwandan "home grown initiatives": Illustrating inherent contradictions of the democratic developmental state. *Development Policy Review*, *36*(3), 309–328.
- Hasselskog, M., & Schierenbeck, I. (2015). National policy in local practice: the case of Rwanda. *Third World Quarterly*, *36*(5), 950–966.
- Heim, B. T., Hunter, G., Isen, A., Lurie, I. Z., & Ramnath, S. P. (2021). Income Responses to the Affordable Care Act: Evidence from a Premium Tax Credit Notch. *Journal of Health Economics*, 76, 102396.
- Hillebrecht, M., Klonner, S., A Pacere, N., & Souares, A. (2020). Community-Based versus Statistical Targeting of Anti-Poverty Programs: Evidence from Burkina Faso. *Journal of African Economies*, 29(3), 271–305.
- Holtz-Eakin, D., Penrod, J. R., & Rosen, H. S. (1996). Health insurance and the supply of entrepreneurs. *Journal of Public Economics*, 62(1-2), 209–235.
- Hong, S.-H. (2013). Measuring the Efffect of Napster on Recorded Music Sales: Difference-in-Differences Estimates Under Compositional Changes. *Journal of Applied Econometrics*, 28, 297–324.
- Houssou, N., Asante-Addo, C., Andam, K. S., & Ragasa, C. (2019). How Can African Governments Reach Poor Farmers with Fertiliser Subsidies? Exploring a Targeting Approach in Ghana. *Journal of Development Studies*, 55(9), 1983–2007.
- Kalisa, I. R., Musange, S. F., Collins, D., Saya, U., & Kunda, T. (2016). *The Development of Community-Based Health Insurance in Rwanda: Experiences and Lessons* (Tech. Rep. No. March). Kigali: Management Sciences for Health.
- Kalk, A., Groos, N., Karasi, J.-c., & Girrbach, E. (2010). Health systems strengthening through insurance subsidies : the GFATM experience in Rwanda. *Tropical Medicine* and International Health, 15(1), 94–97.
- Kidd, S., & Athias, D. (2019). *Hit and miss medicine: An assessment of targeting effectiveness in social protection* (No. March). Sidcup, United Kingdom.
- Kleven, H. J. (2016). Bunching. Annual Review of Economics, 8, 435–464.
- Koch, R., Nkurunziza, T., Rudolfson, N., Nkurunziza, J., Bakorimana, L., Irasubiza, H., ...
 Kateera, F. (2022). Does community-based health insurance protect women from financial catastrophe after cesarean section? A prospective study from a rural hospital in Rwanda. *BMC Health Services Research*, 22(1), 1–11.
- Kwibuka, E. (2017, aug). *Mutuelle de Santé: Officials report improved subscription*. Kigali. Retrieved from https://www.newtimes.co.rw/section/read/217731
- La Ferrara, E., & Milazzo, A. (2017). Customary Norms, Inheritance and Human Capital: Evidence from a Reform of the Matrilineal System in Ghana. *American Economic Journal: Applied Economics*, 9(4), 166–185.
- Landmann, A., & Frölich, M. (2015). Can health-insurance help prevent child labor? An impact evaluation from Pakistan. *Journal of Health Economics*, *39*, 51–59.
- Lannes, L., Meessen, B., Soucat, A., & Basinga, P. (2016). Can performance-based financing help reaching the poor with maternal and child health services? The experience of Rwanda. *International Journal of Health Planning and Management*, 31, 309 – 348.
- Le, N., Groot, W., Tomini, S. M., & Tomini, F. (2019a). Effects of health insurance on labour supply: a systematic review. *International Journal of Manpower*, 40(4), 717–767.

- Le, N., Groot, W., Tomini, S. M., & Tomini, F. (2019b). Effects of health insurance on labour supply: evidence from the health care fund for the poor in Vietnam. *Applied Economics*, 51(58), 6190–6212.
- Lee, K. M. (2019). Health Insurance and the Supply of Entrepreneurs: Evidence from the ACA Medicaid Expansion. *SSRN Electronic Journal*, 2019(1), 12219.
- Liao, P. A. (2011). Heterogeneous impact of Taiwan's National Health Insurance on labor force participation of married women by income and family structures. *Health Care for Women International*, 32(2), 154–173.
- Logie, D. E., Rowson, M., & Ndagije, F. (2008). Innovations in Rwanda's health system: looking to the future. *The Lancet*, *372*(9634), 256–261.
- Lu, C., Chin, B., Lewandowski, J. L., Basinga, P., Hirschhorn, L. R., Hill, K., ... Binagwaho,
 A. (2012). Towards Universal Health Coverage : An Evaluation of Rwanda Mutuelles in Its First Eight Years. *PLoS ONE*, 7(6), e39282.
- Luo, T., & Escalante, C. L. (2020). Public Health Insurance and Farm Labor Supply: Evidence from China's Rural Health Insurance Reform. *China and World Economy*, 28(6), 101– 124.
- Mbabazi, D. (2019, feb). Call for transparency as public weighs in on Ubudehe categorisation process. Kigali. Retrieved from https://www.newtimes.co.rw/article/ 164348/News/call-for-transparency-as-public-weighs-in-on -ubudehe-categorisation-process
- MINALOC. (2016). *Revised Ubudehe Categories are Out*. Retrieved 2019-02-16, from https://tinyurl.com/t746rpmw
- Moffitt, R. A. (2004). Welfare Programs and Labor Supply. *Handbook of Public Economics*, 2, 2393–2430.
- MoH. (2012). *Community Based Health Insurance: Annual Report* (Tech. Rep. No. October). Kigali: Ministry of Health.
- Molina-Vera, A. (2021). The impact of an extension of workers' health insurance on formal employment: Evidence from Ecuador. *World Development*, *141*, 105364.
- Mutanganshuro, L. (2020, dec). Ubudehe: 40% households missed out on classifications. Kigali. Retrieved from https://www.newtimes.co.rw/news/ubudehe-40 -households-missed-out-classifications
- Nirere, M. (2022). Do social protection cash transfers reduce poverty in Rwanda? Evidence from an econometric analysis of Vision Umurenge Program Direct Support. African Development Review, 34(1), 114–126.
- NISR. (2006). Integrated Living Conditions Survey 2005/06: Preliminary Poverty Update Report (Tech. Rep.). Kigali: National Institute of Statistics of Rwanda.
- NISR. (2012). The Third Integrated Household Living Conditions Survey (EICV3): Main Indictors Report (Tech. Rep.). Kigali: National Institute of Statistics of Rwanda.
- NISR. (2015a). Rwanda Integrated Household Living Conditions Survey (EICV) 2013/2014: Social Protection and VUP Report (Tech. Rep.). Kigali: National Institute of Statistics of Rwanda.
- NISR. (2015b). Rwanda Poverty Profile Report 2013/2014: Results of Integrated Household Living Conditions Survey (EICV) (Tech. Rep.). Kigali: National Institute of Statistics of Rwanda.
- NISR. (2018a). The Fifth Integrated Household Living Conditions Survey 2016/2017: Rwanda Poverty Panel Report (Tech. Rep.). Kigali: National Institute of Statistics of Rwanda.

- NISR. (2018b). The Fifth Integrated Household Living Conditions Survey (EICV5) 2016/17: Rwanda Poverty Profile Report (Tech. Rep.). Kigali: National Institute of Statistics of Rwanda.
- NISR, MOH, & ICF. (2021). *Rwanda Demographic Health Survey 2019-20* (Tech. Rep.). Kigali, Rwanda, and Rockville, Maryland, USA: National Institute of Statistics of Rwanda, Mininstry of Health Rwanda and ICF.
- Njuki, J., Poole, J., Johnson, N., Baltenweck, I., Pali, P., & Mburu, S. (2011). *Gender*, *Livestock and Livelihood Indicators* (Tech. Rep. No. October). Nairobi: International Livestock Research Institute.
- Nkurunziza, M. (2023a, jun). Govt to list households in extreme poverty for support. Kigali. Retrieved from https://www.newtimes.co.rw/article/8371/ news/rwanda/govt-to-list-households-in-extreme-poverty-for -support
- Nkurunziza, M. (2023b, mar). What next after govt scraps Ubudehe categories? Kigali. Retrieved from https://www.newtimes.co.rw/article/6292/ news/rwanda/what-next-after-govt-scraps-ubudehe-categories
- Nkurunziza, M. (2024, mar). New social registry system to lift over 900,000 households out of poverty. Kigali. Retrieved from https://www.newtimes.co.rw/article/ 14950/news/rwanda/new-social-registry-system-to-lift-over -900000-households-out-of-poverty
- Ntirenganya, E. (2020, oct). Where do you fall in the new Ubudehe categories? Kigali. Retrieved from https://www.newtimes.co.rw/news/where-do -you-fall-new-ubudehe-categories
- Nyman, J. A. (2001). The income transfer effect, the access value of insurance and the Rand health insurance experiment. *Journal of Health Economics*, 20, 295–298.
- Nyman, J. A. (2008). Health insurance theory: The case of the missing welfare gain. *European Journal of Health Economics*, 9(4), 369–380.
- Oster, E. (2019). Unobservable Selection and Coefficient Stability : Theory and Evidence Unobservable Selection and Coefficient Stability : Theory and Evidence. *Journal of Business & Economic Statistics*, *37*(2), 187–204.
- Premand, P., & Schnitzer, P. (2020). Efficiency, Legitimacy, and Impacts of Targeting Methods: Evidence from an Experiment in Niger. *The World Bank Economic Review*, 0(0), 1–29.
- Ridde, V., Antwi, A. A., Boidin, B., Chemouni, B., Hane, F., & Touré, L. (2018). Time to abandon amateurism and volunteerism : addressing tensions between the Alma-Ata principle of community participation and the effectiveness of community-based health insurance in Africa. *BMJ Global Health*, *3*, e001056.
- Roth, J., Sant'Anna, P. H., Bilinski, A., & Poe, J. (2023). What's trending in difference-indifferences? A synthesis of the recent econometrics literature. *Journal of Econometrics*, 235(2), 2218–2244.
- Rugira, L. (2014, oct). When 'tekiniki' goes sour. Kigali. Retrieved from https://www.newtimes.co.rw/article/112483/Opinions/when -tekiniki-goes-sour
- Rutikanga, C. (2019). A Social Work Analysis of Home-grown Solutions and Poverty Reduction in Rwanda : The Traditional Approach of Ubudehe. In J. M. Twikirize & H. Spitzer (Eds.), *Social work practice in africa: Indigenous and innovative approaches* (pp. 61–80). Kampala, Uganda: Fountain Publishers.

- Ryan, A. M., Burgess Jr, J. F., & Dimick, J. B. (2015). Why We Should Not Be Indifferent to Specification Choices for Difference-in-Differences. *Health Services Research*, *50*(4), 1211–1235.
- Sabates-Wheeler, R., Yates, S., Wylde, E., & Gatsinzi, J. (2015). Challenges of Measuring Graduation in Rwanda. *IDS Bulletin*, 46(2), 103–114.
- Saez, E. (2010). Do taxpayers bunch at kink points? *American Economic Journal: Economic Policy*, 2(3), 180–212.
- Sant'Anna, P. H., & Zhao, J. (2020). Doubly robust difference-in-differences estimators. *Journal of Econometrics*, 219(1), 101–122.
- Sant'Anna, P. H. C., & Xu, Q. (2023). Difference-in-Differences with Compositional Changes. Unpublished Working Paper. Retrieved from http://arxiv.org/abs/ 2304.13925
- Schmidt, J.-O., Mayindo, J. K., & Kalk, A. (2006). Thresholds for health insurance in Rwanda
 : who should pay how much ? *Tropical Medicine and International Health*, 11(8), 1327–1333.
- Schneider, P. (2005). Trust in micro-health insurance : an exploratory study in Rwanda. *Social Science & Medicine*, *61*, 1430–1438.
- Schneider, P., & Hanson, K. (2006). Horizontal equity in utilisation of care and fairness of health financing : a comparison of micro-health insurance and user fees in Rwanda. *Health Economics*(31), 19–31.
- Schnitzer, P. (2019). How to Target Households in Adaptive Social Protection Systems? Evidence from Humanitarian and Development Approaches in Niger. *Journal of Development Studies*, 55(sup1), 75–90.
- Shen, Z., Parker, M., Brown, D., & Fang, X. (2017). Effects of public health insurance on labor supply in rural China. *China Agricultural Economic Review*, 9(4), 623–642.
- Shi, J. (2016). Income Responses to Health Insurance Subsidies: Evidence from Massachusetts. *American Journal of Health Economics*, 2(1), 976–980.
- Stoeffler, Q., Fontshi, F., & Lungela, A. (2020). Targeting in Practice: A Review of Existing Mechanisms for Beneficiary Selection in the Democratic Republic of Congo. *Journal of International Development*, 32(5), 824–829.
- Stuart, E. A., Huskamp, H. A., Duckworth, K., Simmons, J., Song, Z., Chernew, M. E., & Barry, C. L. (2014). Using propensity scores in difference-in-differences models to estimate the effects of a policy change. *Health Services and Outcomes Research Methodology*, 14(4), 166–182.
- The East African. (2015, mar). *Musanze*, *Nyabihu residents protest new Ubudehe categories*. Kigali. Retrieved from https://www.theeastafrican.co.ke/tea/rwanda-today/news/musanze-nyabihu-residents-protest -new-ubudehe-categories--1338362
- Uysal, D. S. (2015). Doubly Robust Estimation of Causal Effects with Multivalued Treatments: An Application to the Returns to Schooling. *Journal of Applied Econometrics*, *30*, 763–786.
- Vogel, L. (2011). Rwanda hikes premiums in health insurance overhaul. *Canadian Medical Association Journal*, *183*(13), 973–974.
- Wagstaff, A., Eozenou, P., & Smitz, M. (2020). Out-of-pocket expenditures on health: A global stocktake. *World Bank Research Observer*, *35*(2), 123–157.
- Wagstaff, A., Flores, G., Smitz, M.-f., Hsu, J., Chepynoga, K., & Eozenou, P. (2018). Progress on impoverishing health spending in 122 countries : a retrospective observational study. *Lancet Global Health*, 6, e180–192.

- Weil, D. N. (2007). Accounting for the effect of health on economic growth. *Quarterly Journal* of *Economics*, 122(3), 1265–1306.
- Williams, T. P., Nzahabwanayo, S., Lavers, T., & Ndushabandi, E. (2020). Distributing social transfers in Rwanda: The case of the Vision 2020 Umurenge Programme (VUP). *Effective States and Inclusive Development Working Papers*(160), 1–32.
- Woldemichael, A., Gurara, D., & Shimeles, A. (2019). The Impact of Community Based Health Insurance Schemes on Out-of-Pocket Healthcare Spending: Evidence from Rwanda. *IMF Working Papers*, 19(38), 1.
- Zeldow, B., & Hatfield, L. A. (2021). Confounding and regression adjustment in differencein-differences studies. *Health Services Research*, 1–10.