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Susmita Baulia

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University of Turku

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To my parents

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ABSTRACT

This doctoral dissertation explores the challenges that lead to underutilisation of welfare products and policies in credit and health, among the poor in developing countries. The essays in the area of Applied Microeconomics included herein shed light on whether resources in welfare policies for the poor are channelised along the right path, and if not, how they could be improved.

The first essay studies take-up issues of microcredit loans. In a decision-making experiment in the laboratory, I find that the take-up increases when prospective borrowers are offered a flexible choice between joint and individual liability loans. Results suggest that more risk-averse borrowers are less willing to take up individual liability loan, and less selfish borrowers are more inclined to take up joint liability loan. The results collectively imply that microloan contracts must be designed according to heterogeneous preferences of borrowers in order to increase take-up; furthermore, there should be enough flexibility in the offered choice-set that leads to better self-selection.

In the second essay, I conduct an empirical investigation of Ugandan households and find that while facing a negative income shock or an adverse health shock, poor households are more prone to take their children for immunisation. The findings highlight that adults in low-income households engage more in their children's preventive healthcare when the opportunity cost of being away from work is low. Therefore, concerning policy, either price subsidies to offset the opportunity cost or strict mandates on healthcare practices are necessary.

The final essay investigates the role of demand-side incentives to mothers and supply-side incentives to community health workers (ASHAs) in improving maternal and child health, in a nationwide health intervention in India. The programme entitled socio-economically backward mothers with cash assistance if they chose to give birth at public health institutions, and simultaneously employed ASHAs to act as a direct link between a pregnant woman and the public healthcare delivery system. Eligible mothers with both cash transfer and ASHA's guidance outperformed the eligible mothers receiving only cash transfer in various maternal and neonatal outcomes. This validates that direct monetary incentives to the mother can improve her uptake of maternal healthcare. Nevertheless, the stronger effect of the ASHA's presence ascertains that information on the importance of health and healthcare can bridge the gap to the low use of healthcare by the poor and that it can be effectively addressed by incentivising the supply-side.

KEYWORDS: Development policy, take-up, microcredit, household shocks, preventive healthcare, time allocation, conditional cash transfer, maternal health, child health

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TIIVISTELMÄ

Tämä väitöskirja tutkii kehittyvissä maissa asuvien köyhien kohtaamia haasteita, jotka johtavat hyvinvointipolitiikan ja -tuotteiden puutteelliseen hyödyntämiseen. Väitöskirja sisältää johdantokappaleen sekä kolme esseetä soveltavan mikrotaloustieteen alalta. Jokaisen esseen kohdalla tarkastellaan sitä, ohjautuvatko köyhiin kohdistetut hyvinvointipolitiikat oikein, ja jos eivät, miten hyvinvointipolitiikkaa voitaisiin tehdä paremmin.

Ensimmäinen essee käsittelee mikroluottojen puutteellista hyödyntämistä. Päätöksentekoa tarkastelevan laboratoriokokeen tulosten mukaan luottoja hyödynnetään enemmän, mikäli lainanottajille tarjotaan mahdollisuus valita joustavasti yhteis- tai yksilövastuullinen laina. Riskiä karttavat lainanottajat ovat haluttomampia ottamaan lainan, josta he ovat yksin vastuussa. Lisäksi itsekkäämmät lainanottajat välttävät ottamasta lainoja, joista he ovat vastuussa yhdessä muiden kanssa. Nämä tulokset yhdessä tarkoittavat, että mikroluottosopimukset tulisi suunnitella huomioimalla lainanottajien yksilölliset preferenssit, jotta niiden hyödyntäminen kasvaisi. Lisäksi tarjotuissa lainoissa tulisi olla riittävästi joustavuutta, joka johtaisi parempaan itsevalikoitumiseen.

Toinen essee käsittelee sitä, miten köyhät ugandalaiset kotitaloudet investoivat ennaltaehkäisevään terveydenhuoltoon kohdatessaan yksilökohtaisia shokkeja. Tulosten mukaan vanhemmat vievät lapsensa rokotettavaksi todennäköisemmin, mikäli kotitalous kohtaa negatiivisen tulo- tai terveysshokin. Löydökset korostavat sitä, että matalatuloiset kotitaloudet käyttävät enemmän aikaa lasten ennaltaehkäisevään terveydenhuoltoon, kun työstä poissaolon vaihtoehtoiskustannus on matala. Politiikan kannalta hintoihin kohdistuvat tuet vaihtoehtokustannuksen tasoittamiseksi tai tiukka sääntely terveydenhuoltoon liittyen näyttäisi olevan välttämätöntä.

Viimeinen essee käsittelee kysyntäpuolelle (äidit) ja tarjontapuolelle (terveydenhoitajat) kohdistettujen kannustimien vaikutuksia äitien ja lasten terveyteen. Intiassa tehty terveydenhuoltoreformi tarjosi huonossa sosioekonomisessa asemassa oleville äideille rahallista avustusta, mikäli he päättivät synnyttää julkisessa terveydenhuoltolaitoksessa. Samanaikaisesti palkattiin terveydenhoitajia ohjaamaan äitejä synnyttämään julkisen terveydenhuollon palveluita hyödyntäen. Ne äidit, jotka avustusten lisäksi saivat ohjausta terveydenhoitajilta, pärjäsivät paremmin useilla mittareilla. Äideille kohdennetut suorat rahalliset kannusteet voivat siis lisätä äideille suunnattujen terveydenhuoltopalveluiden käyttöä. Tämän lisäksi informaatiolla voidaan todeta olevan tärkeä rooli, sillä terveydenhoitajien läsnäollessa reformin vaikutukset voimistuivat, eli informaatio voi lisätä köyhien terveyspalveluiden käyttöä. Informaatiota voidaan myös lisätä tehokkaasti tarjontapuolen kannusteilla.

ASIASANAT: Kehittämispolitiikka, palveluiden hyödyntäminen, mikroluotto, kotitalouden shokit, ennaltaehkäisevä terveydenhuolto, ajankäyttö, ehdollisen rahansiirron järjestelmät, äitien terveys, lasten terveys

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"Everything comes to us that belongs to us if we create the capacity to receive it."
– Rabindranath Tagore

Today, at these crossroads of bidding farewell to one chapter of my professional life and moving on to the next, I reminisce on those moments that summed up to here. A six-years long bittersweet journey - there were times when I feared that I would never be able to arrive at this point, and yet there were moments that propelled me forward to see it to the end. Without any doubt, this endeavour would have been impossible if not for some individuals, who guided me, inspired me and assured me in their own extraordinary ways along this journey. This dissertation is a culmination of their faith in me, and they deserve my deepest gratitude.

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Along the way, I have been fortunate enough to be able to learn and discuss my work with Professor Topi Miettinen of Hanken School of Economics and Dr Nadine Chlass of the University of Jena. Their incredible knowledge and curious questions have helped me immensely to learn my way around experiments. Workshops organised by our Aboa Centre of Economics (now, updated to Turku School of Economics Research) and the Finnish Doctoral Programme in Economics at HECER were beneficial to discuss my work with fellow doctoral students and senior researchers. These rigorous training sessions were indeed instrumental in my development as an economist. I have also been privileged to be able to present my research on various occasions in international workshops and conferences. I deeply value those opportunities since they have given me a better understanding of good research. I have always returned from those places with a positive mind, be it with a new research topic brewing in my head or another method to try.

No PhD journey is complete without trying one's mettle in teaching. I am glad to have had the opportunity to be a teaching assistant to Professor Hannu Salonen and Professor of Practice Ritva Reinikka of Aalto University. Their trust in me with assisting them in advanced courses have trained me to pay attention to details and hone my teaching skills. Professor Salonen has always been patient to discuss with me even the most laborious problem sets, which helped to keep my fundamentals clear. Professor Reinikka came in with remarkable knowledge and experience in the field of development policy. In a short time, she gave me the courage and inspiration to continue my research as a development economist.

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simply lamented together at how mundane our lives as doctoral students are, and sometimes we have revelled together in new research ideas and accomplishments. Especially in these last few months of remote work, I have realised how much of a positive influence they can otherwise have on my work quality and me. In this regard, I extend my deepest gratitude to the entire Department of Economics in Turku School of Economics for their solidarity and for the pleasant academic home they have provided me along these years.

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Finally, I thank my greatest asset Debanga, for loving me unconditionally and believing in me boundlessly. His relentless enthusiasm and undaunted companionship give me the courage in life and teach me perseverance. I am thankful to have grown up with him, discovering life together. In a parallel world, I am probably still hopelessly lost without him.

Turku/Åbo, October 2020
Susmita Baulia

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List of Original Research Articles

This dissertation is based on the following original research articles, which are referred to in the text by their Roman numerals:

I Baulia, Susmita. Take-up of joint and individual liability loans: An analysis with laboratory experiment. *Journal of Behavioral and Experimental Economics*, 2019; 82: 101456.

II Baulia, Susmita. Is household shock a boon or bane to the utilisation of preventive healthcare for children? Evidence from Uganda.

III Baulia, Susmita. Cash incentives to mothers or to community health workers - what contributes better to the health of the mother and the newborn? Evidence from India.

1 Introduction

1.1 Background

The combined wealth of the 26 richest people in the world was the same as the combined wealth of the world's poorest 3.8 billion people in 2018.[1] Since the 2008 financial crisis, the wealth of the billionaires has grown by 12%, whereas over the same period, the wealth of the world's 3.8 billion poorest people declined by 11%. According to the World Bank's most recent estimates in 2015, 10% of the world's population, that is, about 734 million people were living on less than $ 1.90 per day during that time. Although this number has gone down by 26 percentage points since 1990, the share of the poor according to the *multidimensional*[2] definition that includes consumption, education, health and access to basic amenities, is about 50% higher than what the monetary poverty threshold states. For example, in Sub-Saharan Africa, over one-fifth of children between ages 6-11 years are out of school, followed by one-third of youth between the ages of 12-14 years. These are the highest rates for education exclusion in the world.[3] In terms of health, Sub-Saharan Africa and South Asia still have the neonatal mortality rates at least over 25, child mortality (5-14 years) being the highest in the former.[4]

1 Source: Oxfam Report in World Economic Forum, 2019

2 The move from *unidimensional* to *multidimensional* concept of poverty evolved as the approaches such as *basic needs* (as opposed to the increase in income), *social exclusion* and Amartya Sen's *capability approach*, together called for understanding the actual satisfaction of basic needs. In contrast to the income method of measuring poverty, Sen advocated the 'direct method' of poverty identification, which assesses human deprivation in terms of shortfalls from minimum levels of basic needs *per se*. The reasoning for this being - while an increase in purchasing power allows the poor to achieve their basic needs better, markets for all basic needs may not always exist. Alongside, empirical findings that income does not correctly proxy non-monetary deprivations for identifying the poor, have ushered in the importance of having multidimensional measures of poverty (Sen, 1981; Alkire et al., 2015).

3 Source: UNESCO Institute of Statistics, 2020

4 Source: United Nations Inter-agency Group for Child Mortality Estimation (UNIGME), 2019

Even more unsettling than the sheer population size below the monetary poverty line and in all-round deprivation, is that these people do not have the tools to climb out of poverty by themselves. However, what is inspiring is that, with an external positive push, they do stand a chance to do better. Although much has been done in the past few decades by the policy-makers and researchers worldwide, to help them out of poverty through various welfare policies, the goal is still quite far from achieved. Some welfare policies work, many fail. So the question that arises here is that are we doing enough to fight the issue? Or, is it so that the resources are not being channelised in the right path? This dissertation aims to delve into the latter question.

The take-up of new products and services lies where demand and supply meet. Individuals decide on whether to borrow money, open a savings account or buy health insurance based on their needs and preferences, as well as the products and services offered (Karlan et al., 2010). Often the take-up of a particular policy is low because various intrinsic costs exceed its benefits. This dissertation lies at the crux of that issue for the poor. Through the three essays in this dissertation, I explore the challenges that often lead to sub-optimal take-up or underutilisation of welfare products and policies among the poor. While the first two essays investigate some facets of the demand-side challenges, the third essay also highlights a supply-side challenge in parallel. The first essay concerns the take-up of microcredit loans, which has been a prominent credit policy for the poor in developing countries; the next two essays are about the take-up of healthcare.

In this introductory section, I present an overview of the states of credit and health in developing countries and the response of the poor towards some related policies. Then, I discuss what the existing literature says on the utilisation of those policies. In Section 2, I summarise the three essays, present their results and discuss their contributions to the existing literature.

1.2 Credit and Health in Developing Countries

While the need for credit can be considered as the *means* in the resource-constrained poor households, health can be regarded as the *end*. Therefore, ideally, if the means are addressed, they should trickle down to the end. However, it is not easy arithmetic. Research shows that while microcredit only has moderate effects that often do not go beyond business creation for a short term, health investment also does not come that easy for the poor, not at least in their long-term decision-making.

1.2.1 Credit

In this subsection, I discuss the evolution of microfinance in the past few decades. Although different welfare policies on financing the poor have been used in the policy frontier, microfinance is the primary one so far. In what follows, is a review of it.

With the help of financial products such as loans, savings and insurances, individuals can allocate consumption efficiently. However, the absence of functional financial markets for the poor is an obstacle. While in a traditional loan contract, a borrower uses collateral, a poor individual hardly has such resources. Thus, uncollateralised loans to the poor put the burden of loss on the lender in case of defaults, and furthermore, the interest revenue from small loans is not enough to compensate for the costs spent on screening, monitoring and enforcement. Consequently, the poor are pushed into a vicious cycle of poverty and have little chance to smooth the income shocks that they otherwise face frequently. Here came in the revolutionary concept of microfinance to help the "unbanked", initiated by Muhammad Yunus of Bangladesh, who later won the Nobel Prize for Peace for his work.

The concept of microfinance was pioneered by Bangladesh's Grameen Bank in the '80s. Within the framework of microfinance, small loans, also called microcredit loans, were given out to the poor.[5] The main objective of these loans was to help the small and informal firms to expand their businesses.

An innovative feature used in microcredit was the *joint liability* in repayment, by which loans were given to poor borrowers in groups, and the latter were jointly liable for the repayment. This liability structure implied that if one failed to repay, the other members in the group had to pay on behalf of the defaulter. In this way, it reduced the risk of loss due to default for the lending institutions. Through the joint liability mechanism, the borrowers did not necessarily have to provide any collateral, and it incentivised the borrowers in the group to monitor each other against any moral hazard and free-riding issues. The joint liability structure joined with a *fixed repayment* schedule with frequent instalments, and a *dynamic incentive* (whereby the incentive to repay is generated by the promise of access to future loans), gained an impetus around the world in the '90s. Following the successful footsteps of the Grameen Bank, a flourishing microfinance industry emerged across the developing countries, following the Grameen format. In the frontline of that industry were, BancoSol and ACCION in South America, ADIE in Europe and the Mediterranean

[5] *Microfinance* means the broad spectrum of financial services such as loans, insurance, savings provided to the people of low-income groups. However, *microcredit* implies a small loan provided at a low interest rate, to the poor to make them self-employed, i.e. to help the small entrepreneurs start their businesses.

Basin and a dozen MFIs in India. By 2003, the number of borrowers was about 120,000 with a gross loan portfolio of 30 million dollars in Asia, 21,000 borrowers with 22 million dollars portfolio in Latin America and the Caribbean (Helms, 2006).

In theoretical economics research, various mechanisms of the joint liability gained prominence (e.g. peer screening (Ghatak, 1999), peer monitoring (Stiglitz, 1990) and peer enforcement (Besley and Coate, 1995). The general idea of all these models was to shift the burden of default from the lender to a borrowing group that would give them the incentive to use local information and social ties for ensuring repayment. Although this structure helped in the expansion of loan markets in the developing countries, empirical evidence on which theoretical mechanism actually works is relatively thin. In addition to that, field studies that have directly compared default rates and repayment rates in joint liability structure *vis-à-vis* the conventional individual liability structure, have not necessarily found any significant difference (Giné and Karlan, 2014).

Moreover, when it comes to the general impact of microcredit loans, empirical evidence shows only a moderate effect. For example, Banerjee et al. (2015b) summarise from six large-scale studies across different countries that the effects of these loans neither spill over on consumption (not just in the short run, but also in the long run) nor on human capital investment, beyond some business creation.

However, another side observation comes up in all these studies, and that is the low take-up of these credit products. To give some perspective, one of the first field studies in the urban slums of India found that take-up after a study-period of three years was only nine percentage points more among the households that were offered loans in comparison to those who were not (the general take-up rate being 33% in control areas). Moreover, no significant difference in business creation was seen in the treatment and control areas (Banerjee et al., 2015a).

Since the starting point of my research on microcredit is from these consistent findings on low take-up, I discuss in detail in the following subsection, the various insights that recent literature has offered on this issue.

1.2.2 Why the low take-up of credit?

In the body of work on take-up of microcredit, some patterns stand out. The reasons for the low take-up can be broadly classified through the following channels – (a) insufficient information on availability, (b) constraints to entrepreneurial ability, and (c) lack of demand-driven product design.[6]

[6] Note, here I discuss the take-up of loans only. The take-up of other microfinance products, such as insurance, could be affected by another channel, such as liquidity constraint (Cole et al., 2013).

Targeting the poorest of the poor with microfinance products is the most difficult challenge, and this could be partially due to the significant information gap that exists between potential borrowers and lending institutions. Johnston and Morduch (2008) find in the Indonesian context that a large share of creditworthy borrowers is devoid of any loan. To bridge the gap to financial use, dissemination of information through marketing drives is crucial. In addition, on-site assessment of the potential borrowers by the lending institution personnel can reduce information asymmetry by a large extent. However, in reality, the problem lies in the fact that these borrowers bring so limited profit potential through these small loan amounts, that the banks find it too costly to introduce these additional marketing drives. Thus, the absence of information and marketing on credit products is one of the critical reasons that reduce take-up by the target groups. Cole et al. (2013) provide evidence from rural villages in India that receiving a product flyer or getting a visit from a finance educator can significantly increase the take-up rate of credit products. Finally, the passing of information through village networks about existent microloan programmes has also been useful in increasing take-up (Banerjee et al., 2013).

As loans come with liability, not everyone, particularly those who are not keen on expanding a business, are willing to take it. In a study with Tanzanian microentrepreneurs, Berge et al. (2015) find no effect on investment by giving them substantial grants. It is because these small businesses are more often constrained in consumption than in production, and therefore, using the money for consumption purposes is more optimal. On another note, often some microentrepreneurs who could otherwise make profits from the expansion of their existing businesses, have a job or housework which creates frictions for business expansion. Under such circumstances, they might invest if they get a grant but are unwilling to borrow and pay interest in order to invest (Banerjee, 2013). Risk-motivated voluntary withdrawal from the credit market (in other words, "risk-rationing") (Binswanger and Sillers, 1983; Giné and Yang, 2009) is also one of the reasons that uninsured borrowers are deterred from taking loans. It is the risk of high default costs when they are unable to repay, which discourages them. These findings starkly uncover the fact that the borrower's concerns often lie far away from the offered loans. To make things worse, most often these loans are offered with strict purposes, e.g. to be invested in business only, which further inhibits take-up. The above examples identify that these targeted potential borrowers are actually often devoid of the abilities to run a business successfully.

Low take-up of microloans also points out that they may not be tailored as per the client's needs and preferences. The welfare gains from taking a microcredit loan are subjective. Therefore, it is plausible that the take-up rate could be improved if we consider a potential borrower's risk preferences as well as other preferences

discussed in the behavioural science literature. For example, a risk-taking and dynamic individual may prefer not to get involved with a group (e.g. so as not to have to waste time monitoring neighbours) (Banerjee, 2013). This argument indicates that having different contract structures with different criteria of risk and repayment could attract borrowers of heterogeneous preferences. Evidence shows that a strict repayment schedule can discourage illiquid and risky investment (Field et al., 2013) or repel high-revenue borrowers (Barboni, 2017). Such findings prove that a grace period or a variable repayment schedule helps increasing take-up by borrowers with diverse business interests.

While varying certain features of the loan contracts has proven to have a positive effect on take-up rate, in doing so, risk preferences are found to be vital in determining the type of loan chosen. For example, Attanasio et al. (2018) find that subjective risk perceptions on the expected profit affect the demand for loans. They further find that a joint liability loan is preferred over an individual liability one as it encourages risk-sharing among members and reduces the risk involved in any project (particularly, for new business starters). Furthermore, Bertrand et al. (2010) find evidence from South African credit markets that *framing* has a substantial effect on the take-up of loans. For example, having a dominated alternative in the choice-set can drive take-up of the dominating one. Moreover, framing in terms of loss can have a more significant impact on take-up than comparable gain frames (*à la* Kahneman and Tversky (1979)). Finally, time preferences also drive take-up, e.g. individuals with hyperbolic discounting (Laibson, 1997) are more prone to take up microcredit, as difficulty in saving today makes them credit-constrained in future (Bauer et al., 2012). These results summarise that loans need to be personalised according to the preferences of borrowers, and that could be a way of increasing take-up.

All the channels discussed above broadly point out that the lack of take-up can be substantially addressed if the information gap between the lender's offer and the borrower's interests is minimised. Given the intricacies in demand from the borrower's point of view, actively studying take-up with rigorous research designs can provide more definitive answers and help understand how to design better contracts that attract more clients and serve them better. Designs that are more efficient would not only help in having diverse borrowers successfully using their contract types, but also be more profitable for the lenders in terms of spreading their risks over default. In the first essay of this dissertation, I explore how changes in the loan offerings can affect take-up due to the heterogeneity in preferences of borrowers.

In the following subsections, I shift the focus on the topic of health. The following subsection sketches the health status of the poor in developing countries and their approach to healthcare. Then follows a discussion on the insights provided by the existing literature on healthcare utilisation by the poor in these countries.

1.2.3 Health

The brunt of infectious diseases is enormous in developing countries. In 2018, there were an estimated 228 million cases of malaria worldwide, with Africa being home to 93% of the cases. An estimated 49.8 million DALYs[7] occurred from diarrhoeal diseases in 2016, equivalent to 60% of all diarrhoeal deaths, again the developing countries being the major contributors of the statistics.[8] Many of these infectious diseases can be averted by simple and cost-effective investments, e.g. using insecticide-treated bed nets, chlorinating drinking water. While these poor statistics suggest that there is room for tremendous improvement through preventive healthcare technologies, they simultaneously point out how disproportionately high the economic and social impact of these deadly diseases would be if not prevented earlier.

According to the revealed preference interpretation of the human capital theory (Grossman, 2000), a consumer will invest in health if the expected discounted private benefit, including the utility benefit, is higher than the cost, both financial and in utility terms. Therefore, not investing in healthy practices – such as vaccinating the child or chlorinating water, reflects the disutility from preventive actions, or high discount rates (due to *present bias*, or high expected mortality rates), or merely low valuation of life (Kremer and Glennerster, 2011). Empirical evidence from developing countries portrays two distinctly stylised facts in the health behaviour of poor households. First, they spend enormous amounts in curative healthcare, and second, they do not spend enough on preventive healthcare (Dupas, 2011b). It is only logical that the second stylised fact is a precursor of the first. Although in theory, it is easier to address preventive healthcare investment with an affordable solution, it is puzzling as to why it does not happen in reality for the poor. As a result, recent empirical literature is more focussed in its investigation of preventive healthcare behaviour.

In the following subsection, I focus mostly on the literature that concerns take-up or optimal utilisation of preventive healthcare, which inspires the second and third essays of this dissertation by a large extent.

[7] DALY, i.e. *Disability-Adjusted Life Year* is a metric in health statistics. DALYs for a disease or health condition are calculated as the sum of the Years of Life Lost (YLL) due to premature mortality in the population and the Years Lost due to Disability (YLD) for people living with a health condition or its consequences. For example, one DALY can be thought of as one lost year of "healthy" life.

[8] Source: World Health Organization

1.2.4 Why the low take-up of preventive healthcare?

The studies interested in the low adoption of preventive healthcare in developing countries have explored several channels through which the take-up of a range of low-cost public health products (e.g. mosquito nets, vaccinations, chlorine treatment, deworming) could be increased. In those studies, some distinct channels stand out. These channels can be broadly categorised as follows: (a) lack of information, (b) financial constraints, and (c) non-standard (behavioural) channels.

The first factor that affects take-up is information. Information related to individual and local risk factors has been useful in changing health behaviour. For example, informing households that their well water is concentrated in arsenic, can increase the chances that they move to a safer water source (Madajewicz et al., 2007; Bennear et al., 2013), or that their drinking water is contaminated with faecal bacteria can affect their adoption of purification techniques (Jalan and Somanathan, 2008). Similarly, informing adolescent girls on the risks of contracting HIV can change their sexual behaviour (Dupas, 2011a). Social learning (or, the spread of information through peers and neighbours) have also shown a positive impact on take-up behaviour. In particular, when health behaviour comes at a cost (a costly tool or technology), individuals prefer to first know from their peers about the higher returns and then invest themselves. For example, Oster and Thornton (2011) find evidence from Nepal, that having more friends who also received personal hygiene products for free, increased the likelihood of self-adoption of the product by adolescent girls. In a similar example, Dupas (2014) finds from a randomised experiment in Kenya that the individuals exposed to earlier adopters of anti-malarial bed nets, showed a higher inclination in adopting them.

The second factor to the low take-up of health-improving technologies is the financial barrier. In that, the imperfect financial market and subsequent liquidity constraints play a significant contributing role. Often investing in a new water purification technology or an insecticide-treated bed net would require lumpy investments, which the low-income households cannot always necessarily afford. Therefore, access to credit or safe saving technology is crucial for disciplining preventive health behaviour. This credit constraint becomes prominent through the findings that the take-up of preventive tools increases if the poor are allowed time to accumulate funds (Dupas, 2009), or given cash before they are offered the product/technology (Hoffmann et al., 2009), or allowed to use microfinance loans to get those (Tarozzi et al., 2014).

In a similar vein, households that do not have access to credit through borrowing, should still be able to save to acquire these technologies. However, due to the presence of imperfect financial markets, poor households cannot afford reliable saving technologies, which in turn never takes them out of the poverty trap. Dupas and Robinson (2013b) find evidence that individuals would invest more

in health if they had access to better saving technology. Finally, the fact that they respond positively to monetary (or even non-monetary) incentives also bears testimony to the fact that they are liquidity constrained.

The final contributing factors, which have also proven to drive take-up, can be explained only through the channel of specific models based in behavioural theory. In the standard economic model, individuals discount the future at a constant rate. However, in the behavioural paradigm, even though individuals would like to adopt healthy behaviour in the future, they may not yet want to sacrifice any pleasure/resources today (Laibson, 1997). This theory could explain a similar *procrastinating behaviour* while taking up preventive healthcare. While the use of commitment devices can help individuals save up for something, sometimes small nudges or incentives can also help them change this procrastinating behaviour (Dupas and Robinson, 2013b; Tarozzi et al., 2014).

Some other behavioural theories have found empirical validation in this area. For example, that take-up rises right after a promotion campaign and then subsides over time, is borrowed from the *limited attention* model. Kremer et al. (2011b) find some validation of this model in their study on the distribution of dilute chlorine solution to mothers in Kenya. Banerjee et al. (2011) find evidence in a similar vein, in a study on the use of fortified flour in India. Furthermore, Hoffmann et al. (2009) find validation of the *endowment effect* through their finding that individuals put more value to a product that they directly receive than obtaining enough money to buy it. Here, on a slightly different note, one could expect that after getting the products for free for some time, individuals might not be willing to pay to buy them next time. This argument is based on the *price anchoring effect* in the behavioural literature. However, no adverse effect of price anchoring has been found in the related empirical literature, and individuals still show interest in investing in those health technologies after receiving them for free in the past. In this case, *learning* proves to be a more vital channel of effect (Kremer et al., 2011b; Dupas, 2014).

The studies mentioned above, have applied reliable and rigorous methodologies (mostly, randomised controlled trials) in order to explore the importance of the fundamental channels that can substantially affect adoption of preventive healthcare measures. Many of the factors highlighted here have been recognised to be overcome by direct nudges and incentives to poor beneficiaries. These are endeavours to improve demand from the poor. Nonetheless, there are also supply-side drawbacks that can lead to sub-optimal take-ups, such as failed infrastructure and management. In those situations, it is imperative to incentivise the supply providers. The second essay of this dissertation delves into some challenges on the demand-side, and the third essay also highlights a supply-side challenge alongside demand.

2 Overview of the Essays

In this section, I present the summaries of the three essays that constitute this dissertation. While discussing the essays in detail, I highlight the empirical methods used, the findings, and finally, underline their main contributions to the scientific literature.

2.1 Take-up of joint and individual liability loans: An analysis with laboratory experiment

This essay draws its primary motivation from the consistent finding of low take-up of microcredit in earlier literature (Banerjee et al., 2015b). The central argument of this work is that loan selection happens from the borrower-side and thus can be affected by her preferences. Therefore, in this study, I focus on borrowers' heterogeneity in preferences and thereby try to understand if allowing borrowers to self-select into their desired loan-type is a way of increasing take-up.[9] In a laboratory microfinance experiment, I test whether the take-up rate increases when borrowers are offered a flexible choice-set with both joint liability (JL) and individual liability (IL) loans, in contrast to an offer of one loan-type only. This set-up is distinctly different from previous experiments (in lab or field) with microcredit loans where the borrowers were unable to choose between different loan-types (Giné and Karlan, 2014; Banerjee et al., 2015b).

I argue that certain features of JL and IL loan-types could be advantageous or disadvantageous to borrowers according to their preferences. One of the main features of these loans is the dynamic incentive, i.e. the promise of further loans

[9] One might argue that it is not necessarily surprising in itself that offering more loan options in the choice-set would increase the take-up rate. However, a growing body of literature on *cognitive load* in psychology confirms that an increased number of choices can often impair optimal decision-making (Iyenger and Lepper, 2000; Schwartz, 2004). Especially in this study's context, when the prospective borrowers have to choose between profit-making loan options, they might end up making poor and irrational choices, possibly be even discouraged to take any loan at all. In light of this, it is worth checking whether my hypothesis of an increased take-up rate holds for an elaborate choice-set.

from the lender in case of full repayment. With this feature, JL excels over IL because the former ensures a higher probability of repayment through a jointly liable peer-group, which in turn increases the chances of getting more loans in future. Joint liability thus reduces the risk of non-repayment after every period of a loan. Therefore, a borrower who is risk-averse regarding repayment would prefer this loan-type.

On the other hand, as the future time horizon for the availability of future loans (through dynamic incentive) is unknown, the discount factor of the borrower is likely to drive her choice. The borrower, who discounts the future less and values the long-run benefit of receiving further loans, would have a higher willingness to take up JL loan. However, for the borrower who discounts the future heavily, the short-run cost of repaying on behalf of an unsuccessful partner might surpass the fruits of receiving more loans with higher chances in the long run; this would make her prefer individual liability. Therefore, given these features, the *ex-ante* optimisation by borrowers should be influenced by their risk preferences and discount factors.

In addition to that, the taker of JL could also be willing to bear the cost of a partner's burden because she enjoys higher utility from not only her own the expected gains but also her partner's. This possibility originates from the behavioural foundations of preferences which validate that it is not uncommon for an individual to derive additional (positive/negative) utility from other's outcome, i.e. social or *other-regarding* preferences (Levine, 1998). To sum up, the possible differences in risk, discounting and other-regarding preferences incite my hypothesis that when the choice-set of loan-types is constricted, there might not be as many takers as would be with a flexible choice-set.

First, I sketch a theoretical model that includes parameters for risk, time discounting and other-regarding preferences, to analyse the borrower's decision-making when offered a choice of the two loan-types for investment in a business. I keep the model parsimonious to allow the least possible confoundments. The JL structure is designed for a two-person group, with information symmetry between each other; all other features on loan size, interest rate and business risk are kept the same across the two loan-types. Furthermore, in a JL loan, each member gets a loan for herself and can invest in her own business; it is only the repayment where they are "jointly liable", i.e. if one cannot repay, the other has to repay on behalf of the former. The repayment ability is determined by the sole success in business investment of the IL loan borrower, and at least one successful business investment of the group that borrows JL loan. Inability to repay by herself (with IL) or as a group (with JL) indicates the availability of no future loans. To summarise, the fundamental structure of the model is in line with the previous theoretical literature (Armendáriz de Aghion, 1999). Additionally, I introduce an "outside option" for

the non-takers, by designing an employment opportunity (EMPL) which has lesser risk and lesser return compared to what a successful business investment has.

The experiment was conducted with 220 university student subjects in the decision-making lab in Turku, Finland. Three treatment variations were implemented - one group was offered a choice-set of IL, JL and EMPL, another group was offered a choice-set of IL and EMPL, and the last group was offered a choice-set of JL and EMPL. Comparison of the first group with the other two groups helps evaluate whether the flexibility of being able to self-select from a bigger choice-set increases overall take-up or not. I further elicited each subject's risk, social and time preferences through auxiliary choice tasks.

I find statistically significant evidence in favour of my hypothesis that when offered a choice-set with the two loan-types, the take-up proportion is higher than an offer of just one type. With respect to IL-JL-EMPL, take-up was lower by 15.99 percentage points ($p = 0.033$) in IL-EMPL and by 13.42 percentage points ($p = 0.072$) in JL-EMPL. These are equivalent to 20% and 17% lower take-ups than in the flexible choice-set with both loans. I also find interesting effects of heterogeneous preferences, thus confirming their association with the choice-making by the borrower. The willingness to take up IL loan decreases with risk aversion; but in general, the willingness to take up *any* loan decreases with risk aversion. By testing the association between altruism and the chosen loan-type, I find that subjects choosing JL also donated more in a one-shot dictator game.[10] Finally, the findings on heterogeneous preferences validate my theoretical predictions through a reduced-form analysis.

To my knowledge, this study is the first of its kind to have aimed to examine the intertwined channels of different types of preferences that could affect take-up of microloans. To see whether at all multiple preferences are in action, only a controlled experimental set-up in a laboratory could help. Although this methodology implies a trade-off with external validity by not having the natural set-up with a contextual sample, this study's scientific contribution is adequately vital in the empirical literature of microcredit. Firstly, it contributes to the literature of low take-up through the channel of strict contract structures (or, lack of demand-driven loan structures). The finding that a flexible choice-set can increase take-up rate bears validation to that. Secondly, I prove that risk and time preferences matter in the take-up of different loan-types, and thus contribute to the growing literature

[10] The dictator game represents a workhorse in experimental economics to assess how individuals respond to situations where self-interest and equality are opposed to each other (Kahneman et al., 1986). It provides insights into the social preferences of individuals. It could be 'one-shot' or with repetitive rounds, depending on what sort of social preferences we want to learn, *altruism* or *reciprocity*.

on how the take-up of loan-types varies with preferences (Barboni, 2017; Attanasio et al., 2018; Ahlin et al., 2020). I also establish that often controlled lab experiments can successfully provide insights into how to effectively design complex policies that are hard to understand in field experiments due to various confounding factors. Finally, one side-contribution of this study is the detailed sample size and power calculation. It is a crucial yet often unpublished part of experiments, which tremendously helps in replication studies.

In essence, the outcome of this study suggests that based on borrowers' preferences, both loan-types would be valuable to offer. Nevertheless, it is important to reflect a little on how these findings fit the current scenario on microfinance loans across the world. Although my study, along with the related new line of literature, brings out the nuances of various contract structures and how those can primarily drive the take-up rate and welfare of the borrowers, there has been a prominent downward trend in the use of joint liability microloans, in general. This trend has mostly to do with the lender's digression in interest in joint liability since the early years of microfinance. With the flourish of the microfinance industry, growing commercialisation among the lending organisations has led to this shift in interest from JL loan to IL loan (Cull et al., 2009; De Quidt et al., 2018).[11] Ahlin and Suandi (2019), on the other hand, explain that joint liability works in situations where overcoming obstacles in lending, e.g. information asymmetry is problematic. According to them, this phasing out of joint liability structure is the result of an evolution of the best practices in lending, and joint liability structure being superseded by other lending innovations that are fit for a changing lending environment. With the repeated practice of lending, institutions have become experienced in overcoming the impediments like information asymmetries, enforcement limitations and weak social capital. Thus, they have shifted away from group lending with joint liability. Nevertheless, the authors agree that the JL structure could still be an efficient tool for new lending organisations, or even old ones venturing into new areas fraught with information asymmetries and limitations, as it was in the movement's early decades. This view by Ahlin and Suandi (2019) offers a

[11] In a formal analysis, with data from MIXMarket for 2008-2014, De Quidt et al. (2018) show that the shift from non-profit to for-profit organisations, along with rising competition all over the world, has led to this current status. A for-profit organisation essentially targets a different objective function than a non-profit one. The authors further argue that joint liability contracts, in general, maximise borrower's welfare, so non-profits offer JL contracts whenever they can break even while doing so. On the other hand, for-profits require not only to breakeven but also more profitability than IL. It is a strict condition, and hence the latter often end up offering a few JL contracts only.

positive aspect that research on different loan structures, including the joint liability type, still has something to offer.

On a more serious note, some critical incidents resulting from the shift in focus from *non-profit* to *for-profit* nature of organisations over the past years have left policy-makers worried about the welfare consequences of microcredit loans. For example, in India, the unregulated growth of for-profit lending organisations under the umbrella of the flourishing microfinance industry, along with coercive loan-recovery practices, resulted in suicides by hundreds of debt-trapped poor farmers at the end of the last decade. Such usurious practices resulting from the profit-driven nature of lenders defeat the supposed purpose of microcredit of improving the lives of the poor. While these outcomes suggest extreme caution in the use of microcredit loans as welfare products, they also usher in need for innovation in cash policies or alternative ways of financial inclusion of the poor.[12]

2.2 Is household shock a boon or bane to the utilisation of preventive healthcare for children? Evidence from Uganda

This essay is motivated by the second stylised fact in the health behaviour of the poor in low-income households, as highlighted earlier in Section 1.2.3. In this study using secondary panel survey data on Ugandan households, I investigate how their preventive healthcare behaviour changes in times of health and income shocks. First, I aim to understand if the take-up of preventive healthcare varies with the type of shock suffered, be it health or income-related. (Note that, by 'shock' I mean a negative shock that causes a decrease in the current level of health or income.) Second, I explore the channels through which the shocks affect take-up.

The hypothesis that take-up of preventive healthcare may react differently to a health shock *vis-à-vis* an income shock is driven by various economic as well as behavioural theories. For example, Grossman's model recognises that the demand

[12] For instance, Dupas and Robinson (2013a) already find evidence from rural Kenya that helping the poor in opening non-interest-bearing bank accounts improved their savings and then increased their productive investments. Quite interestingly, Dupas and Robinson (2013a) already point out the efficiency of such an intervention over loans; almost 87% of people took up the offered savings account, while less than three per cent of individuals initiated a loan application even after receiving assistance with the collateral requirement as found in another related study.

for healthcare is a *derived* demand, from the demand for health.[13] This means that households with reduced health stock will gain higher marginal utility from health, and hence utilise preventive healthcare more. On the other hand, an increase in preventive healthcare investment could also be related to *salience* after a health shock (Kahneman and Thaler, 2006; Seymour et al., 2007). Perhaps one could even argue through simple *economies of scale* approach that while visiting the health centre to get remedial care for the health shock, the additional cost of receiving some preventive healthcare alongside, is relatively low. All these well-founded theories indicate that a health shock could increase the take-up of preventive healthcare. On the contrary, an income shock could be expected to have a substantial income effect on credit-constrained households and subsequently reduce investment in preventive healthcare.

To test this hypothesis, I take the context of Ugandan households and their take-up behaviour of immunisation for their children. When it comes to preventive healthcare for small children, the best outcome variables to discuss are those related to immunisation. To give some perspective to the general healthcare condition in Uganda, the country holds a rank of 158/189 in the Human Development Index.[14]

The Ministry of Health recognises that 75% of the disease burden could be averted by immunisation, hygiene and sanitation, nutrition and other preventive healthcare practices. The Ugandan National Expanded Programme on Immunisation has been functional for over four decades with a goal that every Ugandan child should be fully vaccinated. Moreover, since 2001, the Ugandan National Minimum Healthcare Package entitles every Ugandan a free basic healthcare coverage at public healthcare facilities. Despite the availability of these programmes, the outcomes on child health have not been promising. In 2011, only 52% of children aged 12-23 months were fully immunised and only 40% of children aged 12-23 months were immunised before their first birthday.[15] According to the Uganda Demographic and Health Survey (2011), the rate of Vitamin A deficiency, which

[13] Michael Grossman's 1972 model is a seminal work in health economics. The model views each individual as both a producer and a consumer of health. Individuals inherit a stock of health that depreciates over time and can be augmented with investments; thus, health is viewed as a sort of capital. The model further acknowledges that improved health has both consumption and investment qualities. As a consumption, it makes us feel better. As an investment, it provides the opportunity to work more hours, more years until retirement, or more productively, either in the market or at home (Grossman, 1972).

[14] Source: United Nations Development Programme, 2018

[15] Source: Uganda Bureau of Statistics, 2012

can threaten overall immunity and cause blindness, was as high as 33% among children under five, despite the availability of immunisation doses.

Using four waves of panel data (2009-2010, 2010-2011, 2011-2012, 2013-2014) from the Uganda Nation Panel Survey, I study 1500 nationally and regionally representative households and their response to preventive healthcare for their children in times of shocks. As the outcome variable on immunisation (or, preventive healthcare), I use the receipt of Vitamin A Supplementation (henceforth, VAS) by children.[16] I further use the incidence of flood or drought as the proxy for an income shock,[17] and illness of any household member as the indicator of a health shock.

I resort to a household fixed effects regression analysis to control for a number of observable and unobservable time-invariant characteristics of the household that could potentially affect the shock incidence as well as the VAS intake by eligible children in the household. A fixed-effects analysis at household level absorbs all the across-household variations and compares children of the same household. Alongside, the effect of idiosyncratic risk is investigated, and while doing so, the time-invariant household risk factors are removed. Finally, the additional use of survey wave fixed effects allows controlling for heterogeneity arising across the survey waves. Besides using several individual-specific covariates in the model, I further examine less-parsimonious versions of the model where I control for time-varying health supply-related variables as well as geographical locations and distances to other amenities, that could confound the effect.

Main results show that the probability of taking a child to get VAS increases by 14 percentage points if an adverse health shock hits the household. Similarly, a negative income shock increases the probability of VAS intake by about nine percentage points. Given an overall sample mean of 73% VAS intake, these increases are 19.7% due to a health shock and 12.9% due to an income shock. However, these findings are statistically significant only at 10% in almost all model-variants; therefore, further research in similar settings is required in order to draw a strong conclusion.

In the Ugandan context, no direct cost is incurred by the households in getting their children immunised; however, they could face indirect costs (e.g. from transportation to healthcare facilities) and/or opportunity cost of time which they

16 The Ugandan Health Ministry and UNICEF strictly recommend that all caretakers of children between 6-59 months should take them to healthcare facilities to receive VAS every six months, as a part of their immunisation schedule.

17 More than 50% of the households in the sample have agriculture as their primary source of income. Therefore, flood and drought are central in determining their income shock.

spend in accessing healthcare services. In the event of a health shock, the latter cost (in other words, time spent away from labour market activities) seems to drive my primary findings. Evidence shows that a typical member of a household hit by a health shock spends significantly less time in labour market activities than one in a shock-free household. Thus, it could mean that the inability to be at work due to illness reduces the opportunity cost of this 'forced' time away from the labour market, which is then used for remedial care and other health-promoting activities for their children. It indeed hints to the economies of scale argument, that the additional cost of getting preventive healthcare for children is low when they already visit the healthcare facility for remedial purposes.

In case of an income shock, it is not easy to pin down the underlying mechanism. The study is able to confirm a positive average effect of the adverse income shock on time spent in the labour market. However, I further find a confirmation that a relatively wealthy household spends lesser time on average in labour activities during the shock. Such an increase in leisure hours is justified, if the household draws down assets, or borrows credit, or receives transfers to insure away the negative income shock, and also, finds it cheaper to substitute time away from the labour market.[18] With the limitation of being able to confirm this channel directly, the study only suggests that the positive effect found on the intake of VAS could be driven by the wealthier households substituting preventive healthcare activities for labour (as their opportunity cost of time away from work decreases).

Essentially, shocks are a way of identifying the *ex-ante* constraints in resources of the household. In Section 1.2.4, mostly the issue of liquidity constraints came up, while discussing the financial barriers to the adoption of preventive healthcare practices. However, this study points out yet another financial barrier. It is the opportunity cost of time spent on accessing health services instead of being spent on income-generating activities. Therefore, this study identifies that low take-up due to high demand on time could also be categorised under the financial constraint challenges in the adoption of preventive healthcare for poor households.[19] In a slightly different vein, several other studies with low-income households have highlighted that it is the distance to availing the healthcare technologies which negatively affects the take-up (Banerjee et al., 2010; Banerjee et al., 2011; Kremer et al., 2011a). I believe that while transportation in itself could impose a cost, the opportunity cost of time could also be valid in explaining their findings. In this regard, the findings of this study give a different perspective on those results.

[18] For example, previous literature already highlights that households offset transitory income shocks by using asset-holdings (either as buffer or as collateral for credit) (Deaton, 1992; Beegle et al., 2006).

[19] Previously, Miller and Urdinola (2010) have found similar results in Columbia.

Since health investment is costly as individuals must trade off time and other resources related to health, it affects the optimal demand for health. Thus I add to the literature that acknowledges that time is a crucial health input (Grossman, 1972; Gronau, 1977; Vistnes and Hamilton, 1995; Miller and Urdinola, 2010). Finally, from the finding that the adults in low-income households engage more in preventive healthcare activities for their children when the opportunity cost of being away from work is low, we could also interpret that it is not in their primary interest. Therefore, concerning policy contribution, either price subsidies to offset the opportunity cost of accessing healthcare or strict mandates on healthcare practices is necessary.

2.3 Cash incentives to mothers or to community health workers – what contributes better to the health of the mother and the newborn? Evidence from India

While financial constraint is one of the primary reasons for low take-up of preventive healthcare practices in low-income households, monetary (and, also non-monetary) incentives provide some relaxation to that constraint. Conditional Cash Transfers (henceforth, CCTs) could be an effective solution. In CCT, a lumpsum monetary incentive generates a short-run income effect. On the other hand, the conditionality of it in adopting good behaviour in health or education can positively affect the well-being, and possibly break the cycle of poverty, in the long run.[20] This essay explores such a health-related CCT in India, the *Janani Suraksha Yojana* (“Safe Motherhood Programme"). In this programme, mothers received cash benefits conditional on giving birth at public healthcare facilities. This incentive was to improve the demand-side challenges in maternal and neonatal healthcare in India. However, the main thrust of the programme lay in that it also recognised the supply-side challenges. To address the supply-side inefficiencies, the programme incentivised community health workers to improve health service

[20] While microcredit has been controversial as a credit welfare programme, CCT is relatively more successful. CCTs started in Latin America, and by the ’90s every country there was running its cash transfer programme. *PROGRESA* in Mexico is the most significant example, which started with approximately 300,000 beneficiary households in 1997 and then spread across almost 5 million households after a decade (Fiszbein et al., 2009). CCT is better and less strict than microcredit loans in the sense that it does not expect everyone to be an entrepreneur and run successful businesses, but gives the money without any repayment liability. However, it does expect the households to send children to school or get them immunised, which is naturally easier.

delivery to the beneficiary mothers. In this essay, I disentangle several intricacies in the programme eligibility and attempt to shed light on the effective channels that induce good health practices among new mothers, for themselves and their newborns. For that purpose, I use data from secondary surveys on mothers with newborns around the programme's timeline and implement a difference-in-difference identification strategy.

In the early '90s India, maternal mortality ratio (MMR) per 100,000 live births was 556 which accounted for almost 19.7% of deaths of women in their reproductive age due to issues related to pregnancy (in absolute terms, this number was as high as 152,000 maternal deaths). In addition to this, the neonatal mortality ratio (NMR) per 1000 live births was 57.4. A decade later, MMR had reduced to 374, which was equivalent to 13% of women's deaths due to maternity, and NMR was still 45.1. Between years 2001-2005, while 48.5% expecting mothers received the three WHO-recommended antenatal care check-ups, only 7.3% Indian women gave birth in the presence of any trained health professional, almost 3% did not give birth at a health facility due to lack of transportation. Only 10% of the new mothers received a postnatal visit by the health worker within two weeks of giving birth. In order to tackle this dire status of maternal and child health, the Government of India introduced this nationwide reform. The JSY programme took effect from April 2005. Its objective was to promote institutional delivery among poor pregnant women. Simultaneously, incentives were offered to village-based health workers, known as the *Accredited Social Health Activists* (ASHAs), to act as an immediate link between the beneficiary mothers and the public health system. The ASHA's primary duties lay in helping expecting mothers in the community with antenatal care, birth and postnatal care.

In its initial years of implementation, the programme underwent a few rounds of revision in terms of the mother's eligibility for the cash assistance and the ASHA's employment across states. However, broadly, eligible mothers for cash assistance were particularly the socio-economically disadvantaged ones. The JSY scheme divided the states into the high-focus and non-high focus ones, which were officially termed as the low-performing states (LPS) and high- performing states (HPS). The ASHAs were only employed in the LPS in the initial few years. I use a difference-in-difference identification strategy, where I incorporate variations across the eligibility of mothers and the presence of ASHAs, to distinctly identify the causal effects along the channel of the mother's incentive *vis-à-vis* the ASHA's incentive.

For empirical analysis, I use repeated cross-sections from the District Level Household Survey of India, which gives a sample of over 300,000 mothers. The survey provides detailed information on antenatal care, delivery (including details of receipt of JSY cash assistance) and postnatal care of the mother's most recent

birth during 2001-2008. Additionally, the survey contains information on the demographic composition and socio-economic characteristics of her household - including caste, religion, and wealth status.

Primary results of this study show that the mothers receiving both cash transfer and ASHA's counselling outperformed the mothers receiving only cash transfer, in outcomes like giving birth at public health facilities and early breastfeeding. To put the difference in perspective, an eligible mother in the high-performing state experienced 2.9 percentage points greater increase in institutional births than the ineligible mother. In contrast, for an eligible mother in a low-performing state, this increase in the difference with the ineligible mother was 7.1 percentage points. These are equivalent to changes of about 11% and 28% in institutional birth rates compared to what an ineligible mother experienced in the pre-intervention period. A similar impact is found for antenatal care and BCG vaccination for the child too. For the rural mothers with up to two births, the ASHA's channel is found to be distinctly more effective for all outcomes. However, the overall implication is that only receiving a cash transfer for giving birth at a public health facility might not be sufficient for a mother to get motivated or to overcome the costs. Continual guidance pre and post-birth by the health worker would lead to better all-round health of the mother and the newborn.

To summarise, this essay investigates a unique quasi-experimental cash transfer policy that recognises both demand-side and supply-side challenges to optimal utilisation of healthcare by the poor. The existing literature consists of studies separately looking at demand-side issues and supply-side issues, and they prove that there are obstacles to take-up on both sides.[21] The other big social welfare programmes in the format of CCT have mostly catered to the demand-side issues (e.g. *PROGRESA* in Mexico, *Bolsa Família* in Brazil). However, then, it is hard to truly understand the effectiveness of the programme if we cannot see how functional the supply-side has been. For example, in several African countries, where the health service delivery system suffers from a severe lack of organisation and management, CCTs to low-income families would not be effective at all (WHO, 2007). Now, in such a situation, experimental set-ups, such as randomised controlled trials, can give better freedom to the researcher in simulating an environment in which the supply-side can also be controlled. For example, Banerjee et al. (2010) implemented a

[21] While the entire Section 1.2.4 is dedicated to studies on demand-side barriers to take-up of healthcare, a body of literature also confirms the supply-side obstacles. For example, issues such as inadequacy of medical equipment in healthcare facilities, absenteeism of health professionals are prevalent in developing countries. In addition, the rate of usage of public healthcare is strongly correlated with absenteeism of health professionals from health facilities (Banerjee et al., 2004; Banerjee and Duflo, 2007).

randomised experiment in rural India to investigate both demand and supply-side issues of child immunisation. They set up immunisation camps in one group of villages (as supply incentive), and they provided food incentives to parents besides setting up immunisation camps in another group (as supply and demand incentives). Then they compared the mean outcomes in the two groups with a control group of villages with no incentive. However, though randomised controlled trials as by Banerjee *et alia* provide the advantage of design and rigour, they are often small-scale and hence, limited in external validity. In that regard, the quasi-experimental nature of the study in this essay gives the most robust external validation possible.[22] Moreover, in terms of findings, this study offers an improvement over the other impact evaluations of the JSY programme through its rigorous inspection of the two distinct channels of effect, namely the mother's incentive vs the ASHA's incentive.[23]

Furthermore, in terms of the channels that affect take-up in the first place, this study underlines the prevalence of financial constraints to the new mother (and her family) which often puts a barrier to the access of better healthcare and health-promoting practices. These costs could arise due to travelling across long distances in order to avail proper healthcare, or through the opportunity cost due to lost wages of the family members who have to help the mother during her pregnancy. Here, we see an example of a direct monetary incentive to the mother, which can redeem this financial barrier. Finally, the positive impact through the ASHA's counselling also ascertains that information on the importance of health and healthcare can bridge the gap to low use of healthcare. Not only that, but it also points out that the problem of low take-up arising through the channel of lack of information, can be addressed by active engagement (and incentivisation) of the supply-side. Particularly in this design, the information channel through the ASHA happens to be stronger than the relaxation of the financial constraint channel through cash assistance to the mother.

22 I acknowledge that quasi-experiments are inferior to randomised experiments in terms of internal validity (due to the lack of random assignment), but then again identification strategies like difference-in-difference are used essentially to bypass the problems arising from non-randomisation.

23 Thus, it complements nascent literature on the JSY programme that examines the two channels of incentives (e.g. (Debnath, 2018)).

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Take-up of joint and individual liability loans: An analysis with laboratory experiment☆

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ABSTRACT

This paper reports a study on decision-making by borrowers regarding take-up of different loan types in a laboratory microfinance experiment. I show that when prospective borrowers are offered a flexible choice of different loan types (here, individual liability (IL) and joint liability (JL)), take-up increases. This is due to heterogeneous borrowers self-selecting into different loan types. Results suggest that more risk averse borrowers are less willing to take up IL loan and less selfish borrowers show signs of higher inclination to take up JL loan. The results collectively imply that microloan offers need to be customized according to the heterogeneous preferences of borrowers; also, there needs to be enough flexibility in the offered choice-set for better self-selection. This would result in a substantial increase in the take-up rate of microloans by the borrowers.

1. Introduction

Joint liability (JL) mechanism of loan repayment is central to microfinance. Through its peer-monitoring strategy, it has the potential for solving the disadvantages of asymmetric information between lenders and borrowers, and thus enforce better repayment. As a result, it has been deemed as a superior method in microloan contracts, especially when collateral cannot be used for poor borrowers.[1] However, over the years, individual liability (IL) loan contracts have also been used in parallel. In this regard, it is not unlikely that these two loan types, with their varied repayment liability schemes, could appeal to the borrowers differently. Borrowers are likely to have heterogeneous preferences; therefore, not every borrower would prefer these loan types equivalently. In this paper, I attempt to study the puzzles in decision-making of the borrowers when they select such loan types. Through a microfinance setting in the laboratory, I try to understand whether being able to self-select into different loan contracts can increase overall take-up of these microloans. And together with that, I try to uncover how heterogeneous preferences among borrowers can play a role in take-up of these two different loan types.

The motivation for this paper arises from the mediocre performance of microcredit as a poverty alleviation tool over the past years. Large-scale field experiments throughout the last decade (in India by Banerjee, Duflo, Glennerster, & Kinnan, 2015a, in Morocco by Crépon, Devoto, Duflo, & Parienté, 2015, in Bosnia-Herzegovina by Augsburg, De Haas, Harmgart, & Meghir, 2015, in Mexico by

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[1] The mechanism became popular through its exceptionally successful implementation by the Grameen Bank in Bangladesh in the 80's. Later, the mechanism was commonly used by microcredit organizations in many developing countries. Theoretical models by Stiglitz (1990), Besley and Coate (1995), Armendáriz de Aghion (1999), Ghatak and Guinnane (1999) bear testimony to the endurance of the JL mechanism in influencing better repayment.

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Angelucci, Karlan, & Zinman, 2015, in Mongolia by Attanasio, Augsburg, De Haas, Fitzsimons, & Harmgart, 2015, and in Ethiopia by Tarozzi, Desai, & Johnson, 2015) fail to find any outstanding effect of microcredit in poverty alleviation.[2] One critical and unanticipated puzzle that all these studies stumble upon is the low take-up of microloans by those poor for whom these are specifically designed.[3] Therefore, the consistent finding of low take-up rate in these experiments calls for an investigation. In my opinion, one possible way of addressing the gap in take-up would be to enquire the issue along the channel of borrowers' preferences.[4] Although the studies mentioned above as well as many others in the related literature[5], have focused on making the loans profitable for the mass of prospective borrowers, they have not incorporated borrowers' heterogeneous preferences into the equation. Therefore, in this study, I focus on borrowers' heterogeneity in preferences and thereby try to understand if allowing borrowers to self-select into their desired loan type is a way of increasing take-up.[6] So, I take a simple hypothesis to the lab to test: whether take-up rate of loan increases when borrowers are offered a flexible choice-set with both JL and IL loans in contrast to when they are offered only one loan type. This set-up is distinctly different from the above mentioned field experiments; there the borrowers were unable to choose between different loan types[7]- either they took the only loan type offered or they did not. Most laboratory and framed field experiments till date, in the context of microcredit (Abbink, Irlenbusch, & Renner, 2006; Cason, Gangadharan, & Maitra, 2012; Kono, 2014), with their aim of studying the superior implementation of repayment through JL in comparison to IL, have also followed a similar design of allowing only one loan type at a time.

One might argue that it is not necessarily surprising in itself that offering more loan options in the choice-set would increase take-up rate. However, a growing body of literature on *cognitive load* agrees on the fact that most often an increased number of choices can impair optimal decision-making.[8] Especially in my context, when the prospective borrowers have to choose between profit-making loan options, they might end up making poor and irrational choices, possibly be even discouraged to take any loan at all. In light of this, it is actually worth checking whether my hypothesis of increased take-up rate holds for an elaborate choice-set. Besides testing the take-up rate, another emphasis of the paper lies in examining if the heterogeneous preferences of borrowers lead to varied take-up. There are certain features of JL and IL loan types that could be advantageous or disadvantageous to the borrowers according to their preferences. One of the main features of microcredit loans is the *dynamic incentive* that comes with them i.e. the promise of further loans from the lender in case of full repayment (Besley, 1995; Morduch, 1999). With dynamic incentive, JL excels over IL because the former ensures a higher probability of loan repayment through a jointly liable peer-group which in turn increases the chances of getting further loans; JL thus reduces the risk of non-repayment after every period of loan. So a borrower who is risk averse regarding repayment, would prefer JL.[9] On the other hand, as the future time horizon for availability of future loans (through dynamic incentive) is unknown, it is likely that the choice might be driven by the discount factor of the borrower. The borrower, who discounts the future less and values long-run benefit of receiving further loans, would have higher willingness to take up JL loan. But for the borrower who discounts future heavily, the short-run cost of repaying on behalf of an unsuccessful partner might surpass the fruits of receiving more loans with higher chances in the long run; hence, she would prefer IL to JL.[10] Therefore, given these features, the *ex ante* optimization by the borrowers should be influenced by their risk preferences and discount factors. In addition to that, the taker of JL might also be willing to bear the cost of a partner's burden because she enjoys higher utility from the expected gains of herself and the partner's as well.[11] This possibility originates from the behavioural foundations of preferences which validate that it is not uncommon that an individual derives additional

[2] An interested reader can find a comprehensive comparative study across these six field experiments in Banerjee, Karlan, and Zinman (2015b).

[3] Furthermore, the unpredicted low take-up of loans in all these studies weakened the statistical power of their corresponding samples and therefore hindered impact evaluation of microcredit.

[4] Harrison and Ng (2016) argue that programmes and policies are most often evaluated by the metric of take-up, which, unfortunately, does not automatically reflect the welfare gain of the product to the adopter. Thus, welfare evaluation requires to include risk and time preferences as well as the individual's subjective beliefs on possible losses from the policy/product in question.

[5] For example, Giné and Karlan (2014) study issues like group pressure, default rates, etc. under both individual and joint loan contracts, with villagers in the Philippines.

[6] In the context of take-up of economic policies, Dasgupta, Gangadharan, Maitra, Mani, and Subramanian (2015) use an artefactual field experiment to show that besides socio-economic and demographic characteristics, behavioural traits such as risk-taking and competitiveness drive take-up of training programmes intended for improving labour market outcomes. They argue that incomplete financial markets failing to smooth economic risks and institutional hurdles rendering investments to be fraught with uncertainty are common phenomena in developing countries. As a result, individuals only with a risk-taking attitude might be interested in investment activities. This implies that risk attitudes play a role in the decision to participate in such programmes/activities.

[7] The aforementioned studies in Ethiopia, India, Mexico and Morocco offered only JL, and in Bosnia-Herzegovina IL was offered; whereas in Mongolia both types were offered, but to separate groups - each being offered to one group.

[8] *Cognitive load* can affect risk-taking and patience while economic decision-making (Deck & Jahedi, 2015) and also, undermine the chooser's subsequent satisfaction and motivation (Iyenger & Lepper, 2000; Schwartz, 2004). I thank an anonymous referee for this discussion.

[9] However, the higher probability of repayment with JL is certain only when peers within a group are fully capable of monitoring each other's actions and investment outcomes. Under less strict scenarios, where information asymmetry among the JL group members is possible, several uncertainties could affect the repayment probability with JL. For example, in the field, an additional uncertainty in joint repayment could arise if a borrower runs away from her commitment when the partner's investment fails, since legal recourse is hardly implementable. Given this possible uncertainty, a prospective borrower could actually be deterred from selecting JL contract since she is not sure of the partner's commitment. Another degree of uncertainty could emerge under information asymmetry, and that is of freeriding. By having the insurance of a jointly liable group, a freeriding partner could actually strategically default by not repaying. Although having dynamic incentives (Giné, Jakiela, Karlan, & Morduch, 2010) and social sanctions (Besley & Coate, 1995) can actually help minimize these additional risks and uncertainties arising with information asymmetry under JL contract, it is not always obvious in the field that JL should outperform IL. While it is true that my assumption of information symmetry among the JL partners leads to a substantial departure from the typical features of microfinance, it is also true that trying to study all these features in one setting would lead to several confounding effects and thus explain nothing.

[10] We commonly relate discount factor to an individual's *patience*, i.e. the more patient she is, the less she minds waiting for something valuable rather than having it immediately. But, another reason for using discount factor is when one is unsure of the time length for which the future will continue, i.e. if one is not sure if tomorrow will really come, she would prefer to have the valuable thing today rather than tomorrow. In this context, one can use the two interpretations interchangeably, that is, the more one believes that the future will last until period t, the more patient she is to wait for the reward until then.

[11] For example, Dreber, Fudenberg, and Rand (2014) study the association between altruistic giving and cooperation in indefinitely repeated PD game. They find evidence of correlation between generosity (in terms of dictator game giving) and cooperation in infinitely repeated PD even when no cooperative equilibria exist; this implies that it is not unlikely that individuals with *other or social regarding preferences* sometimes deviate from the standard predicted way, to cooperate. However, the authors do not find similar correlation when cooperative equilibria exist. Such varied results motivate to check if there is any role of altruistic giving in this study.

(positive/negative) utility from other's outcome i.e. *social* or *other-regarding preferences* (Rabin, 1993; Levine, 1998; Fehr & Schmidt, 1999; Bolton & Ockenfels, 2000). To sum up, these possible differences in risk, discounting and other-regarding preferences incite my hypothesis that when the choice-set of loan types is constricted, there might not be as many takers as would be otherwise. Having described all the possible preferences that could play an active role in such decision-making, I, however, restrict my conclusion to the effects of risk and social preferences only and refrain from making any inference on discount factor in this study.[12]

A key feature of my design is the *outside income option*, to classify the potential non-takers of any loan type and also to understand how the takers vary from them. When analysing (non)/take-up, one needs to pay careful attention to modelling the outside option of the subjects. Considering what goes on in the real field, I assume that the prospective borrowers do not have a constant source of income; therefore when they reject a loan, they merely leave themselves to chances of getting some temporary job that would lead to considerably lower earnings than what a successful investment outcome of the microloan could bring. This aspect has been incorporated in the design in the form of an uncertain outside income, i.e. an employment opportunity, when one does not take up any loan. According to the experimental design, the probability of getting employed is higher than the success probability of the investment project with the loan, but the wage is much lower than the returns from a successful investment. To my knowledge, no previous study has considered the issue of outside income option in the context of microloans.[13]

In a laboratory experiment conducted with 220 university student subjects in the decision-making lab in Turku, Finland, I find statistically significant evidence in favour of the hypothesis which claims that when offered a choice-set with the two loan types, take-up proportion is higher than an offer of just one type.[14] I find interesting effects of heterogeneous preferences, thus confirming their association with the choice-making by the borrower: willingness to take up IL loan decreases with risk aversion; but in general, willingness to take up any loan decreases with risk aversion. By testing the association between altruism and chosen loan type, I find that subjects who take JL loan tend to donate more in a one-shot dictator game (DG).

This paper makes a contribution to the literature on borrower's investment behaviour in the framework of microcredit. A closely related work by Barboni (2017) also deals with the take-up of microloans but focusses on the variability of repayment schedule by offering a menu of rigid and flexible repayment contracts. In a framed field experiment with Indian micro-entrepreneurs, she finds evidence that the high-revenue borrowers prefer flexible schedule and risk averse borrowers stick to the rigid repayment schedule, the second result providing some strength to my hypothesis that the choice of contract types could differ according to borrowers' risk preferences. Other works in this literature study demand for micro-insurance under different contract types involving joint liability (Janssens & Kramer, 2016) and risk-taking choices by borrowers under different microfinance contracts (Giné et al., 2010; Fischer, 2013). While the last two studies investigate optimal risk-taking behaviour given the contract type, my study tries to approach the issue differently - by studying which contract type is optimal for the borrower given the risk and other costs of that contract. This paper also makes a contribution to a very small literature till date, which deals with various issues of microfinance in a laboratory setting, such as loan repayment and monitoring decisions (Abbink et al., 2006; Cason et al., 2012), mechanisms for improved cooperation in social dilemmas (Baland, Gangadharan, Maitra, & Somanathan, 2017), etc. In light of the overarching focus of the paper, which is take-up of microcredit, this study contributes to the literature that concentrates on finding solutions to remove barriers to the take-up of welfare policies. In the context of various welfare products and interventions, such as insecticide-treated bednets in malaria-infested places, free/subsidized account-opening in the bank that influences financial inclusion, vocational training programmes that increase the chances of employment, etc., take-up has been found to be negatively affected by increase in percentage of cost-sharing of the welfare product by the beneficiary (Cohen & Dupas, 2010), lack of trust in the institution to which the intervention is related (Dupas, Green, Keats, & Robinson, 2014), time and resource constraints to the utilization of the product or intervention (Bonan, Le May-Boucher, Scott, & Tenikue, 2017; Jain, Maitra, & Mani, 2019). In connection with these studies, my study finds that it is the lack of flexibility in choice of the product that acts as a barrier to take-up.

On a final note on the contribution, this paper reports elaborately how the sample size and power calculations for the main study were done based on pilot studies. Though this is a necessary step before conducting experiments, it is still rare that studies involving laboratory experiments report theirs. Andersson, Miettinen, Hytönen, Johannesson, and Stephan (2017), who similarly calculate power from pilot studies, use *Cohen's d*, an approach frequently used in estimating sample sizes for statistical testing.[15] I, instead use *Cohen's h*, which measures the difference between two independent proportions, to indicate the necessary sample size. Therefore, this paper, along with Andersson et al. (2017), constitutes a small niche in the literature of lab experiments, that informs about statistical power and sample size calculations from pilot studies.

The remainder of the paper is structured as follows. Section 2 describes the theoretical modelling of the loan types and the borrower's decision-making conditions for taking up a given loan type. In Section 3, I describe the experimental design and the laboratory procedure. This is followed by discussion on results in Section 4. Finally, in Section 5, I conclude.

2. Theoretical framework

In this section, with the help of an economic model, I analyse a borrower's decision when offered a certain loan type.

2.1. Features of IL and JL loan types

As done in previous literature, I keep the features of the loans in their simplest forms for tractability yet without loss of generality.

I assume, the features which are common to both loan contracts are: **1.** Each borrower, if she chooses any loan type, gets loan b to invest in a one-period risky project; **2.** Repayment fee for each borrower is $(1 + r)$. b, where $r > 0$ is the rate of interest; **3.** Probability of success in the risky project is p, which yields return π. If the investment fails with probability $(1 - p)$, b is lost and return is 0. In case of failure, the borrower cannot return her own loan. I assume discrete possibilities,

[12] A drawback of the experimental setting here is, it does not reflect the *patience* factor after every period of loan to such an extent that it could affect the discounting factor of the subjects.

[13] However, the influence of *outside options* has been explored in a variety of contexts within the experimental literature; for example, in the form of the forgone option by the first mover in a lost wallet game and the effect of its size and saliency on the reciprocal nature of the second mover (Cox, Servátka, & Vadovič, 2010), or in the form of an entrant firm's home market profits and its effects on the entrant's competitive behaviour in a contestable market (Dasgupta, 2011).

[14] Besides this study, Abbink et al. (2006) and Cason et al. (2012) have previously used samples of students from developed countries (Germany and Australia respectively) in lab experiments on microfinance.

[15] A lower *Cohen's d* obtained from the difference of two independent means divided by the standard deviation, indicates the necessity of larger sample sizes, and vice versa, as can subsequently be determined together with the additional parameters of desired significance level and statistical power (Cohen, 1988).

i.e. she repays the entire loan with interest in case of success and nothing in case of failure; **4.** The contract has dynamic incentive, i.e. as long as she successfully repays her loan at the end of every period, she is eligible to get a similar loan in the next period which is to be invested in the same way. But if once her investment fails, and she is unable to repay the loan at the end of a certain period, she is no more eligible to get another loan in the latter periods; **5.** Borrowers have no other source of income when they take a loan. And, they cannot use return of one period to repay in another period; **6.** $\alpha = \frac{(1+r).b}{\pi}$ is the repayment ratio. The simple features above are similar to those used by Armendáriz de Aghion (1999).

The additional features of JL loan are: **1.** The loans (having the same features as above) are available for a group of two borrowers (assuming the simplest group formation) and each member has her own project; **2.** The project success probability remains the same for each project, but risk across the projects is uncorrelated; **3.** Success probability p in the individual investment is common knowledge to all group-members; **4.** If one member is successful in her own project while her partner is not, the former has to repay on behalf of the latter - here lies the joint liability. Therefore, for JL to be feasible, $(\pi - 2\alpha\pi) \geq 0 \Rightarrow \alpha < \frac{1}{2}$; **5.** As long as both the loans are repaid by one/both of them after every period, both are eligible to get another loan in the next period. If both investments fail in a certain period and none is able to repay, none of them gets a loan in the latter periods; **6.** I assume that there is symmetric information between the group-members on their respective investment outcomes; hence, no possibility of freeriding[16] by false-reporting own investment outcome. Features **1–5** have been commonly used by Besley and Coate (1995), Armendáriz de Aghion (1999) and Ghatak and Guinnane (1999), etc.

2.2. Features of the outside income option

Since the aim is to study take-up of loan types in an experimental set-up, I also need to consider the situation when the subjects do not take up the offered loan. The easiest way to model an outside option would have been to offer a sure income to the subjects. However, the literature on lab experiments provides strong evidence that the availability of safe and sure options in experimental tasks could bias the subjects towards those (Andreoni & Sprenger, 2012).[17] To avoid such certainty bias, I model the outside option to be safer than loan investment, yet with some uncertainty attached to it. To rationalize this design in the context of microcredit, one could fairly argue that when the prospective borrowers reject a loan, it is not because they have a sure fallback income option; usually they have some temporary job (or merely the prospect of getting one) which brings an income lesser than the returns from a successful microloan investment. In the same context, it is realistic to assume that the chances of obtaining such temporary employment are better than yielding a successful return from loan investment. Thus, I model the outside option as an employment opportunity for the subject if she does not choose any loan. I assume that the probability of obtaining employment is $q(>p)$ and the wage is $s < (\pi - 2\alpha\pi)$. So, if one obtains an employment in any period, then she receives the wage s. I further assume that one's chance to seek employment in the next period is independent of the outcome of her employment search in the current period.[18]

2.3. Theoretical predictions

Stating the assumptions of the three different income possibilities in the setting, I now move on to discussing the discounted expected utility of an individual under these three possible choices. In order to concentrate on the take-up decisions of borrowers, I abstract from the lender's optimization problem and instead take the set of offered loan types as given. Usually, a lender imposes different interest rates for the two loan types (lower for JL). But involving different interest rates could complicate this setting so much so that it would be difficult to single out the role of each preference category in the decision-making. Giné et al. (2010) and Kono (2014), too, follow the same strategy of focussing on the borrower's problem and abstracting from the lender's.

Let each individual i maximize her stream of expected utility by discounting the future at rate $\delta < 1$. Her preferences over payoffs in each period is represented by the utility function $U_i(\gamma; y_i)$, where $U_i'(\gamma; y_i) > 0$, $U_i''(\gamma; y_i) < 0$, $U_i(\gamma; 0) = 0$, and γ is the parameter for risk aversion. Thus she maximizes the discounted utility: $\sum_{t=1}^{\infty} \delta^{t-1} E\,[U_i(\gamma; y_i)]$.[19]

The expected discounted utility of individual i if she selects IL loan, is:

$$EU_{i,IL} = \frac{1}{1-\delta p}.\ p.\ U_i(\gamma; \pi - \alpha\pi), \tag{1}$$

The expected discounted utility of individual i if she selects employment opportunity (henceforth EMPL), is:

$$EU_{i,EMPL} = \frac{1}{1-\delta}.\ q.\ U_i(\gamma; s) \tag{2}$$

Now, it is optimal to choose IL loan over EMPL *iff* (1) > (2)

$$\Rightarrow \frac{p(1-\delta)}{q(1-\delta p)} \geq \frac{U_i(\gamma; s)}{U_i(\gamma; \pi - \alpha\pi)} \tag{3}$$

For example, with a *Constant Relative Risk Aversion (CRRA)* utility function, (3) can be re-written as,

$$\Rightarrow \frac{p(1-\delta)}{q(1-\delta p)} \geq \left[\frac{s}{\pi - \alpha\pi}\right]^{(1-\gamma)} \tag{3'}$$

The general risk-pooling advantage of JL allows a higher probability of loan repayment and obtaining of future loans than IL. With IL loan, the probability of getting the next period of loan is p. With JL loan, the probability of getting the next period loan is $p^2 + p.(1-p) + (1-p).p$ in case of a two-person group. The first term stands for when both the group members are individually successful in their respective projects, second and third terms stand for when one is successful in her project but the other is not and the former is liable to repay on behalf of the latter (Armendáriz de Aghion, 1999; Giné et al., 2010).

[16] In order to concentrate on the differences in features of the loan types that are already present (see Section 1), I abstract from the possibility of freeriding, as it would further complicate the set-up.

[17] The authors highlight that the intuition behind Allais Paradox can be used as a reference to understand such outcomes. When two options are far from certain, then individuals choose as expected utility maximizers, but when one of the two options is made certain, individuals do not necessarily maximize their expected utility.

[18] As pointed out by a referee, this uncertainty in the outside option digresses from the standard assumption in traditional development economics, i.e. the poor always have a safe outside option (e.g. The *Mahatma Gandhi National Rural Employment Guarantee Act* (MGNREGA) introduced in India in 2005 allows any interested poor individual to be able to work at a fixed wage rate). However, by relaxing this assumption in my design, I can confidently eliminate any confounding effect arising from potential certainty bias in decision-making in the lab. To reconcile with the norm in development economics, I propose that it is possible to extend my experimental set-up to one where the outside option comes with zero risks. As long as the outside option is low-return and zero/low-risk, such that the expected income from it is considerably lower than that from the loan(s), I believe our interest in eliciting the effect of flexibility in the choice of loan type (and of the heterogeneity in preferences) on take-up would remain largely unaffected.

[19] She maximizes her discounted utility over an infinite-period horizon since she is unaware for how long the loans would be available in future (provided she is able to repay after every period). Giné et al. (2010) use a similar model under dynamic incentive, but they additionally consider projects with different risks.

Therefore, with JL under information symmetry, individual *i*'s discounted expected utility is:

$$EU_{i,JL} = \frac{1}{1-\delta(2p-p^2)}[p^2U_i(\gamma;\pi-\alpha\pi) + p.(1-p)U_i(\gamma;\pi-2\alpha\pi) + (1-p).pU_i(\gamma;0) + (1-p)^2U_i(\gamma;0)] \quad (4)$$

Therefore, JL is better than IL *iff* $EU_{i,JL} \geq EU_{i,IL}$

$$\Rightarrow \frac{U_i(\gamma;\pi-2\alpha\pi)}{U_i(\gamma;\pi-\alpha\pi)} \geq \frac{1-2\delta p}{1-\delta p} \quad (5)$$

Again by using an example of *CRRA* utility function, (5) becomes,

$$[\frac{\pi-2\alpha\pi}{\pi-\alpha\pi}]^{(1-\gamma)} \geq \frac{1-2\delta p}{1-\delta p} \quad (5')$$

Taken together, the above inequalities lead to the following proposition.

Proposition 1. *Combining the inequalities from (3) and (5), we can obtain the lower threshold of risk aversion above which JL ≥ IL, and the higher threshold of risk aversion until which IL ≥ EMPL. For example, using a CRRA utility function (in (3') and (5')), we can see that JL ≻ IL ≻ EMPL in the range* $\gamma^{**} < \gamma < \gamma^{*}$. *The discount factor δ remains unchanged while this holds.*

Proof. See **Appendix A**. □

Thus we see that the individuals who are comparatively more risk averse ($\gamma > \gamma^{**}$) prefer JL to IL, all other things remaining unchanged. Those who are risk averse at an even higher level ($\gamma > \gamma^{*}$) would prefer EMPL only over IL, and if exceedingly more risk averse (beyond an even higher threshold γ^{***}) then would prefer EMPL over JL too.

For the discount factor δ, we can analogously obtain thresholds across which the preference orders for IL, JL and EMPL change. Since in a microfinance setting with dynamic incentive, the future time horizon of loan availability is unknown to the borrower, it is possible that the borrower's decision on her choice is affected by her belief on the number of periods the loan offerings would last or the number of periods she is willing to wait to enjoy her gains.

Proposition 2. *Combining the inequalities from (3) and (5), we can obtain the lower threshold of discount factor above which JL ≥ IL, and the higher threshold of discount factor until which IL ≥ EMPL. For example, using the utility function in (3') and (5'), we can see that JL ≻ IL ≻ EMPL in the range* $\delta^{**} < \delta < \delta^{*}$. *The risk aversion parameter γ remains unchanged while this holds.*

When risk aversion parameter γ and discount factor δ both vary together, the following corollary is obtained.[20]

Corollary 1. *For a given discount factor, say δ', as the risk aversion level γ decreases below a lower threshold* γ^{**}, *(5') does not hold and JL ⊁ IL anymore. But for a discount factor higher than δ', the risk aversion threshold until which (5') holds is lower than* γ^{**}.

Corollary (1) shows how the interplay of risk preferences and discounting could affect the borrower's selection of loan type. However, one could argue here that the choice between JL and IL is not driven by risk and discounting only. An interesting line of inquiry is to also include utility from other's (here, the partner) income as a component in the utility function of the JL loan borrower. In line with models on *social preferences* (Levine, 1998; Charness & Rabin, 2002), if the borrower puts some positive weight on the partner's payoff in her own utility function, then JL is preferred more easily.

Here, I focus on the altruism aspect of social preferences only. Following the model on pure altruism by Levine (1998), let us try to understand how the utility function of individual *i* looks like when it includes distributed weights on two components:- one being the standard utility from her own income, and the other being the non-standard component that comprises of her utility from partner *j*'s income. Let *i* put a positive weight β ($0 < \beta < 1$) on her utility from *j*'s income, and the remaining weight on the utility from her own.

Formally, let the preferences of *i* in each period be represented by the utility function: $U_i(\gamma; y_i; \beta; y_j) = (1-\beta).U_i(\gamma; y_i) + \beta.U_i(y_j)$; and she maximizes her stream of expected utility by discounting the future at rate $\delta < 1$. Thus she maximizes the discounted utility: $\sum_{t=1}^{\infty}\delta^{t-1}E[U_i(\gamma; y_i; \beta; y_j)]$. The risk parameter only affects her utility from her own possible payoffs but has no effect on her utility from the partner's payoff. To keep the setting in its simplest form, let $\beta > 0$ when the individual chooses JL but $\beta = 0$ when she chooses IL; hence $EU_{i,IL}$ remains the same.

Now the expected discounted utility of individual *i*, when choosing JL, becomes,

$$EU_{i,JL,\beta} = \frac{1}{1-\delta(2p-p^2)}[(1-\beta)(p^2U_i(\gamma;\pi-\alpha\pi) + p.(1-p)U_i(\gamma;\pi-2\alpha\pi)) + \beta U_i(EV_{j,JL})] \quad (6)$$

In Eq. (6), the first component in the square brackets is the $(1-\beta)$-weighted expected utility of *i* from her own payoffs of the different outcomes in each period. And, the second component is the β-weighted utility of *i* from the expected value of *j*'s income in each period, where $EV_{j,JL} = p^2(\pi-\alpha\pi) + p(1-p)(\pi-2\alpha\pi)$. Since the latter term is devoid of *i*'s risk, the expected income of *j* will remain as a constant term in *i*'s utility function in every period.

In this setting, *i* would prefer *JL* to *IL iff* $EU_{i,JL,\beta} \geq EU_{i,IL}$.

By assuming $\beta > 0$ and other things equal, it simplifies to the following condition in a *CRRA* setting.

$$\frac{\beta(1-\delta p)}{(1-p)(1-2\delta p)}[\frac{EV_{j,JL}}{p(\pi-\alpha\pi)^{(1-\gamma)}} - p] \geq 1 - \frac{(1-\beta)(1-\delta p)}{(1-2\delta p)}[\frac{\pi-2\alpha\pi}{\pi-\alpha\pi}]^{(1-\gamma)} \quad (7)$$

Corollary 2 follows from here.

Corollary 2. *If incidence of social or other-regarding preferences is considered, and thus if the borrower of JL loan derives additional positive utility from the partner's payoff, then Corollary 1 could be revised as follows:*

Given discount factor δ' and risk aversion $\gamma'(<\gamma^{**})$, *it is still possible for the borrower to prefer JL over IL as long as the condition given by Eq. (7) holds. And, the more weight she puts on the partner's payoff (i.e. higher β) the stronger is her preference for JL.*

Proof. See **Appendix A**. □

Now, we move on to the experiment to empirically verify if and to what extent the discussed parameters are driving the take-up decisions of the prospective borrowers in the lab. However, in Sections 4.2.2 and 4.2.3, while estimating the effects of these preferences on take-up of loan type, I employ a reduced-form analysis and thus abstain from validating the exact threshold levels given in the propositions. In order to validate the *precise* predictions in Propositions 1 and 2 and Corollaries 1 and 2, one needs to assume a particular form of the utility function. Although I provide examples with a *CRRA* form in the predictions, I refrain from restricting my data analysis to this particular function. The overall implications of the theoretical predictions are that the preference for JL(IL) increases with increase(decreases) in risk aversion, discount factor and other-regarding preferences; and, a reduced-form analysis is sufficient to verify these effects.

[20] Since my main interest lies in the comparison between JL and IL, in the next analytical part of this section I only concentrate on those two.

3. Experimental design & laboratory procedure

The main focus of my experiment is to examine if prospective borrowers at all decide on the basis of their heterogeneous preferences when they take or reject an offered loan type. If so, then possibly a flexible choice-set would help them pick the loan that suits their preference-profile the best; and this, as a result, would also contribute to an overall higher take-up.

3.1. Details of the treatment designs

The experimental design had three treatment variations: one group was offered a choice-set of IL, JL and EMPL, another group was offered a choice-set of IL and EMPL and the last group was offered a choice-set of JL and EMPL. I henceforth address these groups as IL-JL-EMPL, IL-EMPL and JL-EMPL respectively. Comparison of IL-JL-EMPL with the other two groups helps to evaluate whether the flexibility of being able to self-select from a bigger choice-set increases overall take-up or not.

Depending on the treatment being played in a session, the participants were offered to choose any of the income options available in the choice-set.[21] They were told that they would play for several *periods* with their respective chosen options, but that they could make the choice only in the first period. This implies that when they decided on which option to take up, they did so without knowing for how many periods the game would continue. They were only told that after every period, the next period might not occur with a 10% chance; and, should it happen, the game would end.[22] Now, a *round* of the experimental session consisted of this kind of a game.

In every session, I played ten rounds, and at the beginning of each, the participants could revise their choices from the set of income options available in the respective treatment.[23] Summarily, in each round, a participant could choose an income option from the offered menu and then she played an 'infinite'[24] horizon game. As a result, her decision-making over what to choose at the beginning of a round reflects the optimization behaviour presented in the theoretical model.

3.1.1. Design details of the three different income choices

Individual liability loan (IL). If the subject chose this income type at the beginning of a round, then she received a loan of 100 ECU (Experimental Currency Unit) in the first period. She invested that loan in a business which would yield 500 ECU with 50% chance and 0 ECU with 50% chance. At the end of the first period, she had to pay back the loan of 100 ECU plus an interest rate of 20% as a fee for the loan. If the business investment was successful, then the loan could be repaid. As long as 120 ECU could be repaid by the subject, she received another loan of 100 ECU in the next period, which would be invested in the same way. This process of loan availability continued for an indefinite number of periods (i.e. until some period did not occur with a 10% chance). However, if the business investment failed in any period, then the earning was 0 ECU and thus, the repayment could not be done in that period. As a result, she was not eligible to get any further loan in the subsequent periods of that round, even if the periods continued.

Joint liability loan (JL). If the participant chose this income type at the beginning of a round, then she was matched with another participant *(Participant B)*[25] with whom she took the joint loan[26] and then made a business investment. *Participant B* was randomly selected from among all those participants who also had chosen joint loan in the same round[27]. In all periods of the same round, the 'pairs' remained constant. Once the round was over (and should the subject choose JL again), she was randomly re-matched. No communication was possible with *Participant B*.

In the first period, they received a loan of 100 ECU each. Each invested this loan in a business of her own which would yield 500 ECU with 50% chance and 0 ECU with 50% chance. The repayment amount was the same for each of them, i.e. 120 ECU. If both the business investments were successful, each could repay her own loan. If one investment was successful and the other was not, the subject with the successful investment repaid both loans plus both fees, i.e. altogether 240 ECU. As long as 2*120 ECU was repaid in total by one or both of them, each could receive another loan of 100 ECU in the next period, which was to be invested in the same way. This process of loan availability continued for an indefinite number of periods (i.e. until some period did not occur with a 10% chance). However, if both business investments failed together in any period, each earned 0 ECU and thus, no repayment could be done in that period. As a result, no further loan could be obtained by either of them in the subsequent periods of that round, even if the periods continued.[28]

Employment opportunity (EMPL). If the participant chose this income type at the beginning of a round, then she had 67% chance of finding employment in each period. If employment was obtained in a given period, the wage was 50 ECU, otherwise 0 ECU. This process of getting an employment opportunity continued for an indefinite number of

[21] I ran several sessions of the experiment in the lab, and I assigned one session for one treatment type only.

[22] To implement the dynamic incentive of the loans, it was necessary to have multiple periods so that if a loan is repaid successfully, another loan could be obtained. To put it into context, usually with dynamic incentive, the prospective borrowers are unaware of the exact number of periods in future until which the loans would keep coming in the case of successful repayment. So when they optimize their expected discounted utility *ex ante*, they do it over infinite periods. Replicating such a scenario in the lab would require an infinitely/indefinitely repeated game. And, a classic way of implementing it is by using a *random termination* (RT) which links the number of expected repetitions of the periods to the discount factor (Roth & Murnighan, 1978). In RT games, after every period, the game continues for another period with a known probability δ, and ends with probability $(1-\delta)$, as has been implemented in this design. Though other random termination methods are available as well, the RT method is the closest to my context and is the most commonly used method (Fréchette & Yuksel, 2017).

[23] I employed the repetition of the rounds to study the evolution of the game, i.e. the choices of the subjects, as they gained more experience over the rounds (Iyenger & Lepper, 2000; Dal Bó & Fréchette, 2018).

[24] Given the method used to implement it in a lab, the game is not really infinite per se. The term 'infinite' is used more in reference to the theoretical framework which is used to analyse the situation. Hence, the terminologies *infinite* or *indefinite* or *random termination* are used interchangeably in the jargon of lab experiments (Dal Bó & Fréchette, 2018).

[25] The language was kept as neutral possible by avoiding terms like 'partner', 'pair', 'peer', etc. I stressed on avoiding such words in the verbal and written instructions, as well as in the main experiment.

[26] I also avoided using the strong word 'liability' in the JL loan while instructing the subjects; instead, I let them figure out by themselves from the features of the loans. In the instructions and in the experiment, I termed the loans as 'Individual loan' and 'Joint loan'.

[27] If the total number of JL takers in a round was odd, then the unmatched subject was informed on the screen that she could not be matched with a participant in that round, therefore the computer would act as *Participant B* for her and select random numbers to decide *B*'s investment outcome. This could not have harmed the take-up decision, at least in the first round, because the JL taker had already made her choice before this information appeared on screen.

[28] Also, to avoid *priming*, I did not specify to the subjects the collective probability of repayment after every period (i.e. 0.75) with the JL contract. Instead, I vividly explained to them how the success probabilities of the subject and the partner would work and what the different circumstances of repayment were (See Instructions in Supplementary file for more details). However, as pointed out by a referee, this could be a potential drawback of the design, because not telling the subjects the collective probability *explicitly* could actually have caused loss of control on their beliefs that led to their respective choices.

periods (i.e. until some period did not occur with a 10% chance). Unlike the case with loans, the chance of finding employment in any given period was not dependent on whether the subject had found employment in the previous period.

Figures B.1, B.2 and B.3 in Appendix B illustrate how a round with each of the income options proceeds. For more details, see Instructions in the Supplementary File.

3.1.2. Design details of the experimental procedure

To determine the success or failure in loan investment or employment search, the subjects were supposed to click on the screen to obtain a random number (between 0 and 1, where all values are equally likely). If the subject had chosen EMPL and if the random number was $\leq$ 0.33, that would imply that she was unable to obtain employment in that period; if the random number was $>$ 0.33, it would imply she could obtain employment in that period. If the subject had chosen IL or JL and if the random number was $\leq$ 0.50, that would imply that her own investment was unsuccessful in that period; if the random number was $>$ 0.50, it would imply that her own investment was successful in that period.

The presentation of the experimental task was kept neutral without linking it to microfinance; this was to follow the practice in earlier relevant literature (Cason et al., 2012) as well as to avoid any uncontrolled connotation effect (Abbink et al., 2006). I deliberately avoided the use of any effort-involving task to determine the success of investment or employment search, because that could motivate the subjects not to put true effort due to intrinsic moral hazard and freeriding incentives; and in this paper I would like to focus precisely on issues that are already prevalent even after omitting moral hazard and freeriding incentives. Also, no transaction (e.g. disbursal of loan, repayment, etc.) was done physically during the experiment. Instructions were such that each subject would have an earnings account maintained for her during the entire session; for example, if she would choose loan, then in the beginning of each period her account would be credited with 100 ECU, at the end of the period the repayment amount would be debited from the account, etc.

I maintained some further restrictions in the design in order to keep it comparable to the assumptions in the theoretical setting: **1.** The subjects were not able to report falsely about their investment outcomes to the experimenter, and also to *Participant B* in case of JL. This was restricted by directly showing them the respective outcomes of their investments or employment search on their screens, and also their net incomes after each period. In case of JL, information on *Participant B*'s investment outcome also appeared on the subject's screen and necessary repayment amounts were deducted directly from both their accounts. **2.** By allowing no communication between the JL groupmembers, I precluded the possibility of collusion against the lender (experimenter in this case).[29] The aim was to see that when no communication is allowed, what the subjects would instinctively choose in the strictest condition, that is, with no communication, with a randomly chosen partner, etc. **3.** The subjects were told that they would play several rounds of the game during the session, and that each round would have few periods where the next period may not occur with 10% chance. **4.** The subjects were also told that after the experiment ends, each of them would randomly draw one of the rounds played and their respective incomes of the chosen round would be converted to Euros and handed to them.

In summary, the experiment was designed in a way that the subjects could only take action on their choice-making of loan(s) or employment at the beginning of each round. Once the choice was made for a round, they basically had to draw the random probabilities which decided the outcome of their respective chosen option in each period. Otherwise, everything else was hard-coded and no decision-making was needed on their part until that particular round ended with random termination probability of 0.10.

3.2. Elicitation of heterogeneous preferences

Simply by looking at their choices of loan types, it is hard to understand what actually motivates the subjects' decisions. If one chooses IL, then there could be a number of intermingled motivations - she could be simply more risk-seeking, or she could dislike taking a partner's burden. And, if one chooses JL, then it could be due to her risk aversion to non-repayment, or her generosity in sharing the costs of JL with another subject. And, it is the relative strength of these multiple underlying motivations that dictates her choice. There is a possibility of some discount factor playing a role here too, in terms of patience and/or expectation about the length of the game (i.e. discounting between the periods within a round). While making a choice, if one chooses JL, then it could be due to her expectation that there would be several periods of loan availability within the round, and if she patiently plays along, it could lead to higher final gains; thus, she would calculate her *ex ante* expected utility with a considerably high discount factor. But if one chooses IL, then probably it is due to her impatience to secure the income of the immediate period(s) without waiting for bigger expected profits after several periods (which she believes might not occur); then she would calculate her expected utility with low discount factor.

I controlled for generosity[30] of the subjects to check if that affects their choice of loan. As a measure of generosity in sharing income, I used the standard one-shot DG (Forsythe, Horowitz, Savin, & Sefton, 1994), where the subjects were asked to share a reward of 3 Euros with an anonymous participant.[31]

I elicited the subjects' risk preferences using the test designed by Holt and Laury (2002) (henceforth, H&L test). In spite of certain disadvantages of this test, pointed out in recent experimental literature on risk elicitation, I do prefer it because it helps to easily point out and drop the observations that have made inconsistent choices.[32]

I further controlled for the discount rate of the subjects by using the measure proposed by Reuben, Sapienza, and Zingales (2010)[33] to examine if there is at all any association between the subjects' discounting nature in this task and their choices of loan-types in the main experiment. However, I do not stress much on the effects of this measure

[29] Field evidence shows that often due to close ties and information-sharing among group members, group defaults become rampant. When the lender is unable to perfectly oversee, members can collude and maximize the total sum of their utilities (by defaulting together or writing side-contacts with third parties). An interested reader can look into Laffont and Rey (2003), where, in a theoretic model, the authors offer solutions to problems arising from collusion.

[30] In my opinion: since there is neither any communication between the JL partners, nor they have a say in whether they would like to contribute for the partner or not, the JL takers do not necessarily have to show reciprocity to each other for future cooperation; hence, I do not measure that. Rather, I am more interested in their attitude towards pure giving.

[31] Dreber et al. (2014) suggest that DG is better than ultimatum game/ one-shot Prisoner's Dilemma/public goods games in measuring social preferences, because it is the least sensitive to priming and framing effects.

[32] Crosetto and Filippin (2016), in their study on comparison of the popular risk elicitation tests, point out that H&L test is often difficult to comprehend by subjects due to its complicated probabilities, and therefore wrong understanding can lead to multiple switching points. But then again, the authors agree that not a single test is perfect, and once the observations with inconsistent choices (multiple switching points) are removed, H&L test performs considerably well.

[33] In this test, subjects are given a set of 9 decisions- each decision consists of choosing between an amount 50 Euros today and a larger amount $(1 + r)$*50 Euros in 1 week. For the set of decisions, the values of r used are: 0.00, 0.01, 0.03, 0.05, 0.07, 0.09, 0.10, 0.15, and 0.20. With gradual increase in r over the 9 decisions, at some value of r the subject switches from 50 Euros today to $(1 + r)$*50 Euros in a week; the r at switching point serves as the subject's discount rate.

because, in hindsight, the design of my microfinance game was not sophisticated enough to represent the 'waiting' between the periods so well that the subjects' discounting could affect their choice-making *ex ante*.[34] Furthermore, the discount rate elicitation task was not monetarily incentivized. Therefore, later in the discussion of results, I stress more on the effects of risk and social preferences.

3.3. Laboratory procedure

3.3.1. Recruitment

The experiment was conducted in the Public Choice Research Centre (PCRC) decision-making laboratory of University of Turku, Finland. The subjects were recruited using the ORSEE software (Greiner, 2004) and the computerized decision-making task was programmed and conducted on the z-tree software (Fischbacher, 2007). The experiments were conducted during January - March, 2017. Invitations were sent out to 1716 participants registered until that time. The language of communication used in the experimental sessions was Finnish. (See more details in Supplementary file)

3.3.2. Implementation

13 sessions were allotted for the main experiment- 5 for IL-JL-EMPL, 4 for IL-EMPL and JL-EMPL each. In total 65, 63 and 94 invitees turned up to participate in IL-EMPL, JL-EMPL and IL-JL-EMPL respectively. The participants of each session were randomly allocated to the visually isolated computers in the lab. Each subject received an instruction manual, and an associate read it aloud to them. After that, the subjects were given some additional time to go through the instructions by themselves. Before Round 1 started, a few 'test' questions along with multiple-choice answer options appeared on the subjects' screens; the questions were to control for their comprehension of the crucial points of the instructions. If they marked a wrong answer, a hint was provided on the screen to re-read/re-think in case they have misunderstood the steps. (See more details in Supplementary file.)

In each session of the experiment, there were ten rounds of choice-making. Each round started simultaneously for every subject. At the beginning of each round, the subjects were able to select an income type - IL, JL or EMPL depending on what was available in the choice-set in that particular session. Each round continued for a few periods, with each new period having a 10% chance of not occurring.

At the end of the final round, a few more questions were asked which gave them the opportunity to add to their earnings from the main experiment. These questions were the one-shot DG and the H&L risk aversion test. Then the subjects were to randomly draw a number between 1 and 10; this number signified the round from which the experimental earnings (i.e. the sum of earnings from all the periods of that chosen round) were to be converted to the subject's actual earnings from the main experiment.[35] The subjects were then asked a few more questions - these were the test for discount rate; the 10- point-scale standardized survey questions on risk and trust as used in the lab version module on preference elicitation by Falk, Becker, Dohmen, Huffman, and Sunde (2016).

On way out, subjects were paid in cash a total sum of the 3 Euros as participation fee, the earnings from the main experiment and the additional earnings from the one-shot DG and the H&L test. An entire session lasted for about an hour.

4. Results

4.1. Pilot studies, sample size and statistical power

Prior to the main experiment, I also carried out pilot sessions with all the three treatment types. The three pilot sessions, each consisting of one of the three treatment types, were conducted in December, 2016 in the same lab. The subjects of the pilot were also recruited using the ORSEE software (Greiner, 2004). However, these subjects were never invited back to the main experimental sessions. Since there is no similar previous study to get advice on effect size, I had to rely on the observed outcomes of the pilot treatments. In the pilot, the take-up proportions of the loans were 0.50, 0.70 and 0.84 with sample sizes 20, 19 and 20 in the IL-EMPL, JL-EMPL and IL-JL-EMPL treatments respectively. The difference in proportion in terms of *Cohen's h*[36] (Cohen, 1988) is 0.75 between treatments IL-EMPL and IL-JL-EMPL and 0.34 between JL-EMPL and IL-JL-EMPL. The former is a medium-to-large effect while the latter is a small-to-medium effect as per *Cohen's h* standard. Even though drawing inference on effect sizes observed in such small samples is risky, I had to base my sample size and power calculations on that. I wanted to have enough sample size to be well-powered to detect similar effects in the main experiment. Thus, I decided on having around 100 subjects in IL-JL-EMPL group and around 80 each in IL-EMPL and JL-EMPL groups. For a medium-sized effect of $h = 0.44$ (which is equivalent to $0.80 - 0.60 = 0.20$ in terms of absolute difference in proportions) this would give a power of 83%.[37]

4.2. Main experimental findings

This section presents the experimental findings. For analysis of take-up rate and heterogeneity in preferences, I focus only on the take-up rates in Round 1 of the different treatment groups. Concentrating on the first round data will protect from potential threats against statistical independence of the choices made by each subject in the subsequent rounds. Later in Section 4.2.4, I investigate any learning effect among the subjects over the rounds.

4.2.1. Summary statistics

Fig. 1 gives an overview of the take-up proportions in Round 1 across the three different groups. The percentage of take-up was 78.5% in IL-JL-EMPL, whereas in IL-EMPL and JL-EMPL the percentages of take-up were 62.5% and 65.1% respectively. Non-parametric analysis with Chi-square test for independence between the three treatment types and take-up proportion rejects the hypothesis of independence at 10% level of significance ($p = 0.060$); this allows to conclude that take-up behaviour significantly varies across the three groups. When I use this same test to further check separately between groups IL-EMPL & IL-JL-EMPL and then between JL-EMPL & IL-JL-EMPL, I reject independence of treatment type and take-up behaviour at 5% ($p = 0.028$) and 10% ($p = 0.064$) levels respectively.

Fig. 2 gives a closer look at the distribution of JL and IL take-up among the loan takers in Round 1 in the IL-JL-EMPL treatment. The idea is to see which loan type had a relatively higher demand when

[34] My design did not allow the subjects to physically receive the pay-offs after each period at different time points. Instead, all of them received the payments simultaneously at the end of the session. Hence, while they decided on their expected discounted utility *ex ante*, their *patience* most likely did not affect their decision. Even if I would argue that the *continuation uncertainty* of the subsequent periods within a round dictates the discounting more relevantly in this context than *patience* (refer to Footnote 10), then also the measure by Reuben et al. is not suitable, because it elicits *time preferences* rather than *continuation uncertainty*.

[35] i.e. they earned their total income of one of the ten games they played. See Dal Bó and Fréchette (2018) for the choice of payments.

[36] In statistics, *Cohen's h* is a popular measure of difference between two independent proportions. '*h*' is the difference in the arc-sine transformation of the two proportion values. The rule of thumb allows $h = 0.20$ as small, $h = 0.50$ as medium and $h = 0.80$ as large differences.

[37] If the effect sizes would be similar in the main experiment as in the pilot versions, then the decided sample sizes would give a power of 99% while testing between IL-EMPL and IL-JL-EMPL and a power of 61% in case of JL-EMPL and IL-JL-EMPL.

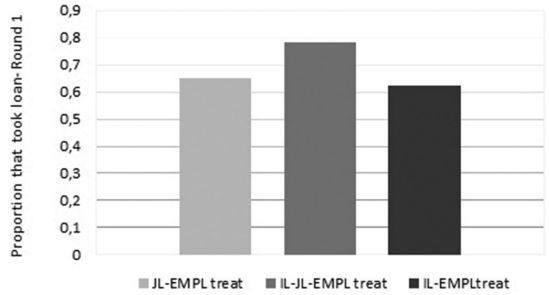

Fig. 1. Take-up of loans in the three treatments - Round 1 **Notes:** This figure describes the proportion of loan takers, irrespective of the loan type, in the first rounds of the three treatments.

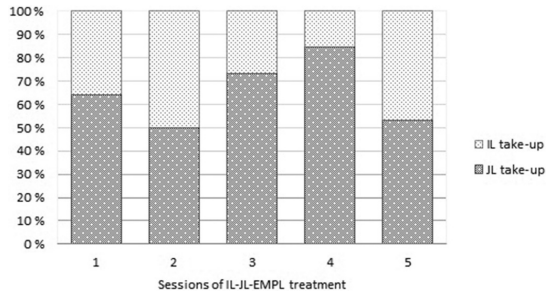

Fig. 2. Distribution of JL and IL take-up among the loan takers in Round 1 of Treatment IL-JL-EMPL **Notes:** This figure highlights the respective shares of IL and JL takers in the first rounds of the five sessions in which treatment IL-JL-EMPL was played. In these rounds, subjects chose EMPL as well, with around 20% takers on average across the five sessions.

both types were available in the choice-set. A binomial test confirms that JL loan take up is significantly higher ($p = 0.014$). This indicates that when borrowers can make sure that partners would not be able to cheat, then JL could excel in take-up rate.

Table 1 comprises the summary statistics of the individual-specific control variables of the subjects. For clarity in understanding the summary statistics of the variables, I only include the observations where no inconsistent choice was made for the corresponding variable.[38] However, while conducting non-parametric tests on the difference of the given variables across the three treatments, I include the inconsistent ones as well; this helps to compare if the inconsistent observations also differ significantly across the three treatments. By using the Kruskal-Wallis H test,[39] I find that for all the three individual-specific control variables, there is no statistically significant difference in the mean ranks across the three treatment groups (Column (7) in Table 1).

4.2.2. Determinants of loan take-up

In addition to the non-parametric tests, I regress take-up of loan on the 3 different treatment categories in Round 1. I then add the individual-specific control variables in the regression. I use the linear probability model specification for each case:

$$Y_i = \alpha + \sum \beta_t * Trt_t + \boldsymbol{X_i'}\gamma + \epsilon_i, \quad (8)$$

where, Y_i is the binary take-up of loan by individual *i*. Trt_t is the dummy variable that takes value 1 if individual i was in treatment *t*, otherwise 0; with β_t being the treatment effect. $\boldsymbol{X_i'}$ is the vector of individual-specific control variables. As already mentioned at the end of Section 2, I only employ a reduced-form analysis here.

Table 2 summarizes the regression results of the treatment effects on take-up of any loan type in Round 1. In Column (1), we see that with respect to IL-JL-EMPL, take-up is lower by 15.99 percentage points (pp.) ($p = 0.033$) and by 13.42 pp. ($p = 0.072$) in IL-EMPL and JL-EMPL respectively. When the individual-specific control variables are included, a portion of the sample is automatically dropped, i.e. those 57 observations whose measure of risk is inconsistent. In Table 2, Column (2) reports only the treatment effect again, with this updated sample, in order to give better comparability with Column (3) estimates. Note, with the updated sample, the take-up rate in JL-EMPL treatment does not vary significantly any more from that in IL-JL-EMPL. This could be

Table 1
Summary statistics of individual-specific control variables.

Variable	N[a]	Mean	Median	St.Dev.	Min.	Max.	*p*-value[b]
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
No.of safe choices in H&L test							
Treatment IL-EMPL	49	5.776	6	2.374	0	10	
Treatment JL-EMPL	45	5.533	5	2.074	0	10	0.15
Treatment IL-JL-EMPL	69	6.551	7	2.153	0	10	
Shared amount in DG							
Treatment IL-EMPL	64	0.852	1	0.776	0	3	
Treatment JL-EMPL	63	0.964	1	0.770	0	3	0.55
Treatment IL-JL-EMPL	93	0.829	1	0.609	0	2	
Discount rate							
Treatment IL-EMPL	64	0.042	0.01	0.047	0	0.2	
Treatment JL-EMPL	60	0.041	0.03	0.042	0	0.2	0.99
Treatment IL-JL-EMPL	92	0.040	0.02	0.045	0	0.2	

Notes: [a] N denotes the number of observations in the respective treatments who did not make any inconsistent choice in the corresponding test. [b] The *p*-values indicate the statistical significance of the Kruskal-Wallis H test.

Table 2
Take-up of loan in Round 1.

Variable	(1)	(2)	(3)
Treatment category:			
IL-JL-EMPL	*(base)*	*(base)*	*(base)*
IL-EMPL	−0.1599**	−0.1585*	−0.1995**
	(0.0745)	(0.0835)	(0.0848)
JL-EMPL	−0.1342*	−0.0560	−0.1092
	(0.0741)	(0.0802)	(0.0814)
Individual specific controls:			
No. of safe lotteries chosen			−0.0441***
			(0.0170)
Shared amount in DG			0.0628
			(0.0420)
Discount rate			0.9591
			(0.8273)
Constant	0.7849***	0.8116***	1.0148***
	(0.0429)	(0.0475)	(0.1284)
Observations	220	163	159
R^2	0.0256	0.0236	0.0767

Notes: 1. ***, **, * denote statistical significance at 1%, 5% and 10% respectively; **2.** heteroscedasticity-robust standard errors are in the parentheses; **3.** H&L risk-inconsistent observations are dropped in Columns (2) & (3); **4.** 4 observations are dropped due to Horrace & Oaxaca trimming in Column (3).

[38] It has been already mentioned before that H&L test is prone to accumulating inconsistent observations, i.e. those who switch among the lotteries more than once. It is not surprising that here too, there were 57 such observations (15 in IL-EMPL, 18 in JL-EMPL and 24 in IL-JL-EMPL).

[39] It is a rank-based non-parametric test that is used to determine if there are statistically significant differences between two or more groups on a continuous or ordinal dependent variable.

Table 3
Effect of heterogeneous preferences on take-up of JL & IL loans across the different treatment groups in Round 1.

	Take-up of JL	Take-up of IL
	(1)	(2)
Treatment category:		
IL-JL-EMPL	*(base)*	*(base)*
JL-EMPL	0.5981*	
	(0.3645)	
IL-EMPL		0.1991
		(0.2946)
Nr. of safe lotteries chosen (in IL-JL-EMPL)	0.0198	−0.0565*
	(0.0320)	(0.0299)
Treatment category *X* Nr. of safe lotteries chosen	−0.0448	−0.0140
	(0.0530)	(0.0401)
Shared amount in DG (in IL-JL-EMPL)	0.2360***	−0.1209
	(0.0928)	(0.0826)
Treatment category *X* Shared amount in DG	−0.1976*	0.1428
	(0.1170)	(0.1107)
Discount rate (in IL-JL-EMPL)	0.2805	−0.5743
	(1.3441)	(1.1014)
Treatment category *X* Discount rate	0.5186	3.176*
	(1.8663)	(1.7066)
Observations	114	115
R^2	0.1060	0.2542

Notes: **1.** *** and * denote statistical significance at 1% and 10% respectively; **2.** in Column (1) only treatments JL-EMPL and IL-JL-EMPL are considered, whereas in Column (2) only treatments IL-EMPL and IL-JL-EMPL are considered; **3.** heteroscedasticity-robust standard errors are in the parentheses; **4.** only H&L risk-inconsistent observations are dropped; **5.** 2 observations are dropped due to Horrace & Oaxaca trimming in Column (1) and 6 observations are dropped due to Horrace & Oaxaca trimming in Column (2).

due to the reason that several observations are now dropped, as described above. Furthermore, calculating with the given values of the parameters in the experimental task, the relative expected gain from choosing IL over EMPL is not as high as it is from choosing JL over EMPL; that is probably why we see that take-up of loan does not differ much between treatments JL-EMPL and IL-JL-EMPL, because JL clearly excels due to its relatively higher expected value. In Column (3), we see that when the individual-specific variables are controlled for, the treatment effects maintain the same direction and the model's explanatory power no doubt increases. I find a statistically significant decline in the probability to take up any loan by 4.4 pp. ($p = 0.010$), with unit increase in the level of risk aversion (i.e. the number of safe lotteries chosen in H&L test). However, no significant effect of either altruism or discount rate found.[40],[41] Finally, the results remain unchanged to robustness checks.[42]

[40] The most pressing shortcoming of LPM as a binary response model is that its estimates do not restrict themselves within zero and one, and this potentially leads to bias and inconsistency. However, a *trimming rule*, proposed by Horrace and Oaxaca (2006), suggests that as long as the number of observations with predicted probabilities outside the unit range is small and as long as these observations are dropped, the estimates from LPM are consistent. Hence, I use this refinement for my estimations.

[41] Also note, I find the predicted probabilities with the LPM model to be very identical to those from logit and probit models, the correlation between the predicted probability vectors being around 0.99 *(logit and probit results not reported here)*.

[42] In my sample, ten subjects shared more than half in the dictator game, five of whom shared the entire sum of 3 Euros. There was no way to verify if these subjects had mistaken the amount to be shared for the amount that they wanted to keep for themselves. Therefore, to avoid predicting the effects based on any confusion, I conduct a robustness check by repeating the regression after dropping these ten observations. The results do not change in any meaningful way. (See the comparison of results in Table C1 in Appendix C).

4.2.3. Determinants of choice between loan types

4.2.3.1. Results of OLS regressions on take-up of the two loan types in Round 1. I separately study the take-up of the two different loan options in Round 1. To estimate the impact on the take-up of each of the two loan types, I again use a linear probability model. Table 3 Columns (1) and (2) respectively report the effects on take-up of JL loan and IL loan across the treatment variations.

For JL take-up. I find that only altruism has a statistically significant effect in group IL-JL-EMPL; a unit increase in DG-sharing increases probability to take up JL by 23.60 pp. ($p = 0.012$). The interaction coefficient −0.1976 gives the difference in the slopes of DG-sharing in groups JL-EMPL and IL-JL-EMPL. Though this interaction term is statistically significant (at 9.5%), the average marginal effect of DG-sharing on JL take-up remains positive in the treatment group JL-EMPL (i.e. main effect + interaction effect = 0.04).[43] In case of risk aversion, I do not find any significant difference in slopes across the treatments.

For IL take-up. The measure of risk aversion has a statistically significant effect in group IL-JL-EMPL; with unit increase in risk aversion, the probability to take up IL loan decreases by 5.65 pp. ($p = 0.062$). The interaction coefficient indicates that the difference in the slopes of risk aversion in groups IL-EMPL and IL-JL-EMPL is not statistically significant. Here, DG-sharing has neither significant main effect nor interaction effect.

Therefore, combining the evidence on effects of the individual-specific control variables across the treatment categories, I can conclude that altruism has a positive effect on probability to take JL loan and risk aversion has a negative effect on probability to take IL loan. And, since there is no strong, statistically significant difference of the average marginal effects across the treatment categories, we can conclude that more or less similar effects persist across the comparable treatments.

Summing up the findings: The risk parameter drives the take-up of IL more strongly than the social preferences parameter, with increased risk aversion reducing inclination for this loan type; whereas for JL loan, the social preferences parameter seems to be the stronger driving factor, with more altruistic subjects showing more willingness to choose it. Thus, relating the findings in the lab to the theoretical predictions, IL choice does relate to less risk aversion and JL choice to more other-regarding preferences.[44] As mentioned previously, due to limitations in the design of the discounting task, its findings are not stressed upon.

Regarding the inference on the association of individual preferences and the take-up decision of the subjects, one could point out the potential risk of reverse causality since the preference measures were elicited after the loan type choice-making. Though this proposition cannot be completely denied, I counter-argue that the chances are quite low. This is because the determination of final earnings of the microfinance experiment was done after the elicitation of social and risk preferences. So at the time the subjects were doing these additional tasks, they simply knew that they would earn according to one of the several rounds which they had just played and that round would be chosen randomly. In addition to that, while studying effects of preferences on take-up type, I use the first round's choices only, and it is unlikely that the first round's choice-making could have particularly affected any subject's performance in the preference elicitation task which came quite later.

[43] In Table C2 in Appendix C this interaction term becomes statistically insignificant, when I drop the ten observations who share more than half in the DG.

[44] Note, the findings in Tables 2 and 3 remain unchanged when I control for additional individual-specific variables, e.g. subject's gender, if study-field is Economics, Mathematics or Statistics, etc.*(results not reported here)*

Table 4
JL & IL take-ups in 10 rounds.

Panel A: JL take-up	
Lagged JL takeup	−0.1294*
	(0.0718)
Lagged earnings outcome	−0.0120
	(0.0622)
Lagged JL takeup *X* Lagged earnings outcome	0.0962
	(0.0711)
Subject fixed effects	Yes
Observations	1026
R^2	
overall	0.0419
within	0.0139
between	0.6719
Panel B: IL take-up	
Lagged IL takeup	−0.1060*
	(0.0548)
Lagged earnings outcome	−0.0213
	(0.0330)
Lagged IL takeup *X* Lagged earnings outcome	−0.0072
	(0.0432)
Subject fixed effects	Yes
Observations	1062
R^2	
overall	0.2362
within	0.0122
between	0.9756

Notes: 1. * denotes statistical significance at 10%; **2.** only treatments JL-EMPL and IL-JL-EMPL are considered in Panel A and only treatments IL-EMPL and IL-JL-EMPL are considered in Panel B; **3.** standard errors clustered by subjects are in the parentheses; **4.** only H&L risk-inconsistent observations are dropped; **5.** in Panel A(B), the coefficient of the interaction term reports the difference in effect of lagged take-up of JL(IL) for *Lagged earnings outcome = 1* when compared with effect of lagged take-up of JL(IL) for *Lagged earnings outcome = 0*.

4.2.4. Impact of treatment on take-ups of different loan types over the rounds

I now move on to the analysis with all rounds' data. Iyenger and Lepper (2000) and Dal Bó and Fréchette (2018), in different contexts inside and outside the lab, mention that individuals learn with experience (or, repetitions of matches/supergames in the lab) and this learning substantially affects the choices they make later. In this capacity, I examine if after playing the same 'infinite' horizon game repeatedly, the subjects ended up with a 'favourite' choice. Each session had ten rounds. In the beginning of each round, the subjects could make a new choice of the income type with which they played an indefinite-period game in that round. I separately study the effect on take-up of the two different loan types. I use the two-way interaction between *lagged take-up* (binary: 1 if the choice type in question was taken in the past round, 0 otherwise) and *lagged earnings result* (binary: 1 if earned more than zero in the past round, 0 otherwise). The lags are by one round. The individual-specific control variables and the treatment type are also present in the specification.

Using LPM with subject fixed effects, I do not find any interesting outcome as such.[45] According to the panel regression results of both JL and IL (given in Table 4 Panels A and B), there is statistically significant evidence at 7% and 5.6% that if earnings in the last round was zero, then willingness to take JL (IL) in the current round by those who had taken it in the last round is 12.94 (10.60) pp. less than those who had not taken it in the last round. This implies that if earned nothing with a certain loan type in the last round, subjects are less willing to repeat take-up of the same loan type in the current round. While comparing the interaction effect (positive in case of JL and negative in case of IL), we can infer the following: in case of IL take-up, even if positive income was earned with IL in the last round, subjects are even less likely to take it again compared to those who earned zero income with it in the last round. But in case of JL, subjects are relatively less likely to turn away from re-take-up if earned a positive income in the last round than those who earned zero income with JL in the last round. However, the interaction effects are not statistically significant to confirm this interpretation. The implications remain similar when I drop the ten observations who share more than half in the DG; however, the main effects of *lagged JL(IL) takeup* on re-take-up become statistically insignificant (See results in Table C3 in Appendix C). In sum, it is safe to conclude that the subjects did not stick to only one choice type; even if they earned some positive income from their chosen type in any round, they showed less willingness to choose the same option again in the next round.

5. Conclusion

The motivation for this study comes from the gradual decline in faith in microcredit loans as a global poverty alleviation tool. From large-scale field experiments implemented in several developing countries, the short-run and long-run welfare effects from microcredit are found to be miles behind what had been projected two decades ago. And, one of the key instruments for this bad performance could be the low demand or take-up of these loans by those poor who are the very target group. This outcome of low take-up is common to many field experiments conducted over the last decade. Such unanticipated findings from big studies give rise to a new interest in the post-analysis of what could have gone wrong. This study makes a modest attempt to explore certain behavioural channels that could provide some answer. I start with a very controlled set-up in the lab where I simply try to dissect the advantageous and disadvantageous sides of IL and JL loan types and understand if individuals with different preferences are inspired by the loan features differently. I argue that if it is so, then borrowers should be able to better self-select into their desired loan type from a flexible choice-set of different types; this, in turn, would be able to lead to the final goal of increasing overall take-up. Therefore, the aim of this paper is to show that the heterogeneity in preferences of prospective borrowers drives take-up. In a lab experiment with student subjects, I study a group who are offered a broader choice-set of both loan types *vis-à-vis* two other groups who are offered only one type each. I do find statistically significant evidence that take-up rate is higher in the former group. More importantly, I find statistically significant evidence that risk averse subjects totally stay away from any loan type and takers of JL type have more than 'self-regarding' preferences.

A limitation of this lab setting is that the JL loan type only allows partnering up with someone anonymous, which is unlike the case in a real setting[46]. In defence of that limitation, if already JL is well in demand in a framework which allows only random anonymous partners, then its demand would certainly be more pronounced in the real situation where the borrowers are willing to partner up with friends and acquaintances.[47] If we think of the real situation, JL loan is actually a safer choice if and when borrowers are fully assured that their partners cannot cheat on them and will help them in case of a genuine investment failure. If such an assurance is there within a group - by means of

[45] Note that the individual-specific variables including the treatment type to which the subject belonged, are automatically omitted in the fixed effects estimation, since they do not vary across the rounds.

[46] although exogenous group formation has been used to some extent in the context of urban slums (Armendáriz de Aghion & Morduch, 2007)

[47] Nevertheless, Wydick (1999) finds evidence from group lending in Guatemala that social ties per se have little or no impact on borrowing group behaviour.

information symmetry on investment outcomes, then it is justified that the borrowers would be convinced to opt for it. However, it would be interesting to check how the interplay of preferences evolves when information asymmetry on investment outcomes is allowed within the group, because that will unleash uncertainties in several aspects. For example, it would be interesting to see if the fear of getting freeridden by the partner demotivates a borrower from choosing JL or if the freeriding incentive actually drives the borrower to choose JL?

Moving on to further conclusions from this study, though we find that JL loan has a higher demand in a flexible choice-set, we cannot, however, ignore the need for IL loan. According to the evidence here, demand for JL is higher among those who show more generosity in sharing income; therefore, those who are not so generous and yet have less risk averse preferences could be more interested in IL loan. As a result, being a lender, it would be useful to elicit the preferences of the prospective borrower and then offer a suitable loan type; or, if preference elicitation is not feasible, it is better to offer a bigger choice-set of loan types so that the borrower has room for self-selection. In the context of lending flexible choice-sets, Ghatak (2000) is the first to explore the feasibility of a menu of JL contracts (where IL type is also present as a special case of JL with zero liability). Ghatak shows that the lender as a *planner* can, under certain conditions, optimally offer different contracts in a separating equilibrium to groups differing in risk types. So far, I have abstracted from discussing optimal lending conditions, but if the lender aims to maximize aggregate surplus as a planner, it would not be impossible to find out conditions in my set-up either, under which she can be better-off by offering optimal separating JL and IL contracts to borrowers who vary in risk and social preferences.[48]

Despite such theoretical feasibility for the lender to offer an optimal menu of various contracts from which borrowers of different types can self-select, in practice, we have seen most microfinance institutions (MFIs) across the world tied to one contract type only. Though some eventually introduced different contracts in parallel, they usually offered different loan types according to the risky nature or business size of the prospective borrower, as opposed to giving the borrower the possibility to self-select from a menu.[49] This way of offering a loan type according to the need of a particular borrower is indeed conducive to better take-up, however, it is still limited to situations where the lender can elicit information on risk and size of investments (e.g. a borrower with a successful repayment history on previous loans from the MFI, can be easily offered an individual loan since she has proven her reliability). But, it is not always possible to elicit this information on the nature of the investment and borrower (e.g. first-time borrowers). This is where my experiment tries to provide some solution. It tests whether under the circumstances, where the lender does not have much information, offering a menu of contract types for self-selection increases take-up.[50]

With a final reflection on the outcomes of the experiment, though the magnitude of effects found here may not be the same in a framed-field/field experiment, the directions of the effects provide significant evidence that loan offers personalized according to heterogeneous preferences of the borrowers would lead to a better take-up rate.

Appendix A

Proof of Proposition 1. By assumption, $s < (\pi - \alpha\pi)$; therefore, the *RHS* in (3') increases with γ. This implies that when γ increases beyond a certain threshold, say γ^*, the *RHS* exceeds the *LHS* and it is not optimal to choose IL instead of EMPL anymore.

Again, as $(\pi - 2\alpha\pi) < (\pi - \alpha\pi)$, with decrease in the value of γ the *LHS* becomes smaller. As γ declines and crosses a certain threshold, say γ^{**}, the inequality in (5') does not hold anymore. (Note, in order to have EMPL preferred to JL, given that JL has a higher expected discounted utility than IL, a borrower has to have an even higher threshold, say γ^{***} ($\gamma^{***} > \gamma^*$).)

Thus, combining the two inequalities in (3') and in (5') together, the thresholds of preferences in case of a *CRRA* utility function can be obtained. This can be further extended for the general functional form, as described by Proposition 1. □

Proof of Corollary 2. $EU_{i,JL,\beta} \geq EU_{i,IL}$ implies

$$\frac{(1-\beta)p}{1-\delta(2p-p^2)}[pU_i(\gamma;\pi-\alpha\pi)+(1-p)U_i(\gamma;\pi-2\alpha\pi)]+\frac{\beta}{1-\delta(2p-p^2)}U_i(EV_{j,JL}) \geq \frac{1}{1-\delta p}.\ p.\ U_i(\gamma;\pi-\alpha\pi) \tag{A.1}$$

By simplifying (A.1) and using a *CRRA* utility function, *JL* is preferred to *IL* as long as the following holds:

$$\frac{\beta(1-\delta p)EV_{j,JL}}{p(1-p)(1-2\delta p)(\pi-\alpha\pi)^{(1-\gamma)}}-\frac{\beta p(1-\delta p)}{(1-p)(1-2\delta p)} \geq 1-\frac{(1-\beta)(1-\delta p)}{(1-2\delta p)}\left[\frac{\pi-2\alpha\pi}{\pi-\alpha\pi}\right]^{(1-\gamma)} \tag{A.2}$$

Of course, when $\beta = 0$ even for the JL utility function, (A.2) simplifies to (5'). Other things equal, when γ is lower than the threshold γ^{**}, (5') does not hold anymore. Therefore, in a $\beta = 0$ model, an individual with risk aversion $\gamma'(< \gamma^{**})$ would optimally always choose IL over JL.

But, by assuming $\beta > 0$ and $\gamma = \gamma'$, while other things remain equal, JL ≻ IL as long as,

$$\frac{\beta(1-\delta p)}{(1-p)(1-2\delta p)}\left[\frac{EV_{j,JL}}{p(\pi-\alpha\pi)^{(1-\gamma')}}-p\right] \geq 1-\frac{(1-\beta)(1-\delta p)}{(1-2\delta p)}\left[\frac{\pi-2\alpha\pi}{\pi-\alpha\pi}\right]^{(1-\gamma')} \tag{A.3}$$

(Note that, it is also possible to find similar conditions by simultaneously varying the discount factor δ.)

To simplify the implications of the mathematical findings above, let's look at an example where IL ≻ JL when we consider a β-free model, but is

[48] Ghatak further shows that a separating equilibrium can exist in a competitive credit market too, where lending firms compete by offering loans of varying amounts of joint liability (including IL with zero liability). Although Ghatak's lending structure differs from my set-up (in terms of dynamic incentive and adverse selection), in essence, a competitive equilibrium can be feasible in my setting too, e.g. where *lender A* offers JL and its competitor *B* lends IL and the borrowers can self-select the lender type (e.g., Ghatak points out that in the 90's Bolivia, Banco Sol offered JL loans and its competitor Caja Los Andes offered IL, and thus catered to different types of borrowers).

[49] For example, both Banco Sol and Grameen Bank after pioneering in group-lending, introduced IL contracts in parallel for the clients who were successful and/or who preferred not being obligated to others by being in a group. Other examples are two Canadian MFIs which offered both IL and JL contracts in the early 2000s, but the borrowers who received IL loans usually had higher income, larger and reliable businesses (Armendáriz de Aghion & Morduch, 2007).

[50] I thank an anonymous referee for this discussion.

not necessarily true when β is positive.

The simplest case is with $\gamma = 0$ and $\delta = 0$ *(a risk neutral individual who discounts future totally)*, where (A.3) boils down to the following condition so that JL can yield higher expected utility than IL,

$$\frac{\beta}{(1-p)}\left[\frac{EV_{j,JL}}{p(\pi - \alpha\pi)} - p\right] \geq 1 - (1-\beta)\left[\frac{\pi - 2\alpha\pi}{\pi - \alpha\pi}\right] \tag{A.4}$$

However, when $\beta = 0$, JL $\succ$ IL as the above inequality never holds ($1 - \frac{\pi - 2\alpha\pi}{\pi - \alpha\pi} \neg \leq 0$). □

Appendix B

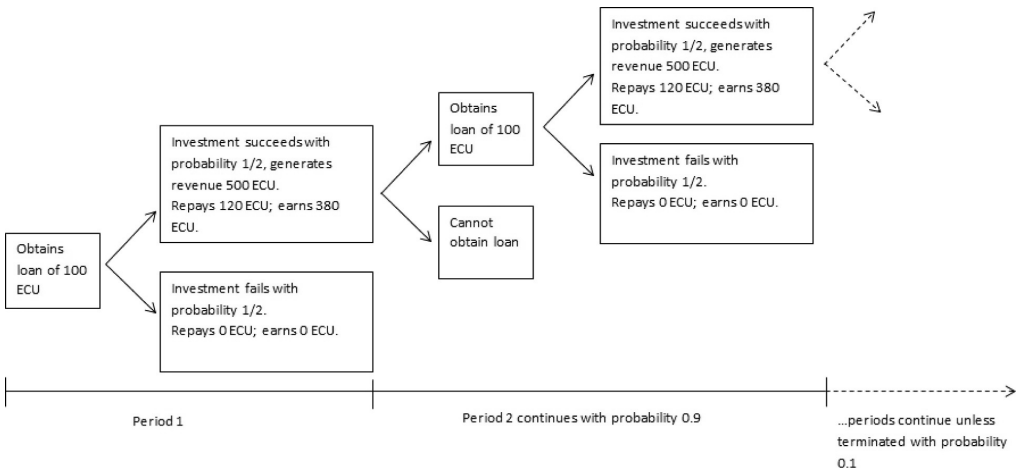

Fig. B1. A Round with IL Loan. **Notes:** This figure describes how the steps of a Round proceed if IL loan is chosen from the set of options (depending on what treatment type is being offered). The sum of the subject's earnings over the respective periods constitutes her income from this particular round.

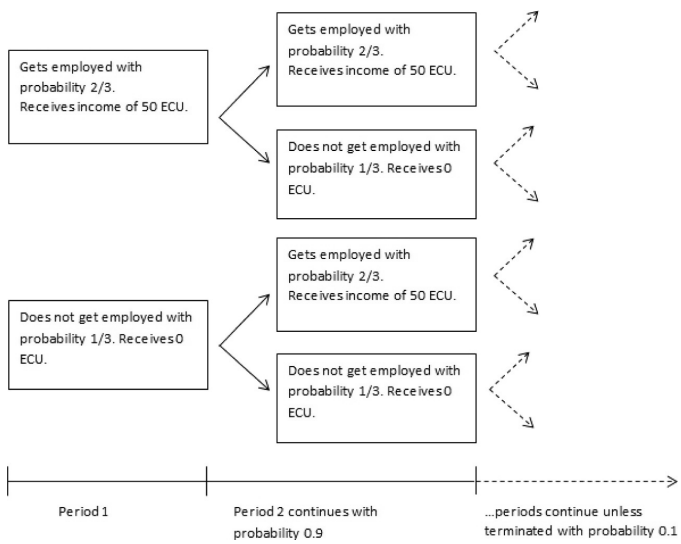

Fig. B2. A Round with EMPL. **Notes:** This figure describes how the steps of a Round proceed if EMPL loan is chosen from the set of options (depending on what treatment type is being offered). The sum of the subject's earnings over the respective periods constitutes her income from this particular round.

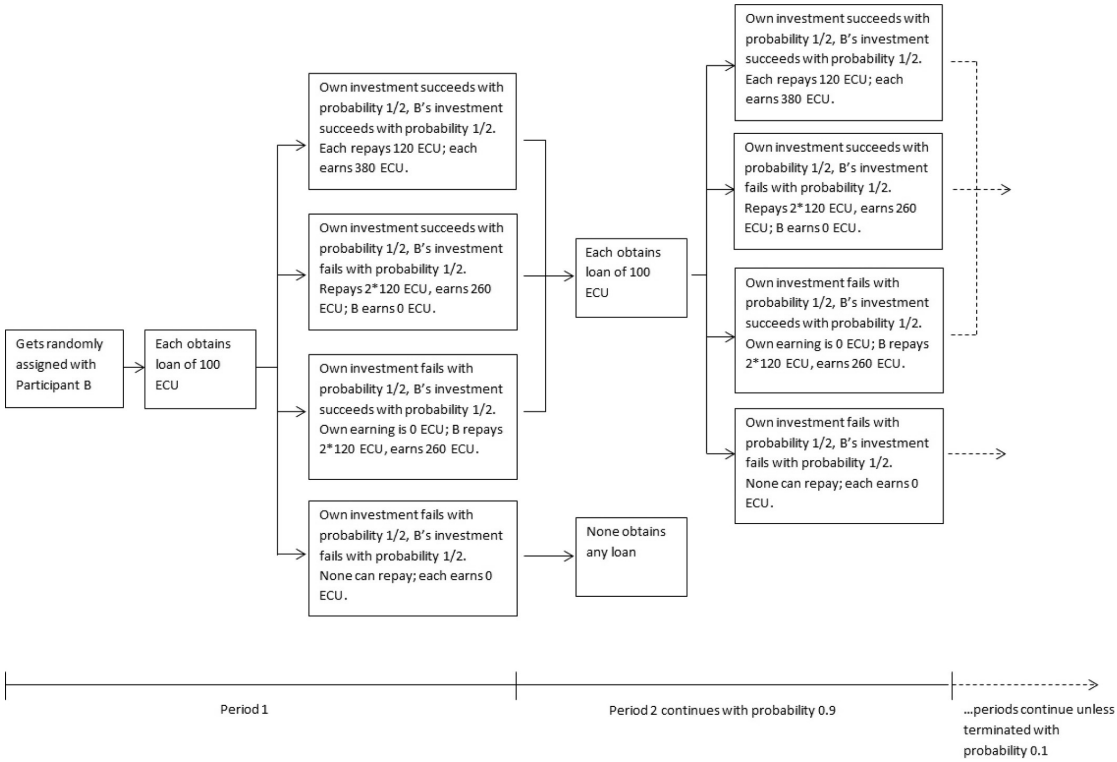

Fig. B3. A Round with JL loan. **Notes:** This figure describes how the steps of a Round proceed if JL loan is chosen from the set of options (depending on what treatment type is being offered). The sum of the subject's earnings over the respective periods constitutes her income from this particular round.

Supplementary material

Supplementary material associated with this article can be found, in the online version, at 10.1016/j.socec.2019.101456 .

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Is household shock a boon or bane to the utilisation of preventive healthcare for children? Evidence from Uganda.

II

Is household shock a boon or bane to the utilisation of preventive healthcare for children? Evidence from Uganda

Susmita Baulia*

Abstract

A stylised fact in the development literature is that poor households in low-income countries invest very little in preventive healthcare. This paper investigates how households invest in children's preventive healthcare during idiosyncratic shocks when resources are even more limited. By using the incidence of flood or drought as the proxy for a negative income shock, and illness of any household member as the indicator for an adverse health shock, I examine the shocks' effects on the intake of Vitamin A Supplementation (VAS) by children. By using four waves of the Uganda National Panel Survey, results from a household fixed effects analysis show that children under the age of two years are significantly more likely to get VAS as a part of their immunisation schedule when the household is under a shock. Further investigation shows that the effect of a health shock results from the increase in average time spent outside the labour market by household adults due to illness. On the contrary, an income shock has a positive effect on the average time spent in the labour market. However, a negative interaction effect of the income shock with the household wealth level suggests that relatively wealthy households could be substituting labour hours with the otherwise time-intensive preventive healthcare activities, thus increasing the VAS uptake.

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1 INTRODUCTION

Although preventive healthcare, such as immunisation, is a proven tool for controlling and eliminating life-threatening diseases, a stylised fact in low-income countries is that poor households do not invest much in preventive healthcare (Dupas, 2011). According to Dupas, one possible explanation for this could be the high opportunity cost of time for resource-constrained households.[1] Building on this argument, one can expect that in times of adverse shocks to the household, when resources are even more limited, parents would postpone immunisations of their children in order to cater to the bigger crisis at hand.

Now, shocks are events manifested through risks and cause significant negative welfare effect to a group or an individual. In the form of illness, unemployment, or natural calamities, shocks may themselves cause or compound poverty (Canagarajah et al., 2002; Marques, 2003). Particularly, poor households in developing countries face substantial risks of such shocks. Most often, due to the lack of reliable coping mechanisms through savings and insurance, vulnerability to poverty linked to such risks remains high (Dercon, 2002).[2]

Shocks being so widespread among poor households, the question that this study aims to answer is how the adversities due to these shocks affect the utilisation of preventive healthcare among the poor. Given the scarcity of resources and the struggle for coping mechanisms, to what extent do the households trade off investments in preventive healthcare practices? More importantly, does this trade-off vary with the shock type suffered? To elucidate, in credit-constrained households, a negative income shock is likely to have a substantial income effect on healthcare, especially on preventive healthcare. However, on the other hand, if the shock is health-related, e.g. a household member is ill, it could bring awareness in the household about the importance of health. Regarding health shock, one can argue that if demand for healthcare is a *derived* demand from that for health (Grossman, 1972), households with reduced health stock will gain higher marginal utility from health, and hence invest more in preventive healthcare. Besides, one could even point out that if a household member has to stay away from work due to illness and/or has to visit the health-centre for remedial care, then the additional cost of taking the child along for preven-

[1] While a high opportunity cost of time could be considered a consequence of resource/liquidity constraints, other reasons for low investment in preventive healthcare that have been recognised in the literature, are lack of information and behavioural biases (Kremer and Glennerster, 2011).

[2] Adverse shocks to the poor in low-income countries are associated with uninsured risks and results in income and non-income losses for households. Dercon (2002) summarises in the context of Ethiopia, that almost 80% of rural households faced severe risk-related hardships through harvest failure over decades. Other prevailing problems are policy shocks, labour problems and livestock issues. Wagstaff and Lindelow (2014) find in the context of Laos, that health shocks are more common than most other shocks and more concentrated among the poor. Health-related shocks are more idiosyncratic, more costly and highly likely to lead to a cut in consumption.

tive healthcare would be lower. This latter argument holds provided preventive healthcare for children is publicly available for free and that the household only needs to invest time to use that. These various theoretical possibilities justify the need for an empirical investigation into the relationship between different types of shocks faced by resource-constrained households and their preventive healthcare investments. This paper attempts to shed light on that, and while doing so, also examines underlying mechanisms of the effects.

With an empirical investigation of Ugandan households, I find that while facing a decrease in total income or a deterioration in health status, poor households are more prone to take their children for immunisation, and this is associated with the increase in time away from the labour market in times of shock.

In the literature involving income shocks and healthcare for children, the focus, so far, has been mainly on the relative strength of income and substitution effects of aggregate shock, and the evidence on that from developing countries is quite nuanced. Two most relevant studies in this context are by Miller and Urdinola (2010) and Fichera and Savage (2015). However, their findings are contrasting. By using world coffee price fluctuations as a proxy for aggregate income shock, Miller and Urdinola (2010) find evidence of countercyclical time-intensive child health investments in Columbia, i.e. a stronger substitution effect. They find that when coffee prices are high, parents choose to work and thus do not have time for health investment. On the other hand, Fichera and Savage (2015), who instrument a positive income shock with rainfall measurements, find evidence of a stronger income effect in Tanzania, in which a rise in income reduces illness and increases vaccinations for children under six.[3]

According to the broader literature of child human capital formation, which follows a similar conceptual framework, most studies based on developing countries have investigated the income shock at an aggregate level. Björkman-Nyqvist (2013) finds that a negative income shock, measured by reduced rainfall, lowers children's educational hours in Uganda; whereas Shah and Steinberg (2017) find evidence from India that a positive aggregate shock measured by rainfall, increases the opportunity cost of schooling and thus increases the school dropout rate. Beegle et al. (2006) conduct one of the very few studies that focus on the effects of an idiosyncratic income shock. They look into children's educational outcomes in Tanzania; findings imply that households, when hit by a negative income shock (proxied by sudden crop loss), tend to increase the use of child labour to substitute adult labour in household activities.

The bias in attention on aggregate income shocks in comparison to idiosyncratic ones comes from some strong arguments put forward by the advocates

[3] According to Fichera and Savage (2015), this difference in outcomes of the two studies could be due to insufficient weather-related variations to income compared to that from coffee price variations which in turn did not affect the opportunity cost of time. Furthermore, a stronger income effect than substitution effect in their study could have resulted from the better access to health-centres which also did not affect the opportunity cost of time that heavily.

of the former. Ferreira and Schady (2009) argue that an idiosyncratic shock is less interesting because it usually has no substitution effect unlike aggregate shock; Townsend (1994) proposes that an idiosyncratic shock may lack strong manifestation because it can be easily insured away by formal and informal mechanisms. While these arguments are pretty well-founded, one must also consider certain difficulties that arise in estimating the real effect of an aggregate income shock. Hyder et al. (2015) stress that measured aggregate shocks are effectively the average of individual shocks that vary considerably within heterogeneous communities; in this regard, the use of individual idiosyncratic shocks rather than community averages may represent with less measurement error what individual households experience. Another drawback of an aggregate shock is that often it interferes with the supply of services and thus makes it difficult to understand true demand. For example: when the public sector is an important provider, if public spending on health or education is procyclical and if expenditures and service quality are linked, then cuts in public expenditure on these services may reduce the value of schooling and healthcare to households during recessions. Under such circumstances, the income effect gets more pronounced (Ferreira and Schady, 2009). In light of these arguments, studying the effect of an idiosyncratic income shock could provide a better and more accurate understanding of the demand by households.

The literature of health shocks shows that this shock ranks the highest in terms of incidence, idiosyncrasy, costs and impact among the poor (Wagstaff and Lindelow, 2014). However, the literature is quite scarce when it comes to health shocks and their effects on healthcare. To date, there is only some evidence on its effects on children's educational outcomes. Studies by Bratti and Mendola (2014), Alam (2015) and Dhanaraj (2016) based in low-income settings confirm that parent's illness affects educational outcomes of children of different age cohorts to varying extents.

We can summarise from the above studies that the literature associating household-level income or health shocks and investment in children's preventive healthcare (or broadly, in child human capital) in developing countries is quite scarce. It sufficiently invigorates our curiosity to empirically examine the effects, if any, of both types of shocks on preventive healthcare for children. Uganda provides a suitable set-up with poor households to test the effects of idiosyncratic income and health shocks on their investments in preventive healthcare for children. Moreover, when it comes to preventive healthcare for small children, the best outcome variables to discuss are those related to immunisation.[4] Therefore, by using four waves from the Uganda National Panel Survey, I examine the effect of a health shock and an income shock on the intake of Vitamin A Supplementation (VAS) by children under two years, as a part of their immunisation schedule. I use the illness of any household member as an indica-

[4] The words *immunisation* and *vaccination* are often used interchangeably in this context.

tor of an adverse health shock and the incidence of flood or drought as the proxy for a negative income shock.

Primary results from a household fixed effects analysis show that the probability of taking the child to get VAS increases significantly if a negative health shock hits the household. Similar evidence is obtained in case of a negative income shock too. As mentioned above, no direct cost is incurred by Ugandan households in getting their children immunised; however, they could face indirect costs (e.g. from transportation to healthcare facilities) and/or opportunity cost of time which they spend in accessing healthcare services. In the event of a health shock, the latter cost (in other words, time spent away from labour market activities) seems to drive the primary finding. Evidence shows that a typical member of a household hit by health shock spends significantly less time in labour market activities than one from a shock-free household. This finding could indicate that the inability to be able to work due to illness reduces the opportunity cost of this 'forced' time away from the labour market, which is then used for remedial care and health-promoting activities for their children.

In case of an income shock, it is, however, difficult to pin down the underlying mechanism. The study does confirm that the average effect of an income shock on time spent in the labour market is positive. Further, there is confirmation that a relatively wealthy household spends lesser time on average in labour activities during the shock. However, with the limitation of being able to confirm directly, the study can only suggest that the positive effect found on the intake of VAS is driven by the wealthier households substituting preventive healthcare activities for labour (as their opportunity cost of time away from work decreases).

Thus, this study points out that the effect of a health shock *vis-à-vis* an income shock works through different pathways in the utilisation of preventive healthcare in poor households. Finally, keeping in mind the caveat of health shock confounding with income shock, the findings are put through a battery of robustness checks, and the results remain consistent. However, it is important to point out that most findings of this study are statistically significant at only 10%; therefore, further research in similar settings is required in order to draw a strong conclusion.

In its contributions, this paper offers a comparison of the effects of both income and non-income shocks on child human capital investment. Thus it complements a similar investigation done by Bandara et al. (2015) in the context of child labour in Tanzania. Furthermore, the results obtained in this paper contribute to the line of literature which supports the primacy of time in households for child healthcare (Grossman, 1972; Gronau, 1977; Vistnes and Hamilton, 1995; Miller and Urdinola, 2010). This body of literature recognises that health investment is costly as individuals must trade off time and other resources related to health; therefore, it affects the optimal demand for health.

Now, a shock is merely an identification of the dimensions along which the

household is constrained *ex ante*.[5] In this vein, a growing literature on the effect of unconditional and conditional cash transfers in low-income settings on the uptake of preventive healthcare such as vaccinations (Barham and Maluccio, 2009; Robertson et al., 2013), regular cancer screening and HIV tests (Ranganathan and Lagarde, 2012), and other health and nutritional outcomes such as stunting and obesity (Behram and Hoddinott, 2005; Fernald et al., 2008a,b) identifies that financial barriers are primarily responsible for the underutilisation of preventive healthcare. Among these financial barriers, one is the opportunity cost of time spent on accessing health services instead of being spent on income-generating activities. In this connection, this paper contributes to this body of literature which identifies the demand-side financial barriers in utilising healthcare.

The remainder of the paper is organised as follows: Section 2 gives an overview of child health and healthcare status in Uganda and motivates its suitability in studying my research question. Section 3 introduces the data, study variables and the summary statistics; Section 4 consists of the empirical specification; Section 5 summarises the results, followed by a conclusion in Section 6.

2 THE UGANDAN CONTEXT

Uganda is a financially developing country in Sub-Saharan Africa that currently holds rank 159 out of 189 countries in the Human Development Index in 2018. The under-five child mortality rate of the country is 45.8 per 1,000 live births in 2019.[6] The Ugandan Ministry of Health had already recognised in 2010 that 75% of the disease burden in the country could be averted by immunisation, hygiene and sanitation, nutrition and other preventive healthcare practices and health-promoting activities. The Ugandan National Expanded Programme on Immunisation has been functional for over four decades with a goal that every Ugandan child should be fully vaccinated. Also, since 2001, the Ugandan National Minimum Healthcare Package entitles every Ugandan a free basic healthcare coverage at public healthcare facilities. Despite the availability of these programmes, the outcomes on child health are not promising. In 2011, only 52% of children aged 12-23 months were fully vaccinated, and only 40% of children aged 12-23 months were immunised before their first birthday (Uganda Bureau

[5] As an aside, note that while this study focusses on the direct effects of shocks *per se*, previous literature has most often considered shocks as an identification strategy to learn the dimensions the poor are constrained *ex ante* or how intra-household bargaining works.

[6] Child health statistics obtained from https://data.unicef.org/country/uga/.

of Statistics, 2012). According to the Uganda Demographic and Health Survey (2011), the rate of Vitamin A deficiency, which can threaten overall immunity and cause blindness, was as high as 33% among children under five, despite the availability of immunisation doses.

Given this status, it seems logical to investigate if an income or a health shock at the household-level acts as a demand-side barrier and potentially hinders the utilisation of preventive healthcare for children; and also, if the household's trade-off on investment for preventive healthcare varies with the type of shock suffered.

3 DATA AND SUMMARY STATISTICS

3.1 Data

The data consists of four waves of the Uganda National Panel Survey (UNPS) collected in 2009-2010, 2010-2011, 2011-2012 and 2013-2014. The Uganda Bureau of Statistics implemented the UNPS with financial and technical support from the Living Standards Measurement Study-Integrated Surveys on Agriculture (LSMS-ISA) programme of the World Bank. For the analysis, I use data from the household and community modules of the survey.

In the survey, a *household* is defined as a group of people who have been living and eating their meals together for at least 6 of the 12 months preceding the interview. Therefore, the members of the household are defined by their usual place of residence. The first wave of UNPS consists of 2975 such households tracked and interviewed from a nationally and regionally representative sample of 3123 households that were initially interviewed in the Uganda National Household Survey (UNHS), another survey in 2005-2006.[7] The 2975 households of the first wave of UNPS include 2607 households retained from those 3123 households after attrition and an additional 368 *split-off* ones.[8] The second and third waves consist of 2716 and 2850 households respectively, after accounting for attrition and tracking of split-off households. The retention rate of the original households between waves 1 and 2 is 89% and that between waves 2

[7] The 2009 UNPS is a sub-sample of the 2005-2006 UNHS. For the UNPS sample, the UNHS sample was divided into five strata (Kampala, Central, Eastern, Northern and Western). Within each stratum, Enumeration Areas were selected using simple random sampling.

[8] Before the fieldwork of the first wave in 2009-2010, a random sub-sample of 20% from each Enumeration Area that added up to a total of 643 households was drawn from the already sampled panel households. If a chosen household indicated that any person who was a member in the 2005-2006 survey had left, that 'mover' referred to as split-off would be followed. 430 split-offs were interviewed in the first UNPS and by that time they formed 368 households (from UNPS Reports on waves 1 and 3).

and 3 is 92.4%. In the fourth wave of UNPS, a part of the third wave sample was dropped and replaced with a 'fresh' sub-sample of households. This fresh sub-sample was extracted from updated sample frames developed by Ugandan Bureau of Statistics as a part of the 2012 Uganda Population and Housing Census.

3.2 Variables

As the outcome variable on immunisation (or, preventive healthcare), I use the receipt of Vitamin A Supplementation by children. In the immunisation guideline for children, the Ugandan Health Ministry strictly recommends that all caretakers of children between 6-59 months should take them to healthcare facilities to receive Vitamin A Supplementation every six months, as a part of their immunisation and health promotion schedule.[9] Among other immunisation categories available from the survey (e.g. measles and DPT-3), I prefer VAS as the study variable because it is not a one-time dose like the other categories and thus allows me to have a richness of observations by including one child more than once in the analysis. The household questionnaire of the survey asks the question whether the child had received VAS during the last six months starting from the survey interview date. Since the question concerns intake of VAS in the last six months from the date of the survey interview, only those children who are at or above 12 months of age during the interview, were eligible for VAS intake in the period as mentioned earlier. In my analysis, I include only the children eligible for VAS. However, this dataset has information on VAS intake by children up to the age of 24 months only. Therefore, this analysis is restricted to households that have at least one child between 12-24 months during the survey. Also, since the analysis spans over four annual waves, it is unlikely that one household with only one child between 12-24 months of age would be present in all the four waves unless another child is born in that household. It gives the panel an unbalanced structure. Aiming for a balanced structure of the panel would constrict the dataset to a reduced number of observations.

In this study, I identify household shocks of two types - a health shock and an income shock. Throughout the paper, a *shock* implies a *negative* shock that causes a decrease in the current level of health or income. As the proxy for an income shock, I use any household-reported shock due to flood or drought/irregular rains in the past six months. The use of agriculture and weather-related variables as proxies for income shocks is a common practice in the context of developing countries where cultivation is the main livelihood. Some of the proxies used in the literature are accidental crop loss at the house-

[9] A detailed guideline on immunisation for Ugandan children, with recommendations from the World Health Organization and UNICEF, can be obtained from https://www.unicef.org/uganda/key-practice-immunization.

hold level (Beegle et al., 2006; Bandara et al., 2015), rainfall variation at the district level (Björkman-Nyqvist, 2013; Shah and Steinberg, 2017). In my sample, 50% of the households (having children between 12-24 months), have agriculture as their primary income source, with 4% of them engaging in commercial farming and 96% in subsistence farming.[10] Therefore, it is reasonable that drought or flood would be ideal candidates for an income shock here.

For a health shock, I use household-reported information on severe illness or death of any household member in the past six months. In related literature, Bratti and Mendola (2014) focus on the self-reported health status of the parents of the child as a measure of health shock, whereas Alam (2015) uses the self-reported health status of other household members too. In contrast, Bandara et al. (2015) use death as a measure of health shock, whereas Dhanaraj (2016) uses both severe illness and death as a health shock.

The information on shocks is available from the same survey module. The relevant questions are polar, i.e. if the household suffered a shock due to flood or drought/irregular rains in the past year and if the household suffered a shock due to severe illness/death of a household member in the past year. Last but not least, I define the incidence of the shock (health/income) as the presence of a shock in the household during the last six months from the interview, and this shock could have started even before these last six months but continued into these six months.[11] The reason behind focussing on only the past six months for the experience of shock is because the information on VAS dose is available for the prior six months only. Thus I get the best possible overlap of the two events.

3.3 Summary statistics

Table 1 summarises a comparison of the households which faced any shock, health or income, in the past six months with the households that did not. The following statistics pertain only to those households that had at least one child of 12-24 months in at least one survey wave. In **Appendix 2 Table B1**, I present a similar comparison of households with at least one child up to the age of five, and the findings do not vary as such.[12] Typically, the head of a shock-free

[10] In Uganda, above 60% of the population, is engaged in agricultural activities and more than 80% of the farming community consists of subsistence or smallholder farmers. These subsistence farmers usually cultivate less than one hectare of land and own a few heads of cattle. They produce crops or raise livestock for family consumption with a little surplus for the market. Subsistence farming is mostly labour-intensive and is mainly run by household members with no/limited ability to hire outside labour. Usual lack of transport and high transaction costs along value chains often lead them to sell the surplus in local markets and to local traders with negligible profits.

[11] The survey contains information on the start date and the end date of a shock and thus allows me to calculate its incidence in the six months before the interview.

[12] The comparison with a bigger sample is to ensure that the households with children between 12-24 months are not, in any meaningful way, different from other households with small children (here,

Table 1: Summary statistics of households with and without shock

Variable	HH without shock		HH with shock		
	Mean	Std. Dev.	Mean	Std. Dev.	Diff.
	(1)	(2)	(3)	(4)	(5)
Age of household-head (yrs.)	39.39	12.83	41.44	13.53	2.05***
Household-head ever attended school	0.89	0.31	0.83	0.38	0.06***
Household-head married	0.88	0.32	0.84	0.37	0.05***
Household members away due to work	0.08	0.30	0.07	0.28	0.01
Members living in household all year	4.13	2.41	4.36	2.54	-0.23*
Log total household assets' value (in UGX)	14.96	1.77	14.78	1.74	0.17**
Avg. no. of weeks in the labour market	12.04	13.71	14.20	14.86	-2.15**
No. of children under five in household	2.01	0.92	2.21	0.98	-0.20***
Avg. age of infants (12-24 mo.) in household	18.12	3.89	18.02	3.90	0.10
Household facing a health-related shock	0	0	0.26	0.44	-0.26***
Household facing an income-related shock	0	0	0.83	0.38	-0.83***
Number of observations	1486		553		

Notes: These statistics are obtained by comparing the households with at least one child of 12-24 months. A household can appear multiple times across waves 1-4 of UNPS. In Column (5), are the difference in means that are given in Columns (1) and (3). A t-test compares the means across the two types of households. The corresponding two-tailed p-values are reported. *** is $p < 0.001$, ** is $p < 0.05$ and * is $p < 0.01$.

Table 2: Summary statistics by panel waves

Variable	Mean	Std. Dev.
Child related variables:		
Infants (12-24 mo.) who received VAS in last six months from interview date	0.73	0.44
Infants (12-24 mo.) who has received DPT3 vaccine	0.85	0.36
Infants (12-24 mo.) who has received measles vaccine	0.84	0.37
Infants (12-24 mo.) who were breastfed	0.96	0.19
Infants (12-24 mo.) who slept under bednet the prior night	0.60	0.49
Infants (12-24 mo.) whose mother lives in the same household	0.92	0.27
Infants (12-24 mo.) whose father lives in the same household	0.73	0.44
Infants (12-24 mo.) whose mother has no education	0.002	0.05
Infants (12-24 mo.) whose father has no education	0.02	0.14
Household related variables:		
Household with main income source as agriculture	0.52	0.49
Household with main income source as subsistence farming	0.50	0.50
Household members away from household due to work	0.08	0.29
Household members present in household all year round	4.20	2.45
Number of children up to five years present in household	2.06	0.94
Average number of weeks in the labour market	12.61	14.05
Health Shock related variables:		
Households suffering from health shock in the last six mo.	0.07	0.26
Number of months suffered due to health shock	2.83	3.07
Income Shock related variables:		
Households suffering from income shock in the last six mo.	0.22	0.42
Number of months suffered due to income shock	3.54	2.04

Notes: This table provides the mean over all four waves of UNPS. The household and shock statistics are only for those households which had at least one child between 12 to 24 months in at least one wave, the number of such households being 1604. The child-related statistics span over 2061 children of 12-24 months.

household is about 40 years old, with about 89% chance of ever attending any

under five).

school and 88% chance of being married, and the average number of weeks spent in the labour market by a household member is about 12. These above figures are slightly different in a household that suffered a shock in the past six months. Among some variables, the means remain more or less similar across both the groups, with about one person in the household being away from home for work, about four permanent members being present all the year-round, and the mean age of children under two being 18 months. Across both the categories, about two children in the household are under five, and the logarithm of the total value of household assets is about 15 Ugandan shillings (UGX).[13]

Table 2 provides the means of the variables of interest across all the four waves. On average, 73% of eligible children received VAS in the past six months interval from the interview date. More than 80% of the children were reported to have received their DPT-3 and measles vaccines. Among other child-related variables, 97% of children were breastfed at birth, and 92% had their mothers, and 73% had fathers living with them in the same household. Compared to these figures, healthy lifestyle measures such as sleeping under a bed net was still not very common - only 60% of children were reported to have slept under bed nets the previous night. Among the health shock-related measures, 7% of the households suffered from a shock in the past six months. The absolute span of health shock, i.e. the total number of months of suffering from the shock, was 2.83 months on average (the maximum recorded being 12 months). 22% of the households reportedly suffered from an income-related shock in the last six months; the total span of an income shock was 3.54 months on average (the maximum recorded being 12 months). These figures validate that the households in the sample suffered from these shock types only for a few months. Hence, the possibility of any related chronic condition can be discarded. Also, the percentage of households suffering from an income shock suggests that the shocks were not all-pervasive to be considered at a large aggregate level (given that it is proxied by flood or drought). Finally, **Appendix 2 Table B2** reports these statistics for the bigger sample of households with children under five years.

[13] This is equivalent to a total assets value of about 3261097.37 UGX, roughly 1300 USD (1 UGX ≡ 0.0004 USD, between 2009-2013).

4 IDENTIFICATION STRATEGY

I use a linear probability model specification as given below, where I include each shock type as an independent variable.[14]

$$Y_{iht} = \beta_0 + X'_{iht}\beta_1 + \beta_2 HealthShock_{ht} + \beta_3 IncomeShock_{ht} + \alpha_h + \mu_t + \epsilon_{iht} \quad (1)$$

Here, the subscripts index over child i, household h and survey wave t. Y is the binary outcome variable on the intake of VAS by child i in household h during six months interval before the interview date in survey wave t. *HealthShock* and *IncomeShock* are binary variables denoting the experience of a negative shock related to health and income, by the household during the same time interval.[15] An income shock is proxied by the incidence of flood or drought; and, health shock is indicated by the illness (or, death) of any household member. X is a set of controls consisting of child and household characteristics that vary over survey wave t. I further include household fixed effects α_h and survey wave fixed effects μ_t. The primary coefficients of interest are β_2 and β_3; they measure the effect of the negative shock experienced by household h in survey wave t on the intake of VAS by child i in the same household in that wave.

I control for individual-level variables for the children, such as quality of care received. These include: if the mother lives in the same household if the father lives in the same household, if the mother received any education, if the father received any education, if the child had been breastfed at birth.[16] Here, one must use caution while controlling for various individual-level variables on the (quality of) care received, because those variables themselves could potentially be affected by the shock.[17] In this vein of argument, I stress that the control variable on breastfeeding is recorded during birth and is unlikely to be affected by a shock that occurs in the past six months in the household with a child of age 12-24 months. Furthermore, among child-related controls, I use a separate indicator denoting if the child was in the sixth month of its age during the past six months. The reason behind including this indicator is that children start their VAS eligibility schedule at the age of six months, so the parent/caregiver in the household is likely to be more alert about the first schedule.

[14] A linear probability model specification is preferable here because of the *incidental parameters problem* where the use of non-linear panel data models with fixed effects potentially leads to biased and inconsistent estimates (Greene, 2004; Wooldridge, 2010).

[15] In reality, income and health shocks are bound to confound with each other. Therefore, having both the shocks together in one regression equation could posit multicollinearity challenges. On a positive note, the correlation between the two shock types in the sample is only 0.027, and thus the concern for multicollinearity is minimum. Nevertheless, I again discuss this inseparability issue of the two shocks in greater detail in Section 5.3.2.

[16] Note here, the eligible children within a household do not necessarily belong to the same set of parents.

[17] The *bad controls* problem in econometrics.

I further use household fixed effects to control for several observable and unobservable time-invariant characteristics of the household that could potentially affect shock incidence as well as VAS intake by the eligible children in the household. The use of household fixed effects absorbs all the across-households variation and produces an estimate of the predictor's average effect within households. With the inclusion of household fixed effects, the effect of idiosyncratic risk is investigated, and while doing so, the time-invariant household risk factors are eliminated. Finally, the additional use of survey wave fixed effects allows controlling for heterogeneity arising across the survey waves.

To further minimise the possibility of *omitted variable bias* through time-variant household features, I control for the number of under-five children present in the household during the survey (this number potentially affects the amount and quality of information which a household has on child healthcare), the total number of permanent[18] members in the household (the more the number of adults in the household the more their cumulative work-free time which could be invested in childcare), and the logarithm of the total value of household assets. I further control if the family relocated in the past few years, if the survey interview was being done in a rainy season, and if the household lived in a flood-prone region. The last two controls take into account any possible seasonal effect.

In a less-parsimonious version of the model, I also include some health supply-related variables. They include information on the nearest health facility - whether it provides general outpatient care, whether it provides immunisation doses, whether a significant limitation of the facility is its remoteness and whether the lack of skilled staff is a significant limitation. These health supply-related covariates are likely to be correlated with the income shock as well as VAS uptake; if flood or drought affects a community, the health supply and the demand, both are susceptible to getting affected. Note that these are time-variant for the following two reasons, and hence, cannot be absorbed by a household fixed effect: the survey design allowed tracking of the sample households or their split-off parts that moved within parishes/communities , and also, in the five years of the four-panel waves, many of these health supply facilities expanded.

In another version of the model, by the same reasoning on the time-variance, I control for more variables which indicate the locational advantage and road-networking of the household. I include the distance of the household to the nearest facilities; these are - distances to the public health facility, to the markets selling agricultural inputs, agricultural produce, non-agricultural produce, to the primary market for livestock, to the nearest major roads with gravel and tarmac, to the nearest feeder road.[19]

[18] A *permanent* household member is the one who resides in the household all year round.

[19] To give some perspective, the development of road networks has been on the rise in Uganda; re-

Regarding the identification strategy, one concern is with the exogeneity of shocks. One could argue that shocks could more likely hit more vulnerable households. In addition to having household fixed effects, I address the issue by including time-variant household features that could potentially contribute to its vulnerability in being hit by a shock. These variables are the location of the household in a flood-prone region, the season of survey interview, the number of household members present in the past year, and relocation of the family in recent years. Nevertheless, one must bear in mind that time-variant unobserved heterogeneity of the households cannot be entirely removed with survey data analysis and hence, leaves some chance for bias in case it drives some of the effects.

As a final note regarding the chosen identification strategy, a better alternative of an income proxy could be to instrument the household income with the reported shock due to drought or flood and proceed with a two-stage least squares (2SLS) model, as, Fichera and Savage (2015) use the weather shock of period $t-1$ as an instrument of income in period t. However, here arises the problem of data limitation of the survey. Only sparse information is available for the first three waves on the previous year's household income from crop-farming, and other agricultural and non-agricultural enterprises. Thus, it causes a sample size challenge in 2SLS estimation. As a result, I resort to the currently chosen strategy, given by **Equation 1**, which can be considered a reduced-form of the IV strategy.

5 RESULTS

5.1 The effects of shocks on intake of VAS

In **Table 3**, I present the regression estimates of Equation 1. Column (1) depicts the findings for only the two shock types without any controls and Column (2) includes the controls, excluding household fixed effects. It is only in case of the income shock that a precise positive effect is found when controls are included (in Column (2)). Columns (3)-(5) include the household fixed effects, but each column differs in the degree of time-varying controls.

Given the panel structure of data and the possibility to have unobserved, household-specific time-invariant confounders, it is only logical to put emphasis on the models that include household fixed effects. As discussed earlier, a

portedly, from 2009-2012 paved roads increased to over 3,500 kilometres with 1,500 kilometres of major roads under construction during that time. In 2013, construction of over 1,000 kilometres of roads started. So, the new road networks had quite an impact on access to all amenities in life.

household fixed effect regression, as opposed to a pooled regression, captures all the unobserved heterogeneity across the households, which could confound the estimated effects. To be precise, if the unobserved characteristics specific to households are correlated with the shocks, then a pooled OLS estimate will be inconsistent. In that regard, a household fixed effects regression will render more reliable estimates.

Now, in Column (3) are the estimates of the model where I do not include the indicators of health supply and distances to other facilities. We see that experience of a health shock in the household in the last six months increases the probability of VAS intake by the child during the same time interval by 14.4 percentage points (pp.) ($p = 0.067$), while an income shock has an effect of 9.4 pp. ($p = 0.090$) during the same time interval. Compared to the overall sample mean of 73% VAS intake, these increases are about 19.7% due to health shock and 12.9% due to income shock. Column (4) shows that when the health supply indicators are included as controls, the two estimates remain unchanged. With further inclusion of controls on distances to the other facilities, the income shock estimate loses its precision ($p = 0.122$) (Column (5)). Note that the income measure is given by drought or flood, which is likely to affect areas within a few kilometres radius alike. Therefore, when I constrict the geographical boundaries by including the distance controls, it is plausible that not too much variation arises in the income shock incidence on households within these cluster areas.

Table 3: Effect of the shocks on VAS intake by child in the household

Dependent variable:	If the child received a VAS dose in last 6 months				
	(1)	(2)	(3)	(4)	(5)
Health Shock	0.026	0.051	0.144*	0.145*	0.147*
	(0.038)	(0.038)	(0.079)	(0.078)	(0.081)
Income Shock	0.018	0.050**	0.094*	0.091*	0.088
	(0.024)	(0.024)	(0.056)	(0.056)	(0.057)
Controls[a]	No	Yes	Yes	Yes	Yes
Health supply covariates	No	Yes	No	Yes	Yes
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	2109	2109	2109	2109	2109
No.of households	1592	1592	1592	1592	1592
R-sq.	0.001	0.095	0.074	0.081	0.131

Notes: (1) Dep.var. dummy=1 if the child under two years in the hh. received a VAS dose in last 6 months. Indep.var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(2)** [a] includes household level controls - log of total value of household assets, number of children under five, number of permanent members, if the family had moved in recent past, if interviewed in a rainy season, if lived in a flood-prone region; and individual level controls as neonatal care received (breastfed at birth), presence of mother, presence of father, education of mother and father and if attained 6 months of age 6 months ago. **(3)** Health supply covariates include - if the nearest health facility offered general outpatient care, if it offered immunisation service, if a major limitation was its distance, and if a major limitation was lack of skilled staff. **(4)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(5)** Standard errors in parentheses, clustered at household level. **(6)** The overall mean of VAS intake is 0.73. **(7)** The R-squared for the household fixed effects models in Columns (3)-(5) is the within-R-squared. **(8)** * indicates significance at 10%. **(9)** Refer to **Appendix 2 Table B3** for an extended version of this table.

5.2 Investigating the channels of effects

One plausible channel of the effect of the shocks on VAS intake (which is available free-of-charge) is the time spent away from the labour market by the household adults. One could reasonably expect that the total labour hours of the household would decrease under a health shock, due to hours of illness spent at home or due to seeking remedial healthcare. Under such circumstances, the opportunity cost of getting some preventive healthcare for the child is low, e.g. while visiting the health-centre for remedial purposes, the child is carried along to get immunisation doses. Dillon (2013), for example, find evidence from northern Mali that a morbidity shock in the household increases the time spent in childcare. However, when it comes to an income shock, low-income households would likely look for ways to smooth the shock, and as a result, the average time spent in the labour market would probably increase. On the contrary, the result obtained in Table 3, that VAS intake increases with the incidence of an income shock, hints towards a different possibility and thus calls for further investigation.

To examine how the labour participation reacts to the incidence of shocks, I regress the average weeks spent in their main activity in the labour market by a permanent household member on the two shock types, after controlling for the usual covariates.[20]

In **Table 4**, Column (1) shows the average effects of the two shocks across households without any controls; only the income shock has a significant positive effect. With controls, Column (2) shows that this effect of an income shock holds, and a health shock has a negative and statistically significant effect on the average labour hours across households. On including household fixed effects in Columns (3)-(5), I find similar results. Column (3) shows that with the experience of a health shock in the household in the last six months, the average labour weeks spent by a permanent household member decrease by 6.3 units ($p = 0.007$). Similar findings are seen in Column (4) and Column (5) with more controls. This result supports the argument that as productivity is negatively affected by illness, the opportunity cost of the 'forced' time away from labour market activities due to illness should get lower. So, during this time, taking the child for preventive healthcare is not costly in this regard. Also, it becomes easier for the household members to take the child(ren) along for VAS doses when they visit the health-centre for remedial care.[21] Note that this finding on

[20] According to the survey questionnaire, the *main activity* of an individual is the one in which s/he spent the most time in labour force participation in the previous year. This activity could be her/his primary job (sometimes also a second job or a third job), and it could be any income-generating (in cash/kind) work in the agricultural or non-agricultural area, paid domestic work, work in own/household business (sometimes even without being paid), work with/without pay as apprentices, work in a household's farm (e.g. tending crops, feeding animals).

[21] One could be curious about what happens when the health shock in the household is due to the illness of the child itself. Then it can be argued that an ailing child in the household implies that the

Table 4: Effect of the shocks on the household's labour force participation

Dependent variable:	Avg. weeks spent in the labour force by a HH member				
	(1)	(2)	(3)	(4)	(5)
Health Shock	-0.113	-2.127**	-6.330***	-5.895***	-4.473**
	(1.229)	(0.911)	(2.333)	(2.464)	(1.985)
Income Shock	3.460***	1.133*	3.957**	3.860**	4.368***
	(0.962)	(0.685)	(1.766)	(1.736)	(1.793)
Controls[a]	No	Yes	Yes	Yes	Yes
Health supply covariates	No	Yes	No	Yes	Yes
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	1518	1518	1518	1518	1518
No.of households	1255	1255	1255	1255	1255
R-sq.	0.011	0.542	0.650	0.653	0.716

Notes: (1) Dep.var. is the number of weeks a permanent hh.member spends in the labour market on average. Indep.var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(2)** This analysis involves only the survey years 2010, 2011 and 2013 since the labour participation hours or weeks cannot be calculated for 2009 due to lack of data. **(3)** [a] includes household level controls - log of total value of household assets, number of permanent members, number of members in their prime age, if the family had moved in recent past, if interviewed in a rainy season, if lived in a flood-prone region. **(4)** Health supply covariates include - if the nearest health facility offered general outpatient care, if it offered immunisation service, if a major limitation was its distance, and if a major limitation was lack of skilled staff. **(5)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(6)** Standard errors in parentheses, clustered at household level. **(7)** The overall mean of household labour weeks is 12.61. **(8)** The R-squared for the household fixed effects models in Columns (3)-(5) is the within-R-squared. **(9)** ***,**,* indicate significance at 1%, 5% and 10% respectively. **(10)** Refer to **Appendix 2 Table B4** for an extended version of this table.

the labour force participation also holds for a bigger sample of households with children up to the age of five years, and is not just any selection effect. Although data limitation hinders the validation of whether the number of visits to health facilities by the household members also increases or not when struck by a health shock, one interesting finding from the coefficients of the health supply covariates can be used to support the mechanism to some extent. That finding is, a stronger positive effect of the availability of general outpatient care at the nearest health facility on VAS uptake (in Column (4) of **Appendix 2 Table B3**). This result hints toward *economies of scale* approach, i.e. while visiting the health centre for remedial purposes, the additional cost of investing in some preventive healthcare alongside is relatively low. In a similar vein, Goldman and Grossman (1978) point out that the time price is a fixed cost since it does not depend on the number of services received per visit. They show that adults with higher fixed cost obtain more healthcare services per visit. This reasoning rightly explains my finding.

In **Table 4**, the coefficient of an income shock reveals a strong income effect, with the average labour hours increasing by almost 4 units ($p = 0.025$) (Column (3)) from the mean of about 12 weeks in a household facing no income shock. This result is robust to controls on the supply-side variables and distance con-

closest caregiver is 'forced' to stay away from the labour market. Moreover, if the child is taken to the health-centre for remedial care, it is more probable that its vaccination doses are updated too. I further discuss this concern in Section 5.3.4.

trols (Columns (4) and (5)). However, this does not reconcile with the main finding of increased VAS intake with income shock in Table 3 Columns (3)-(5).

Table 5: Effect of the shocks on labour force participation in households with different wealth levels

Dependent variable:	Avg. weeks spent in the labour force by a HH member				
	(1)	(2)	(3)	(4)	(5)
log of total assets' value	-0.019	-0.051	-0.029	-0.041	0.157
	(0.216)	(0.191)	(0.664)	(0.679)	(0.710)
Health shock	-3.271	-13.015**	-14.932	-16.229	-24.805*
	(10.122)	(6.408)	(12.227)	(13.078)	(13.889)
Health shock X log of total assets' value	0.208	0.728*	0.622	0.743	1.423
	(0.689)	(0.421)	(0.862)	(0.925)	(0.954)
Income shock	-4.84	-1.368	34.612*	35.089*	32.473*
	(8.795)	(6.921)	(19.130)	(19.305)	(19.574)
Income shock X log of total assets' value	0.563	0.168	-2.092*	-2.133*	-1.921
	(0.596)	(0.464)	(1.261)	(1.278)	(1.301)
Controls[a]	No	Yes	Yes	Yes	Yes
Health supply covariates	No	Yes	No	Yes	Yes
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	1518	1518	1518	1518	1518
No.of households	1255	1255	1255	1255	1255
R-sq.	0.012	0.542	0.657	0.660	0.722

Notes: **(1)** Dep.var. is the number of weeks a permanent hh.member spends in the labour market on average. Indep.var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(2)** The interaction terms give the differences in slope of *log of total assets* when under respective shocks to that under no shock. **(3)** This analysis involves only the survey years 2010, 2011 and 2013 since the labour participation hours or weeks cannot be calculated for 2009 due to lack of data. **(4)** [a] includes household level controls - log of total value of household assets, number of permanent members, number of members in their prime age, if the family had moved in recent past, if interviewed in a rainy season, if lived in a flood-prone region. **(5)** Health supply covariates include - if the nearest health facility offered general outpatient care, if it offered immunisation service, if a major limitation was its distance, and if a major limitation was lack of skilled staff. **(6)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(7)** Standard errors in parentheses, clustered at household level. **(8)** The overall mean of household labour weeks is 12.61. **(9)** The R-squared for the household fixed effects models in Columns (3)-(5) is the within-R-squared. **(10)** **,* indicate significance at 5% and 10% respectively.

A further investigation by interacting the shock with the logarithm of household assets' value, provides some explanation to reconcile the two findings related to an income shock in Tables 3 and 4. This specification would help understand how households with different wealth levels cope with the shock. Focussing on the household fixed effects estimation in Columns (3)-(5) in **Table 5**, we see that in case of an income shock, a relatively wealthy household puts lesser time on average in the labour market. The levels of significance on the interaction coefficient are 9.7% and 9.5% in Columns (3) and (4) in the basic model with household fixed effects and the one which further controls for health supply-related variables. In Column (5), however, where the distances to other facilities are included, the coefficient of interaction between the income shock and household wealth becomes less precise (significant at 14%) even though the magnitude is similar. The coefficient of the direct effect of an income shock is positive and statistically significant at 10% in all the three columns. Now, the

interaction coefficient in say, Column (3) is interpreted as follows: in the time of an income shock, with a 10% increase in the household asset value (mean log value being = 12.61), the difference in the expected mean time spent in labour market decreases by $2.09^{*}log(1.10) \approx 0.09$ week $\equiv$ 14.5 hours.

A decrease in the slope of wealth level during an income shock implies that the more wealth a household has, the less is the urgency to put extra working hours in the labour market. Such an increase in leisure hours is justified if the household draws down assets, or borrows credit, or receives transfers to insure away the negative income shock, and also, finds it cheaper to substitute time away from the labour market. For example, previous literature already showcases that households offset transitory income shocks by using asset-holdings (either as buffer or collateral for credit) (Deaton, 1992; Beegle et al., 2006). The findings of Table 5 hold even for a bigger sample of households with children up to five.[22]

Furthermore, to understand the direct effect of this mediating factor, i.e. the time spent in the labour market during shock or otherwise on VAS uptake, I carry out a regression by interacting the time spent in the labour market with each of the shocks and examine the effect of this interaction on VAS uptake. Columns (4) and (5) in **Appendix 1 Table A1**, which include the most number of controls along with household fixed effects specification, show that the main effects of the shocks on VAS intake are positive (as large as about 20 pp.) and statistically significant. However, the interaction coefficients with the average weeks spent at the labour market have a negative effect of size 0.4 pp. for the health shock and 0.1 pp. for the income shock, and are imprecise. The imprecise interaction coefficients between the shocks and the mean labour hours of households could also be due to low statistical power since not all the survey waves have information on the time spent in the labour market.

5.3 Robustness checks

5.3.1 Attrition and other sample issues

One potential concern regarding the dataset could be that of attrition. More than 90% of the households interviewed in the first wave were retained after the end of the third wave. However, whether the shock significantly affects the probability of exiting the sample, would be ideal for checking. I regress the probability of exiting the sample on the incidence of a shock in the household in the previous wave, and I find no significant effect. In **Table A2** in **Appendix 1**, I report a summary of the findings.

[22] Note, however, that a similar examination of the effects of shocks on households at different wealth levels on the uptake of VAS does not provide any precise estimates to directly confirm that a relatively wealthy household is more prone to take up VAS for the child during an income shock.

Secondly, since the sample of the fourth wave was 'refreshed' by dropping a substantial number of households and adding new ones, one could argue that the sample loses its originality by the end of the four waves. Therefore, I re-do the analysis with a sample which omits households that were the newest inclusion in the last wave of the survey. The findings as shown in **Appendix 1 Table A3** remain similar to the main results obtained in Table 3.

5.3.2 (In)separability of health and income shock

The relation between health and income is often interconnected (Weil, 2014). Thus, the possibility of a shock to one of being correlated with a shock to the other is difficult to preclude. In this section, I argue with the help of some robustness checks that the two channels of shocks in this study are indeed mutually exclusive.

First, I examine the results with each shock in separate regressions, given that there could be spillovers between the shocks. That is, for example, household members could be ill due to adverse health effects of excessive rainfall. A comparison of the direct effects of the shocks in two separate regressions given in Panels A and B of **Appendix 1 Table A4**, shows that the coefficients do not change much with respect to the findings of the main specification. The estimates in the separate models are, however, relatively more precise (coefficients of the health shock are statistically significant at 5% in all household fixed effects models and that of the income shock are statistically significant at 6%).

Next, I also vary the study definitions to some extent to minimise any confoundment between the two shock types. The indicator variable for a health shock which has been used in the study so far takes the value 1 if any household member became ill or died in the last six months from the survey interview date, and this includes even the primary income earner of the household. However, one could argue that a health shock to the primary income earner potentially interferes as an income shock to the household. Hence, I conduct a robustness check by omitting the health shock of the primary income earner from the measure. Now, the dummy variable for *HealthShock* takes the value 1 if any household member except the primary income earner was affected (i.e. got ill or died) in the past six months.

Table A5 Panel A in **Appendix 1** summarises the results. In all the specifications, we can see that the main finding on VAS intake does not change in any meaningful way. The magnitude of the positive effect of a health shock remains similar across all the specifications that include household fixed effects, i.e. about 15 pp. (Columns (3)-(5)). Only in Column (5), the coefficient of *HealthShock* loses precision (significant at 12.6%), but this could be due to the fact that the fraction of households suffering from the health shock reduces (from 7% to about 4%) as I revise the dummy variable. (Now, in relation to the health shock measure, it could be argued that *death* does not qualify as a health

shock under all circumstances; for example, death due to old age or a road accident. Therefore, I conduct the same analysis by considering only the illness of any household member except the primary income earner, as an indicator of a health shock. The findings are summarised in **Table A5 Panel B**. Now the fraction of households suffering from health shock reduces even further, and that is why the precision degrades. The magnitude of the positive effect of the health shock reduces a little compared to Panel A results.

Despite the robustness measures in Table A5, a caveat remains. Other household members are likely to participate in the labour force too and thus contribute to the household's income pool. As a result, shocks to their health could also affect the household's income. An easy solution to this problem could be examining if the experience of health shock by the household in the last six months has any effect on the household's wealth. By examining this, I do not find any statistically significant effect while using the original proxy of health shock. In contrast, the effect of an income shock in the last six months on the log of overall asset holdings of the household is negative and statistically significant. It implies that the health shock measure used here does not confound as an income shock. Results are reported in **Table A6**.

While these above robustness tests still do not foreclose the possibility of an income shock causing a health shock, one can (optimistically) deduce from the positive effect of an income shock on the average labour hours of the household, that this path is unlikely, at least in this sample.

5.3.3 Effect on other child-health measures

As a further interest, I examine how the shocks affect other health measures of the children in the household. Through the following exercise, I also intend to find the shocks' effects on children up to five.[23] Some standard health measures for children under five years, recommended by the World Health Organization, are anthropometric measures such as *weight-for-height, height-for-age* and *weight-for-age*.[24] With the given sample, I measure the *weight-for-height* and the related *z-score* for children up to five years in the household. This anthropometric measure reflects current body weight relative to the current height of a child of any age. A low measure that indicates malnutrition can be due to thinness (not due to a pathological process) or wasting (due to acute starvation or disease). On the other hand, a high measure indicates obesity. The measure is recorded for the household children during the time of the survey.

By using the same identification strategy used for VAS, I find that with household fixed effects, neither shock has any statistically significant effect on the

[23] Note that the other health(care) measures such as vaccinations on measles, DPT-3 or breastfeeding are mostly available for children under two, with very sparsely available data points for children between 2-5 years.

[24] For children under two years, length replaces height in these measures.

z-score of *weight-for-height*, but the estimates suggest that the income shock has a negative effect of about 4 pp (**Table A7**).[25] This finding is different from that found for VAS intake but is quite rational (though only suggestive). While receiving VAS doses can be thought of as a function of time outside the labour market for the household adults, the *weight-for-height* measure is a direct function of consumption. It is plausible to argue that when a negative income shock hits, the household consumption is directly affected, which in turn takes a toll on the nutritional intake of the children. On the contrary, VAS intake could still increase if the time outside the labour market is utilised to get the doses. Similar evidence is found for another anthropometric measure *weight-for-age*.

5.3.4 Other sensitivity checks

In this section, I discuss a variety of robustness examinations. Firstly, I investigate if the main effects of the shock types differ across different age brackets of children under two. The only noteworthy finding is: the main effect on VAS intake by the children who were between 6-9 months during the shock window, is significantly higher (about 8-10 pp., significant below 10%) than among the other children under two and this effect remains the same irrespective of shock incidence. This finding is justified, as the child who is between 6-9 months old is more likely to receive VAS because at this age it is eligible to get other vaccinations too (e.g. for measles), but after that age, only a few other vaccines are required. Therefore, it potentially affects the frequency at which the caregiver in the household visits the health-centre or the motivation/awareness of household adults to take the child for VAS. Results are reported in **Appendix 2 Table B5**.

Next moving on to a discussion on the choice of income shock proxy, here I have included the incidence of flood or drought as a measure of the shock. Another close candidate for the shock could be erosion as well. However, it is left out intentionally, as often soil erosion is caused by agricultural practices such as overgrazing of cattle, over-cropping and deforestation. So, this anthropogenic nature of erosion could affect the exogeneity of soil erosion as a proxy for income shock.[26] Furthermore, some other potential candidates for an income shock measure were available in the survey, such as agricultural crop loss due to disease, livestock loss due to disease, the exorbitant price of agricultural inputs and unusually low price of agricultural outputs. However, it makes more sense to choose flood and drought instead, because these natural shocks are often precursors of the others.

Here, I also discuss another issue related to the health shock measure. From the survey, it is not possible to identify if a child who is eligible for VAS itself suffered from illness in the past six months. It could be so that the driver

[25] The findings also hold when the shock window is extended to past one year.

[26] Also, none of the households in my sample reported having suffered from erosion in the past six months. Therefore, the inclusion of erosion besides flood and drought, would not affect my results.

of the health shock in the household is the illness of the child itself. So far, with household fixed effects specification, the reported effect of a health shock on VAS intake is the expected mean for the eligible children within one household. However, this average estimate for the children in the household would be inaccurate if there are several omitted factors specific to the ailing child who could be triggering the health shock in the first place. One solution would be to conduct a child fixed effects analysis that essentially utilises the variation within a single child over multiple survey waves. The idea of using this estimator is that the household to which the child belongs, could face a health shock (even arising from the illness of the eligible child itself) in one survey wave and could face no health shock in another survey wave, however, a child fixed effects will absorb all the observables and unobservables intrinsic to that particular child and will provide an estimate with a minimised error. Implementing a child fixed effects analysis, however, suffers a major drawback here. In the presence of only a few observations[27] and potentially too little within-group variation, the assessment of any effect could be difficult. Nevertheless, by using a specification with child fixed effects and controlling for some time-varying child-specific characteristics, I find some evidence that a child is more likely to get VAS when the household to which it belongs experiences a health and income shock compared to when it does not. Results are reported in **Appendix 2 Table B6**.

On further introspection, I think it is possible to estimate the shock's effect imperfectly if we only consider whether it occurred or not. In the specification given by Equation 1, I have defined *HealthShock* and *IncomeShock* as an experience of a health or income shock by the household in the past six months before the interview. It is the same time interval when the household member(s) should have taken the eligible child(ren) in the household for VAS. However, according to my definition, the onset of this shock could have occurred earlier and then continued into the last six months. Now suppose, a household faced the shock that had started four months before the last six months and continued for an additional month into these last six months of our interest. Let, a second household faced a shock which had started a month before the last six months but continued for three months into these last six months. Clearly, the relative suffering during the last six months by the second household is more than that experienced by the first one. To account for this difference, I control for a measure of 'relative intensity' of shock in the past six months. It is given by the ratio of the number of months of the shock suffered in the last six months to the number of months of suffering before that due to the same.[28] In the findings,

[27] Only 71 children between 12-24 months appear in multiple waves of the survey (however, not more than twice). Despite having children of 12-24 months in the analysis and the survey waves being about a year apart, these children appear twice because some households were interviewed at the latter part of say, wave 1 (2009-2010) and they were revisited for wave 2 in the early part of 2010-2011.

[28] Naturally, this measure only works for those households in which the shock started earlier than the last six months from the interview. Also note, this measure is capable of taking into account the

the effects of shocks do not change much in magnitude; however, the precision of most estimates degrades to around 11-12% level of significance. The main and interaction effects of the intensity measures are statistically insignificant. Results are reported in **Appendix 2 Table B7**.

Finally, another concern regarding the use of self-reported shocks, especially for health, is a potential selection bias resulting from the fact that the poor are less likely to report being ill. Often patients' internal assessment may be seriously limited by their social experience. Sen (2002) argues that in a community fraught with many diseases and few medical facilities, one is often inclined to deem certain symptoms as "normal" when they are indeed clinically preventable, and this is especially the case for the poor. Significant under-reporting of subjective health status (compared to objective measures) has been found among low socioeconomic groups in developing countries (Vellakkal et al., 2013; Onur and Velamuri, 2018). To verify whether the sample in this study suffers from such reporting bias, I test if there is any significant correlation between the wealth level and the incidence of shock due to self-reported illness. As reported in **Appendix 2 Table B8**, there is no significant correlation, and thus I discard the possibility of any significant selection bias in this sample.

6 CONCLUSION

This paper aims to empirically examine how low-income households in developing countries trade off investment in their children's preventive healthcare during idiosyncratic shocks when resources get even more limited. Starting with hypotheses that a negative income shock possibly has a substantial income effect on healthcare, especially preventive healthcare, and that an adverse health shock may bring awareness and increase marginal utility from health stock, I examine the effects of both shocks on preventive healthcare utilisation by more than 1500 nationally and regionally representative households present in the four waves of Ugandan National Panel Survey. In Uganda, immunisations to small children are publicly provided free-of-cost at public health facilities in the community. So, the only costs that a household could incur while taking the children to get immunised are the indirect cost of transportation and/or the opportunity cost of time spent in accessing that service instead of spending on income-generating activities.

By using the incidence of flood or drought as a proxy for negative income

start date of the shock and then accordingly weighing the suffering during the last six months. In this regard, it qualifies as a better measure of the intensity of a shock compared to a measure such total number of months of shock.

shock, and illness of any household member as an indicator for negative health shock, I examine how the receipt of VAS doses by children under two years, as a part of their immunisation schedule, is affected. I find that both health and income shocks have positive effects on VAS take-up, the result of the income shock being contrary to what I expected. Further investigation of the mechanisms shows that the effect of health shock results from the increase in the average time outside the labour market by the members of a shock-hit household. It could mean that the inability to be able to work due to illness reduces the opportunity cost of the time away from the labour market which the household adults use in seeking remedial care and other health-promoting activities for their children. This argument should also hold if a child or an elderly member (who is not a labour force participant) in the household is ill; then some working adult member is 'forced' to stay away from the labour market to take care of the former. In case of income shock, though the average effect on time spent in the labour market is positive, an interaction with the wealth level of the household shows relatively wealthy households allocating lesser time on average in the labour market during an income shock. This finding hints toward the possibility that an income shock in terms of flood or drought reduces the opportunity cost of time away from work for at least the relatively wealthy who probably are capable of using buffer stocks. As a result, the increase in VAS uptake due to income shock could be driven by them substituting their time with preventive healthcare instead of labour.

To summarise, the findings in this study imply the importance of time in child health development, as has been previously shown by Miller and Urdinola (2010) in a developing country setting. Nevertheless, it is worth mentioning here that the results obtained in this study are not very strong in terms of statistical significance; therefore, more research in similar socio-economic settings is necessary before reaching a firm conclusion.[29]

Furthermore, some caveats on the interpretation of the mechanism remain and call for further data and research in a similar context. For example, it is crucial to be able to distinguish if the increase in VAS intake during income shock is caused only through leisure hours? Or, does being in an adverse weather shock such as drought or flood makes the adults more aware of the health of their children? As mentioned earlier, other potential agriculture-based proxies for an income shock were available in the survey, but those provide too little number of households exposed to the shock. That would cause an estimation challenge.

Another deeper insight that this paper fails to provide is the household dynamics. The otherwise intriguing results of the paper give rise to further ques-

[29] To clarify my statement further, the primacy of time in child healthcare is already established in the literature (Grossman, 1972; Gronau, 1977; Vistnes and Hamilton, 1995), but the channels that I study in this paper to reach that conclusion on the primacy of time are not statistically precise. Therefore, the channels need further validation.

tions: for example, how is the work division in the household - are the males in the household responsible for all the income-generating work and the women for child-rearing? If so, then why would a change in the time away from the labour market necessarily affect childcare? Also, how much are the elder children in the household involved in the home production? Often multiple nuclear families live in the same household in this context (and that is why the sample often consists of multiple children with different parents across multiple waves within households), then are there multiple primary income earners under the same roof? The most that can be answered by exhausting this survey and consulting related literature is that most Ugandan households are engaged in agricultural farming in their own cultivation lands, and the household members, especially women and adolescents, are the main additional labour input providers. So to reconcile the findings of this paper, during a health shock, the women are likely to divert time away from those agricultural tasks to look after the ailing. During an income shock, the household members probably seek out other income-generating activities, but the wealthier the household is the lesser the need for everyone in the household to search work for income smoothing. For instance, the women and adolescents who were working in the household's lands can then utilise the 'leisure' to engage in other time-intensive activities. More research is needed to shed light into these gender dynamics within households in times of shock.

All things considered, it can be concluded that in this paper we see an example where even though preventive healthcare for children is available for free, it is the opportunity cost of accessing that which is high. Taking the child to get immunised or investing time in other preventive healthcare practices is something that the adults do during the time outside their labour hours. It indicates that simply providing preventive healthcare services for free is necessarily not enough. Parents and other caregivers in the household should be incentivised through the right channel so that they can easily engage in preventive healthcare activities. However, incentives like a paid day off work would not be meaningful in the framework of Uganda and other similar countries where the informal labour market has a considerable share. Instead, innovative nudges and incentives could work. For example, Banerjee et al. (2010) provide evidence from a randomised controlled trial in India, where they found that setting up of immunisation camps increased immunisation rates. However, it was more effective when the parents or caregivers were offered an incentive of receiving a kilo of lentils per vaccination. This incentive helped offset the opportunity cost of taking a child to get immunised and thus, was successful.

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1 APPENDIX

Table A1: Effect of the shocks on VAS intake by child in households with different labour participation

Dependent variable:	If the child received a VAS dose in last 6 months				
	(1)	(2)	(3)	(4)	(5)
avg. week in labour force	-0.002**	0.002*	0.002	0.002	0.004
	(0.001)	(0.001)	(0.004)	(0.004)	(0.003)
Health shock	0.047	0.056	0.170	0.178	0.213*
	(0.055)	(0.057)	(0.149)	(0.150)	(0.125)
Health shock X avg. week in labour force	-0.001	0.002	0.001	-0.002	-0.004
	(0.004)	(0.005)	(0.011)	(0.011)	(0.010)
Income shock	0.012	0.043	0.162	0.178*	0.230**
	(0.034)	(0.033)	(0.106)	(0.107)	(0.098)
Income shock X avg. week in labour force	-0.000	-0.001	0.002	0.000	-0.001
	(0.002)	(0.002)	(0.005)	(0.005)	(0.005)
Controls[a]	No	Yes	Yes	Yes	Yes
Health supply covariates	No	Yes	No	Yes	Yes
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	1511	1511	1511	1511	1511
No.of households	1251	1251	1251	1251	1251
R-sq.	0.005	0.112	0.151	0.161	0.308

Notes: (1) Dep.var. dummy=1 if the child under two years in the hh. received a VAS dose in last 6 months. Indep.var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. The interaction terms give the differences in slope of *average week in the labour force by a hh.member* when under respective shocks to that under no shock. **(2)** This analysis involves only the survey years 2010, 2011 and 2013 since the labour participation hours or weeks cannot be calculated for 2009 due to lack of data. **(3)** [a] includes household level controls - log of total value of household assets, number of children under five, number of permanent members, if the family had moved in recent past, if interviewed in a rainy season, if lived in a flood-prone region; and individual level controls as neonatal care received (breastfed at birth), presence of mother, presence of father, education of mother and father and if attained 6 months of age 6 months ago. **(4)** Health supply covariates include - if the nearest health facility offered general outpatient care, if it offered immunisation service, if a major limitation was its distance, and if a major limitation was lack of skilled staff. **(5)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(6)** Standard errors in parentheses, clustered at household level. **(7)** The overall mean of VAS intake is 0.73. **(8)** The R-squared for the household fixed effects models in Columns (3)-(5) is the within-R-squared. **(9)** **,* indicates significance at 5% and 10%.

Table A2: Effect of shock in one survey wave on the probability of attrition at the end of the wave

	(1)
Health shock in last one year	-0.004
	(0.014)
Income shock in last one year	0.002
	(0.009)
Households FE	Yes
Survey wave FE	Yes
No. of obs.	11516
No.of households	4520
R-sq. within	0.000

Notes: (1) The sample includes all households. Dependent variable dummy=1 if the household in wave t could be tracked in survey wave $t+1$. Indep.var. *Health Shock* dummy=1 in wave t if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 in wave t if the hh. faced flood or drought/irregular rains in last 6 months. **(2)** number of other shocks experienced in in wave t by the household in the past year is controlled for. **(3)** SE in parentheses, clustered at household level.

Table A3: Effect of the shocks on VAS intake by child in the household - checking with only the first three survey waves

Dependent variable:	If the child received a VAS dose in last 6 months				
	(1)	(2)	(3)	(4)	(5)
Health shock	0.046	0.066*	0.145*	0.146*	0.149*
	(0.040)	(0.040)	(0.079)	(0.078)	(0.081)
Income shock	0.019	0.053**	0.094*	0.091*	0.088
	(0.026)	(0.026)	(0.056)	(0.056)	(0.057)
Controls[a]	No	Yes	Yes	Yes	Yes
Health supply covariates	No	Yes	No	Yes	Yes
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	1864	1864	1864	1864	1864
No.of households	1357	1357	1357	1357	1357
R-sq.	0.001	0.087	0.075	0.082	0.132

Notes: (1) The regression includes households with children of 12-24 months in the first three survey waves only. **(2)** Dep.var. dummy=1 if the child under two years in the hh. received a VAS dose in last 6 months. Indep.var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(3)** [a] includes household level controls - log of total value of household assets, number of children under five, number of permanent members, if the family had moved in recent past, if interviewed in a rainy season, if lived in a flood-prone region; and individual level controls as neonatal care received (breastfed at birth), presence of mother, presence of father, education of mother and father and if attained 6 months of age 6 months ago. **(4)** Health supply covariates include - if the nearest health facility offered general outpatient care, if it offered immunisation service, if a major limitation was its distance, and if a major limitation was lack of skilled staff. **(5)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(6)** Standard errors in parentheses, clustered at household level. **(7)** The overall mean of VAS intake is 0.69 in the first three survey waves. **(8)** The R-squared for the household fixed effects models in Columns (3)-(5) is the within-R-squared. **(9)** **,* indicate significance at 5 % and 10%.

Table A4: Effects of the two shocks in separate regressions on VAS intake of child in the household

Dependent variable:	If the child received a VAS dose in last 6 months				
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Health Shock	0.027	0.056	0.151**	0.152**	0.150**
	(0.038)	(0.038)	(0.079)	(0.079)	(0.081)
Controls[a]	No	Yes	Yes	Yes	Yes
Health supply covariates	No	Yes	No	Yes	Yes
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	2109	2109	2109	2109	2109
No.of households	1592	1592	1592	1592	1592
R-sq.	0.001	0.093	0.067	0.075	0.125
Panel B:					
Income Shock	0.019	0.052**	0.098*	0.096*	0.090
	(0.024)	(0.023)	(0.056)	(0.056)	(0.058)
Controls[a]	No	Yes	Yes	Yes	Yes
Health supply covariates	No	Yes	No	Yes	Yes
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	2109	2109	2109	2109	2109
No.of households	1592	1592	1592	1592	1592
R-sq.	0.001	0.094	0.068	0.076	0.125

Notes: (1) Panel A shows the estimates of the regression with only *Health Shock* as the predictor and Panel B shows the estimates of the regression with only *Income Shock* as the predictor. **(2)** Dep.var. dummy=1 if the child under two years in the hh. received a VAS dose in last 6 months. Indep.var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(3)** [a] includes household level controls - log of total value of household assets, number of children under five, number of permanent members, if the family had moved in recent past, if interviewed in a rainy season, if lived in a flood-prone region; and individual level controls as neonatal care received (breastfed at birth), presence of mother, presence of father, education of mother and father and if attained 6 months of age 6 months ago. **(4)** Health supply covariates include - if the nearest health facility offered general outpatient care, if it offered immunisation service, if a major limitation was its distance, and if a major limitation was lack of skilled staff. **(5)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(6)** Standard errors in parentheses, clustered at household level. **(7)** The overall mean of VAS intake is 0.73. **(8)** The R-squared for the household fixed effects models in Columns (3)-(5) is the within-R-squared. **(9)** **,* indicate significance at 5 % and 10%.

Table A5: Effect of health shock of any household member except the main income earner on VAS intake of child in the household

Dependent variable:	If the child received a VAS dose in last 6 months				
	(1)	(2)	(3)	(4)	(5)
Panel A:					
Health Shock	0.063	0.079*	0.157*	0.145*	0.144
	(0.044)	(0.044)	(0.089)	(0.086)	(0.094)
Income Shock	0.018	0.051**	0.095*	0.093*	0.090
	(0.024)	(0.024)	(0.056)	(0.056)	(0.057)
Controls[a]	No	Yes	Yes	Yes	Yes
Health supply covariates	No	Yes	No	Yes	Yes
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	2109	2109	2109	2109	2109
No.of households	1592	1592	1592	1592	1592
R-sq.	0.001	0.096	0.072	0.079	0.128
Panel B:					
Health Shock	0.046	0.068	0.102	0.086	0.076
	(0.048)	(0.049)	(0.086)	(0.082)	(0.092)
Income Shock	0.018	0.051**	0.096*	0.093*	0.089
	(0.024)	(0.024)	(0.056)	(0.056)	(0.057)
Controls[a]	No	Yes	Yes	Yes	Yes
Health supply covariates	No	Yes	No	Yes	Yes
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	2109	2109	2109	2109	2109
No.of households	1592	1592	1592	1592	1592
R-sq.	0.001	0.095	0.070	0.077	0.126

Notes: (1) Panel A reports the estimates of the regression where indep.var. *Health Shock* dummy=1 if any hh.member except the main earner was **ill or died** in last 6 months. Panel B reports the estimates of the regression where *Health Shock* dummy=1 if any hh.member except the main earner was **only ill** in last 6 months. **(2)** Dep.var. dummy=1 if the child under two years in the hh. received a VAS dose in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(3)** [a] includes household level controls - log of total value of household assets, number of children under five, number of permanent members, if the family had moved in recent past, if interviewed in a rainy season, if lived in a flood-prone region; and individual level controls as neonatal care received (breastfed at birth), presence of mother, presence of father, education of mother and father and if attained 6 months of age 6 months ago. **(4)** Health supply covariates include - if the nearest health facility offered general outpatient care, if it offered immunisation service, if a major limitation was its distance, and if a major limitation was lack of skilled staff. **(5)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(6)** Standard errors in parentheses, clustered at household level. **(7)** The overall mean of VAS intake is 0.73. **(8)** The R-squared for the household fixed effects models in Columns (3)-(5) is the within-R-squared. **(9)** **,* indicate significance at 5 % and 10%.

Table A6: Effect of the shocks in the last six months on household wealth

Dependent variable:	Logarithm of total wealth in the household				
	(1)	(2)	(3)	(4)	(5)
Health shock	0.100	0.065	0.060	0.059	0.047
	(0.102)	(0.099)	(0.079)	(0.080)	(0.079)
Income shock	-0.253***	-0.261***	-0.142***	-0.145***	-0.140***
	(0.059)	(0.056)	(0.046)	(0.046)	(0.046)
Controls[a]	No	Yes	Yes	Yes	Yes
Health supply covariates	No	Yes	No	Yes	Yes
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	9826	9826	9826	9826	9826
No.of households	3018	3018	3018	3018	3018
R-sq.	0.004	0.067	0.101	0.105	0.118

Notes: (1) The regression includes households with children under five. **(2)** Dep.var. is the logarithm of total assets' value of the household. Indep. var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(3)** [a] includes household level controls - number of permanent members, number of members in prime years of their age, if the family had moved in recent past, if interviewed in a rainy season, if lived in a flood-prone region. **(4)** Health supply covariates include - if the nearest health facility offered general outpatient care, if it offered immunisation service, if a major limitation was its distance, and if a major limitation was lack of skilled staff. **(5)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(6)** Standard errors in parentheses, clustered at household level. **(7)** The overall mean value of logarithm of household's assets is about 15 UGX. **(8)** The R-squared for the household fixed effects models in Columns (3)-(5) is the within-R-squared. **(9)** *** indicates significance at 1%;

Table A7: Effect of shocks in the past six months on other health measures of children under five in the household

Dependent variable:	z-score of weight-for-height of child under five				
	(1)	(2)	(3)	(4)	(5)
Health Shock	-0.056**	-0.045*	0.007	0.007	0.011
	(0.025)	(0.027)	(0.037)	(0.037)	(0.035)
Income Shock	-0.006	0.003	-0.044	-0.042	-0.038
	(0.030)	(0.029)	(0.038)	(0.038)	(0.036)
Controls[a]	No	Yes	Yes	Yes	Yes
Health supply covariates	No	Yes	No	Yes	Yes
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	7033	7033	7033	7033	7033
No.of households	2556	2556	2556	2556	2556
R-sq.	0.001	0.008	0.006	0.007	0.014

Notes: (1) The regression includes households with children under five. **(2)** Dep.var. is the z-score of *weight-for-height* of the child under five. Indep. var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(3)** [a] includes household level controls - log of total value of household assets, number of children under five years, number of permanent members, if the family had moved in recent past, if interviewed in a rainy season, if lived in a flood-prone region; and individual level controls as neonatal care received (breastfed at birth), presence of mother, presence of father, education of mother and father and if under two years of age interacted with whether received relevant vaccines on measles and DPT-3. **(4)** Health supply covariates include - if the nearest health facility offered general outpatient care, if it offered immunisation service, if a major limitation was its distance, and if a major limitation was lack of skilled staff. **(5)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(6)** Standard errors in parentheses, clustered at household level. **(7)** The overall mean value of *weight-for-height* z-score is 0.005. **(8)** The R-squared for the household fixed effects models in Columns (3)-(5) is the within-R-squared. **(9)** **, * indicate significance at 5% and 10%.

2 APPENDIX

Table B1: Summary statistics of households with and without shock

Variable	HH without shock		HH with shock		
	Mean	Std. Dev.	Mean	Std. Dev.	Diff.
	(1)	(2)	(3)	(4)	(5)
Age of household-head (yrs.)	40.88	13.22	42.96	13.79	-2.09***
Household-head ever attended school	0.89	0.32	0.82	0.39	0.07***
Household-head married	0.86	0.35	0.83	0.38	0.03***
Household members away due to work	0.07	0.28	0.07	0.28	0.00
Members living in household all year	4.08	2.32	4.43	2.40	-0.35***
Log total household assets' value (in UGX)	14.98	1.74	14.79	1.75	0.19***
Avg. no. of weeks in the labour market	12.55	14.00	13.98	14.53	-1.43***
No. of children under five in household	1.70	0.90	1.86	0.95	-0.15***
Avg. age of under-five children in household (mo.)	30.51	16.35	30.79	16.10	-0.27
Household facing a health-related shock	0	0	0.28	0.45	-0.28***
Household facing an income-related shock	0	0	0.79	0.41	-0.79***
Number of observations	4748		1677		

Notes: These statistics are obtained by comparing the households with at least one child under five. A household can appear multiple times across the waves 1-4 of UNPS. In Column (5), are the difference in means that are given in Columns (1) and (3). A t-test compares the means across the two types of households. The corresponding two-tailed p-values are reported. *** is $p < 0.001$.

Table B2: Summary statistics by panel waves

Variable	Mean	Std. Dev.
Household related variables:		
Household with main income source as agriculture	0.50	0.50
Household with main income source as subsistence farming	0.49	0.50
Household members away from household due to work	0.07	0.28
Household members present in household all year round	4.17	2.35
Number of children up to five years present in household	1.74	0.91
Average number of weeks in the labour market	12.89	14.14
Health Shock related variables:		
Households suffering from health shock in the last six mo.	0.07	0.26
Number of months suffered due to health shock	2.82	3.09
Income Shock related variables:		
Households suffering from income shock in the last six mo.	0.21	0.40
Number of months suffered due to income shock	3.63	2.04

Notes: (1) This table provides the mean over four waves of UNPS. The household and shock statistics are only for those households which had at least one child under five years in at least one wave, the number of such households being 3022. **(2)** The child statistics are not shown here as they are only available for children under two and thus can be found in Table 2.

Table B3: Effect of the shocks on VAS intake by child in the household

Dependent variable:	If the child received a VAS dose in last 6 months				
	(1)	(2)	(3)	(4)	(5)
Health Shock	0.026	0.051	0.144*	0.145*	0.147*
	(0.038)	(0.038)	(0.079)	(0.078)	(0.081)
Income Shock	0.018	0.050**	0.094*	0.091*	0.088
	(0.024)	(0.024)	(0.056)	(0.056)	(0.057)
Individual-specific controls:					
Breastfed at birth		0.056	0.008	0.008	0.008
		(0.052)	(0.13)	(0.13)	(0.120)
At 12th month now		-0.013	-0.032	-0.034	-0.037
		(0.033)	(0.060)	(0.059)	(0.062)
Mother in same household		0.052	0.092	0.096	0.125
		(0.042)	(0.090)	(0.087)	(0.091)
Father in same household		0.050**	0.103	0.091	0.087
		(0.026)	(0.066)	(0.067)	(0.067)
Mother with no education		-0.400***	0.061	0.072	0.097
		(0.151)	(0.238)	(0.234)	(0.226)
Father with no education		0.007	0.256	0.251	0.199
		(0.081)	(0.173)	(0.171)	(0.153)
HH-specific time-variant controls:					
Log of total household assets		0.008	0.017	0.019	0.014
		(0.006)	(0.18)	(0.018)	(0.019)
Total permanent members		-0.002	0.015	0.014	0.016
		(0.004)	(0.017)	(0.017)	(0.017)
Total children under five		-0.009	-0.003	-0.005	-0.014
		(0.011)	(0.031)	(0.030)	(0.030)
If relocated		-0.031	-0.061	-0.002	0.011
		(0.035)	(0.075)	(0.074)	(0.073)
If lives in flood-prone region		0.157***	0.905***	0.899***	0.960***
		(0.022)	(0.124)	(0.143)	(0.152)
If interviewed in rainy season		-0.038**	-0.033	-0.030	-0.025
		(0.019)	(0.040)	(0.041)	(0.041)
Health supply covariates:					
General outpatient care offered		-0.195***		0.397**	0.188
		(0.055)		(0.175)	(0.223)
Immunisation offered		-0.066		-0.316*	-0.156
		(0.048)		(0.166)	(0.209)
If too far		0.006		-0.045	-0.073
		(0.044)		(0.099)	(0.104)
If no skilled staff		-0.082		-0.039	-0.001
		(0.052)		(0.099)	(0.100)
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	2109	2109	2109	2109	2109
No.of households	1592	1592	1592	1592	1592
R-sq.	0.001	0.095	0.074	0.081	0.131

Notes: (1) Dep.var. dummy=1 if the child under two years in the hh. received a VAS dose in last 6 months. Indep.var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(2)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(3)** Standard errors in parentheses, clustered at household level. **(4)** The overall mean of VAS intake is 0.73. **(5)** The R-squared for the household fixed effects models in Columns (3)-(5) is the within-R-squared. **(6)** ***,** and * indicates significance at 1%, 5% and 10%.

Table B4: Effect of the shocks on the household's labour force participation

Dependent variable:	Avg. weeks spent in the labour force by a HH member				
	(1)	(2)	(3)	(4)	(5)
Health Shock	-0.113	-2.13**	-6.330***	-5.895***	-4.473**
	(1.23)	(0.911)	(2.333)	(2.464)	(1.985)
Income Shock	3.46***	1.133*	3.957**	3.860**	4.368***
	(0.962)	(0.685)	(1.766)	(1.736)	(1.793)
HH-specific time-variant controls:					
Log of total household assets		0.030	-0.381	-0.397	-0.086
		(0.174)	(0.621)	(0.630)	(0.640)
Total permanent members		-0.668***	-0.326	-0.352	-0.218
		(0.115)	(0.482)	(0.492)	(0.512)
Total members in prime years		0.608***	0.111	0.398	0.324
		(0.201)	(0.839)	(0.850)	(0.832)
If relocated		-0.569	-0.749	-1.052	-1.522
		(0.835)	(1.975)	(2.011)	(1.874)
If lives in flood-prone region		-0.327	-20.159***	-18.981***	-19.943***
		(0.522)	(5.206)	(5.917)	(6.618)
If interviewed in rainy season		1.027**	-0.511	-0.614	-1.074
		(0.473)	(1.228)	(1.250)	(1.192)
Health supply covariates:					
General outpatient care offered		7.887**		-8.039	-5.115
		(3.405)		(8.847)	(6.984)
Immunisation offered		-1.166		8.460	6.662
		(1.472)		(8.532)	(6.228)
If too far		-0.780		-2.345	-2.882
		(1.211)		(4.464)	(5.012)
If no skilled staff		1.404		3.657	4.747
		(1.397)		(4.691)	(5.165)
Distance covariates	No	Yes	No	No	Yes
Households FE	No	No	Yes	Yes	Yes
Survey wave FE	No	Yes	Yes	Yes	Yes
No. of obs.	1518	1518	1518	1518	1518
No.of households	1255	1255	1255	1255	1255
R-sq.	0.011	0.542	0.650	0.652	0.716

Notes: (1) Dep.var. is the number of weeks a permanent hh.member spends in the labour market on average. Indep.var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(2)** This analysis involves only the survey years 2010, 2011 and 2013 since the labour participation hours or weeks cannot be calculated for 2009 due to lack of data. **(3)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(4)** Standard errors in parentheses, clustered at household level. **(5)** The overall mean of household labour weeks is 12.61. **(6)** The R-squared for the household fixed effects models in Columns (3)-(5) is the within-R-squared. **(7)** ***,**,* indicate significance at 1%, 5% and 10% respectively.

Table B5: Effects of shocks on VAS intake of child in different age brackets

Dependent variable:	If the child received a VAS dose in last 6 months		
	(1)	(2)	(3)
Age 6-9 months	0.106**	0.105**	0.089*
	(0.050)	(0.050)	(0.050)
Health Shock	0.124	0.123	0.117
	(0.098)	(0.098)	(0.104)
Age 6-9 months X Health Shock	0.015	0.017	0.046
	(0.143)	(0.143)	(0.156)
Income Shock	0.120**	0.119**	0.111*
	(0.061)	(0.062)	(0.062)
Age 6-9 months X Income Shock	-0.094	-0.097	-0.080
	(0.090)	(0.091)	(0.090)
Controls[a]	Yes	Yes	Yes
Health supply covariates	No	Yes	Yes
Distance covariates	No	No	Yes
Households FE	Yes	Yes	Yes
Survey wave FE	Yes	Yes	Yes
No. of obs.	2109	2109	2109
No.of children	1592	1592	1592
R-sq.	0.083	0.090	0.137

Notes: **(1)** Dep.var. dummy=1 if the child under two years in the hh. received a VAS dose in last 6 months. Indep.var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(2)** Dummy variable *Age 6-9 months*=1 if the child was between 6-9 months old in the last 6 months. The interaction terms give the differences in slope of this age variable when under respective shocks to that under no shock. **(3)** [a] includes household level controls - log of total value of household assets, number of children under five, number of permanent members, if the family had moved in recent past, if interviewed in a rainy season, if lived in a flood-prone region; and individual level controls as neonatal care received (breastfed at birth), presence of mother, presence of father, education of mother and father. **(4)** Health supply covariates include - if the nearest health facility offered general outpatient care, if it offered immunisation service, if a major limitation was its distance, and if a major limitation was lack of skilled staff. **(5)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(6)** Standard errors in parentheses, clustered at household level. **(7)** The overall mean of VAS intake is 0.73. **(8)** The R-squared for the household fixed effects models in Columns (1)-(3) is the within-R-squared. **(9)** ** and * indicates significance at 5% and 10%.

Table B6: Effects of shocks on VAS intake using child fixed effects

Dependent variable:	If the child received a VAS dose in last 6 months		
	(1)	(2)	(3)
Panel A:			
Health Shock	0.214	0.284	0.337
	(0.218)	(0.227)	(0.283)
Income Shock	0.247*	0.234	0.516***
	(0.146)	(0.153)	(0.180)
Controls[a]	Yes	Yes	Yes
Health supply covariates	No	Yes	Yes
Distance covariates	No	No	Yes
Child FE	Yes	Yes	Yes
Survey wave FE	Yes	Yes	Yes
No. of obs.	142	142	142
No.of children	71	71	71
R-sq.	0.257	0.275	0.685
Panel B:			
Health Shock	0.214	0.284	0.337
	(0.207)	(0.212)	(0.234)
Income Shock	0.247*	0.234*	0.516***
	(0.138)	(0.143)	(0.149)
Controls[a]	Yes	Yes	Yes
Health supply covariates	No	Yes	Yes
Distance covariates	No	No	Yes
Child FE	Yes	Yes	Yes
Survey wave FE	Yes	Yes	Yes
No. of obs.	2121	2121	2121
No.of children	2050	2050	2050
R-sq.	0.257	0.275	0.685

Notes: (1) Panel A shows the estimates of the regression with child FE with only the children that appear more than once across the panels. **Panel B** shows the estimates of a regression with child FE including also the singleton observations. Maintaining singleton groups in linear regressions where fixed effects are nested within clusters does not affect the coefficient estimates but can overstate statistical significance and lead to incorrect inference (Correia, 2015). Here, we see that the cluster-robust SEs decrease a little in Panel B, however the corresponding statistical significances do not change that much that they affect the findings. **(2)** Dep.var. dummy=1 if the child under two years received a VAS dose in last 6 months. Indep.var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(3)** [a] includes household level controls - log of total value of household assets, number of children under five, number of permanent members, if the family had moved in recent past, if interviewed in a rainy season, if lived in a flood-prone region; and individual level controls as presence of mother and presence of father. **(4)** Health supply covariates include - if the nearest health facility offered general outpatient care, if it offered immunisation service, if a major limitation was its distance, and if a major limitation was lack of skilled staff. **(5)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(6)** Standard errors in parentheses, clustered at child level. **(7)** The overall mean of VAS intake is 0.73. **(8)** The R-squared for the household fixed effects models in Columns (1)-(3) is the within-R-squared. **(9)** *** and * indicates significance at 1% and 5%.

Table B7: Effects of shocks on VAS intake using controls on shock intensity

Dependent variable:	If the child received a VAS dose in last 6 months		
	(1)	(2)	(3)
Health Shock	0.128	0.126	0.139*
	(0.083)	(0.083)	(0.085)
Health Shock Intensity	0.091	0.100	0.063
	(0.087)	(0.092)	(0.093)
Income Shock	0.103*	0.098**	0.081
	(0.060)	(0.060)	(0.061)
Income Shock Intensity	-0.015	-0.011	0.009
	(0.039)	(0.039)	(0.038)
Controls[a]	Yes	Yes	Yes
Health supply covariates	No	Yes	Yes
Distance covariates	No	No	Yes
Households FE	Yes	Yes	Yes
Survey wave FE	Yes	Yes	Yes
No. of obs.	2109	2109	2109
No.of children	1592	1592	1592
R-sq.	0.075	0.082	0.131

Notes: (1) Dep.var. dummy=1 if the child under two years in the hh. received a VAS dose in last 6 months. Indep.var. *Health Shock* dummy=1 if any hh.member was ill or died in last 6 months. Indep.var. *Income Shock* dummy=1 if the hh. faced flood or drought/irregular rains in last 6 months. **(2)** Variable *Health Shock Intensity* is the ratio of the number of months of health-related shock suffered in the last six months to the number of months of suffering before that due to the same. Variable *Income Shock Intensity* is similarly defined for income-related shock. **(3)** [a] includes household level controls - log of total value of household assets, number of children under five, number of permanent members, if the family had moved in recent past, if interviewed in a rainy season, if lived in a flood-prone region; and individual level controls as neonatal care received (breastfed at birth), presence of mother, presence of father, education of mother and father. **(4)** Health supply covariates include - if the nearest health facility offered general outpatient care, if it offered immunisation service, if a major limitation was its distance, and if a major limitation was lack of skilled staff. **(5)** Distance covariates include - categorical distances to the nearest public health facility, the market selling agricultural inputs, agricultural and non-agricultural outputs, and the trunk and feeder roads. **(6)** Standard errors in parentheses, clustered at household level. **(7)** The overall mean of VAS intake is 0.73. **(8)** The R-squared for the household fixed effects models in Columns (1)-(3) is the within-R-squared. **(9)** ** and * indicates significance at 5% and 10%.

Table B8: Effect of household wealth status on health shock measure

Dependent variable:	If the HH suffered a health shock in last 6 months
	(1)
Log of total assets' value	0.003
	(0.004)
Controls[a]	Yes
Households FE	Yes
Survey wave FE	Yes
No. of obs.	9826
No.of households	3108
R-sq. within	0.018

Notes: (1) The sample includes all households. Dep. var. dummy=1 if the household suffered from a health shock in the last 6 months. Indep.var. *log of total assets' value* is continuous. **(2)** [a] includes the number of permanent members in the household, number of members in prime years of their age and number of children under five in the household. **(3)** SE in parentheses, clustered at household level.

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Cash incentives to mothers or to community health workers – what contributes better to the health of the mother and the newborn? Evidence from India.

III

Cash incentives to mothers or to community health workers - what contributes better to the health of the mother and the newborn? Evidence from India

Susmita Baulia*

Abstract

This paper investigates the role of demand-side incentives to mothers and supply-side incentives to community health workers (ASHAs) in improving maternal and child health. These conditional cash benefits were part of a nationwide health intervention *Janani Suraksha Yojana*, introduced in India in 2005. By its unique dual focus on demand and supply, the programme entitled socio-economically backward mothers with cash assistance if they chose to give birth at public health institutions, and simultaneously employed ASHAs to act as a direct link between a pregnant woman and the public health delivery system. By using variations in the eligibility of mothers, and the differential implementation of ASHAs across low-focus and high-focus states in a difference-in-difference framework, the maternal and neonatal health outcomes are evaluated. Results show causal evidence that eligible mothers who received both cash benefits and ASHA's guidance outperformed the eligible mothers receiving only cash benefits, in outcomes such as institutional births and breastfeeding practices. To elucidate, a mother with both cash benefits and ASHA's counsel experienced a 7.1 percentage points increase in institutional birth rate than her ineligible counterpart; whereas, the corresponding increase for a mother only eligible for cash benefits was 2.9 percentage points. A similar impact is found in the case of antenatal care check-ups of the expecting mother and BCG vaccination of the newborn.

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1 INTRODUCTION

Conditional Cash Transfer (CCT) programme is one of the proven methods for increasing human capital investment by resource-constrained households in low-income countries. In addition to short-term poverty reduction through income transfer, it is designed to serve as a powerful incentive for households in adopting a behaviour that could positively affect their well-being and break the cycle of poverty in the long run. Latin American countries were the first ones to experiment with this poverty-reduction tool in the early '90s in order to improve health and educational outcomes, and they showed positive results. *PROGRESA* (later named *Oportunidades*) in Mexico, *Bolsa Família* in Brazil are the major CCTs in the world, that have impacted lives of millions of beneficiaries till date (Fiszbein et al., 2009). As these examples suggest, CCTs are essentially targeted to address the demand-side problems of inadequate investment in human capital and/or uptake of health and education-promoting services. However, in many of these socio-economic settings of our interest, problems could be at the supply end (too), e.g. mediocrity of the service delivery system. For example, in several African countries, where the health service delivery system suffers from a severe lack of organisation and management (WHO, 2007), CCTs to low-income families would not be optimal at all. Under such circumstances, a CCT with only demand-side incentives is not only likely to leave behind mixed outcomes but also bigger unanswered questions such as, is the underutilisation triggered by lack of demand or lack of supply? The *Janani Suraksha Yojana* ("Safe Motherhood Programme") in India is a healthcare policy which by its unique features helps investigate such an unanswered question. This CCT incentivised both the mothers as well as community health workers for improving maternal and neonatal health outcomes. By this unique dual focus, the programme brings forth the possibility to examine both the demand and the supply-side issues. By exploiting several intricacies of the programme's features, this paper tries to uncover which is a more effective way to improve the health outcomes of the mother and the newborn - a demand-side incentive or a supply-side one, or are they complementary to each other?

The Janani Suraksha Yojana (henceforth, JSY) programme started in April 2005 as an initiative of the Ministry of Health and Family Welfare in India and is still ongoing; the number of beneficiaries of the scheme increased from 0.738 million in 2005-2006 to 10.438 million in 2014-2015, with about 0.9 million health workers involved. As of 2010, it had a budget of 15 billion INR (1 USD ≡ 46 INR in 2010). Fifteen years later, in 2020, with the introduction of parallel schemes and with the redistribution of budgets, JSY now has 4.28 million beneficiaries and works parallel to another nationwide maternity benefit scheme, the *Pradhan Mantri Matru Vandana Yojana*.

The programme divided the states as the high-focus and non-high focus ones, officially termed as the low-performing states (LPS) and high-performing states (HPS), and the cash incentives were to vary according to that categorisation.[1] In both the LPS and HPS, pregnant women above 19 years of age belonging to below-poverty-line families and giving birth in public health centres were eligible for cash assistance for up to two live births. On the other hand, incentives were offered to village-based health workers, known as the *Accredited Social Health Activists* (ASHAs), who were to act as the direct link between the local community and the public health system. The ASHA's primary duties regarding maternal and child health were to identify pregnant women in the community and to facilitate prenatal care for them. They were also supposed to escort the expecting mothers to the health centre for delivery or other pregnancy-related complications that needed medical help, and finally, to ensure postnatal care for the mother as well as the newborn. However, LPS being the focus areas of the programme, the ASHAs were introduced only there in the initial few years. This paper aims to use the absence of ASHAs in HPS in the early years, along with the eligibility criteria of mothers for the cash assistance, and thus distinguish the effects of the two channels on relevant health outcomes of the mother and the newborn.[2]

Using data from the second, third and fourth cross-sectional waves of the District Level Household Survey, I analyse a sample of more than 300,000 mothers with their most recent births in the time frame of 2001-2008. In a difference-in-difference (DiD) framework, I use multiple treatment groups categorising "eligible mothers in low-performing states with ASHA" and "eligible mothers in high-performing states without ASHA", and compare them with a control group of "mothers ineligible for any benefit". I find that the mothers receiving both the cash benefits and ASHA's counselling outperformed the mothers with only cash benefits in giving birth at public health facilities and in starting to breastfeed within 1-2 hours of birth. To put the difference in perspective, the eligible mother in the high-performing state experienced 2.9 percentage points (pp.) greater increase in institutional births[3] than the ineligible mother. In contrast, for an eligible mother in a low-performing state, this increase in the difference with the ineligible mother was 7.1 pp. These are equivalent to changes of about 11% and 28% in institutional birth rates compared to what an ineligible mother

[1] The LPS were Uttar Pradesh, Uttaranchal, Bihar, Jharkhand, Madhya Pradesh, Chattisgarh, Assam, Rajasthan, Orissa and Jammu and Kashmir, where institutional delivery rates were alarmingly low (less than 25%) compared to the HPS which comprised of the remaining states. (See **Appendix 2 Figure B2** for a distribution of institutional birth rates across states in the pre-intervention period.)

[2] Note, however, that the effects studied here are *intent-to-treat* effects since the analysis considers the eligibility of the mothers and the availability of the ASHAs as per the guidelines of the programme regardless of whether an eligible mother actually received the cash transfer or an ASHA actually counselled her in a low-performing state.

[3] By 'institutional birth', I mean birth at a public healthcare facility which has been approved by the JSY programme.

would experience in the pre-intervention period. Outcomes on antenatal care and BCG vaccination of the newborn also face a similar impact. For the rural mothers with up to two births, the ASHA's channel is distinctly more effective for all outcomes. However, the overall implication is that only receiving cash benefits for giving birth at public health facilities might not be sufficient for a mother to get motivated or overcome the costs. Continual guidance pre and post-birth by the health worker would lead to better all-round health of the mother and the newborn. An additional robustness analysis with the eligible mothers only in the HPS, who also received the ASHA's guidance post-2008, highlights a similar superior effect of the ASHA in antenatal and birth outcomes.

Previous studies on the JSY programme have mainly concentrated on the overall impact evaluation of the programme, i.e. its uptake and impact on reproductive and child health outcomes. The earliest of the studies is by Lim et al. (2010). They find that the intervention reduced perinatal and neonatal deaths and that the poorest and the least educated women in the LPS were not the most probable ones to receive the cash assistance. Powell-Jackson et al. (2015), by exploiting the heterogeneous intensity in the implementation of the programme across districts, find evidence of increased uptake of maternity services by women due to cash incentives, but no substantial evidence on reduction in neonatal mortality.

Contrary to Lim et al.'s findings, Powell-Jackson et al. find that women of low socio-economic status utilised the programme more. Similar results are reported by Carvalho et al. (2014) with regards to immunisation rates, postpartum check-up rates, and healthy breastfeeding practices. By exploiting differences due to treatment eligibility of the programme in a sample of over 400,000 women, Joshi and Sivaram (2014) find moderate effects of the programme. They find that compared to the broader population, the targeted population experienced only three percentage points increase in medically supervised births; however, no increase in antenatal or postnatal care was found. Randive et al. (2014) analyse only the LPS by using a combination of surveys to see the change in socio-economic inequality in institutional deliveries after the implementation. They use concentration curve and concentration index and further decompose the index to understand channels of the inequalities.

Despite this substantial evidence on JSY on the whole, the related literature still lacks a more in-depth examination of which side of the financing has been more effective. It is where this paper's contribution lies. Although the nature of the programme allows for only a short-run analysis of this research question (due to the features becoming more or less uniform across all states over the years and also later getting combined with other state-level programmes), this study is able to explore the different channels sufficiently and provide some insights into how efficiently they can improve maternal and child health status in developing countries.

Another recent study by Debnath (2018) also focusses on the channels. While I consider the treatment variations through the mother's eligibility criteria for cash assistance and the availability of ASHA, Debnath exploits the variation in the amounts of cash given as incentives to the ASHA and the eligible mothers. He finds that larger cash incentives to the health workers are associated with relatively high utilisation rates of these services compared to larger incentives to mothers. His further findings show an increase in the use of pre and post-natal healthcare services and a reduction in early neonatal deaths. However, Debnath's study does not take into account that the ASHAs were unavailable in the HPS until 2008, and also omits the revisions made in the JSY guideline on the eligibility of mothers in late 2006. Furthermore, the fact that the ASHAs in rural areas received additional compensation to arrange for transportation of the expecting mother to the healthcare facility, while her own cash incentive remained the same as in urban areas, has not been clearly discussed by Debnath; hence, the inference from the estimated effect of the ASHA's monetary incentive might suffer from some bias. My study offers an improvement over any such estimation bias by incorporating all relevant programme details.

Furthermore, this paper acknowledges the indispensability of public/community health workers in the improvement of health outcomes. They act as knowledge-base on health and sanitation practices and childcare to the women in developing countries (Block, 2007; Hirvonen et al., 2017). Also, incentive programmes to public healthcare providers have proved to be effective in the use and service quality of healthcare, e.g. better performance of public health agents in promoting HIV prevention in Zambia (Ashraf et al., 2014), decrease in absenteeism of nurses in India (Banerjee et al., 2008). A paper worth mentioning here is by Basinga et al. (2011) which provides evidence that a randomised trial with performance-based pay to health workers increased institutional deliveries by 23% and receipt of postnatal care by newborns by over 50% in Rwanda. My paper lies at the junction of these two strands of literature - one, on the effectiveness of health workers as a knowledge source, and two, on the improvement of the service delivery by them through incentives.

In the literature on CCTs, the ones focused on various health aspects have shown improvement in preventive healthcare and healthy behaviour, such as vaccination coverage for children in rural Nicaragua (Barham and Maluccio, 2009), cervical cancer screening, annual HIV tests in low and middle-income countries (Ranganathan and Lagarde, 2012), and also nutritional outcomes in Mexican children (Behram and Hoddinott, 2005; Fernald et al., 2008b) and adults (Fernald et al., 2008a). In this vein, my paper contributes to the literature on health-based CCTs in terms of maternal and neonatal health outcomes. Also, this paper contributes to the literature in the impact evaluation of welfare programmes, which investigates the complementarity between supply and demand-side nudges and incentives. In other studies, Banerjee et al. (2010) have

examined the impact on child immunisation by setting up immunisation camps in rural India vs providing food incentives to parents besides setting up camps, and Singh (2015) has examined the effect on children's nutritional outcomes by incentivising the health worker through performance-based pay vs providing information to mothers besides worker incentives. Furthermore, to my knowledge, in the literature on JSY evaluation using the District Level Household Survey, this is the first paper that incorporates the latest available fourth survey round in order to investigate the programme's impact in the high-performing states after the ASHAs were introduced there post-2008. Finally, this paper adds to the growing number of empirical studies that incorporate coarsened exact matching technique along with difference-in-difference in their methodologies, e.g. (Rellstab et al., 2020; Bertoni et al., 2020).

The remainder of the paper is structured as follows. Section 2 sketches the organisational background and the details of the CCT programme. Section 3 suggests the mechanisms. In Section 4, I introduce the data and empirical strategy for the analysis, followed by the main results in Section 5. In Section 6, I discuss some robustness issues and finally, in Section 7, I conclude.

2 THE INDIAN CONTEXT

2.1 Maternal and child health in India

In the early '90s India, maternal mortality ratio (MMR) per 100,000 live births was 556 which accounted for almost 19.7% of deaths of women in their reproductive age due to issues related to pregnancy (in absolute terms, this number was as high as 152,000 maternal deaths). In addition to this, the neonatal mortality ratio (NMR) per 1000 live births was 57.4. A decade later, MMR had reduced to 374, which was equivalent to 13% of women's deaths due to maternity, and NMR was still 45.1.[4]

This severity in the mortality rates often results from the missed utilisation of several necessary steps of care during maternity. Giving birth under the skilled supervision of health professionals is of utmost importance to necessitate the safety of the mother and the child. However, between 2001-2004, only 7.4% of Indian women gave birth in the presence of any trained professional, and almost 3% did not give birth at a health facility due to lack of transportation. When it comes to the antenatal phase, it is prescribed that an expecting mother should receive at least three antenatal care (ANC) check-ups during the preg-

[4] Source: WHO, UNICEF, UNFPA, World Bank Group, and United Nations Population Division Maternal Mortality Estimation Inter-Agency Group.

nancy to track her own and the child's health. These check-ups encompass an examination of weight, blood pressure, abdomen, ultra-sound tests of the pregnant woman and can be conducted in any health-centre, clinic or hospital. (A public one would be available for free, however.) Together with the check-ups, the expecting mother also receives advice on breastfeeding, need for cleanliness during delivery, family planning and nutrition for herself and the child. During these check-ups, she is also given Iron Folic Acid tablets/syrups and Tetanus injections.[5] However, my pre-intervention sample shows that only 48.5% expecting mothers received the recommended three ANCs.

Over the past decades, continued evidence on the positive effect of breastfeeding on child health, particularly in developing nations (Attanasio, 2015), has led to the conclusion that early breastfeeding by the mother reduces chances of child mortality. WHO recommends new mothers to start breastfeeding already at the first hour of birth, followed by exclusive and continued breastfeeding until the first six months of birth. However, the pre-intervention sample shows that only 27.2% were breastfed at birth. Finally, it comes to postnatal care of the mother and the child. My data shows that only 10% of the new mothers between 2001-2004 received any postnatal visit by a health worker within two weeks of giving birth. Administering vaccines in the early months is crucial to the child's well-being at a later age. When looking at the receipt of the BCG vaccine,[6] we see that only 68% of newborns were vaccinated, even though it is recommended that BCG should be given to all newborns within two weeks of birth, as part of the National Immunisation Programme.[7]

For these inferior outcomes on maternal and child health, both the demand and the supply ends are partially responsible. Although the public healthcare system in India allows for free and low-cost maternal and child healthcare, the uptake of those services remains relatively low (and often, only constricted to mothers of upper socio-economic background, urban residence and sometimes, certain religions). The major contributing factors of this underutilisation of maternal and child healthcare are high indirect costs, the practice of informal pay-

[5] According to WHO guidelines, daily oral iron and folic acid supplementation with 30 mg to 60 mg of elemental iron and 400 μg (0.4 mg) folic acid is recommended for pregnant women to prevent maternal anaemia, puerperal sepsis, low birth weight, and pre-term birth. WHO further recommends, a pregnant mother should receive two doses of tetanus toxoid containing vaccine. Neonatal infection usually occurs through the exposure of the unhealed umbilical cord stump to tetanus spores, which are universally present in the soil, and newborns need to have received maternal antibodies via the placenta to be protected at birth.

[6] In 1993, Tuberculosis (TB) was recognised by the WHO as a global emergency in public health. In 1995, out of the total TB cases in the world, the South-East Asia region accounted for 42% of the cases. The highest number of TB patients were found in the Indian sub-continent, with estimated incidence rates of more than 100 per million population (WHO, 1997). The *bacille Calmette-Guérin*, most commonly known as BCG vaccine, has existed for over 80 years and has a documented protective effect against tubercular meningitis and disseminated TB in children. Thus, the BCG vaccine is included in the national childhood immunisation programme in almost all countries, including India.

[7] The discussed statistics related to the births in pre-intervention years are obtained from author's calculations using DLHS 2 and 3.

ments, socio-cultural norms, gender inequality and broadly prevalent economic inequality. Gender inequality, together with traditional patriarchal norms and practices in society, often deprives women of their rights of accessing basic healthcare (Drèze and Sen, 2002). At the same time, the quality of healthcare provision has not been cooperative either. A body of literature exploring various aspects of the healthcare system of India, confirms issues such as inadequacy of medical equipment in healthcare facilities and absenteeism of health professionals. Furthermore, the rate of usage of public healthcare has been found to be strongly correlated with absenteeism of health professionals from health facilities (Banerjee et al., 2004; Banerjee and Duflo, 2007).

2.2 *The Janani Suraksha Yojana Programme*

In order to address these demand and supply-related blockages leading to such a dire status of maternal and child health in the country, the Government of India introduced this nationwide intervention. The JSY programme, as a component of the *National Rural Health Mission*,[8] took off in April 2005. Its objective was to reduce maternal and neonatal mortality by promoting institutional delivery among poor pregnant women. The intervention was ambitious with a dual focus. It intended to increase the demand for (quality) maternal and neonatal healthcare by giving cash assistance to mothers for institutional delivery, and simultaneously, aimed to improve the healthcare service delivery to the poor by engaging ASHAs to serve as village-based health workers. The main thrust of the programme, however, lay on the low-performing states that were the worse performers in maternal and child health indicators compared to the high-performing states. (**Figure B1** in **Appendix 2** shows the two categories of states.) The JSY programme is still ongoing and has been joined by other state-level similar schemes over the years.[9]

During its initial few years, the programme underwent a few rounds of revision. The first set of guideline took effect nationwide from April 2005. Soon

[8] The National Rural Health Mission, an initiative of the Ministry of Health and Family Welfare of India, aimed to provide equitable, affordable and quality health care to the rural population, especially the vulnerable groups. The mission focussed on establishing a fully functional, community-owned, decentralised health delivery system with inter-sectoral convergence at all levels, to ensure simultaneous action on a wide range of determinants of health such as water, sanitation, education, nutrition, social and gender equality.

[9] Note, before the introduction of JSY, another scheme *National Maternity Benefit Scheme* (NMBS) was introduced in 1995 as a component of a broader National Social Assistance Programme. This scheme gave the pregnant women who belonged to below-poverty-line households and had attained 19 years of age, the right to receive 500 INR of financial assistance per live birth for up to two live births. This scheme was uniformly prevalent across the entire country, unlike the JSY that introduced a graded scale of assistance based on the categorisation of states and residence status. NMBS was transferred from the Ministry of Rural Development to the Ministry of Health and Family Welfare during 2001-2002. It is plausible that despite the presence of NMBS, the looming difference in health outcomes of the mother and the newborn, across different states, led to the replacement of NMBS by JSY.

after, it went through a revision in October 2006. It was followed by further revisions, for example after 2008 (taking effect from April 2009), when the ASHAs spread to the HPS as well. Later in June 2011, with additional features on eliminating all out-of-pocket expenditures, the programme came to be known as the *Janani Shishu Suraksha Karyakram* ("Mother Safety Child Programme"). Another conditional benefit scheme at national-level, named the *Indira Gandhi Matritva Sahyog Yojana* was conceived in 2010 (and took effect from late 2011) and focussed on the compensation of wage loss of women during and after pregnancy.

2.2.1 Eligible mothers

According to the first set of guidelines,[10] which was published by the Ministry of Health and Family Welfare in April 2005, cash incentives to mothers were to vary between the low-performing states and high-performing states (inclusive of the union territories).[11] In both the LPS and HPS, pregnant women above 19 years of age and belonging to below-poverty-line families,[12] giving birth in public healthcare facilities (such as primary health centres, community health centres, first referral units, general wards of district and state hospitals or accredited private institutions), were eligible for the cash assistance for up to two live births. However, the eligibility criteria in the LPS and HPS were different. The LPS being under extra attention, the women from both rural and urban areas in the LPS could avail the cash assistance, but, the women from only rural areas could do so in the HPS.

However, this set of rules was soon criticised since it did not effectively encompass all the socio-economically backward women in the country among whom the institutional birth rates were the lowest. As a result, the eligibility criteria were made less restrictive. Consequently, from October 2006 onwards, all women in the LPS irrespective of their residence type, income level and birth history could avail the cash assistance if they chose to give birth at public healthcare facilities. In the HPS, the previous eligibility also spread across the urban areas; and the women who belonged to socially disadvantaged castes like *Scheduled Caste* or *Scheduled Tribe* families also came under the coverage now.[13]

In the empirical analysis in Section 4.2, I consider the eligibilities of the

[10] downloaded from "www.ilo.org/dyn/travail/docs/683/JananiSurakshaYojanaGuidelines/MinistryofHealthandFamilyWelfare.pdf"

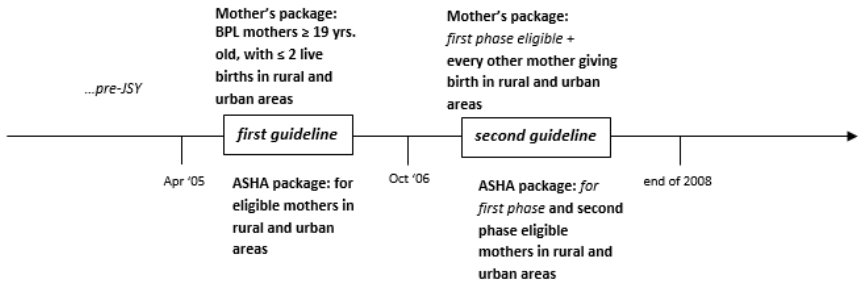
[11] The union territories of India come directly under the administration of the Central Government whereas the states of the country have decentralized governments. The union territories of India during this study period were Andaman & Nicobar Islands, Chandigarh, Dadra & Nagar Haveli, Daman & Diu, Delhi, Lakshadweep and Puducherry.

[12] A below-poverty-line household is the one which has insufficient income to purchase two basic meals per day. This *poverty line* value differs across rural and urban areas and states. According to the Tendulkar Committee, the *poverty line* based on per capita consumption expenditure per day was 27 INR in rural areas and 33 INR in urban areas in 2009 (Tendulkar et al., 2009).

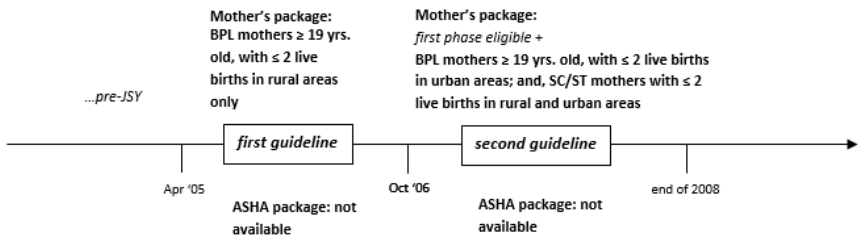
[13] downloaded from "https://mohfw.gov.in/sites/default/files/FEATURES%20FREQUENTLY%20ASKED%20QUESTIONS.pdf"

mothers (in terms of their socio-economic indicators, residence type and birth history) for the cash assistance as per the first and second guidelines of the programme. Taking into account the revisions of the guidelines on the eligibility, I consider two categories of eligible mothers. The *first phase eligible mothers* satisfy the eligibility criteria of the first guideline that took effect from April 2005, and the *second phase eligible mothers* do not satisfy the eligibility criteria of the first guideline but of the second guideline that took effect from October 2006.

Figure 1 summarises the eligibility structures as given by the first two guidelines of the programme. Therefore, the time period used in my main analysis is between April 2005 to the end of 2008.



(a) Low performing states (LPS)



(b) High performing states (HPS)

Figure 1: JSY programme details - the first two guidelines

Notes: This is a simple representation of the eligibilities of the new mothers according to the first two guidelines for the mother's package and the ASHA's package. BPL is below-poverty-line, SC, ST are *Scheduled Caste* and *Scheduled Tribe* classes. A more detailed guideline which also include the scale of monetary incentives is given by Appendix 2 Figure B4.

2.2.2 ASHAs

The village-based health workers called the *Accredited Social Health Activists* or ASHAs constituted an integral part of the JSY programme. They were em-

ployed only in the LPS, according to the first two guidelines of JSY; later in 2009, they spread across the HPS too.[14]

Typically, an ASHA was a female resident of a village, within the age group of 25-45 years, and with primary education. The norm was to have one ASHA per 1000 population. ASHAs were selected by a committee composed of self-help groups, the village health committee and the village council, together with the presiding officers at the district and sub-district levels. The selected ASHAs were trained in basic health care practices. The ASHA's primary duties regarding maternal and child health were to identify a pregnant woman in the community, register her with the programme and counsel her throughout pregnancy. Besides, she was to facilitate prenatal care, to escort the expecting mother to the health centre for delivery or other pregnancy-related complications that needed medical help, and finally, to ensure postnatal care for the mother as well as the newborn. An Auxiliary Nurse Midwife stationed at health sub-centres and primary health centres was to supervise an ASHA.[15]

Note that, as a treatment variation in the main estimation strategy in Section 4.2, I use the presence of ASHA in the LPS and her consequent absence in the HPS during the time period when the first two guidelines were in effect.

2.2.3 Cash incentives and timings of payments

As treatment variations, this paper focusses on using the eligibility status of the mother for the cash assistance and the availability of ASHA at state-level. Nevertheless, in this section, I briefly outline how the cash incentives were designed and revised, and where lay the conditionality of the incentives.

The cash assistance to the pregnant woman, i.e. the 'mother's package' per live birth varied in scale across rural and urban areas. According to the first guideline, in rural areas, the mother's package was 700 INR in both the LPS and HPS and 600 INR in the urban areas of the LPS. According to the second guideline, the cash amounts for the rural HPS remained the same with an addition of 600 INR mother's package in the urban HPS. Also, the amounts in the LPS were increased to 1400 INR and 1000 INR in rural and urban areas. The monetary incentives in the LPS were quite substantial - around 63% of the poverty line expenditure cut-off, and 68% of the average delivery cost in a government medical facility (Tendulkar et al., 2009). The mother received the cash assistance at the time of discharge from the health facility after she had given birth.

The 'ASHA's package' per live birth, however, was only available in the LPS; 600 INR in rural areas and 200 INR in urban areas. Of this 600 INR, only 200

[14] However, it is unclear from the official guidelines, if the ASHAs were employed in the union territories. Therefore, later in the analysis, I exclude the union territories.

[15] Note that, apart from the JSY programme, the ASHAs were also involved in spreading awareness across households in the community regarding general health, family planning and sanitation needs.

INR was her incentive for all her duties and the remaining was for organising transportation of the beneficiary to the nearest health centre for delivery and her own logistics support when she stayed with the expecting mother at the health facility for delivery. In rural areas of LPS, the transportation and logistics compensation was paid to the ASHA in advance. However, her cash incentive came in instalments - the first one was paid during/after the discharge of the mother from the health facility, and the second, a month after the delivery on confirming postnatal care visit and administering the BCG vaccine to the newborn. In urban areas, only the incentive part of 200 INR was available to the ASHA since it was not mandatory for her to arrange for transportation for the mother nor stay with the mother at the health facility during labour.

3 CONCEPTUAL FRAMEWORK

One of the most cited reasons among the poor women in India for not having institutional deliveries is the attached costs. Women living in remote villages would often require to travel long distances in order to avail proper healthcare; hence they choose to give birth at home or in the absence of skilled supervision. Besides the indirect costs due to transportation for the expecting mother in labour as well as other accompanying family members, another cost that usually arises is the opportunity cost due to their lost wages of the days spent at the health facility due to birth. Under such circumstances, the cash assistance given to the mother (or the promise of it until she delivers the baby) through the programme, should ease the budget constraint of the household while choosing institutional births. Particularly for women in rural areas where these costs are relatively high, the cash incentives are likely to have a more substantial effect.

Another channel for the lack of institutional births among socio-economically backward women is the dearth of right knowledge and information on hygiene and safety issues of giving birth. Most of these mothers are often unaware of the health risks involved with pregnancy. In that regard, an ASHA is a positive addition to the JSY programme. The fact that the ASHA counsels and helps the expecting mother through every step of her pregnancy would lead to informed decision-making by the latter and thus reduce the chances of giving birth in unhealthy conditions.

Note that the mother's package was available in the high-performing states, and both the mother's package and the ASHA were available in the low-performing states. One could expect to examine by comparing the eligible mothers in the HPS with the ineligible mothers whether the mother's cash incentive was effective in overcoming the indirect costs due to transportation and opportu-

nity cost. Then by comparing the eligible mothers in the LPS with the ineligible mothers, it is possible to investigate if the dual incentives - one to the mother and the other to the ASHA, were useful to offset both the mother's indirect costs as well as the information gap. Now, by comparing the outcomes of the eligible mother in the LPS vs the HPS, it is possible to understand whether additional employment of the ASHA necessarily improved the outcomes. A positive difference would suggest that the information cost was a significant deterrent of the utilisation of maternal and neonatal services, and incentivising the health workers did provide an effective solution to that. On the other hand, if both the state-types faced similar improvements, with no added effect in the LPS, then it can be concluded that incentivising the mother to offset the indirect costs was enough and there was no information gap, in this context.

However, here it is crucial to recognise that eliciting the effect of the ASHA's channel merely by comparing the LPS and HPS mothers, who could be compositionally quite different, is not straightforward. Therefore, it would be worthwhile to compare a homogeneous sample across the two state-types in order to conclude on this better. When the LPS eligible mothers are demographically identical to those in the HPS, then it is easy to pin down the added effect on the former precisely through the ASHA's presence (also, given that the mother's cash incentive was of the same amount in the two state-types). In the later sections, I address these issues.

On a related note, there is some rationale behind the choice of outcome variables. The criteria for the programme benefits tell that the mother's incentive was only conditional on her having an institutional birth, and did not mandate her to go through antenatal care and postnatal care check-ups. However, the ASHA's performance-based pay required her to complete a set of duties during the antenatal, birth and postnatal phase. So looking at those particular antenatal and postnatal outcomes could also provide a precise angle on the ASHA's channel of effect.

4 DATA AND EMPIRICAL STRATEGY

4.1 Data

The District Level Household Survey (DLHS) is the most useful health-related data source at the district level in India. I use the second and third rounds of this repeated cross-sectional survey (henceforth, DLHS 2 and 3) to obtain information on births by mothers across all districts, before and after the implementation of the programme. The DLHS is administered by the Institute for International

Population Studies (Mumbai, India) and its partner organisations. To date, they have implemented four rounds of the survey. The DLHS 2 has information on 507,622 married women sampled across the districts in 2002-2004, and DLHS 3 has information on 643,944 married and unmarried women sampled during 2007-2008. These two surveys contain detailed information on ANC, delivery (including details of receipt of JSY cash assistance) and PNC of the most recent birth by mothers during 1998-2004 and 2004-2008 respectively. Additionally, the surveys contain information on the demographic composition and socio-economic characteristics of the household - including caste, religion, and wealth status. [16]

By merging DLHS 2 and 3, the sample for my analysis is comprised of every woman who is married and is of age 15-49 years and had her most recent birth between 2001-2008. It leaves a sample of a little over 300,000.[17]

In the analysis, I use the information on the mother's characteristics which include her age while giving birth, her maximum schooling years, the total number of births she had in the past, if she was a Hindu or Muslim, if she belonged to one of the socially disadvantaged castes (*Scheduled Caste, Scheduled Tribe or Other Backward Classes*), her residential status - rural/urban, household wealth status (given in deciles of wealth distribution). All these information are directly available from the woman's questionnaire modules of DLHS 2 and 3. I formulate the wealth index by using *principal components analysis* with a variety of assets owned by the households.[18]

For further information on village health infrastructure, I use the information on the presence of early childhood development services such as the *Integrated Child Development Services*[19] in the village. An indicator for this should control for whether the mother benefitted from any other welfare and early childcare related programmes. Furthermore, I control for distance to the nearest primary health centre, to the nearest community health centre and the nearest public district hospital. The information on these indicators is available from the village

[16] The DLHS is more relevant to the reform's timeline than any another potential data source on India (e.g. the combination of India Human Development Surveys I and II) since it offers more number of observations with birth details, from the pre-intervention period.

[17] Although DLHS 2 has information on births from 1998-1999, I restrict the sample from 2001 onward to have the *pre* and *post* windows equally distributed around the reform.

[18] Since DLHS 2 does not have direct information on the below-poverty-line status, I compute this wealth index and use the bottom-most quintile of the distribution to define the poor. Joshi and Sivaram (2014) use a similar methodology. Many different indices have been used to decide on the poverty line in India. As per India's Planning Commission figures of poverty between 2004-2005, the rural and urban percentages below the poverty line were 22.7% and 21.9%. In this regard, defining the poor as the bottom-most quintile of wealth distribution is quite acceptable.

[19] Since 1975, the *Integrated Child Development Services* scheme is one of the flagship programmes undertaken by the Government of India, on early childhood care and development. The beneficiaries under this scheme are children of 0-6 years, pregnant women and lactating mothers. The offered health services are supplementary nutrition, health check-up, referral services and immunisation. They are usually provided by Auxiliary Nurse Midwives and *Anganwadi* workers (*Anganwadi* being a type of rural childcare centre in India). Other development services include pre-school and non-formal education.

questionnaire module of the survey.

Among the binary outcome variables used are institutional births, at least three ANCs received by the mother, if the mother started breastfeeding within an hour or two of giving birth, and if the baby received BCG vaccine after birth. The choice of these outcome variables is driven by the fact that these are the immediate components which can be affected by the programme and can significantly contribute to the alleviation of maternal and child morbidity and mortality over time, which is the main aim of the programme.

The institutional birth rate is the most vital outcome to look at in this context because this is what the programme wanted to impact the most. The binary variable on institutional birth is obtained from information in the woman's questionnaire on whether she had her latest birth at a public health facility.[20] It is recommended to have a standard number of three ANCs during pregnancy. As a measure of ANC received by the expecting mother, a dummy variable is used to indicate whether she received at least three ANCs. Then the binary variable on early breastfeeding is denoted by the information on whether the mother breastfed the baby within 1-2 hours of birth. Finally, the information if the child received a BCG vaccine is obtained from the two waves of DLHS. Although DLHS 3 data allows recognising precisely whether the child received the vaccine within a month of birth, the DLHS 2 does not. Therefore, I resort to using a more general variable that only suggests whether the child got vaccinated with BCG or not.

Lastly, for additional analysis on the introduction of ASHAs in the HPS (as discussed in Section 6.3), I combine the latest DLHS 4 wave undertaken in 2012-2014, with DLHS 2 and 3. Here, I use similar information on mothers of age 15-49 years who had their latest birth from 2001 until May 2011, only in the high-performing states. The relevant sample contains over 145000 observations.

4.2 Empirical strategy

The goal of this paper is to estimate the causal effect of the JSY programme, through its different channels, on the health outcomes of the mother and newborn. Given the roll-out of the programme, two major *phases* should be considered - one when the first guideline determined the mother's eligibility, and the other when the second guideline determined the eligibility. This is because, after the second guideline was introduced, the new eligibility status then encompassed more mothers who would otherwise be in the 'control' group should

[20] A public health facility, approved by the JSY programme, includes public hospital, dispensary, primary health centre, community health centre, urban health centres (Urban Health Centre/Urban Health Post/Urban Family Welfare Centre), AYUSH hospital/clinic.

I only consider the first guideline's eligibility.[21] This essentially gives rise to two sets of eligible mothers along the timeline, - the ones who became eligible according to the first guideline, and then the ones who were not eligible according to the first guideline, but became so after the second guideline took effect. Alongside them, remains a set of mothers who never became eligible.

Then comes the variation in the availability of the ASHA at the state-level. Throughout the period when the first and second guidelines were prevalent, ASHAs were available to counsel the eligible mothers only in the low-performing states and not in the high-performing states.

Taking into account the above variations, I have four treatment groups - "eligible mothers according to the first guideline without the presence of ASHA", "eligible mothers according to the first guideline with the presence of ASHA", "newly eligible mothers according to the second guideline without the presence of ASHA" and "newly eligible mothers according to the second guideline with the presence of ASHA". The control group consists of those mothers who never become eligible for any assistance.

Therefore, to estimate the causal impact of the programme, I resort to a DiD specification. For a mother i giving birth in a state j in a year t, I estimate the following:

$$\begin{aligned} Y_{ijt} = \alpha &+ \beta_1 EligiblewithoutASHA_{ij} + \beta_2 EligiblewithASHA_{ij} \\ &+ \beta_3(EligiblewithoutASHA_{ij} * Post1_t) + \beta_4(EligiblewithASHA_{ij} * Post1_t) \\ &+ \beta_5 AddEligiblewithoutASHA_{ij} + \beta_6 AddEligiblewithASHA_{ij} \\ &+ \beta_7(AddEligiblewithoutASHA_{ij} * Post2_t) + \beta_8(AddEligiblewithASHA_{ij} * Post2_t) \\ &+ X'_{ij}\delta + \mu_t + \gamma_j + \epsilon_{ijt} \quad (1) \end{aligned}$$

Y_{ijt} is the outcome variable of the mother i in the state j giving her latest birth in the year t. $EligiblewithoutASHA_{ij}$ is one of the treatment variables which takes value 1 for the mother who is eligible for cash assistance as per the first guideline in the state where ASHA is not available, i.e. the HPS. Similarly, $EligiblewithASHA_{ij}$ is her counterpart group in the state where ASHA is available, i.e. the LPS. They are whom I refer to as the *first phase eligible* mothers. β_1 and β_2 capture the difference in means of these two groups with the control group (i.e. the *Ineligibles*) before the intervention. $Post1_t$ takes value 1 for the births that took place after April 2005. An interaction of $Post1_t$ with each of these two eligible groups gives how different the expected mean change in outcome was from before to after the first guideline was introduced, between the respective eligible group and the control group. In other words, β_3 gives the treatment effect of only the mother's package on the first phase mother in the HPS, and β_4 gives the treatment effect of the mother's package and ASHA's

[21] Joshi and Sivaram (2014) also consider the intervention period in two phases. However, they do not incorporate the updated eligibility status of the mothers in the second phase.

presence on the first phase eligible mother in the LPS. Now, by comparing β_3 and β_4, one would be able to say that the ASHA's presence drives the difference in effect.

Then comes the $AddEligiblewithoutASHA_{ij}$ which is a dummy variable for the mother who was not eligible according to the first guideline, but newly eligible for cash assistance as per the second guideline in the HPS (where ASHA remained unavailable). Similarly, $AddEligiblewithASHA_{ij}$ is a dummy variable for her counterpart in the LPS. They are whom I refer to as the *second phase eligible* mothers. β_5 and β_6 capture the difference in means of these two groups with the *Ineligibles* before the intervention. $Post2_t$ takes value 1 for the births that took place after October 2006. An interaction of $Post2_t$ with the $AddEligiblewithoutASHA_{ij}$ and $AddEligiblewithASHA_{ij}$ respectively, gives the difference-in-difference effect of the programme on the mothers who became eligible only after the second guideline took effect, in comparison to the control group. Essentially, β_7 gives the treatment effect of only the mother's package on the second phase mother in the HPS, and β_8 gives the treatment effect of the mother's package and ASHA's presence on the second phase eligible mother in the LPS. A comparison of β_7 and β_8 tells that the ASHA's presence drives the difference in effect.

To take into account any confounding effect arising from the heterogeneity in the roll-out of the programme across states, I include state fixed effects denoted by γ_j. Also, it is worth mentioning here that a few other maternity programmes were in effect during JSY's study period.[22] However, these programmes were implemented at state-level; therefore, a state fixed effect should eliminate any confounding effect arising from their availability. Furthermore, any unobservable effect arising due to specific birth-years of the latest births by the mothers is absorbed by a birth-year fixed effect denoted by μ_t.

Finally, X_{ij} constitutes the set of control variables at individual, household and village infrastructure levels. The individual-specific variables are the number of births the mother has had, her maximum schooling years, her age during the latest birth. Household-specific variables comprise wealth decile, socially backward class status and religion of the household. Of the village infrastructural variables, the presence of any child welfare development programme and the distances to the nearest primary health centre, community health centre and district-level public hospital are included.

[22] To my knowledge, *Dr Muthulakshmi Maternity Benefit Scheme* which was launched in 1987 in the state of Tamil Nadu, continued to be in effect during JSY. The state of Orissa launched a conditional electronic cash transfer programme *Mamata* in October 2011, which is later than JSY's study period in this paper.

4.3 Summary statistics

In **Table 1**, I present the pre-intervention summary statistics of the dependent variables of interest and also, various covariates related to the mother and the household across the five comparison groups. Some of the covariates' means are significantly different across these groups because of the very nature of the groups' composition that depended on the residence, wealth status, social status, age and birth history. Moreover, it can be seen from the mean values of the dependent variables that the first phase eligible mothers in the LPS, denoted by *EligiblewithASHA* in Column (3) of Table 1, were the worst performers in the pre-intervention years. Naturally, on them lay the highest focus of the programme.

In **Table 2**, I also report the summary statistics related to the births before and after the introduction of the programme; I present this as an average across all the groups to verify if the average composition of the mothers giving birth pre and post-intervention changed substantially. Table 2 summarises that in the pre-intervention years, a typical mother would have less than two live births including her latest birth, would be about 24 years old, with at most four years of schooling and belonging to a household in the third quintile of wealth distribution. In the post-intervention years, however, a typical mother would have, on average 2.7 births, including her latest, and would be about 25 years old. Concerning other characteristics such as maximum schooling years and wealth level of the household, the average remained the same as in pre-intervention years. With regards to characteristics such as religion, the averages pre and post-intervention remain similar, - with almost 80% of the mothers being Hindu by religion and about 14% mothers being Muslim. However, rural births increased from 74% to over 80%, and the socially backward classes representation rose from 74 to 77% after the programme started.

The increase in the average live births by a mother (1.42 to 2.68) in Table 2 suggests that mothers' birth rate rose post-intervention. Nonetheless, the presence of a similar percentage of eligible mothers (any type) both in the pre and post-intervention columns suggest that all groups, including the *Ineligibles*, experienced this increase in the birth rate. Closer scrutiny *(not shown here)* reveals that in each eligible group, the mean of total births by the mother increased, but remained below 2 in the post-intervention period. The only exceptions were $AddEligiblewithASHA_{ij}$ (i.e. the second phase eligibles in LPS) whose mean jumped from 1.65 to 3.13 and the *Ineligibles* who moved from an average of 1.38 to 2.27, thus pushing the average across all groups over 2.[23]

[23] As an aside, one could argue that the increase in the birth rate in all the treatment groups is an unanticipated effect of the intervention itself (i.e. it made giving birth a more accessible choice in the families). However, a similar increase in the mean birth rate among the *Ineligibles* does not confirm that it is exclusively due to the intervention.

Table 1: Summary statistics of key variables before intervention

Variables	(1) Ineligibles	(2) Eligible withoutASHA	(3) Eligible withASHA	(4) AddEligible withoutASHA	(5) AddEligible withASHA
	Mean(SD)	Mean(SD)	Mean(SD)	Mean(SD)	Mean(SD)
Dependent variables					
Institutional births	0.25 (0.43)	0.21 (0.40)	0.06 (0.24)	0.27 (0.45)	0.15 (0.36)
Atleast 3 ANCs	0.80 (0.40)	0.56 (0.50)	0.16 (0.37)	0.69 (0.46)	0.38 (0.49)
Breastfed at birth	0.43 (0.49)	0.39 (0.49)	0.13 (0.34)	0.41 (0.49)	0.21 (0.40)
Child received BCG	0.88 (0.33)	0.74 (0.44)	0.43 (0.49)	0.84 (0.37)	0.64 (0.48)
Individual control vars.					
Total births by mother	1.38 (0.99)	1.07 (0.26)	1.04 (0.18)	1.07 (0.26)	1.65 (1.56)
Age during latest birth	23.40 (4.63)	24.23 (4.61)	25.81 (5.27)	22.25 (4.59)	24.03 (5.62)
Mother's schooling yrs.	6.85 (4.99)	1.89 (3.56)	0.90 (2.48)	4.56 (4.70)	3.82 (4.88)
Hindu	0.75 (0.43)	0.80 (0.40)	0.88 (0.33)	0.82 (0.38)	0.83 (0.38)
Muslim	0.16 (0.37)	0.15 (0.36)	0.11 (0.31)	0.20 (0.14)	0.15 (0.36)
SC/ST/OBC	0.56 (0.50)	0.80 (0.40)	0.88 (0.33)	0.99 (0.08)	0.75 (0.44)
Wealth quintile	3.73 (1.12)	1 (0)	1 (0)	3.05 (1.11)	2.97 (1.26)
Rural	0.64 (0.48)	1 (0)	0.91 (0.28)	0.76 (0.43)	0.72 (0.45)
Observations	40172	4124	25209	14600	80401

Notes: Data from DLHS 2 and 3. The sample consists of mothers with reported last birth between 2001-2004.

Table 2: Summary statistics of key variables pre and post intervention

Variables	Births post-intervention Mean (SD)	Births pre-intervention Mean (SD)	Std. Difference
Total births by mother	2.68 (1.89)	1.42 (1.22)	0.79
Age during latest birth	24.85 (5.22)	23.99 (5.31)	0.16
Max.schooling yrs. of mother	4.31 (4.92)	4.13 (4.95)	0.04
Hindu	0.80 (0.40)	0.81 (0.39)	-0.03
Muslim	0.14 (0.35)	0.14 (0.34)	0.02
SC/ST/OBC	0.77 (0.42)	0.74 (0.44)	0.07
Wealth quintile	2.80 (1.41)	2.81 (1.41)	-0.01
Rural	0.82 (0.38)	0.74 (0.44)	0.20
Eligible for one/both package	0.74 (0.44)	0.76 (0.43)	-0.03
Observations	280840	102465	

Notes: Data from DLHS 2 and 3. The sample consists of mothers with reported last birth between 2001-2008. The last column gives the standardised difference in means.

5 MAIN RESULTS

This section presents the main results based on **Equation (1)**. As a general overview, the results suggest that the introduction of cash assistance had a positive and statistically significant effect on the eligible women in both phases of the programme in comparison to the women who never became eligible. Evidence shows that eligible women in the LPS, i.e. those who received both the cash assistance as well as the ASHA's counselling, benefitted more than the

eligible women in the HPS, i.e. those who only received the cash assistance.

In the following subsections, I discuss the effects on different outcome variables.

5.1 Institutional births

Table 3 Column (1) reports the effect on public institutional births. The coefficient of interaction between *Eligible without ASHA* and $Post1$ indicates that the increase in the institutional birth rate among the first phase eligible mothers in a high-performing state where ASHA was not available, was 2.9 pp.($p = 0.024$) greater than that among the control group *Ineligibles*. In contrast, the coefficient of interaction between *Eligible with ASHA* and $Post1$ indicates that the increase among the first phase eligibles in a low-performing state where ASHA was available, was 7.1 pp. ($p = 0.000$) larger than that among *Ineligibles*. Therefore, the first set of mothers who were eligible for the cash assistance as well as the ASHA's advice experienced more than two times increase in institutional birth rates than that by the first set of mothers who only had the cash assistance. Given only 25% of ineligible mothers gave birth at public health facilities before the intervention, this increase was about 11% in the HPS and about 28% in the LPS.

Now, we look at the mothers who were not eligible for the programme benefits in the earlier phase but became eligible from the end of 2006. Among them, the ones who only received cash assistance (i.e. the ones in the HPS) experienced a 1.8 pp. ($p = 0.039$) increase in institutional births and the ones who received both the cash assistance and ASHA's guidance, experienced 6.7 pp. ($p = 0.006$) increase relative to the ineligible mothers. It indicates that receiving the cash assistance helped the mothers in opting for birth at public institutions, and those who received the additional ASHA counselling showed an even better outcome. Now, any further conclusion regarding the separation of channels of effects requires caution here, as it needs comparison across the treatment groups which are compositionally quite different from each other.

It is hard to ignore that the various treatment groups, along with the control group, are compositionally quite different from each other in terms of their socio-economic indicators, birth history and residence. (For reference, see the various group compositions in **Figure B3** of **Appendix 2**) Firstly, the control group, i.e. the *Ineligibles* consists of a very heterogeneous mix of mothers who are either above-poverty-line, or below-poverty-line but below 19 years of age, or not in a socially backward class, or with more than two births, in the rural and urban areas of HP states. This composition is quite different than, say, the *Eligible with ASHA* group, who are below-poverty-line and above 19 years old, with up to two live births in the rural and urban LP states. Therefore, this

Table 3: Effect on Institutional births

Dependent variable:	If mother gave birth at public health facility		
	(1)	(2)	(3)
First phase eligibles			
Eligible without ASHA	0.008	0.017*	0.001
	(0.012)	(0.010)	(0.012)
Eligible with ASHA	0.011	-0.048	0.005
	(0.045)	(0.048)	(0.054)
Eligible without ASHA*Post1	0.029**	0.023*	0.009
	(0.013)	(0.013)	(0.013)
Eligible with ASHA*Post1	0.071***	0.058***	0.050***
	(0.007)	(0.007)	(0.008)
Second phase eligibles			
Additional Eligible without ASHA	0.066***	0.048***	0.045***
	(0.007)	(0.007)	(0.008)
Additional Eligible with ASHA	0.047	-0.032	0.034
	(0.045)	(0.049)	(0.054)
Additional Eligible without ASHA*Post2	0.018**	0.021**	0.020**
	(0.009)	(0.010)	(0.010)
Additional Eligible with ASHA*Post2	0.067***	0.069***	0.078***
	(0.006)	(0.007)	(0.008)
Constant	0.338***	0.389***	0.376***
	(0.032)	(0.033)	(0.033)
State FE	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
F-test of equality between interaction coefficients of first phase eligibles			
F-statistic	7.88	5.96	7.70
p-value	0.005	0.015	0.006
F-test of equality between interaction coefficients of second phase eligibles			
F-statistic	19.26	16.50	21.79
p-value	0.000	0.000	0.000
Baseline mean of Ineligibles	0.250	0.237	0.239
Observations	326338	255433	181590
R^2	0.118	0.124	0.126

Notes: (1) The unit of observation is the mother who had her latest birth between 2001-2008. **(2)** Column (1) presents the estimates of a regression with the entire sample, Column (2) with only rural mothers and Column (3) with rural mothers with up to two live births. **(3) EligiblewithoutASHA** and **EligiblewithASHA** denote the respective pre-intervention difference in means of the first phase eligible mothers in HPS and LPS with the control group (Ineligibles). **EligiblewithoutASHA*Post1** and **EligiblewithASHA*Post1** denote the respective difference-in-difference effect of the programme on the first phase eligible mothers in HPS and LPS. **Add.EligiblewithoutASHA** and **Add.EligiblewithASHA** denote the respective pre-intervention difference in means of the second phase eligible mothers in HPS and LPS with the control group (Ineligibles).**Add.EligiblewithoutASHA*Post2** and **Add.EligiblewithASHA*Post2** denote the respective difference-in-difference effect of the programme on the second phase eligible mothers in HPS and LPS. **Post1** and **Post2** denote the births taking place after the onset of first guideline (Apr 2005) and the ones after the onset of the second guideline (Oct 2006). **(4)** An F-test of equality between the coefficients **EligiblewithoutASHA*Post1** and **EligiblewithASHA*Post1** (i.e.the first phase eligibles) show that they significantly different from each other (below 1% level). A similar F-test of equality between the coefficients **Add.EligiblewithoutASHA*Post2** and **Add.EligiblewithASHA*Post2** (i.e.the second phase eligibles) show that they significantly different from each other (below 1% level). **(5)** Control variables include number of live births the mother has had, maximum schooling years of the mother, her religion, if her family belongs to one of the socially backward classes, the wealth quantile of her household, and some village-specific health infra-structure controls like presence of other child welfare programme in the village, distance to the nearest primary health centre, community health centre, to the nearest district hospital. **(6)** Standard errors clustered at district level within parentheses. **(7)** The unadjusted R^2 values are reported here. **(8)** The mean of the outcome variable in the control group (Ineligibles) in the pre-intervention period is reported. **(9)** ***,**,* implies p < 0.01, <0.05, < 0.10 respectively.

differential selection into the control and treatment groups might lead to some discrepancy in the interpretation of results. Secondly, the issue gets even more pronounced when one tries to decipher the additional effect of the ASHA's presence by comparing the eligible mothers in LPS with that in HPS. Under these circumstances, trying to homogenise the composition of the groups along the demographic indicators of the mothers could offer some solution to this twofold problem.

It is logical to expect that rural women do not vary much in their socio-economic indicators. Table 3 Column (2) shows the estimates of the same analysis but restricted to only rural mothers. However, the effect sizes remain more or less identical to those found with the entire sample.[24]

A further constricted sample with rural and relatively new mothers, i.e. with less than two births, gives similar effect sizes for the respective treatment groups (Column (3)). However, note that the estimates of the first phase eligible mothers in the HPS lose their statistical significance in the sub-samples (Columns (2) and (3)). Thus, one cannot reject the possibility that the mother's incentive alone does not affect the institutional birth rate of these mothers.

Note that, when the groups are now more homogeneous with rural (and with less than two births) mothers, the first phase groups - *Eligible without ASHA* and *Eligible with ASHA* become demographically identical, i.e. they are below-poverty-line, above 19 years old, have less than two births and belong to rural areas. Due to this homogeneity, we can stress that the added effect seen in the eligible mothers in LPS compared to HPS is not due to any other systematic difference between the state-types, but due to the ASHA's presence. Moreover, with this same argument that the first phase mothers in the LPS and HPS do not differ demographically in this sub-sample, we can minimise the possibility that the LPS mothers see better results due to the combined effect of the cash assistance and the ASHA's presence. The confidence to claim the latter possibility comes from the finding that only a cash transfer to such mothers does not help improve institutional birth rates in the HPS; in contrast, an additional ASHA's presence does that in the LPS. Besides, the fact that the amount of cash transfer to these mothers was the same (700 INR) across the LPS and HPS, further consolidates that the added effect on the rural mothers (with less than two births) in the LPS must be through the ASHA.[25]

[24] As an aside, the only difference in Column (2) compared to Column (1) is that the respective baseline differences in means of the eligible mothers in LPS of both phases with the control group are now negative (see the coefficients of *Eligible with ASHA* and *Add.Eligible with ASHA*). It suggests that in the case of the whole sample (Column (1)), the relatively large baseline mean in institutional births in these two treatment groups is contributed by the urban population in the LPS. Moreover, in the rural sample, this anticipated negative difference in the baseline means in births of the eligible women in LPS to the ineligible women confirms the diagnosis that the women in the rural LPS had worse institutional birth outcomes in the first place.

[25] Note when only the rural (and with less than two births) sub-sample is considered, the control group of *Ineligibles* still consists of a heterogeneous mix of mothers who are either above-poverty-line, or

Having said that, it is to be noted that the use of the sub-sample of rural (and with less than two births) mothers renders only the first phase mothers in the LPS and HPS to be demographically identical. There, however, remains some difference in this regard among the second phase mothers across LPS and HPS.

5.2 Antenatal care

In this section, I discuss the effect of the programme on the utilisation of ANC check-ups by the mothers.

The outcome variable considered here is a binary variable that takes value 1 for mothers who received at least three ANCs. **Table 4** reports the effect of the programme on antenatal care outcomes of the different treatment groups.[26] In Column (1), the coefficient of interaction between *Eligible without ASHA* and *Post*1 indicates that the increase in the probability of having at least three ANCs by this group was 4.4 pp. ($p = 0.026$) more than the *Ineligibles*. In contrast, a similar interaction of the time variable with *Eligible with ASHA* indicates that the increase in the probability of receiving ANC by this group was 13.1 pp.($p = 0.000$) higher than that experienced by the *Ineligibles*. Given that only 79.6% of the control group mothers had at least three ANCs in the baseline, the increase in the HPS mothers was 5.5% and about 16% in the LPS mothers.

Among the mothers who only became eligible from the end of 2006, those who received cash assistance only (i.e. the ones in the HPS) experienced a 1.2 pp. ($p = 0.090$) increase in ANC uptake and the ones who received both the cash assistance and ASHA's counsel, experienced a 4.0 pp. ($p = 0.000$) increase relative to the ineligible mothers.

The above findings indicate that receiving cash assistance helped the mothers significantly in utilising the ANC services. Nevertheless, those who received the additional ASHA counselling showed better outcomes. This pattern was similar in both phases.

Intending to conclude better on the ASHA's channel, I further examine if a similar result persists in a relatively more homogenised sample. Column (2) of Table 4 consists of analysis with only rural mothers, and Column (3) consists of analysis with rural mothers with less than two live births. We see that the DiD coefficients remain more or less unchanged in magnitude. However, only the findings on the eligible mothers in the low-performing state hold their precision in both phases (Columns (2) and (3)). As found in the case of institutional birth,

below-poverty-line but below 19 years of age, or not in a socially backward class, (or with more than two births) in the rural areas of HP states. However, drawing the above inference should not be a problem if the control group differs by some extent in composition, as long as we compare the eligible mothers in LPS and HPS with the same control group.

[26] Note that the number of observations used in the ANC analysis is smaller than that in institutional births because of missing data.

Table 4: Effect on Antenatal care

Dependent variable:	If mother received at least three ANCs		
	(1)	(2)	(3)
First phase eligibles			
Eligible without ASHA	0.024	0.009	0.011
	(0.016)	(0.015)	(0.015)
Eligible with ASHA	-0.115***	-0.145***	-0.138***
	(0.033)	(0.035)	(0.044)
Eligible without ASHA*Post1	0.044**	0.035*	0.030
	(0.020)	(0.019)	(0.019)
Eligible with ASHA*Post1	0.131***	0.115***	0.116***
	(0.009)	(0.010)	(0.010)
Second phase eligibles			
Additional Eligible without ASHA	0.014***	0.005	0.001
	(0.006)	(0.006)	(0.006)
Additional Eligible with ASHA	-0.070**	-0.104***	-0.116***
	(0.034)	(0.036)	(0.045)
Additional Eligible without ASHA*Post2	0.012*	0.009	0.009
	(0.007)	(0.009)	(0.009)
Additional Eligible with ASHA*Post2	0.040***	0.041***	0.068***
	(0.006)	(0.007)	(0.008)
Constant	0.650***	0.703***	0.687***
	(0.033)	(0.033)	(0.033)
State FE	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
F-test of equality between interaction coefficients of first phase eligibles			
F-statistic	15.70	13.40	16.11
p-value	0.000	0.000	0.000
F-test of equality between interaction coefficients of second phase eligibles			
F-statistic	9.83	9.80	27.94
p-value	0.002	0.002	0.000
Baseline mean of Ineligibles	0.796	0.755	0.759
Observations	262024	198977	156835
R^2	0.301	0.280	0.313

Notes: **(1)** The unit of observation is the mother who had her latest birth between 2001-2008. **(2)** Column (1) presents the estimates of a regression with the entire sample, Column (2) with only rural mothers and Column (3) with rural mothers with up to two live births. **(3)** **EligiblewithoutASHA** and **EligiblewithASHA** denote the respective pre-intervention difference in means of the first phase eligible mothers in HPS and LPS with the control group (Ineligibles). **EligiblewithoutASHA*Post1** and **EligiblewithASHA*Post1** denote the respective difference-in-difference effect of the programme on the first phase eligible mothers in HPS and LPS. **Add.EligiblewithoutASHA** and **Add.EligiblewithASHA** denote the respective pre-intervention difference in means of the second phase eligible mothers in HPS and LPS with the control group (Ineligibles).**Add.EligiblewithoutASHA*Post2** and **Add.EligiblewithASHA*Post2** denote the respective difference-in-difference effect of the programme on the second phase eligible mothers in HPS and LPS. **Post1** and **Post2** denote the births taking place after the onset of first guideline (Apr 2005) and the ones after the onset of the second guideline (Oct 2006). **(4)** An F-test of equality between the coefficients **EligiblewithoutASHA*Post1** and **EligiblewithASHA*Post1** (i.e.the first phase eligibles) show that they significantly different from each other (below 1% level). A similar F-test of equality between the coefficients **Add.EligiblewithoutASHA*Post2** and **Add.EligiblewithASHA*Post2** (i.e.the second phase eligibles) show that they significantly different from each other (below 1% level). **(5)** Control variables include number of live births the mother has had, maximum schooling years of the mother, her religion, if her family belongs to one of the socially backward classes, the wealth quantile of her household, and some village-specific health infra-structure controls like presence of other child welfare programme in the village, distance to the nearest primary health centre, community health centre, to the nearest district hospital. **(6)** Standard errors clustered at district level within parentheses. **(7)** The unadjusted R^2 values are reported here. **(8)** The mean of the outcome variable in the control group (Ineligibles) in the pre-intervention period is reported. **(9)** ***,**,* implies p < 0.01, <0.05, < 0.10 respectively.

this last finding on ANCs again underlines the possibility that when we consider the sub-sample(s) of only the rural mothers (and with less than two births), it is through the ASHA that the effect takes place.

5.3 Breastfeeding practices

In **Table 5** Column (1), with the entire sample, there was no statistically significant difference in the average increase in effect for the first set of eligible mothers in the HPS where there was no ASHA. However, the first set of eligible mothers in the LPS (who were 24.5 pp.($p = 0.000$) less likely than the ineligible mothers to start breastfeeding within an hour in the baseline) experienced a disproportionately large increase of size 3.6 pp.($p = 0.001$) in the probability of early breastfeeding in the post-intervention phase.

Among the second phase eligible mothers, the ones in the HPS did not experience any statistically significant difference in effect than the *Ineligibles*. In contrast, the eligible mothers in the LPS experienced an increase which is 3.2 pp.($p = 0.000$) higher than their ineligible counterparts. Therefore, to summarise, it is only the eligible mother in the LPS in both phases, with the ASHA by her side, who experienced a statistically significant effect of the programme on early breastfeeding practices.[27]

Similar results persist when only the mothers in rural areas are considered, and also when rural mothers with less than two live births are considered (Table 5 Columns (2) and (3)).

Here, we clearly see that there was no effect of only the mother's package on the HPS mothers. Furthermore, the analysis with rural mothers (with less than two live births) confirms that if there was no effect of the cash transfer to the mother in the HPS, it was likely not to affect a demographically similar mother in the LPS either. So whatever effect we see in the LPS, was through the ASHA's presence.

5.4 BCG vaccinations

I examine if the programme had any effect on BCG vaccination given to the newborns. As already mentioned, to make the relevant variable on BCG coherent across the two waves of DLHS survey, I use a binary variable that only indicates whether the child received the BCG vaccine or not.

Let us first discuss the estimates for the mothers who became eligible according to the first guideline, as reported in **Table 6** Column (1). The eligi-

[27] Note, however, the estimated post-intervention difference in means with the control group remains negative for the eligibles of LPS in both phases (main effect+interaction effect < 0).

Table 5: Effect on Breastfeeding practices

Dependent variable:	If child was breastfed within 1-2 hours of birth		
	(1)	(2)	(3)
First phase eligibles			
Eligible without ASHA	0.042***	0.037***	0.038***
	(0.015)	(0.015)	(0.014)
Eligible with ASHA	-0.245***	-0.252***	-0.268***
	(0.053)	(0.053)	(0.065)
Eligible without ASHA*Post1	-0.006	-0.013	-0.018
	(0.019)	(0.018)	(0.018)
Eligible with ASHA*Post1	0.036***	0.030***	0.029***
	(0.010)	(0.010)	(0.011)
Second phase eligibles			
Additional Eligible without ASHA	0.021***	0.011	0.019**
	(0.008)	(0.009)	(0.009)
Additional Eligible with ASHA	-0.238***	-0.248***	-0.263***
	(0.055)	(0.054)	(0.067)
Additional Eligible without ASHA*Post2	-0.004	-0.006	0.003
	(0.010)	(0.011)	(0.011)
Additional Eligible with ASHA*Post2	0.032***	0.028***	0.046***
	(0.007)	(0.008)	(0.010)
Constant	0.688***	0.703***	0.661***
	(0.035)	(0.035)	(0.034)
State FE	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
F-test of equality between interaction coefficients of first phase eligibles			
F-statistic	3.60	4.00	4.97
p-value	0.058	0.046	0.026
F-test of equality between interaction coefficients of second phase eligibles			
F-statistic	8.03	6.68	8.12
p-value	0.005	0.010	0.005
Baseline mean of Ineligibles	0.426	0.420	0.415
Observations	309440	242111	171327
R^2	0.173	0.177	0.186

Notes: **(1)** The unit of observation is the mother who had her latest birth between 2001-2008. **(2)** Column (1) presents the estimates of a regression with the entire sample, Column (2) with only rural mothers and Column (3) with rural mothers with up to two live births. **(3)** **EligiblewithoutASHA** and **EligiblewithASHA** denote the respective pre-intervention difference in means of the first phase eligible mothers in HPS and LPS with the control group (Ineligibles). **EligiblewithoutASHA*Post1** and **EligiblewithASHA*Post1** denote the respective difference-in-difference effect of the programme on the first phase eligible mothers in HPS and LPS. **Add.EligiblewithoutASHA** and **Add.EligiblewithASHA** denote the respective pre-intervention difference in means of the second phase eligible mothers in HPS and LPS with the control group (Ineligibles).**Add.EligiblewithoutASHA*Post2** and **Add.EligiblewithASHA*Post2** denote the respective difference-in-difference effect of the programme on the second phase eligible mothers in HPS and LPS. **Post1** and **Post2** denote the births taking place after the onset of first guideline (Apr 2005) and the ones after the onset of the second guideline (Oct 2006). **(4)** An F-test of equality between the coefficients **EligiblewithoutASHA*Post1** and **EligiblewithASHA*Post1** (i.e.the first phase eligibles) show that they significantly different from each other (below 5% level). A similar F-test of equality between the coefficients **Add.EligiblewithoutASHA*Post2** and **Add.EligiblewithASHA*Post2** (i.e.the second phase eligibles) show that they significantly different from each other (below 1% level). **(5)** Control variables include number of live births the mother has had, maximum schooling years of the mother, her religion, if her family belongs to one of the socially backward classes, the wealth quantile of her household, and some village-specific health infra-structure controls like presence of other child welfare programme in the village, distance to the nearest primary health centre, community health centre, to the nearest district hospital. **(6)** Standard errors clustered at district level within parentheses. **(7)** The unadjusted R^2 values are reported here. **(8)** The mean of the outcome variable in the control group (Ineligibles) in the pre-intervention period is reported. **(9)** ***,**,* implies p < 0.01, <0.05, < 0.10 respectively.

Table 6: Effect on BCG vaccination

Dependent variable:	If the newborn received BCG vaccine		
	(1)	(2)	(3)
First phase eligibles			
Eligible without ASHA	0.043***	0.039**	0.040***
	(0.017)	(0.016)	(0.015)
Eligible with ASHA	-0.071***	-0.076***	-0.051
	(0.028)	(0.030)	(0.035)
Eligible without ASHA*Post1	0.043**	0.025	0.004
	(0.019)	(0.019)	(0.018)
Eligible with ASHA*Post1	0.224***	0.206***	0.190***
	(0.011)	(0.011)	(0.011)
Second phase eligibles			
Additional Eligible without ASHA	0.037***	0.037***	0.039***
	(0.005)	(0.006)	(0.006)
Additional Eligible with ASHA	0.029	0.018	0.027
	(0.027)	(0.030)	(0.035)
Additional Eligible without ASHA*Post2	0.008	-0.002	0.002
	(0.007)	(0.009)	(0.009)
Additional Eligible with ASHA*Post2	0.063***	0.059***	0.087***
	(0.005)	(0.006)	(0.008)
Constant	0.657***	0.678***	0.551***
	(0.040)	(0.040)	(0.041)
State FE	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes
Controls	Yes	Yes	Yes
F-test of equality between interaction coefficients of first phase eligibles			
F-statistic	67.10	67.06	73.99
p-value	0.000	0.000	0.000
F-test of equality between interaction coefficients of second phase eligibles			
F-statistic	37.23	36.21	58.95
p-value	0.000	0.000	0.000
Baseline mean of Ineligibles	0.877	0.867	0.865
Observations	326338	255433	181590
R^2	0.154	0.147	0.190

Notes: (1) The unit of observation is the mother who had her latest birth between 2001-2008. **(2)** Column (1) presents the estimates of a regression with the entire sample, Column (2) with only rural mothers and Column (3) with rural mothers with up to two live births. **(3) EligiblewithoutASHA** and **EligiblewithASHA** denote the respective pre-intervention difference in means of the first phase eligible mothers in HPS and LPS with the control group (Ineligibles). **EligiblewithoutASHA*Post1** and **EligiblewithASHA*Post1** denote the respective difference-in-difference effect of the programme on the first phase eligible mothers in HPS and LPS. **Add.EligiblewithoutASHA** and **Add.EligiblewithASHA** denote the respective pre-intervention difference in means of the second phase eligible mothers in HPS and LPS with the control group (Ineligibles).**Add.EligiblewithoutASHA*Post2** and **Add.EligiblewithASHA*Post2** denote the respective difference-in-difference effect of the programme on the second phase eligible mothers in HPS and LPS. **Post1** and **Post2** denote the births taking place after the onset of first guideline (Apr 2005) and the ones after the onset of the second guideline (Oct 2006). **(4)** An *F*-test of equality between the coefficients **EligiblewithoutASHA*Post1** and **EligiblewithASHA*Post1** (i.e.the first phase eligibles) show that they significantly different from each other (below 1% level). A similar *F*-test of equality between the coefficients **Add.EligiblewithoutASHA*Post2** and **Add.EligiblewithASHA*Post2** (i.e.the second phase eligibles) show that they significantly different from each other (below 1% level). **(5)** Control variables include number of live births the mother has had, maximum schooling years of the mother, her religion, if her family belongs to one of the socially backward classes, the wealth quantile of her household, and some village-specific health infra-structure controls like presence of other child welfare programme in the village, distance to the nearest primary health centre, community health centre, to the nearest district hospital. **(6)** Standard errors clustered at district level within parentheses. **(7)** The unadjusted R^2 values are reported here. **(8)** The mean of the outcome variable in the control group (Ineligibles) in the pre-intervention period is reported. **(9)** ***,**,* implies p < 0.01, <0.05, < 0.10 respectively.

ble mothers in a high-performing state with no ASHA present experienced 4.3 pp.($p = 0.022$) increase in the BCG vaccination of their newborns. On the other hand, the eligible mothers in the low-performing state, who had a baseline mean difference of 7.1 pp.($p = 0.010$) with the *Ineligibles*, experienced a disproportionately large difference in the effect of size 22.4 pp.($p = 0.000$).

Among the mothers who were not eligible at first but later became so according to the second guideline, those in the LPS experienced a relatively large difference in effect (6.8 pp.($p = 0.000$)) than the *Ineligibles*, compared to their counterparts in the HPS who did not experience any significant effect.

Furthermore, by restricting the sample to only rural mothers, and then to rural mothers with less than two live births, the results do not change in any meaningful way. However, only the findings on the eligible mothers in LPS hold their precision in both phases (Columns (2) and (3) respectively of Table 6). This result is again quite similar to the findings of the other outcome variables. Therefore, similarly again, we can conclude on the superiority of the ASHA's channel on the rural mothers (with less than two live births).

Summarising the results of Section 5, we can say that the eligible mothers in a low-performing state experienced large effects in all outcomes. In contrast, the eligible mothers in a high-performing state had a relatively small effect (in institutional birth, ANC and BCG) or no effect (in breastfeeding). Also, the effect sizes are more prominent for the first phase mothers in comparison to the second phase mothers. Finally, examining the effects across a more comparable sub-sample of rural mothers (and with less than two births), the effect remains statistically significant only for the eligible mothers in the LPS. This last finding implies that the mother's package alone did not affect those mothers. Also, since it is possible to compare demographically identical mothers across the LPS and HPS (only the first phase ones) with the sub-sample of rural mothers (and with less than two births), we can confidently attribute the effect found in the LPS to the ASHA's performance. This is under the assumption that demographically identical mothers in rural areas (and with up to two births) are otherwise unlikely to behave differently across LPS and HPS.

6 ROBUSTNESS CHECKS

In this section, I discuss various issues that could bias the above findings and the measures that I take to mitigate them. In Section 6.1, I argue the causality of the effects found, by validating the parallel pre-trends assumption of a difference-in-difference strategy. In Section 6.2, I return to the issue of heterogeneity in the

observable characteristics of the mothers that could render the findings to suffer from differential selection bias. Therefore, as a robustness measure, I combine coarsened exact matching with DiD. Finally, in Section 6.3, I return to the issue of systematic differences in the low and high-performing states that could potentially bias my conclusion. As a solution, I present an extended analysis with only the HPS sample before and after the introduction of ASHA and compare the findings with the previous ones.

6.1 Parallel pre-trend issue

The identifying assumption of a DiD approach is that the growth of the outcome variables would follow the same trend in the treatment and control groups in the absence of the intervention. Under this assumption, it is possible to decipher the average causal effect on the treated in the post-intervention period. Although the very construction of the assumption renders itself untestable, a typical way of getting the closest validation is to check if the outcomes of interest evolved with a similar trend in the treatment and control groups in the pre-intervention years.

When it comes to the JSY programme, the eligibility was determined based on the health outcomes, e.g. institutional births; therefore, it is evident that the different eligible groups should be at different levels in the pre-intervention period. However, as long as we can ensure that the pre-trends are parallel across the groups, the estimated effects should be unbiased. While a mere visual inspection of the trends of the outcome variables across the groups is a good starting point, a more robust way to verify this is to plot the estimated coefficients of the interaction of the respective group and year of the latest birth by the mother. Also, as there are multiple treatment groups in the model, it is more meaningful to investigate the plots of the relevant coefficients in the estimated model than to look at the raw trend plots of each treatment category with respect to the control group.

I estimate a variant of Equation (1) as follows:

$$Y_{ijt} = \alpha + \mu_t + \beta_1 EligiblewithoutASHA_{ij} + \sum_{t=2001}^{2008} \beta_t EligiblewithoutASHA_{ij} * lastbirthyear_t + \beta_2 EligiblewithASHA_{ij} + \sum_{t=2001}^{2008} \beta_t EligiblewithASHA_{ij} * lastbirthyear_t + \beta_3 AddEligiblewithoutASHA_{ij} + \sum_{t=2001}^{2008} \beta_t AddEligiblewithoutASHA_{ij} * lastbirthyear_t + \beta_4 AddEligiblewithASHA_{ij} + \sum_{t=2001}^{2008} \beta_t AddEligiblewithASHA_{ij} * lastbirthyear_t + X'_{ij}\delta + \gamma_j + \epsilon_{ijt} \quad (2)$$

Note in **Equation (2)**, $lastbirthyear_t$ are the year dummies of the latest births by the mothers. The other notations bear the same meaning as in Equation (1).

6.1.1 Institutional birth pre-trends

Figure 2 suggests no statistically significant difference in the effect on institutional birth for the first phase eligible mothers in the high-performing state as well as those in the low-performing state in comparison to the ineligible mothers, before the intervention started. This finding confirms the presence of parallel pre-trends in institutional births for the first phase eligible mothers compared to the ineligible mothers. It is only post-2005, i.e. after the roll-out of the first phase, one can see some positive difference in effect for these two eligible groups. The parallel pre-trend assumption also holds strong for the group of mothers that became eligible in the second phase in the LPS in late-2006; until that year, the interaction coefficient remains statistically insignificant. However, the assumption breaks for the group of second phase eligible mothers in HPS. As plot (c) of Figure 2 suggests, the difference in means with the control group already becomes statistically significant for this treatment group in 2005, which is one year before this particular group became eligible for the programme benefits. Perhaps, the finding is not that surprising. Since the first phase eligible mothers started receiving the programme benefits already in 2005, it could be attributed to a positive spillover of information that led these otherwise socio-economically similar mothers in the urban areas to opt for institutional births already in 2005. (Note: in an HPS, a share of the newly eligible second phase mothers are socio-economically almost similar to the first phase eligible mothers; they only differ in that they come from the urban areas.) Furthermore, it could be so that due to fuzzy information on mother's eligibility in the first phase, the below-poverty-line mothers in urban areas of HPS also believed that they were eligible in the first phase, while in reality, only their rural counterparts

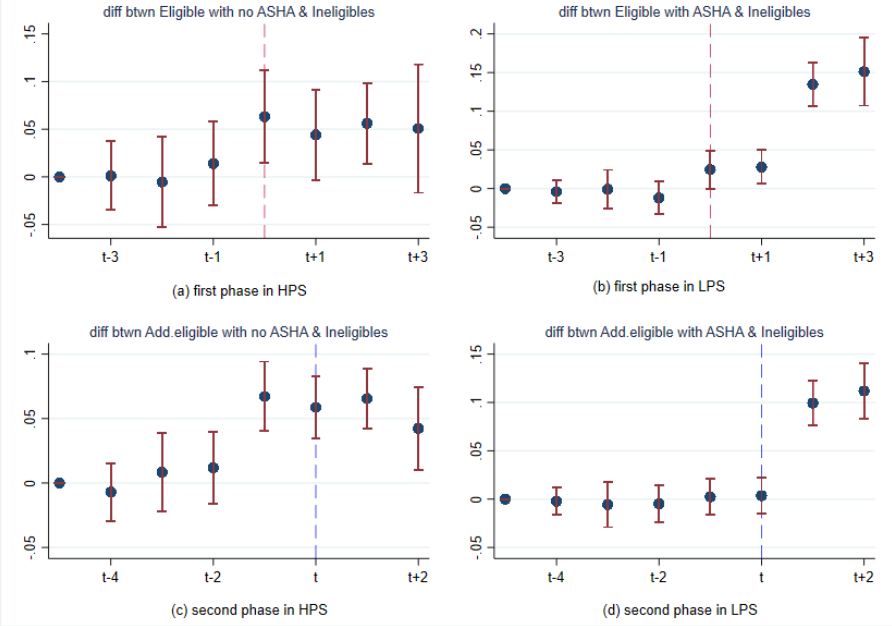


Figure 2: Time-varying difference in institutional births between the respective treatment groups and control group.

Notes: This figure shows the difference between each treated sub-group and the control group in the probability of giving birth at public healthcare institutions during 2001-2008. The two figures in the upper panel show the intervention onset in t=2005 for the first phase eligibles, and the two figures below show the intervention onset in t=2006 for the second phase eligibles.

were.

6.1.2 Antenatal care pre-trends

Figure 3 invalidates the prevalence of parallel trends across all the groups, in the pre-intervention phase, except for the first phase eligibles in the HPS. Focusing on plots (a) and (b) of Figure 3 which investigate the first phase mothers, one can see that there was some clear effect on the two eligible groups' ANC utilisation, especially in the LPS. However, that particular treatment group, i.e. *Eligible with ASHA* seems to have evolved in its ANC uptake in a different way than the *Ineligibles*, in the baseline. As a result, one cannot stress the causality in the effect of the programme on this group's ANC take-up. While plots (c) and (d) in the same graph indicate some positive spillover effect already in 2005 on the second phase eligibles, a statistically significant difference in the means of the eligibles in the LPS and the control group already in years 2003 and 2004 cautions us from drawing any causal inference.

Furthermore, a closer scrutiny of the raw pre-trends of the variable of interest

(**Figure A1** plot (a) in **Appendix 1**) shows that the *Ineligibles* and the *Additional Eligible with ASHA* (i.e. the second phase eligibles in LPS) followed a different trend than the other three groups; while the latter showed an upward trend in this outcome, the former showed a dip in the year 2004. Though there is no firm explanation for this, one must note that the three groups *Eligible with ASHA, Eligible without ASHA* and *Additional Eligible without ASHA* (i.e. the first phase eligibles in both state-types and the second phase eligible in HPS) comprise mostly of mothers with similar characteristics, i.e. with up to two live births, belonging to below-poverty-line and socially disadvantaged families. On the other hand, the other two groups *Additional Eligible with ASHA* and *Ineligible* comprise of a heterogeneous mix of mothers (above-poverty-line, or not socially disadvantaged or more than two live births). Another raw plot (b) of pre-trends by using the sub-sample of only rural mothers with up to two live births, however, gives a more comparable sketch of the trends across the groups. Thus again referring to the full sample plot (a), the socio-economically disadvantaged mothers with less than two births, appear to have received information and/or service related to ANC in a different way than the other mothers during 2004. The simultaneous dip in the uptake of ANC in 2004 in the two groups *Ineligibles* and the *Additional Eligible with ASHA* in the plot (a), could also imply reallocation of limited supply resources in the year immediately before the intervention was announced.

6.1.3 Breastfeeding practices pre-trends

As given by the event-study plots in **Figure 4**, the parallel pre-trends assumption holds for the first phase eligibles in both state-types. It only fails in case of the second phase eligible mothers in the LPS. Arguably, the smoothly rising trend in the last plot of the graph most likely captures the general increasing trend in early breastfeeding practices of the newly eligible mothers of the second phase in the LPS, because this particular treatment group is basically a representation of all mothers who gave birth during the second phase, irrespective of their socio-economic backgrounds or birth history.

6.1.4 BCG vaccination pre-trends

From the event study plots in **Figure 5**, we can see that there is no statistically significant difference in the receipt of BCG vaccinations in the pre-intervention years for the first phase eligible mothers in the LPS, in comparison to the ineligible mothers (see plot(b)). However, all other eligible groups seem to have evolved in a different path in their BCG uptake. Again a closer investigation of the raw pre-intervention trends reveals that all groups suffered a drop in BCG uptake in 2004, some of them quite sharp ones compared to the control group (**Appendix 1 Figure A2**). Even by restricting the sample to rural mothers with

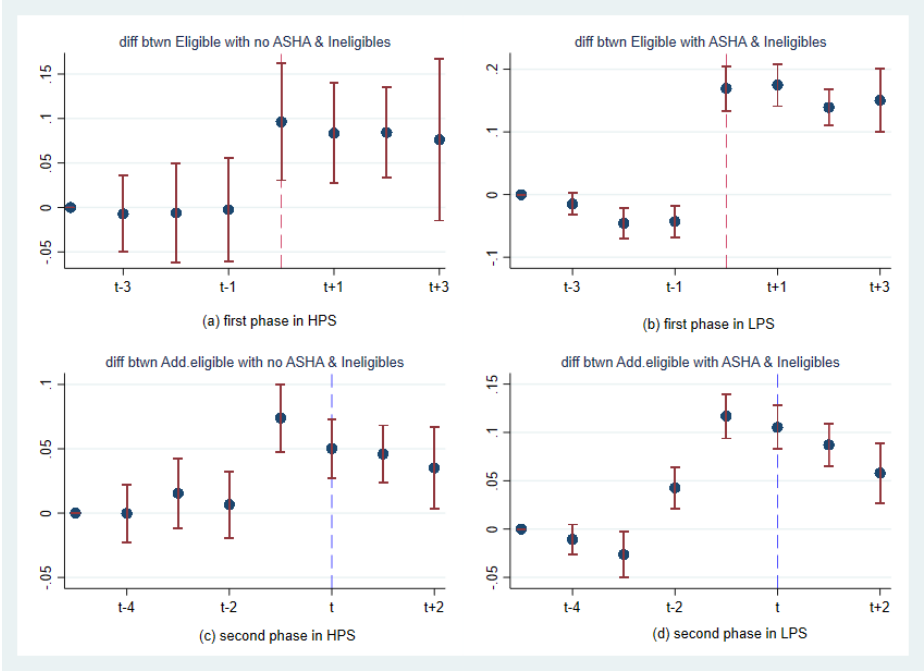


Figure 3: Time-varying difference in antenatal care check-ups between the respective treatment groups and control group

Notes: This figure shows the difference between each treated sub-group and the control group in the probability of receiving atleast 3 ANCs during 2001-2008. The two figures in the upper panel show the intervention onset in t=2005 for the first phase eligibles, and the two figures below show the intervention onset in t=2006 for the second phase eligibles.

less than two live births, similar trend pattern prevails.[28]

Summing up the verification results of the pre-intervention trends of the variables of our interest in Section 6.1, it can be safely concluded that there was clearly some causal effect of the programme on institutional births and breastfeeding practices, and clearly quite large for the first phase eligible mothers in the low-performing states where ASHA was available. Though I cannot confirm that the outcome was similar for the second phase eligible mothers, at the same time one cannot expect a clean presence of parallel pre-trends for the late-eligible groups, because interventions of such immense magnitude are bound to

[28] Note that some data issues could also drive this drop in the uptake. The variable on BCG uptake used here is created from two different variables in the two waves of the survey. DLHS 2, which is until 2004, has information on whether or not the BCG vaccine was given to the child, whereas DLHS 3, which starts with later births, has information on the date of BCG vaccination received. It is difficult to discard any discrepancy arising from the construction of the study variable. Also, it is not easy to compare with findings across other studies because they either vary in their specification of the treatment groups or use of data (e.g.Debnath (2018) avoids sub-categorisation of eligibles unlike me, and uses only one survey wave DLHS 3 for his analysis).

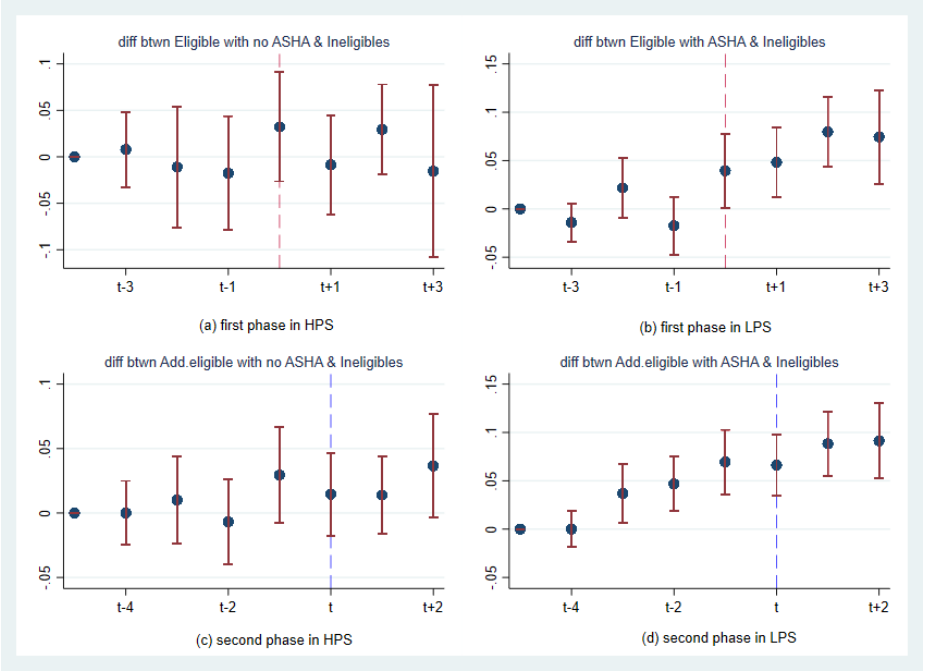


Figure 4: Time-varying difference in breastfeeding practices between the respective treatment groups and control group

Notes: This figure shows the difference between each treated sub-group and the control group in the probability of breastfeeding at birth during 2001-2008. The two figures in the upper panel show the intervention onset in t=2005 for the first phase eligibles, and the two figures below show the intervention onset in t=2006 for the second phase eligibles.

have spillover effects. Keeping this in mind, I argue that the confirmation of parallel pre-trends only for the first phase eligibles would be sufficient to assert that the introduction of the programme had a causal effect on institutional births and early breastfeeding.

However, for ANC and BCG uptakes, the parallel pre-trends verifications do not hold simultaneously even for the two first phase eligible groups. Hence, it is difficult to compare the causality in effects across the LPS and HPS for these outcomes at all.[29]

6.2 Coarsened Exact Matching

As a further robustness check to the empirical results obtained in the previous section, I employ a Coarsened Exact Matching (CEM) approach to the sample. CEM is a monotonic imbalance-reducing matching method implementable on

[29] Note that an introduction of group-specific linear trends is not feasible here. Due to the presence of multiple treatment groups in the model, one of the groups gets omitted due to multicollinearity when group-specific linear trends are used.

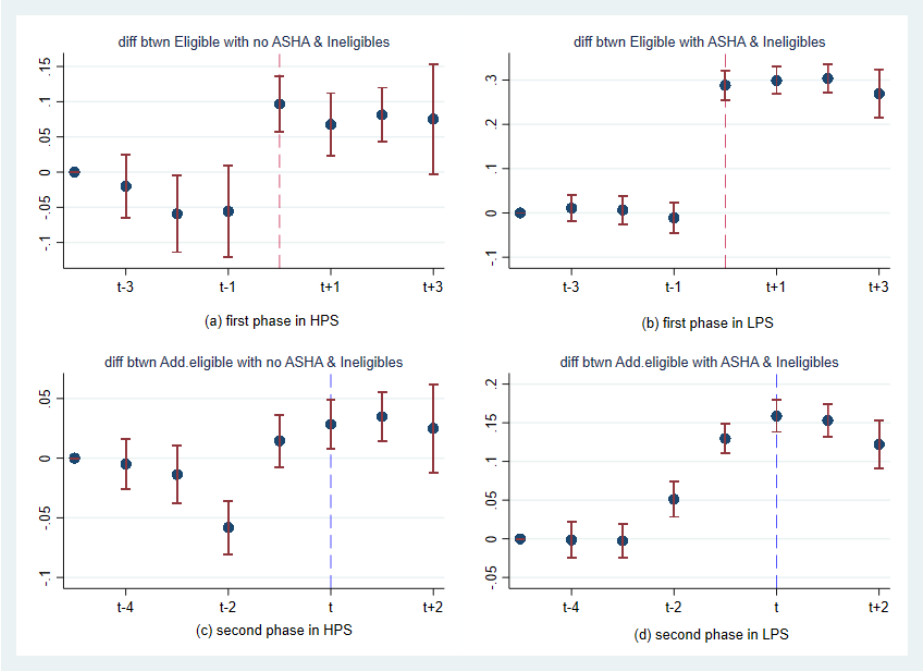


Figure 5: Time-varying difference in BCG vaccination between the respective treatment groups and control group

Notes: This figure shows the difference between each treated sub-group and the control group in the probability of getting the child BCG vaccinated during 2001-2008. The two figures in the upper panel show the intervention onset in t=2005 for the first phase eligibles, and the two figures below show the intervention onset in t=2006 for the second phase eligibles.

observational data and is capable of improving the estimation of causal inference (Iacus et al., 2012). In order to minimise the imbalance between the treatment and control groups in my heterogeneous sample of mothers, I employ CEM based on observable characteristics of the mother. The covariates which I use for this procedure are the number of live births by the mother, her age when she gave birth to her last child, her religion - Hindu or Muslim, social caste and wealth decile of her household.

The global balance between the treated and control units in the data is measured using the $\mathcal{L}_1$ index (Iacus et al., 2011, 2012),

$$\mathcal{L}_1 = \frac{1}{2} \sum_{l_1 \ldots l_k \in H(x)} |f_{l_1} \ldots f_{l_k} - g_{l_1} \ldots g_{l_k}| \tag{3}$$

where $f_{l_1} \ldots f_{l_k}$ and $g_{l_1} \ldots g_{l_k}$ are the relative frequencies of treated and control observations belonging to the cells with coordinates $l_1 \ldots l_k$ in the multivariate cross tabulation (H). Conditioning on the coarsening level of the covariates, the empirical distributions before and after CEM are entirely separated when

$\mathcal{L}_1 = 1$, while if the distributions perfectly overlap $\mathcal{L}_1 = 0$. A good matching performance is achieved if $\mathcal{L}_1$ of the matched population is less than $\mathcal{L}_1$ of the original population.

One issue with using CEM, however, is that it allows for only one treatment and one control group. Under such circumstances, there are two approaches that I could take here: (i) classify those who have been treated at any point with either mother's package or both packages, as *treated*, and those who have never been treated with any package, as the *control* and then *coarsen* over specific covariates;[30] (ii) If there are multiple treatment groups, consider each pair (with the control group) separately and estimate the *average treatment effect on the treated* for each group.

On applying approach (i) in my sample by using the mothers' characteristics mentioned above, the $\mathcal{L}_1$ value of imbalance between the treated and control group reduces from 0.56 to 0.18. In the process of achieving this lesser imbalance, 1464 and 96926 observations are dropped from the original control and treatment groups, respectively. This pruning results in the loss of 30% of the original number of observations.

When the sample becomes more balanced through CEM, the effect on the eligibles in the high-performing state in both phases becomes statistically insignificant. In contrast, the effects on eligibles in the low-performing state of both phases remain of similar size and precision, as in the original analysis. This finding is similar to that found in the main analyses with the homogenised sub-sample of rural mothers (and with less than two births). **Table 7** Column (1) shows that in the case of institutional births, the difference in effect for the *Eligible with ASHA* and *Additional Eligible with ASHA* groups relative to the control group are 7.1 pp. ($p = 0.000$) and 6.8 pp. ($p = 0.000$) respectively. The findings on ANC and BCG uptake also show a similar pattern, i.e. only the eligible mothers in LPS in both phases see any statistically significant difference in effect in the CEM sample (Columns (2) and (4) of Table 7). For the outcome on breastfeeding at birth, only the estimate of the second phase eligible mothers in the LPS remains precise (Column (3) of Table 7).

Moreover, the parallel pre-trends assumption holds for the outcome variables (except for one time-point in case of BCG) for the first phase eligible mothers in both LPS and HPS. Hence, allowing for the leniency that the second phase eligible mothers can face some spillover effects before actually starting with their benefits, these findings on the first phase eligibles give more strength to the causal inference. (See **Figure A3** in **Appendix 1**) It is quite plausible that the evolution of trends in the pre-intervention years become similar across the

[30] One caveat, however, regarding this approach is that it would not always provide at least one "exact match" *per se* in each of the treated sub-group with respect to the control group. To elucidate, since the matching would be done without taking into account the sub-groups of the *treated*, it is likely that there would not be a one-to-one matching for every observation in the control group with a corresponding one in a treated sub-group.

groups as they become more "balanced" through CEM.

Table 7: Effects on the outcome variables after Coarsened Exact Matching

	(1)	(2)	(3)	(4)
Dependent Variable:	Institutional births	ANCs	Breastfeeding	BCG
First phase eligibles				
Eligible without ASHA	0.016	0.017	0.012	0.062***
	(0.024)	(0.028)	(0.025)	(0.025)
Eligible with ASHA	-0.024	-0.119***	-0.283***	-0.049*
	(0.044)	(0.036)	(0.059)	(0.030)
Eligible without ASHA*Post1	0.041	0.035	-0.007	-0.005
	(0.032)	(0.041)	(0.038)	(0.038)
Eligible with ASHA*Post1	0.071***	0.098***	0.020	0.196***
	(0.013)	(0.020)	(0.015)	(0.019)
Second phase eligibles				
Additional Eligible without ASHA	0.151**	-0.017	-0.020	0.034
	(0.064)	(0.069)	(0.065)	(0.055)
Additional Eligible with ASHA	-0.0003	-0.090***	-0.265***	0.021
	(0.044)	(0.036)	(0.061)	(0.028)
Additional Eligible without ASHA*Post2	0.005	0.148	0.069	-0.023
	(0.155)	(0.094)	(0.131)	(0.109)
Additional Eligible with ASHA*Post2	0.068***	0.030***	0.027***	0.053***
	(0.007)	(0.008)	(0.008)	(0.007)
Constant	0.427***	0.716***	0.726***	0.641***
	(0.040)	(0.049)	(0.045)	(0.045)
State FE	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
F-test of equality between interaction coefficients of first phase eligibles				
F-statistic	0.77	1.89	0.44	21.96
p-value	0.381	0.170	0.508	0.000
F-test of equality between interaction coefficients of second phase eligibles				
F-statistic	0.17	1.56	0.11	0.48
p-value	0.684	0.212	0.743	0.489
Baseline mean of Ineligibles	0.250	0.796	0.426	0.878
Observations	227991	179062	215777	227991
R^2	0.119	0.256	0.169	0.123

Notes: **(1)** Sample contains observations 'matched' with CEM only. **(2)** The unit of observation is the mother who had her latest birth between 2001-2008. **(3)** **EligiblewithoutASHA** and **EligiblewithASHA** denote the respective pre-intervention difference in means of the first phase eligible mothers in HPS and LPS with the control group (Ineligibles). **EligiblewithoutASHA*Post1** and **EligiblewithASHA*Post1** denote the respective difference-in-difference effect of the programme on the first phase eligible mothers in HPS and LPS. **Add.EligiblewithoutASHA** and **Add.EligiblewithASHA** denote the respective pre-intervention difference in means of the second phase eligible mothers in HPS and LPS with the control group (Ineligibles).**Add.EligiblewithoutASHA*Post2** and **Add.EligiblewithASHA*Post2** denote the respective difference-in-difference effect of the programme on the second phase eligible mothers in HPS and LPS. **Post1** and **Post2** denote the births taking place after the onset of first guideline (Apr 2005) and the ones after the onset of the second guideline (Oct 2006). **(4)** An F-test of equality between the coefficients **EligiblewithoutASHA*Post1** and **EligiblewithASHA*Post1** (i.e.the first phase eligibles) show that they are however, not significantly different from each other, except for the outcome variable on BCG. A similar F-test of equality between the coefficients **Add.EligiblewithoutASHA*Post2** and **Add.EligiblewithASHA*Post2** (i.e.the second phase eligibles) also show that they are not significantly different from each other. **(5)** Control variables include number of live births the mother has had, maximum schooling years of the mother, her religion, if her family belongs to one of the socially backward classes, the wealth quantile of her household, and some village-specific health infra-structure controls like presence of other child welfare programme in the village, distance to the nearest primary health centre, community health centre, to the nearest district hospital. **(6)** Standard errors clustered at district level within parentheses. **(7)** The unadjusted R^2 values are reported here. **(8)** The mean of the outcome variable in the control group (Ineligibles) in the pre-intervention period is reported. **(9)** ***,**,* implies $p < 0.01$, <0.05, < 0.10 respectively.

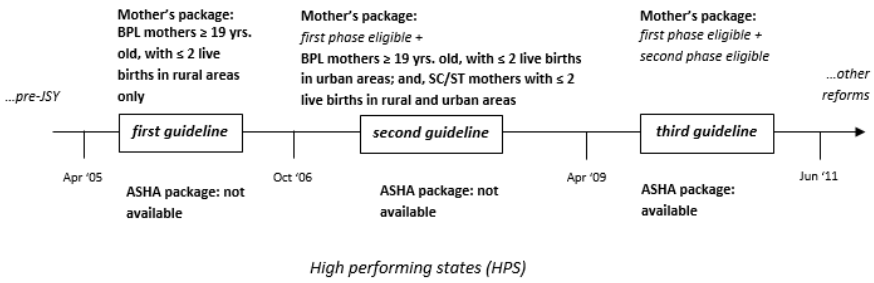


Figure 6: Roll-out of JSY in the HPS

6.3 Issue of differential trend of unobservable characteristics of the LPS and HPS

The regions which had the central focus of the programme were the low-performing states, due to their poor performance on maternal and neonatal outcomes in the pre-intervention period. Therefore, one could argue that the roll-out of the programme, for example, the strategy and speed of roll-out of funding and other facilities, could have been different for the LPS compared to the HPS. That is, if the determinants of the programme implementation are systematically related to underlying trends in the institutional births in the LPS and HPS, one should worry about differences in the characteristics of the two state-types that could potentially lead to meaningless/biased conclusion, as merely a state-fixed effect analysis would not take care of it.

There are a few ways of dealing with this issue. Debnath (2018) deals with it by using districts on the border separating the geographically clustered LPS and HPS, in order to minimise the difference in characteristics. I, on the other hand, utilise the further spread of ASHAs in the HPS in consequence to a third revision to the JSY guideline. Thus, I study the eligible mothers only in the HPS who were exposed to the mother's cash transfer from the beginning of the intervention, and then also received an ASHA's guidance from April 2009 onwards.[31] This analysis consolidates my results in three ways. (i) By only using the HPS sample, it avoids any systematic difference in the underlying trends of institutional births in the LPS and HPS that could have affected the implementation. (ii) It gives a more robust way of eliciting the ASHA's effect through comparison of characteristically similar mothers in the same state-type before and after ASHA. (iii) Fewer eligible sub-groups make it easier for the parallel pre-trends assumption to be validated.

Figure 6 briefly restates the revisions to the JSY guideline in the HPS. According to the first guideline, only the mother's package was introduced to eco-

[31] The information on the initiation of this phase is available from MOHFW (2009).

nomically disadvantaged mothers. These mothers were above the age of 19, had up to two live births and belonged to below-poverty-line families in rural areas. In the second guideline, these eligibility criteria were relaxed to include an identical cohort from the urban areas; besides, mothers with up to two live births belonging to either of the two socially backward classes -*scheduled caste* and *scheduled tribe*, were also given the eligibility status. From April 2009, another revision, that is the third guideline, came into effect. Now, besides the mother's package that was already available to the eligible mothers, the ASHAs were also introduced to guide the eligible mothers. In the following analysis, I will consider the timeline until May 2011 (as afterwards, other health reforms were initiated and bridged with JSY).

I consider the following DiD specification for estimation:

$$Y_{ijt} = \alpha + \beta_1 Eligible_{ij} + \beta_2 AddEligible_{ij} + \beta_3 (Eligible_{ij} * Post1_t) + \beta_4 (AddEligible_{ij} * Post2_t) + \beta_5 (Eligible_{ij} * Post3_t) + \beta_6 (AddEligible_{ij} * Post3_t) + X'_{ij}\delta + \mu_t + \gamma_j + \epsilon_{ijt} \quad (4)$$

Y_{ijt} is the outcome variable of the mother i in some HP state j giving her latest birth in the year t. $Eligible_{ij}$ is the treatment group which takes value 1 for the mother who was eligible for cash assistance in an HPS as per the first guideline. $AddEligible_{ij}$ is another treatment group that takes value 1 for the additional mothers who were not eligible for the cash assistance in the first phase but became so in the second phase. $Post1_t$ captures the time window between April 2005 - March 2009 and takes value 1 for all the births that took place during this time. Essentially, an interaction of $Eligible_{ij}$ with $Post1_t$ should give the treatment effect of only the mother's cash transfer on the mothers who became eligible according to the first guideline. Similarly, $Post2_t$ captures the time window between October 2006 - March 2009 and takes value 1 for all the births that took place then. The interaction of $AddEligible_{ij}$ with $Post2_t$ should give the treatment effect of only the mother's cash transfer on the mothers who became eligible for it only after the second guideline took effect. Finally, $Post3_t$ is a dummy variable for the births from April 2009 onwards, when all eligible mothers received the ASHA's counselling besides cash assistance. From the respective interactions of $Post3_t$ with the two treatment groups, we would get the combined effect of the mother's cash transfer and the ASHA's guidance for each of the groups. Thus, the coefficients of interest here would be β_3,β_4,β_5 and β_6. Comparison between β_5 and β_3 would give an idea of the additional effect of having the ASHA's counselling for the first phase eligible mothers. In the same way, a comparison between β_6 and β_4 would indicate the same for the additional eligible mothers of the second phase. The remaining notations of this equation indicate the same as in Equation (1).

The estimates reported in **Table 8** suggest that the effect of the mother's cash assistance on all outcome variables was more prominent in magnitude for the

first phase eligible mothers than for the second phase eligible mothers (all differences in effects being statistically significant at 1%, except breastfeeding). This bigger *average treatment effect* (ATE) could also be because the first phase eligibles were exposed to the benefits for a more extended period than the second phase eligibles. The results after the third guideline took effect, are, however, more nuanced. The first phase eligible mothers showed a substantially large difference in effect in institutional births and ANCs in the period when both mother's cash transfer and ASHA were available. On the contrary, the ATE on the second phase eligible mothers remained more or less similar in both *Post*2 and *Post*3, thus implying that an additional presence of ASHA did not provide any value-added in affecting their institutional birth rates and ANCs (see Columns (1) and (2)). In the case of BCG vaccination, the first phase mothers still experienced a statistically significant positive effect. However, the effect on the second phase mothers was relatively small and also imprecise (in Column (4)). On early breastfeeding, I find no precise effect of the mother's incentive on the eligible mothers of both phases (in Column (3)). This result complements the earlier findings on early breastfeeding in the main analysis, where too, I see that the eligible mothers in HPS did not benefit significantly from the mother's package in early breastfeeding practices. Finally, with the introduction of ASHAs in the HPS, there is no change in effect either.

When it comes to the robustness of the results in this section, the first phase eligible mothers in the HPS (according to Equation (4)) show clear parallel pre-trends in all the outcome variables in the baseline years, with respect to the ineligible mothers. The second phase eligible mothers, however, experience some spillover effects already before attaining eligibility in late-2006 (See **Figure A4** in **Appendix 1**).

To sum up, the takeaway from this analysis is that with the ASHA by their side, the HPS mothers eligible according to the first guideline experienced a bigger increase in institutional births and ANC take-ups. However, the effect on breastfeeding and BCG vaccination subsided with the introduction of ASHAs (the finding of breastfeeding being only suggestive). On the other hand, the mothers eligible according to the second guideline did not experience any additional positive effect with the introduction of ASHAs. Note that the first phase mothers in the HPS are those below-poverty-line, with less than two live births, and living in rural areas. As discussed earlier in the paper, these mothers are a vulnerable group, and as per the main findings of Section 5, they benefitted more from the ASHA's guidance. Similarly, now in the analysis with only the HPS sample, this particular group improved, at least in institutional births and ANC take-ups, after ASHAs were introduced.

Now at this point, some caveats should be borne in mind. Before comparing the original findings of Section 5 with the findings here, it is important to stress

that this analysis with only the HPS serves as an extension rather than a replication of the previous findings. It is because, when we look at the analysis with the high-performing states only, the empirical model changes by a large extent, with the number of treatment categories being reduced to *Eligible* and *Additional Eligible* and the control group of *Ineligible*. Essentially, the $Eligible_{ij}$ of Equation (4) is equivalent to $EligiblewithoutASHA_{ij}$ of Equation (1) and $AddEligible_{ij}$ of Equation (4) is equivalent to $AddEligiblewithoutASHA_{ij}$ of Equation (1). Moreover, since $Post1_t$ and $Post2_t$ vary in time-frame in the two equations, in addition to the presence of other variables, one cannot expect effects of similar magnitude in the two corresponding analyses.

On a final note, the mixed findings on the ASHA's effect in the analysis with HPS only, also makes one curious about how the ASHA's incentive was paid. Increased effects in the pre-birth outcomes (institutional births and ANC checkups) and reduced effects in the post-birth outcome (BCG vaccinations) raise questions on certain possibilities. Did the ASHA's full payment come directly during the birth with no direct incentive for postnatal care? Or, was the first payment (full/instalment) so delayed that the ASHA felt discouraged to carry on with her postnatal services? Unfortunately, the programme guidelines do not clearly state how the ASHA's incentive was disbursed after her introduction in HPS. As a result, these questions remain beyond the scope of investigation.

7 DISCUSSION AND CONCLUSION

This study investigates a CCT programme at the national level in India, which had a unique feature of incentivising both the demand end and the supply end of maternal and child healthcare in the country. The shocking rates of maternal mortality and neonatal mortality in the country expedited the need for an intervention like this. The programme allowed for one-time cash assistance to mothers if they chose to give birth at public healthcare facilities under the supervision of trained personnel. Also, a performance-based pay was to be given to trained health workers, called ASHAs, appointed by the programme if they helped the expecting mothers in the community during pregnancy, birth and postnatal phase.

Though it was a nationwide programme, there was heterogeneity in terms of eligibility of the mother entitled to the cash benefit, and that depended on her social and economic status, caste, residence and number of births. At the same time, there was a heterogeneous implementation of the health-workers across states, i.e. the low-performing states and the high-performing states. I exploit these two variations in the programme implementation and try to under-

Table 8: Effects on the outcome variables in HPS only

	(1)	(2)	(3)	(4)
Dependent variable:	Institutional births	ANCs	Breastfeeding	BCG
First phase eligibles				
Eligible	-0.103***	-0.057***	0.008	-0.039***
	(0.010)	(0.017)	(0.014)	(0.015)
Eligible*Post1	0.098***	0.066***	0.023	0.119***
	(0.013)	(0.020)	(0.018)	(0.017)
Eligible*Post3	0.218***	0.179***	0.021	0.098***
	(0.019)	(0.019)	(0.021)	(0.019)
Second phase eligibles				
Additional Eligible	0.024***	-0.032***	0.009	0.012**
	(0.007)	(0.006)	(0.007)	(0.005)
Additional Eligible*Post2	0.054***	0.053***	0.015	0.036***
	(0.008)	(0.009)	(0.009)	(0.006)
Additional Eligible*Post3	0.050***	0.052***	-0.010	0.003
	(0.011)	(0.007)	(0.012)	(0.007)
Constant	0.411***	0.368***	0.355***	0.739***
	(0.040)	(0.030)	(0.039)	(0.025)
State FE	Yes	Yes	Yes	Yes
Birth year FE	Yes	Yes	Yes	Yes
Controls	Yes	Yes	Yes	Yes
F-test of equality between the coefficients of Eligible*Post1 & Eligible*Post3				
F-statistic	35.87	89.09	0.01	3.99
p-value	0.000	0.000	0.934	0.045
F-test of equality between the coefficients of Add.Eligible*Post2 & Add.Eligible*Post3				
F-statistic	0.08	0.05	5.27	18.98
p-value	0.780	0.828	0.023	0.000
Baseline mean of Ineligibles	0.284	0.582	0.479	0.890
Observations	146085	128227	140143	146085
R^2	0.068	0.447	0.111	0.067

Notes: (1) Sample contains observations from HPS only **(2)** The unit of observation is the mother in HPS, who had her latest birth between 2001- May 2011. **(3) Eligible** denotes the pre-intervention difference in means of the first phase eligible mothers with the control group (Ineligibles). **Additional Eligible** denotes the pre-intervention difference in means of the second phase eligible mothers with the control group (Ineligibles). **Eligible*Post1** and **Additional Eligible*Post2** denote the respective difference-in-difference effect of the mother's incentive only on the first phase and second phase eligible mothers respectively. **Eligible*Post3** and **Additional Eligible*Post3** denote the respective difference-in-difference effect of the mother's incentive and ASHA's presence on the first phase and second phase eligible mothers respectively. **Post1** denotes the births taking place between Apr 2005-Mar 2009, **Post2** denotes the births taking place between Oct 2006-Mar 2009, and **Post3** denotes the births taking place between Apr 2009-May 2011. **(4)** An F-test of equality between the coefficients **Eligible*Post1** and **Eligible*Post3** (i.e.the DiD effect on first phase eligibles with one package and with two packages) show that they are significantly different from each other for all variables except early breastfeeding. A similar F-test of equality between the coefficients **Add.Eligible*Post2** and **Add.Eligible*Post3** (i.e.the DiD effect on second phase eligibles with one package and with two packages) show that they are significantly different from each other only for early breastfeeding and BCG. **(5)** Control variables include number of live births the mother has had, maximum schooling years of the mother, her religion, if her family belongs to one of the socially backward classes, the wealth quantile of her household, and some village-specific health infra-structure controls like presence of other child welfare programme in the village, distance to the nearest primary health centre, community health centre, to the nearest district hospital. **(6)** Standard errors clustered at district level within parentheses. **(7)** The unadjusted R^2 values are reported here. **(8)** The mean of the outcome variable in the control group (Ineligibles) in the pre-intervention period is reported. **(9)** ***,**,* implies p < 0.01, <0.05, < 0.10 respectively.

stand through a difference-in-difference identification strategy as to whether the mother's incentive or the ASHA's incentive plays a more significant role in several outcomes related to maternal and neonatal health. Though I do not check the direct effects on maternal and neonatal mortality rates (which led to the in-

troduction of the programme in the first place), I do check the direct effects on the factors that in turn affect those mortality rates, e.g., birth at public health facilities, the required number of ANC check-ups, early breastfeeding practices and BCG vaccination. More importantly, these four outcomes of interest are also the ones on which the cash transfers to the mother or the ASHA were conditional. Therefore, in a short-run analysis, these outcomes should experience the most immediate effects, if any. Nevertheless, I agree that it is equally important to understand the bigger picture as well, that is, whether there was a substantial change in the mortality rates eventually.

By taking into account all the intricacies in the implementation of the programme in its initial years, I investigate the differential effects of the mother's eligibility for the cash benefits and the ASHA's presence on maternal and child health outcomes.

The most important result of this study shows that the mothers eligible for both the mother's incentive and the ASHA's help, experienced a bigger positive effect in institutional births and early breastfeeding than the mothers with mother's incentive only. This result is statistically significant. While this could qualify as causal evidence for the first phase eligible mothers of the programme (through the presence of clean parallel pre-trends in institutional births and breastfeeding practices), it is hard to confirm that the effect on the eligibles of the latter phase was also causal, due to possible spillover effects of a programme as big as this. I further find evidence that eligible mothers receiving cash benefits as well as the ASHA's guidance showed bigger improvement in receiving the recommended three antenatal care check-ups and also in having their newborns vaccinated with BCG. In contrast, the mothers getting just the cash assistance showed relatively small or no effect. Although the findings on the ANC utilisation and BCG vaccination could not be confirmed as causal evidence with the original sample, a CEM-refined sample validates the evidence on ANC as causal.

To offer a better conclusion on the channel of ASHA's effect, I restrict the sample to only rural mothers (and with up to two births). This analysis with more homogenised groups minimises the possible bias in inference which could arise from a differential selection. At the same time, it also makes the groups of first phase eligible mothers in LPS and HPS demographically identical. While it is a plausible argument that comparing the LPS and HPS mothers to pin down the ASHA's effect could be flawed due to systematic differences, homogenising the two groups across their demographic indicators could, however, offer some strength to the comparison. Note that when I analyse only the sub-sample of rural mothers, the first phase eligible mothers in both LPS and HPS are above 19 years old, with less than two live births, belonging to below-poverty-line households in rural areas. Compared to the control group of ineligible mothers (who are a mix of either above-poverty-line, or below-poverty-line but less than

19, or not belonging to socially disadvantaged castes, or with more than two births, in rural HPS), the first phase eligible mothers in HPS see a small or no effect with only the mother's cash assistance. In contrast, the first phase eligible mothers in the LPS see a large effect with the cash assistance and the ASHA's guidance. Given that the eligible mothers of the first phase in the rural areas are the most vulnerable ones in the LPS and HPS alike, they are likely to benefit in the same way from the cash assistance. However, the findings of the HPS cohort already suggest that only a cash incentive to the mother does not suffice. A similar outcome persists when I further constrict the sample to rural mothers with up to two live births. Therefore, one can conclude that at least for the rural sub-sample (with less than two births) the programme's effect is coming though the ASHA's performance.

In a similar exercise, where I use a sample homogenised by coarsened exact matching, the treatment effect on the eligible mothers in the LPS remains robust. In contrast, the effect on the eligible mothers in HPS becomes imprecise. This finding also does not reject the possibility that only a cash incentive to the mother does not improve the health outcomes. Thus the results of the matched sample, further consolidate that the effect on LPS mothers is through the ASHA's channel.

Another scrutiny with only the HPS sample (who were introduced to ASHAs from April 2009 onwards), however, shows that the added effect of the ASHAs can be seen only in case of institutional births and ANC take-ups by the first phase eligible mothers in the HPS. This finding reconciles with the previous findings on rural mothers in general. The fact that in the HPS analysis, the first phase eligible mothers (i.e. the rural mothers above 19 with up to two births and belonging to below-poverty-line households) showed more improvement in these two outcomes, after the introduction of ASHAs post-2008, mirrors the conclusion from the other analyses on ASHA's superior effect on such mothers.

Finally, concerning the nuances in the interpretation of the results, one needs to use caution when deducing that the better effect can only be offered through the presence of the ASHAs, and hence incentivising the supply-side (here, the ASHA) would, in general, offer a better solution. In this paper, I compare the eligible mothers in the low-performing and high-performing states in the most characteristically homogenised way possible, in order to confidently deduce that the additional effect found in the LPS must come through the presence of ASHA. However, this study's set-up allows for the possibility of homogenisation only for the rural sample (with up to two births) of the first phase mothers across the LPS and HPS.[32] Therefore, one can pin down the sole effect of ASHA in

[32] Trying to minimise the groups further with more specific characteristics (e.g. only below-poverty-line or only below 19 years) often results in the omission of some treatment groups due to multicollinearity in the analysis, given that some of the groups are already formed with precise specifications.

so far as these mothers are concerned. Now, no doubt, these mothers who are below-poverty-line and above 19 years, rural and with less than two births, were the main focus group of the programme (i.e. those who became eligible at the very first phase), and this analysis is able to point out that the ASHA's channel is a better way of improving their outcome. (A similar result, at least for some outcomes, in the analysis with only the HPS sample, further consolidates the conclusion on these vulnerable mothers.) However, some caution is necessary while discussing the effects on mothers beyond this vulnerable group. For the rest of the groups, the analysis could still suffer from potential selection bias due to heterogeneity in the composition of mothers and the different state-type; hence, pinning down the overall better outcome of the LPS mothers only through the ASHA's channel may be faulty. It is still possible that the LPS being historically the worse performer and thus the mothers there being the "most needy" of the intervention, simply could react strongly to both the cash transfer and the ASHA's guidance, and not just the latter.

Keeping the above in mind, it is, however, safe to conclude here that only cash benefits to a mother for giving birth at public health facilities under skilled supervision may not be sufficient to motivate her (or, to overcome the costs). A mother needs frequent counselling and guidance pre and post-birth by the health worker in the community, to attain all-round health improvement of herself and the newborn.

On a final note, from the public policy perspective, it is vital to assess the cost-effectiveness of the packages. We must remember that employing the ASHA in addition to the mother's package comes with additional costs in the public health sector. Therefore, I compare the first phase eligible mothers in the HPS and the first phase eligible mothers in the rural LPS to determine how cost-effective it was.[33] I find that for 1 pp. increase in institutional birth rate compared to pre-intervention, 45926.99 INR (the mother's package cost times the number of births) was spent in HPS. In contrast, that corresponding cost was 2483000 INR in the rural LPS with the dual package. The latter expenditure is about four times that spent in the former, with the first phase mothers. However, then again, the number of eligible mothers in the rural LPS is about five times of that in HPS, so the dual package in rural LPS was more cost-effective even if we are looking at only institutional birth rates. Needless to say, the additional gains of ASHA is also present in the antenatal and postnatal outcomes and in general, overall healthy lifestyle.

[33] I compare the eligibles only from rural LPS because the eligibles from the HPS were only from the rural areas in the first phase. Therefore, I intend to keep the comparison groups as homogeneous as possible.

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1 APPENDIX

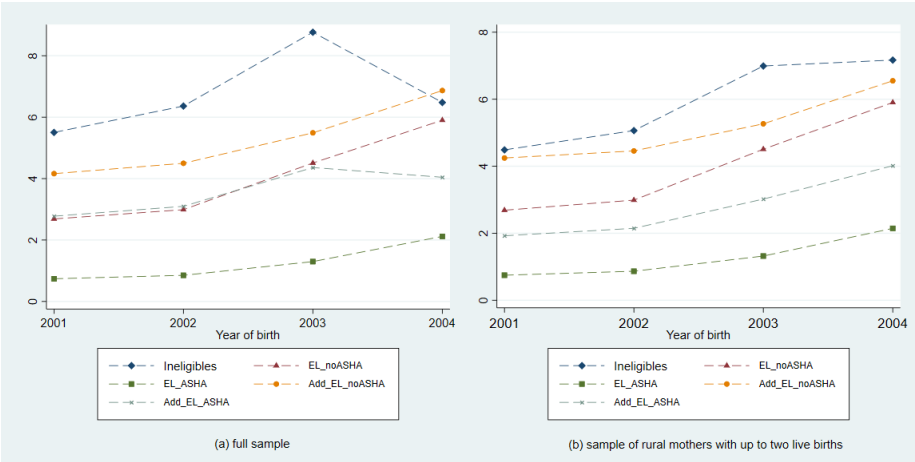

Figure A1: Raw trends in Antenatal care in the pre-intervention years

Notes: The first plot (a) in the graph is for the full sample of mothers who had their most recent birth in 2001-2004. The second plot (b) is for the rural mothers with less than two live births, who had their recent birth in 2001-2004.

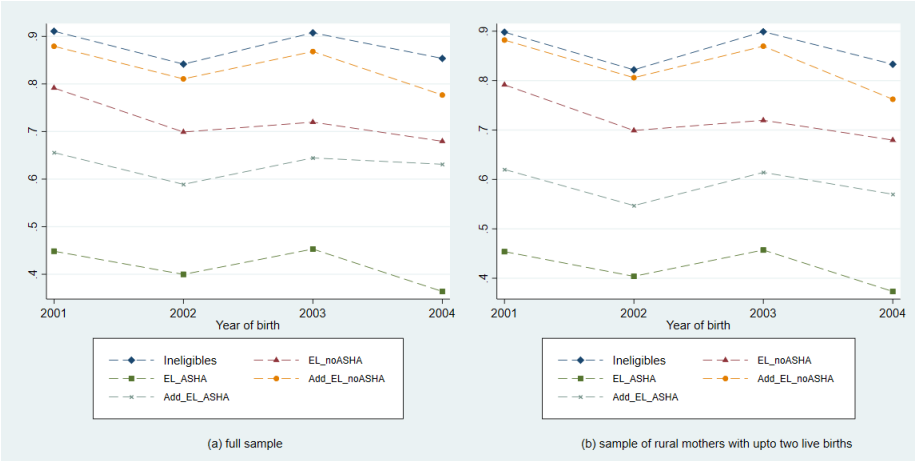

Figure A2: Raw trends in BCG vaccination in the pre-intervention years

Notes: The first plot (a) in the graph is for the full sample of mothers who had their most recent birth in 2001-2004. The second plot (b) is for the rural mothers with less than two live births, who had their recent birth in 2001-2004.

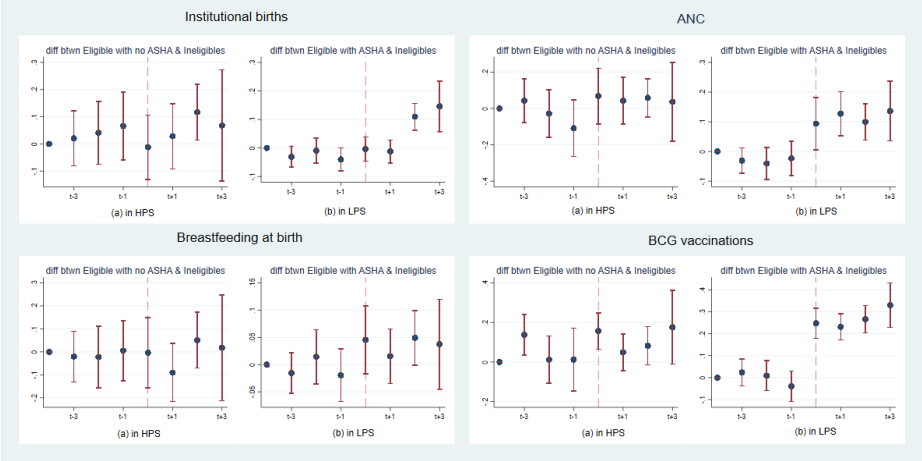

Figure A3: Time-varying difference in the outcome variables between the first-phase treatment groups and control group - with CEM sample

Notes: The sample used for this graph contains observations 'matched' with CEM only. This graph summarises for each outcome variable the difference between the first phase eligible mothers in HPS (without ASHA) and the control group in (a), and the difference between the first phase eligible mothers in LPS (with ASHA) and the control group in (b). The timeline spans from 2001-2008 and the onset of the intervention is shown at t=2005. The parallel pre-trends assumption holds for the first phase eligible mothers both in LPS and HPS in case of all outcome variables (except for one earlier time point in HPS for BCG)

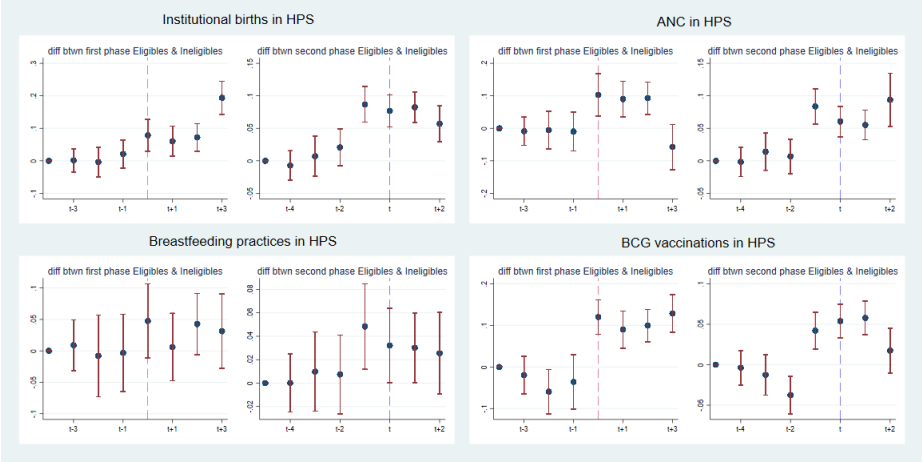

Figure A4: Time-varying difference in the outcomes variables between the treatment groups and control group in HPS only

Notes: This graph summarises for each outcome variable the difference between the first phase eligible mothers with the control group, and the difference between the second phase eligible mothers with the control group in HPS. The timeline spans from 2001-2008 and the onset of the intervention is at t=2005 for first phase and t=2006 for second phase. The parallel pre-trends assumption holds for the first phase eligible mothers in case of all outcome variables.

2 APPENDIX

Figure B1: Categorisation of the low and high-performing states in India according to JSY programme

Notes: (a) This is a representation of the low and high-performing states according to the JSY programme. (b) The North-Eastern States (NES) and the Union Territories (UTs) are left out of the categorisation as they are not included in this study. (c) The political map of India is according to Census Data 2001.

Figure B2: Distribution of institutional birth rate across states in the time period 2001-2004

Notes: (a) This is a dot density plot of average institutional birth rate across the states between 2001-2004. Each dot in the graph implies 1 institutional birth per 1000 births. (b) The North-Eastern States (NES) and the Union Territories (UTs) are left out of the categorisation as they are not included in this study. (c) The political map of India is according to Census Data 2001.

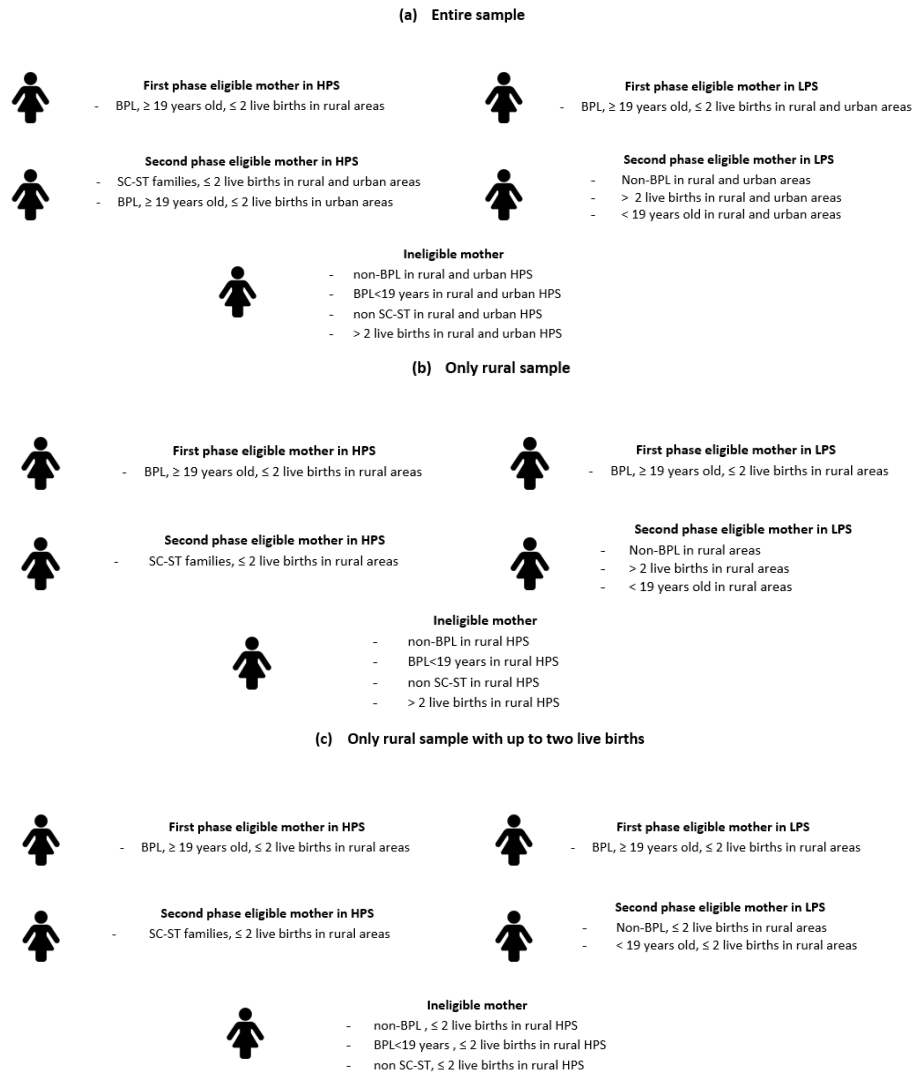

Figure B3: Composition of the treatment groups

Notes: This is a representation of each group's composition used in the empirical analysis of Section 4.2 and in the results in Section 5. Figure (a) describes the groups when the entire sample is used, Figure (b) describes the groups when only the rural sample is used, and Figure (c) describes the corresponding groups for rural mothers with up to two live births. Note that under all cases, the eligible groups in LPS had further guidance from ASHA while the eligible groups in HPS did not.

	Low Performing States		High Performing States	
First set of guidelines: April 2005- October 2006				
Eligibility of mothers	- pregnant women (≥19 y.o), belonging to BPL families and choosing to deliver in public health facilities; - Upto 2 live births - Both in rural and urban areas		- pregnant women (≥19 y.o), belonging to BPL families and choosing to deliver in public health facilities; - Upto 2 live births - Only in rural areas	
ASHAs	Available		Not available	
Scale of assistance	*Rural*	*Urban*	*Rural*	*Urban*
	Mother's package: 700 INR	Mother's package: 600 INR	Mother's package:700 INR	Mother's package:N/A
	ASHA's package: 600 INR	ASHA's package: 200 INR	ASHA's package:N/A	ASHA's package:N/A
Second set of guidelines: October 2006- end of 2008				
Eligibility of mothers	- All pregnant women choosing to deliver in public health facilities; - No limit on no.of births - In rural and urban areas		- Only BPL women (≥19 y.o) and SC/ST women choosing to deliver in public health facilities; - Upto 2 live births; - In rural and urban areas	
ASHAs	Available		Not available	
Scale of assistance	*Rural*	*Urban*	*Rural*	*Urban*
	Mother's package: 1400 INR	Mother's package: 1000 INR	Mother's package:700 INR	Mother's package:600 INR
	ASHA's package: 600 INR	ASHA's package: 200 INR	ASHA's package:N/A	ASHA's package:N/A

Figure B4: JSY programme details - the first two guidelines

Notes: This is a detailed representation of the eligibilities of the new mothers for the mother's package and the ASHA's package according to the first two guidelines.

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